Anti-poverty Programs Can Reduce Violence: India's Rural Employment Guarantee and Maoist Conflict*

Aditya Dasgupta† Kishore Gawande‡ Devesh Kapur§

September 13, 2014

Abstract

We estimate the effects of the National Rural Employment Guarantee Scheme (NREGS), one of the world's largest anti-poverty programs, on the Maoist conflict in India. Difference in differences analyses, based on the phased roll-out of NREGS across districts between 2006 and 2008 and a new panel dataset on Maoist conflict violence based on local language press sources, show that NREGS adoption caused a roughly 80% reduction in violent incidents and deaths. This effect was not driven by pre-adoption trends and emerged after three quarter-years of program adoption. We provide evidence for an opportunity cost channel by showing that NREGS' violence reducing effects impacted all targets of violence and were larger in districts experiencing negative rainfall shocks. The results provide new evidence that large-scale anti-poverty programs represent an important policy tool for mitigating violent civil conflict.

*All authors contributed equally. For valuable feedback we thank seminar participants at Harvard and Yale and at the Workshop on India's Maoist Insurgency at Princeton University. All errors are our own.

†PhD Candidate, Department of Government, Harvard University. Email: dasgupta@fas.harvard.edu

‡Century Club Professor in Business, Government and Society, University of Texas, Austin. Email: kishore.gawande@mccombs.utexas.edu

§Associate Professor of Political Science and Director, Center for the Advanced Study of India, University of Pennsylvania. Email: dkapur@sas.upenn.edu

1 Introduction

More than half of all nations have experienced violent civil conflict since 1960 (Blattman and Miguel, 2010). In the existing literature, poverty is arguably the dominant explanation for civil conflict. Theoretical work suggests that poverty may contribute to civil conflict because of the low economic opportunity costs of participating in conflict or by generating grievances among the poor (Grossman, 1991; Collier and Hoeffler, 2004; Dal Bó and Dal Bó, 2011). Empirically, cross-national analyses have found that a lower per capita income level is among the best predictors of civil conflict (Collier and Hoeffler, 1998; Fearon and Laitin, 2003). A large literature utilizing rainfall shocks as a source of exogenous variation in agricultural productivity shows that negative productivity and income shocks contribute to the intensification of conflict (Miguel, Satyanath and Sergenti, 2004).

Given the extensive evidence of a poverty-violence relationship, it is widely believed that anti-poverty programs represent a viable policy intervention to reduce violence (Collier et al., 2003). Yet empirical evidence of anti-poverty programs that have succeeded in mitigating violent civil conflict by improving economic conditions is surprisingly rare. Crost et al. (Forthcoming) find that the KALAHI-CIDSS, a participatory local public goods program in the Philippines, *increased* violence in the early stages of program implementation due to rebel attempts to sabotage the program. Berman et al. (2011) provide evidence that CERP, a US military funded public services program in Iraq, reduced insurgent violence. However, they argue that this reduction was due not directly to improved economic conditions or to changes in opportunity costs; rather, they argue that conditional access to development programs incentivizes civilians to inform against rebels and therefore contributes to the success of counter-insurgency operations. Several cross-national analyses examine the impact of foreign aid upon civil conflict, finding surprisingly that foreign aid often increases conflict (Nielsen et al., 2011; Besley and Persson, 2011; Nunn and Qian, Forthcoming).

In contrast to the findings of the existing literature, we provide evidence that large-scale anti-poverty programs *can* mitigate violent civil conflict by improving economic conditions and increasing the economic opportunity costs of participation in insurgency. We estimate the impact of the National Rural Employment Guarantee Scheme (NREGS) on the Maoist conflict in India. Enacted in 2005, NREGS guarantees every rural household in India up to 100 days of local public works employment annually, making it one of the largest anti-poverty programs in the world. The Maoist insurgency, concentrated in the poorest districts of India's central and eastern 'red belt' states, draws support from and claims to represent the interests of the rural poor. The Maoist insurgency seeks to violently establish a zone of control outside of the Indian state and in 2006 was termed by India's prime minister as the country's "gravest internal security challenge". According to our data, in the six red belt states of India between 1999 and 2009 the conflict between government security forces and the Maoist militia resulted in at least 7,900 deaths and the displacement and disruption of the lives of many more.

There are several reasons to believe that the impact of NREGS may have differed from that of previously studied development programs. Unlike the CERP and KALAHI-CIDDS programs in Iraq and the Philippines (Berman, Shapiro and Felter, 2011; Crost, Felter and Johnston, Forthcoming), respectively, NREGS has had a large impact on rural labor markets, because the program provides employment to households (as opposed to public goods) and because of its much greater scale. In 2009, the Indian government spent over $8 billion on the program and national sample survey data show that 24 percent of rural household participated in NREGS, with each participating household receiving on average 37 person-days of employment. NREGS therefore provides a better setting for testing whether anti-poverty programs can reduce violence by increasing the economic opportunity costs of participation in insurgency. NREGS is also a domestically funded national anti-poverty program. The program does not possess the budget constraint-relaxing and transitory characteristics of foreign aid, which have been found to exacerbate conflict (Nielsen et al., 2011; Collier and Hoeffler, 2007).

Our first contribution is to assemble a new district-level panel dataset on Maoist conflict violence compiled from multiple local language press sources, enabling us to study the primarily rural conflict with unprecedented accuracy. The dataset set covers 144 districts between 1999 and 2009 across the six central and eastern states– Andhra Pradesh, Bihar, Chhattisgarh, Jharkhand, Orissa, and West Bengal– in which over 90% of Maoist conflict deaths occur. The dataset geo-codes attacks and deaths to districts on a monthly basis. We use this data to measure deaths and violent incidents associated with the Maoist conflict, our dependent variable. This dataset measures a greater number of attacks and deaths, with much greater accuracy and over a longer time span, than do existing datasets based upon English language news reports.

Our second contribution it to identify the causal effect of NREGS adoption on Maoist conflict violence. To estimate the effects of NREGS, we take a difference in differences identification strategy based upon the roll-out of NREGS across districts in three phases between 2006 and 2008. Such an empirical strategy permits a comparison of changes in violence outcomes in districts receiving NREGS versus control districts, while controlling for district and year fixed effects. The inclusion of district fixed effects absorbs any time invariant omitted variables and means that we identify the effects of NREGS utilizing within-district variation over time. To account for possible non-parallel trends across districts arising from the assignment of poorer districts to earlier phases of NREGS implementation, we control for the 'backwardness index' score used to assign districts to NREGS phases in interaction with time dummy variables. We provide evidence that controlling for the backwardness index score achieves balance on major pre-treatment covariates thought to be related to the Maoist conflict. We also provide evidence that our results are not driven by non-parallel pre-adoption trends in violence.

Poisson regression analyses show that NREGS caused a large long-run reduction in violence.[1]

[1] We utilize Poisson quasi-maximum likelihood regression models because they are appropriate for our violence outcome data, which are count variables. Poisson QML models are also robust to the inclusion of unit fixed effects and to arbitrary distributional assumptions (Wooldridge, 1999), unlike other count models. We also report parallel OLS regression results, which are similar, in the appendix.

The estimates suggest that after two years of NREGS adoption the rate of violent incidents and deaths per district-year fell by roughly 80%, an extremely large violence reducing effect. The estimates are consistent across a range of specifications, including, in addition to the control variables described above, state-year fixed effects and lagged dependent variables. Additional analyses of the timing of the effect show that NREGS's violence reducing effect was not driven by a pre-adoption trend and emerged after three quarter years of NREGS adoption.

Our third contribution is to test for the channel for these violence reducing effects. We provide evidence that NREGS mitigated violence through a labor market and opportunity cost channel. First, we show that NREGS adoption reduced violence against all targets: government forces, civilians, and Maoists. This is consistent with an opportunity cost channel, in which the program dis-incentivizes participation in the insurgency and thereby reduces the intensity of both Maoist attacks and government counter-insurgency operations. Secondly, we provide direct evidence for an opportunity cost channel by exploiting rainfall shocks as a source of exogenous variation in agricultural productivity and labor demand. We show that NREGS's violence reducing effects were larger in districts experiencing a negative rainfall shock; using program monitoring data, we provide evidence that these larger effects were connected to greater NREGS employment provision during these periods.

The paper contributes to the large literature, reviewed by Miguel and Blattman (2010), on the economics of violent civil conflict. It also contributes to the nascent literature on the impact of development programs on conflict, though it differs from Crost et al. (Forthcoming) in finding that anti-poverty programs *can* reduce violence and from Berman et al. (2011) in finding evidence for a labor market and opportunity cost channel for the effect. The findings are consistent with a recent field experiment which finds that better employment opportunities resulting from a job training program reduced conflict participation in Liberia (Blattman and Annan, 2014). We show that the direct provision of public employment via a large-scale anti-poverty program reduces violence as well.

The remainder of the paper is structured as follows. We first provide background on the Maoist conflict and on NREGS. We then develop the theoretical argument about the impact of NREGS on the Maoist conflict. We then describe the data and empirical strategy before reporting the results. The final section concludes.

2 Background

The Maoist Conflict

The stated goal of India's Maoist insurgency is to violently establish a zone of control outside of the Indian state. The movement subscribes to a Maoist ideology and advocates pro-rural poor agenda, including land redistribution and the prevention of displacement of tribal populations by mining projects. The Maoist insurgency can be traced historically to the "Naxalite" movement, which originated in 1967 as an anti-landlord peasant uprising in Naxalbari, a village in West Bengal (Banerjee, 1980). Though the Naxalite movement in West Bengal was violently repressed, the movement survived over the next three decades in the form of several splinter groups. The conflict between the Maoist insurgency and the Indian government resurged in the 2000s, in particular after 2004, when the two major Maoist movement factions joined forces to form the Communist Party of India (Maoist).

Organizationally, the Maoist insurgency consists of a military and a political wing, with a decentralized leadership structure. Though details are murky, the Maoist militia is estimated to possess several thousand armed fighters, with considerable variation in strength across districts. In areas it controls, the Maoist movement provides social services, such as healthcare, and also enforces its own regulations, for instance alcohol prohibitions and a ban on participating in elections (for overviews see Chakravarti (2009) and Pandita (2011)).

In 2006, Indian prime minister Manmohan Singh termed the insurgency the "country's gravest internal security challenge" and the central government has aggressively sought to crack down on the insurgency through the deployment of central reserve police forces (CRPF), trained specifically to fight against the Maoist insurgency. States have also pursued their own strategies to crack down on the insurgency. For instance, Andhra Pradesh has created a counter-terrorism force, the Greyhounds, specifically trained for jungle warfare. More controversially, the state of Chhattisgarh has provided support and training to the *Salwa Judum*, a vigilante civilian militia that has been accused of human rights violations and atrocities.

The conflict between the government and Maoist insurgency has resulted in a large number of civilian, Maoist, and government security force deaths. The Maoist insurgency has inflicted large casualties on government security forces, especially via planned ambushes. Security forces have inflicted large numbers of casualties on the Maoist insurgency via heavily armed counter-insurgency operations and incursions. Civilians are often victims of collateral damage arising from “encounters" between government and Maoist forces.

Figure 1 displays total violent incidents and deaths associated with the Maoist conflict in the six red belt states of India – Andhra Pradesh, Bihar, Chhattisgarh, Jharkhand, Orissa, and West Bengal – between 1999 and 2009. Notably, violence spikes following the unification of the CPI (Maoist) in 2004, but appears to decline between 2006 and 2008, the time period during which NREGS was rolled out across districts.

[Figure 1 About Here]

A growing body of research analyzes the determinants of Maoist conflict, finding that poverty and socio-economic marginalization are major determinants of the strength of the insurgency. Borooah (2008) finds that Maoist conflict violence increases across districts in the poverty rate and falls in the literacy rate. Hoelscher, Miklian and Vadlamannati (2012) find that conflict increases with forest cover, prevalence of conflict in neighboring districts, and the population share of members of scheduled castes and tribes. Gomes (2012) finds a strong effect of land inequality on Maoist violence. Vanden Eynde (2011) examines the strategic choices of targets and the intensity of violence committed by Maoist insurgents and finds a reduced form effect of negative rainfall shocks on conflict. Gawande, Kapur and Satyanath (2012) show that negative rainfall shocks increase Maoist conflict violence via effects on the livelihoods of forest-based tribal populations.

The findings of the quantitative literature are consistent with ethnographic evidence that the insurgency recruits and draws support from the rural poor, especially India's marginalized tribal population (Bhatia, 2005; Harriss, 2010; Guha, 2007). The Indian home ministry itself believes the Maoist insurgency to originate in poverty and socioeconomic marginalization, reporting in its 2005-06 annual report that the insurgency "...is not merely a law and order problem but has deep socioeconomic dimensions" and in its 2010-11 annual report that "Left Wing Extremists operate in the vacuum created by functional inadequacies of field level governance structures, espouse local demands and take advantage of prevalent dissatisfaction and feelings of perceived neglect and injustice among the under privileged and remote segments of population" (Government of India, 2005, 2011).

NREGS

Enacted in 2005, NREGS guarantees each rural household up to 100 days of employment on local public works projects. With nearly 70 percent of India's 1.2 billion population living in rural areas, NREGS ranks as one of the largest anti-poverty programs in the world. In 2009, according to official statistics, the national government spent $8 billion on NREGS. National sample survey data from 2009-10 show that 24 percent of India's rural households participated in NREGS in that year, with each participating household receiving on average 37 person-days of employment (Dutta et al., 2012).

NREGS stands apart from previous anti-poverty programs in India because access to the program is not means-tested. Like "workfare" programs generally (Besley and Coate, 1992), NREGS is designed to be "self-targeting", with the employment requirement for receipt of benefits intended to screen those with better outside employment options. NREGS participants are employed typically on small-scale local public works projects, such as ditch irrigation. Wages paid under NREGS vary by state and are adjusted over time, but in all states effectively amount to a minimum wage, on average $2/day. An innovation in the administrative design of the program is that locally elected village councils possess significant control of the daily administration of the program, including the distribution of job cards, the handling of employment requests, and the management of wage payments.

A growing program evaluation literature suggests that overall NREGS has had very large labor market impacts. Though studies document considerable corruption in the program (Imbert and Papp, 2011), mainly in the form of stolen wages and fictional employment (Niehaus and Sukhtankar, 2013), several studies utilizing a difference-in-differences identification strategy similar to that employed in this paper find that NREGS has improved living standards of rural households. Imbert and Papp (Forthcoming) find that NREGS adoption caused an increase in real earnings of rural households, notably for program non-participants as well as participants via labor market equilibrium effects. Berg et al. (2012) similarly find that NREGS adoption has after 6-12 months passed through to higher wages for agricultural laborers. The consensus of a large number of studies on NREGS is that the program has a had a significant impact on a number of different labor market outcomes (see edited volumes by Khera (2011) and Mann and Pande (2012) for a comprehensive review).

3 Theoretical Framework

To understand the impact of NREGS upon the Maoist conflict, we take as a theoretical starting point economic opportunity cost theories of civil conflict (Grossman, 1991; Collier and Hoeffler, 2004; Dal Bó and Dal Bó, 2011). These theories hold that poverty and low returns to peaceful economic activity make participation in violent insurgency relatively more attractive.

As discussed, several accounts, both popular and academic, stress the role of poverty in increasing the appeal of joining or supporting a Maoist insurgency that – while highly risky – promises an alternative economic regime, including social services, protection against corrupt bureaucrats, and an alternative stream of income. Previous research provides evidence for an opportunity cost theory of the Maoist conflict (Gawande, Kapur, and Satyanath, 2012; Vanden Eynde, 2011), showing that negative rainfall shocks increase Maoist violence.

To the extent that NREGS increases real earnings and provides an effective safety net to rural households, the program plausibly reduces the relative appeal of supporting and joining the insurgency. This is especially true of direct recruitment into the Maoist cadres, but also of latent civilian support for the insurgency, which previous research shows is an important dimension of rebel success and failure in civil conflict (Kalyvas, 2006). This is because lending informal or indirect support to the Maoist movement runs the risk of attracting reprisal and indiscriminate violence from government security forces.

Thus, by reducing recruitment and support, in the long run NREGS adoption plausibly mitigates the ability of the Maoists to launch attacks and also plausibly reduces the intensity of violence arising from government counter-insurgency operations, which are targeted primarily at strongholds of the insurgency. This yields the first hypothesis:

H1: NREGS adoption caused a long-run decrease in Maoist conflict-related violence.

Previous research on CERP, a public goods program operated by the US military in Iraq, suggests that an alternative to an opportunity cost theory of the impact of development programs on conflict is a 'hearts and minds' theory (Berman, Shapiro and Felter, 2011). However, NREGS differs in design and scale from CERP in ways that makes a labor market and opportunity cost channel more plausible. Unlike CERP, NREGS provides employment directly to households and has had a large impact on rural household earnings and labor markets in India. NREGS also exceeds CERP in scale and coverage. CERP was allocated discretionarily and accounted for just

$3.1 billion in spending over over a five year period. By contrast, NREGS covers all rural households and accounts for over $8 billion in spending annually.

Qualitative evidence also supports an opportunity cost theory in the case of NREGS. An important part of the 'hearts and minds' theory is that service provision is conditional on government control, thereby creating incentives for civilians to inform against rebels. There is no evidence for such a scenario in the case of NREGS. In line with its view that the Maoist insurgency has "deep socioeconomic dimensions" the Indian government has actively *targeted* the program at districts impacted by the Maoist insurgency rather than at districts safely under government control.[2] Surprisingly, though the Maoist insurgency's 2013 manifesto termed NREGS an "imperialist conspiracy" there is little evidence that the insurgency has systematically interfered in the program's implementation (Banerjee and Saha, 2010).[3]

Ultimately, we treat the impact of NREGS on Maoist conflict violence as an empirical issue, and test for a labor market and opportunity cost channel. We do this first by dis-aggregating the effects of NREGS by target of violence. A 'hearts and minds' theory of NREGS's effects arguably predicts an *increase* in violence against Maoist forces as information-sharing by civilians increases the effectiveness of counter-insurgency operations. By contrast, an opportunity cost theory predicts that NREGS adoption reduces violence against all targets because the program dis-incentivizes participation in the insurgency, diminishing the ability of the Maoist militia to launch attacks against security forces, and reducing the intensity of counter-insurgency operations, which are targeted primarily at strongholds of the insurgency. This yields the second hypothesis:

[2] Districts classified as "left wing extremism" affected were prioritized for the first phase of NREGS adoption. In 2010, the Indian government introduced an Integrated Action Plan program for left wing-extremism affected districts, sending additional grant aid for spending on development programs to each district.

[3] The best available data for gauging NREGS program uptake is a 2009 national household sample survey. The survey finds that in the 'red belt' states under analysis, rural household participation in NREGS was as follows: Andhra Pradesh (35.4%), Bihar (9.9%), Chhattisgarh (47.9%), Jharkhand (19.2%), Orissa (22.0%), and West Bengal (43.2%). These levels are close to the national average of 24.9% participation.

H2: NREGS adoption caused a long-run reduction in violence against all targets.

Finally, we develop the testable implication that if NREGS reduced violence by increasing economic opportunity costs of joining and supporting the insurgency, its impact on reducing violence should have been larger in districts with low levels of agricultural productivity and labor demand. The problem with testing this directly with data on employment or wage *levels* is that these variables are plausibly endogenous with respect to a number of variables, such as state capacity, which might have shaped the effects of NREGS on violence. However, rainfall shocks provide a valuable source of exogenous variation in productivity and labor demand in rain-fed agricultural economies (Jayachandran, 2006; Miguel, Satyanath and Sergenti, 2004), allowing us to test whether NREGS's violence reducing effects were larger in districts experiencing a negative shock to productivity and labor demand, as an opportunity cost theory predicts. This yields the final hypothesis:

H3: The violence reducing effects of NREGS adoption were larger after negative rainfall shocks.

4 Data

Dependent Variable

Our dependent variable is Maoist conflict violence, measured in terms of violent incidents and deaths. One of the challenges of studying a rural insurgency is that violence is difficult to measure and easily available data sources are often susceptible to severe reporting bias. Our empirical strategy focuses on within-district variation over time and therefore requires collecting data with a good degree of precision at the district level. To deal with these challenges, we assemble a new district-level panel dataset on Maoist violence compiled from multiple local language press sources. The dataset covers 144 districts between 1999 and 2009 in the six eastern states

– Andhra Pradesh, Bihar, Chhattisgarh, Jharkhand, Orissa, and West Bengal – in which over 90% of Maoist conflict deaths occur. The dataset geo-codes attacks and deaths to districts on a monthly basis, though we use year and quarter-year as our time unit of analysis.

Section 1 of the Appendix details the process of how the dataset was constructed. In brief, over two years for each state a team of researchers examined archives of daily editions of four distinct media sources for mentions of Maoist conflict incidents: the national English press, the regional English press, the local language (vernacular) dailies, and two wire services. By analyzing local language press sources we obtain much better coverage of the conflict in rural areas, which attracts little national English press coverage except in the case of major conflict incidents.[4] Each incident was geo-coded at the district level. For each incident, the number of civilian, government security personnel and Maoist deaths were recorded.[5] Our core measures of violence are the sum of total violent incidents and the sum of civilian, government security force and Maoist deaths (some analyses disaggregate by target of violence).

Because some districts split over the time frame under analysis, we utilize 2001 census district boundaries. We restrict the analysis to the six eastern states because the Maoist conflict is overwhelmingly concentrated in this region, with over 90% of deaths occurring in these states. The six states together contain 144 districts and over 378 million people. Figure 2 displays a map of average annual deaths associated with the Maoist conflict between 1999 and 2009 by district.

[Figure 2 About Here]

[4]We are able to capture a substantially larger number of casualties than the widely used South Asia Terrorism Portal database. See section 1 of the Appendix for more details.

[5]A handful of incidents spanned multiple districts. To deal with this, we evenly divided the casualties associated with these incidents between the spanned districts.

Explanatory Variables

Our difference in differences empirical strategy leverages the roll out of NREGS in three phases across district in India. Phase 1 districts received the program in February 2006, Phase 2 districts in April 2007, and Phase 3 districts in April 2008.[6] Our core explanatory variable 'pools' across these different phases with a binary treatment indicator that takes a value of 1 following program adoption in a district and 0 otherwise. Figure 3 displays a map of districts in the analysis by phase group.

[Figure 3 About Here]

Districts were assigned to NREGA implementation phases non-randomly. In all major states, districts were scored on a 'backwardness index' (BI), a technocratic score constructed by India's Planning Commission prior to the enactment of NREGA (Nayyar, 2003). A district's composite score was based upon percentage of disadvantaged minorities (scheduled caste and tribal groups) in the population (M), agricultural output per worker (O), and agricultural wage rate (W), measured with census and sample survey data from the 1990s. The formula used to compute a district's BI score was:

$$BI_i = \frac{max(M) - M_i}{max(M) - min(M)} + \frac{O_i - min(O)}{max(O) - min(O)} + \frac{W_i - min(W)}{max(W) - min(W)}, \tag{1}$$

with a lower score indicating a relatively lower level of development. This score was then used to assign districts to NREGA implementation phases in a particular way. First, a fixed number of slots for each NREGS adoption phase was assigned by the Planning Commission to states. Second, within states poorer districts were recommended to earlier phases of NREGS adoption in order of their intra-state rank on the backwardness index. Thirty two-districts, however, were

[6] Three completely urban districts in the states under analysis, Hyderabad, Vishakhapatnam, and Kolkata, did not receive the program. These districts drop out from the analysis.

prioritized for phase 1 adoption regardless of score due to being classified as "left wing extremism affected" by the Indian government. In line with their constitutional authority over social programs, states also retained discretion in the allocation of districts to phases.

This process of non-random selection raises concerns about the parallel trends assumption that underpins a difference in difference identification strategy. However, these concerns are mitigated by the fact that the timing of the NREGS adoption by phase was decided on a national rather than on a localized basis. These concerns are also mitigated by the fact that we pool the treatment effect of NREGS adoption across all three phase groups. We also deal with the issue by controlling directly for a district's score on the backwardness index in interaction with time dummy variables,[7] and by directly testing for pre-NREGS adoption trends in violence in the later empirical analysis

We assess the effect of controlling for the backwardness index in a cross-sectional OLS regression of different pre-treatment development indicators that might be related to the dynamics of Maoist violence – average real agricultural wages between 2004-2005, percentage of disadvantaged minorities (scheduled castes and scheduled tribes) in the population, and share of villages with a primary school – on phase group dummy variables. The results of these regressions, before and after the inclusion of the backwardness index score variable, are reported in Table 1.

[Table 1 About Here]

Predictably, phase 1 districts are characterized by lower real wages and higher tribal and lower caste population shares, though they surprisingly do not seem to possess significantly fewer schools. Controlling for a district's backwardness index score improves balance across

[7] Five predominantly urban districts, Patna, Raipur, Dhanbad, Ranchi, and Howrah, in the states under analysis did not receive a score on the backwardness index. We construct a score for each of these districts by averaging the backwardness index scores of neighbouring districts. All results are also robust to simply dropping these districts from the analysis.

phase groups on these pre-treatment measures of development, especially in terms of the percentage of disadvantaged minority groups in the population. This highlights the importance of controlling for the backwardness index score in the later empirical analysis.

To test hypothesis 3, which examines variation in the effects of NREGS as a function of rainfall shocks, we utilize annual data on district-level rainfall and various NREGS program monitoring indicators. Annual district-level rainfall data in total millimetres comes from the Indian Meteorological Department, which creates monthly gridded rainfall maps based on rainfall gauges throughout the country. We standardize this annual rainfall variable by taking its natural log and subtracting the sample mean. Since all regressions include district fixed effects, they implicitly identify the effects of over time variation within districts i.e. rainfall shocks. Our program monitoring indicators for NREGS at the district-year level are total employment (in 100,000 person-days), total expenditure (in million rupees), total number of public works projects (1,000 projects), and employment for women, scheduled tribe, and scheduled caste participants, respectively (in 100,000 person-days). These data were obtained from the Ministry of Rural Development, which compiles these statistics on the basis of monthly progress reports submitted by district-level bureaucrats.

Table 2 reports descriptive statistics for the major variables in the analysis. We note considerable within-district variation over time in Maoist conflict violence. Since our analysis includes district fixed effects, we investigate the role that NREGS adoption played in mitigating violence within districts over time.

[Table 2 About Here]

5 Results

We utilize a difference in differences identification strategy based upon the phased roll-out of NREGS across districts between 2006 and 2008. Such an approach controls for unobservable omitted variables with time invariant effects as well as time-based trends and shocks that affect all districts equally, but relies on the crucial assumption of parallel trends across districts, for which we provide evidence.

To estimate the effects of NREGS adoption, we utilize a Poisson quasi-maximum likelihood (QML) regression model, which is appropriate for our violence outcome data, a count variable, and for which coefficient estimates are robust to arbitrary distributional assumptions as long as the conditional mean is specified correctly (Wooldridge, 1999).[8] The Poisson model is also a consistent estimator in the the presence of unit fixed effects, unlike other count models, which suffer from the 'incidental parameters' problem. We estimate a regression equation of the form,

$$\mathbb{E}(Violence_{it}) = \exp\left(\gamma_i + \tau_t + \beta_1 NREGS_{it} + \beta_2 NREGS_{i,t-1} + \delta\mathbf{X}'\right), \tag{2}$$

where $Violence_{it}$ is a measure of violence in district i and year t, either total incidents or total deaths. The variable $NREGS_{it}$ represents NREGS adoption, pooling across all three phases of NREGS adoption (in 2006, 2007, and 2008, respectively). We include a lagged value of the treatment indicator to estimate the long-run effects of NREGS. This is important because considerable research shows the NREGS uptake was gradual rather than immediate. To compute the long run-effect of NREGS adoption on violence, the substantive quantity of interest, we we exponentiate the sum of the coefficients on the lagged and contemporaneous treatment indicator (giving us the incidence rate ratio), and subtract one, giving us the estimated percentage change in violence caused by two or more years of NREGS adoption.

[8] We also estimate and report a parallel set of results using OLS in the appendix. All results are qualitatively similar.

All regressions include district fixed effects, γ_i. This means we utilize within-district variation over time to estimate NREGS's effects on violence and can rule out time-invariant omitted variables, such as terrain and geography. All regressions include year fixed effects, τ_t, which absorb time-based shocks and trends common to all districts. The term $\mathbf{X}'$ is a vector of covariates. All regressions control for a district's score on the backwardness index, in interaction with year dummy variables, in order to minimize concerns about non-parallel time trends across different phase groups arising from the assignment of less developed districts as measured on the backwardness index to earlier phases of NREGS adoption. Some specifications additionally control for state-year fixed effects or lagged values of the dependent variable, to rule out the possibility that non-parallel trends across states or auto-correlation drive the results. All models estimate robust standard errors adjusted for clustering by district, to account for over-time correlation within districts in violence. Robust standard errors also account for possible over-dispersion in our violence data. We report the Poisson regression estimates in Table 3.

[Table 3 About Here]

The table reports both the raw coefficient estimates as well as estimates of the substantive quantity of interest, the estimated percentage change in violence after two or more years of NREGS adoption, as well as a 95% confidence interval for this estimate. The regression estimates suggest that NREGS adoption caused a large long-run reduction in incidents of Maoist conflict violence as well as total deaths. In the base specification reported in columns (1) and (4), NREGS adoption is estimated to have caused on average a 82% long-run reduction in Maoist conflict incidents and a 87% long-run reduction in Maoist conflict deaths. These are massive reductions in violence in percentage terms. To gain a sense of the implied effects in terms of absolute numbers, take the average number of deaths per district-year (5) and multiply this by the estimated long-run effects of NREGS on violence (-87%). This computation suggests that NREGS adoption in the long run caused on average a 4.35 reduction in deaths per district-year, a number close to the OLS estimates reported in Table A1 of the appendix. The coefficient estimates suggest that this effect emerged in large part in the second year of program adoption, with large, negative, and statistically significant estimates of the coefficient on the lagged treatment indicator.

The estimates are stable across different specifications. Columns (2) and (5) additionally control for a lagged value of the dependent variable, to account for possibly dynamics in the relationship between NREGS adoption and violence. Finally, columns (3) and (6) control for state-year fixed effects, thereby absorbing any time trends and shocks specific to states. This specification rules out concerns about non-parallel trends across states. While the coefficient estimates shrink slightly, the estimates suggest that NREGS adoption caused a 78-81% long run-reduction in Maoist conflict incidents and a 78-86% long-run reduction in total deaths.

Timing of Effect

The central assumption that underpins a difference-in-differences identification strategy is the parallel trends assumption i.e. the assumption that, conditional on covariates, districts adopting NREGS were not experiencing declining trends in violence independent of program adoption. A natural 'placebo test' of this assumption is to examine whether districts about to receive NREGS were experiencing reductions in violence *prior* to program adoption.

To investigate time patterns of changes in violence before and after the adoption of NREGS at a more fine-grained level, we utilize quarter-yearly data instead of yearly data, and regress our measures of violence on indicators for each of the four quarters prior to NREGS adoption and for each of the eight quarters including and following NREGS adoption (the final dummy variable is an indicator of the eighth quarter onward). As before, we estimate a Poisson QML regression model that includes district and year fixed effects and controls for the backwardness index score in interaction with year dummy variables. We display the estimated coefficients on the time period indicators in Figure 4. Vertical bars represent 95% confidence intervals (with robust standard errors adjusted for clustering within districts).

[Figure 4 About Here]

The analysis shows that the estimated effects of NREGS adoption on Maoist conflict violence were not driven by a pre-adoption trend. Rather, both incidents and deaths started to fall in a district in roughly the third quarter-year of program adoption, with the effect emerging fully in the second year of program adoption, consistent with main results already reported. This finding lends credence to the main findings in their realism. As discussed, it took several months, and sometimes more than a year, for formal program adoption to pass through to jobs and wages on the ground (Bhatia and Dreze, 2006; Berg et al., 2012), and to have a substantial impact on the labor market in a way that might have mitigated the conflict.

Testing the Opportunity Cost Channel

We argue that NREGS adoption reduced violence by improving economic conditions and increasing the opportunity costs of supporting and participation in the insurgency. We conduct two separate tests of this posited channel. First, we dis-aggregate the violence reducing effects of NREGS adoption by the *targets* of violence. Secondly, we investigate heterogeneity in the effects of NREGS adoption as a function of rainfall shocks, which provide a source of exogenous variation in agricultural productivity and labor demand.

An opportunity cost theory predicts that NREGS adoption reduces violence against all targets: civilians, government security forces, and Maoists. This is because increasing opportunity costs diminish the ability of the Maoist militia to launch attacks against security forces, reduce the intensity of counter-insurgency operations, which are targeted primarily at strongholds of the insurgency, and reduce the number of clashes resulting in civilian deaths. By contrast, a 'hearts and minds' theory of NREGS's effects arguably predicts an *increase* in violence against

Maoist forces as information-sharing by civilians increases the effectiveness of counter-insurgency operations. Table 4 reports estimates of the impact of NREGS adoption on deaths dis-aggregated by target of violence.

[Table 4 About Here]

Strikingly, the estimates suggest that across the board NREGS adoption caused a long-run reduction in violence against all targets. These patterns are consistent with an opportunity cost channel, with NREGS adoption dis-incentivizing participation in the insurgency and reducing the intensity of government counter-insurgency operations. By contrast, there is no evidence that the program increased the effectiveness of counter-insurgency operations, with Maoist deaths falling, not increasing, following program adoption. The estimated long-run effects on violence against government security forces are negative, but imprecisely estimated. There is some evidence of a small increase in violence against security forces in the first year of program adoption followed by a large decline in the second year of program adoption. However, the coefficients on the contemporaneous indicator of program adoption, while positive, are statistically insignificant, while the coefficients on the lagged indicator of program adoption are large, negative and statistically significant.

We now show that NREGS's violence reducing effects were larger in districts experiencing a negative rainfall shock, providing further support for an opportunity cost channel. As discussed, rainfall shocks provide a source of exogenous variation in agricultural productivity and labor demand. If NREGS reduced violence by increasing opportunity costs, its violence reducing effects should have been especially large in districts experiencing a negative rainfall shock. We estimate a model of the form,

$$\mathbb{E}(Violence_{it}) = \exp\big(\gamma_i + \tau_t + \beta_1 NREGS_{it} + \beta_2 NREGS_{i,t-1} + \delta \mathbf{X}' + \tag{3}$$

$$+\theta_1 Rain_{it} + \theta_2 Rain_{it} \times NREGS_{it} + \theta_3 Rain_{it} \times NREGS_{i,t-1}\big), \tag{4}$$

where the long run effect of NREGS now varies as a function of rainfall. Our measure of rainfall is the natural log of annual mm of rainfall, standardized by subtracting the sample mean. Since we include district fixed effects, we implicitly identify the effects of within district rainfall variation over time, or rainfall shocks. An opportunity cost-based theory would expect to see a positive estimate for the coefficients θ_2 and θ_3, implying that more negative rainfall realization results in a larger reduction in violence after the adoption of NREGS (or a more positive rainfall realization a smaller reduction in violence). We can also estimate the same equation with *lagged* values of rainfall, to assess whether previous year rainfall shocks shape the impact of NREGS.

We report the results of these analyses graphically in Figure 5, displaying the long-run effect of NREGS adoption on violence as a function of within-district standard deviations of rainfall variation from the standardized sample mean of zero. Again, to obtain the substantive quantity of interest, we exponentiate the sum of the coefficients on the contemporaneous and lagged treatment indicators ($\beta_1 + \beta_2 + \theta_2 Rain_{it} + \theta_3 Rain_{it}$), which now varies as a function of rainfall, and subtract one to compute the percentage change in the violence rate caused by two or more years of NREGS adoption. Table A2 in the appendix reports the full set of regression results in table form.

[Figure 5 About Here]

The results are strongly consistent with an opportunity cost channel. NREGS's violence reducing effects are found to vary strongly with rainfall shocks, with a larger violence reduction is districts experiencing a worse rainfall realization i.e. a negative shock to agricultural productivity and labor demand. In all four models, F-tests strongly reject the null hypothesis that the coefficients on the interaction terms between rainfall and NREGS adoption are equal to zero ($\theta_2 = \theta_3 = 0$). Interestingly, we find that the impact of NREGS adoption on both incidents and total deaths varies as a function of rainfall shocks, and that both current year and previous year rainfall shocks modify the relationship between NREGS adoption and violence. These results are consistent with Fetzer (2014), who investigates the role of NREGS adoption in moderating the rainfall-violence relationship in India in detail.

To verify that the larger impacts of NREGS in districts experiencing a negative rainfall realization are connected to the greater intensity of the program in these areas, we utilize program monitoring data to assess whether negative rainfall shocks cause increases in NREGS employment provision. We focus on the sample of district-years in which NREGS has already been adopted, and estimate OLS regression of different program monitoring indicators on measures of current year and previous year rainfall. Our indicators of program are intensity are total employment (in 100,000 person-days), total expenditure (in million rupees), total public works projects (1,000 projects), and employment for female, scheduled caste, and scheduled tribe recipients, respectively. As before, the inclusion of district fixed effects means that we identify the effects of within district rainfall variation over time, or rainfall shocks. The OLS regression results are reported in Table 5.

[Table 5 About Here]

The results provide additional confirmation of an opportunity cost channel. Employment provision across categories, total works, and expenditure under the program rise sharply in districts experiencing a more negative rainfall realization, rationalizing the larger violence-reducing effects of NREGS adoption in these areas. The magnitude of the estimates of the responsiveness of NREGS program intensity to rainfall shocks are large. For instance, a one within-district standard deviation (0.25) shortfall in previous year rainfall is estimated to cause an increase ($-0.25 \times -30.604 \times 100,000$ person-days) of 765,100 person-days of NREGS employment in a district-year. This is roughly 15 percent of overall average employment provision and roughly a third of a within-district standard deviation in employment provision. This estimated increase is present across all categories of employment, including employment for both female and disadvantaged minority recipients. The results show that both current year and previous year rainfall shocks impact program intensity, though previous year rainfall is estimated to have larger effects than current year rainfall.[9] This rationalizes the reported estimates of heterogeneity in the violence reducing effects of NREGS adoption as a function of both current year and previous year rainfall shocks.

6 Conclusion

Utilizing a new district-level panel dataset on Maoist conflict violence based on multiple press sources, including those in local languages, and a difference in differences identification strategy based on the phased roll-out of NREGS across districts, we have shown that the program caused a large reduction in violence. Placebo tests show that this effect was not driven by a pre-adoption trend. The effects of the program emerged after three quarter years of program adoption and reduced the rate of violent incidents and total deaths by approximately 80%, an extremely large effect.

We have also provided evidence that NREGS reduced violence through labor market impacts that increased the opportunity costs of joining and supporting the Maoist insurgency. NREGS adoption reduced violence against all targets, including Maoists, supporting an opportunity cost channel over a hearts and minds channel. NREGS's violence reducing effects were larger when, due to exogenous within-district rainfall variation, agricultural productivity and labor demand were low, also consistent with an opportunity cost channel. The larger violence reducing effects of NREGS in districts experiencing a negative rainfall shock were connected to greater program intensity and employment provision during these periods.

The paper contributes to the large literature on the poverty-violence relationship by directly

[9] This is not surprising. The monsoon rainfall, which occurs typically between June and September, is the primary determinant of agricultural productivity and labor demand in India. Since years in our analysis are defined by calendar convention from January to December, agricultural productivity and labor demand during the pre-monsoon months in a district-year as defined in our analysis are determined primarily by monsoon rainfall in the previous calendar year.

testing the policy implication that anti-poverty programs ought to reduce violence. In sharp contrast to previous research, we *do* find that large-scale anti-poverty programs can mitigate violent civil conflict by improving economic conditions and increasing the opportunity costs of participation in insurgency. We have argued that this is plausibly due to the employment-oriented design and massive scale of NREGS, which guarantees 100 days of public works employment annually to rural households and has had a large impact on rural labor markets in India. A valuable topic for future research is how the design and scale of different development programs shape their impacts on civil conflict.

References

Banerjee, Kaustav and Partha Saha. 2010. "The NREGA, the Maoists and the Developmental Woes of the Indian State." *Economic and Political Weekly* 45(28):42–7.

Banerjee, Sumanta. 1980. *In the Wake of Naxalbari: A history of the Naxalite movement in India.* Subarnarekha Calcutta.

Berg, Erlend, Sambit Bhattacharyya, Rajasekhar Durgam and Manjula Ramachandra. 2012. "Can Public Works Increase Wages? Evidence from India." *University of Oxford Working Paper Series* 5.

Berman, Eli, Jacob N Shapiro and Joseph H Felter. 2011. "Can Hearts and Minds be Bought? The Economics of Counterinsurgency in Iraq." *Journal of Political Economy* 119(4):766–819.

Besley, Timothy and Stephen Coate. 1992. "Workfare versus Welfare: Incentive Arguments for Work Requirements in Poverty-alleviation Programs." *American Economic Review* 82(1):249–261.

Besley, Timothy and Torsten Persson. 2011. "The Logic of Political Violence." *The Quarterly Journal of Economics* 126(3):1411–1445.

Bhatia, Bela. 2005. "The Naxalite Movement in Central Bihar." *Economic and Political Weekly* pp. 1536–1549.

Bhatia, Bela and Jean Dreze. 2006. "Employment Guarantee in Jharkhand: Ground realities." *Economic and Political Weekly* pp. 3198–3202.

Blattman, Christopher and Edward Miguel. 2010. "Civil War." *Journal of Economic Literature* 48(1):3–57.

Blattman, Christopher and Jeannie Annan. 2014. "Can Employment Reduce Lawlessness and Rebellion? A Field Experiment with High-Risk Youth in a Fragile State." *Mimeo* .

Borooah, Vani K. 2008. "Deprivation, Violence, and Conflict: An Analysis of Naxalite Activity in the Districts of India." *International Journal of Conflict and Violence* 2(2):317–333.

Chakravarti, Sudeep. 2009. *Red Sun: Travels in Naxalite Country.* Penguin Books India.

Collier, Paul and Anke Hoeffler. 1998. "On Economic Causes of Civil War." *Oxford Economic Papers* 50(4):563–573.

Collier, Paul and Anke Hoeffler. 2004. "Greed and Grievance in Civil War." *Oxford Economic Papers* 56(4):563–595.

Collier, Paul and Anke Hoeffler. 2007. "Unintended Consequences: Does Aid Promote Arms Races?*." *Oxford Bulletin of Economics and Statistics* 69(1):1–27.

Collier, Paul, V. L. Elliott, Havard Hegre, Anke Hoeffler, Marta Reynal-Querol and Nicholas Sambanis. 2003. *Breaking the Conflict Trap: Civil War and Development Policy.* World Bank Publications.

Crost, Benjamin, Joseph Felter and Patrick Johnston. Forthcoming. "Aid Under Fire: Development Projects and Civil Conflict." *American Economic Review* .

Dal Bó, Ernesto and Pedro Dal Bó. 2011. "Workers, Warriors, and Criminals: Social Conflict in General Equilibrium." *Journal of the European Economic Association* 9(4):646–677.

Dutta, Puja, Rinku Murgai, Martin Ravallion and Dominique van de Walle. 2012. "Does India's Employment Guarantee Scheme Guarantee Employment?" *Economic & Political Weekly* 47(16):55.

Eynde, Oliver Vanden. 2011. "Targets of Violence: Evidence from India's Naxalite Conflict." *Mimeo* .

Fearon, James D and David D Laitin. 2003. "Ethnicity, Insurgency, and Civil War." *American Political Science Review* 97(01):75–90.

Fetzer, Thiemo. 2014. "Can Workfare Programs Moderate Violence? Evidence from India." *Mimeo* .

Gawande, Kishore, Devesh Kapur and Shanker Satyanath. 2012. "Natural Resource Shocks and Conflict in India's Red Belt." *Mimeo* .

Gomes, Joseph Flavian. 2012. "The Political Economy of the Maoist conflict in India: an Empirical Analysis." *Mimeo* .

Grossman, Herschel I. 1991. "A General Equilibrium Model of Insurrections." *American Economic Review* 81(4):912–21.

Guha, Ramachandra. 2007. "Adivasis, Naxalites and Indian Democracy." *Economic and Political Weekly* 42(32):3305–3312.

Harriss, John. 2010. "The Naxalite/Maoist Movement in India: A Review of Recent Literature." *Working Paper* .

Hoelscher, Kristian, Jason Miklian and Krishna Chaitanya Vadlamannati. 2012. "Hearts and Mines: A District-level Analysis of the Maoist Conflict in India." *International Area Studies Review* 15(2):141–160.

Imbert, Clément and John Papp. 2011. *Estimating Leakages in India's Employment Guarantee Using Household Survey Data.* Oxford University Press.

Imbert, Clement and John Papp. Forthcoming. "Labor Market Effects of Social Programs: Evidence from India's Employment Guarantee." *AEJ: Applied Economics* .

Jayachandran, Seema. 2006. "Selling Labor Low: Wage Responses to Productivity Shocks in Developing Countries." *Journal of Political Economy* 114(3):538–575.

Kalyvas, Stathis N. 2006. *The Logic of Violence in Civil War*. Cambridge University Press.

Khera, Reetika, ed. 2011. *The Battle for Employment Guarantee*. Oxford University Press.

Mann, Neelakshi and Varad Pande. 2012. *MGNREGA Sameeksha: An Anthology of Research Studies on the Mahatma Gandhi National Rural Employment Guarantee Act*. Orient Blackswan.

Miguel, Edward, Shanker Satyanath and Ernest Sergenti. 2004. "Economic Shocks and Civil Conflict: An Instrumental Variables Approach." *Journal of Political Economy* 112(4):725–753.

Nayyar, Rohini. 2003. *Identification of Districts for Wage and Self Employment Programmes*. Planning Commission.

Niehaus, Paul and Sandip Sukhtankar. 2013. "Corruption Dynamics: The Golden Goose Effect." *American Economic Journal: Economic Policy* 5(4):230–269.

Nielsen, Richard A, Michael G Findley, Zachary S Davis, Tara Candland and Daniel L Nielson. 2011. "Foreign Aid Shocks as a Cause of Violent Armed Conflict." *American Journal of Political Science* 55(2):219–232.

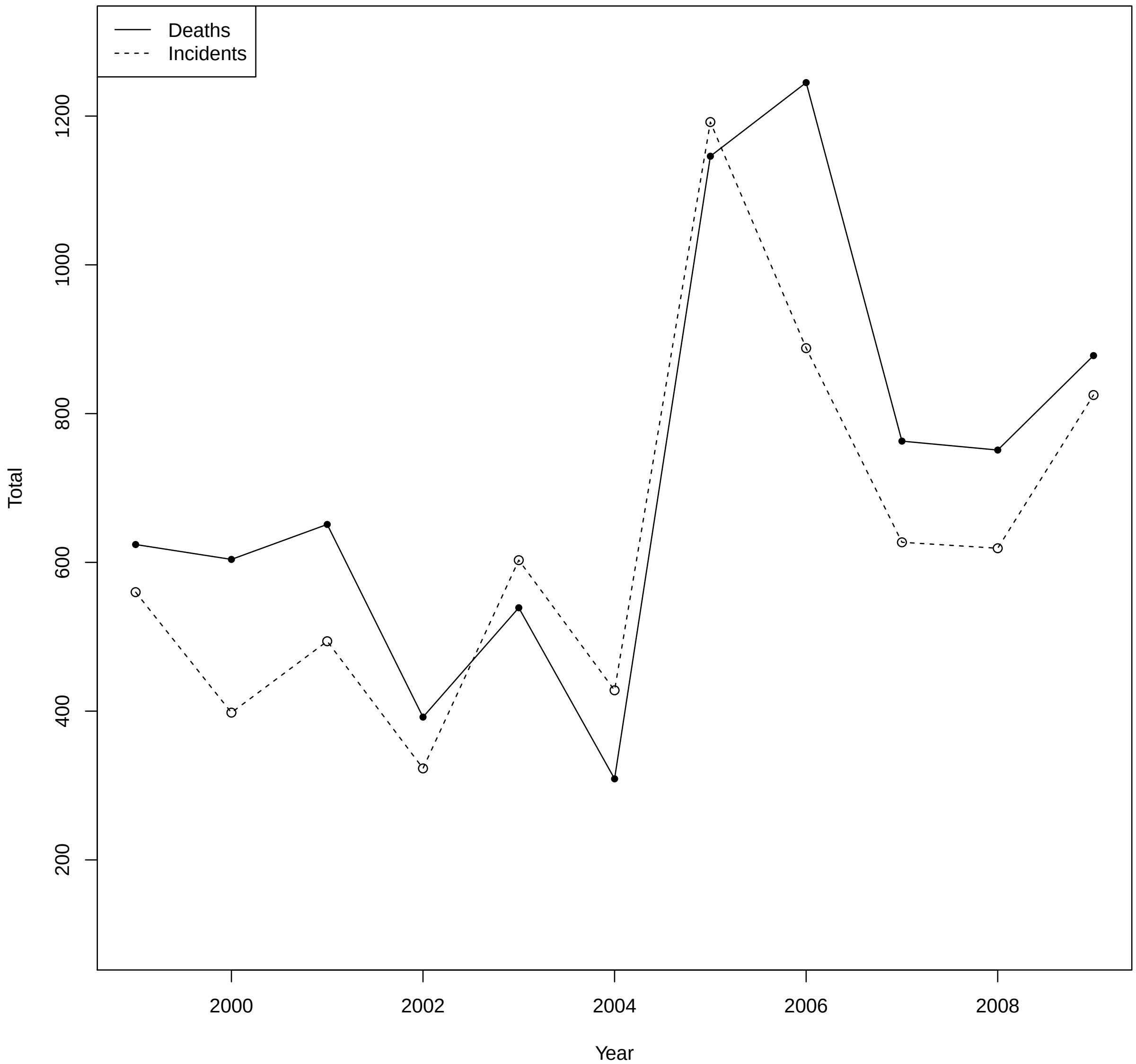
Nunn, N and N Qian. Forthcoming. "Aiding Conflict: The Impact of US Food Aid on Civil War." *American Economic Review* .

Pandita, Rahul. 2011. *Hello Bastar*. Westland.

Wooldridge, Jeffrey M. 1999. "Distribution-free Estimation of Some Nonlinear Panel Data Models." *Journal of Econometrics* 90(1):77–97.

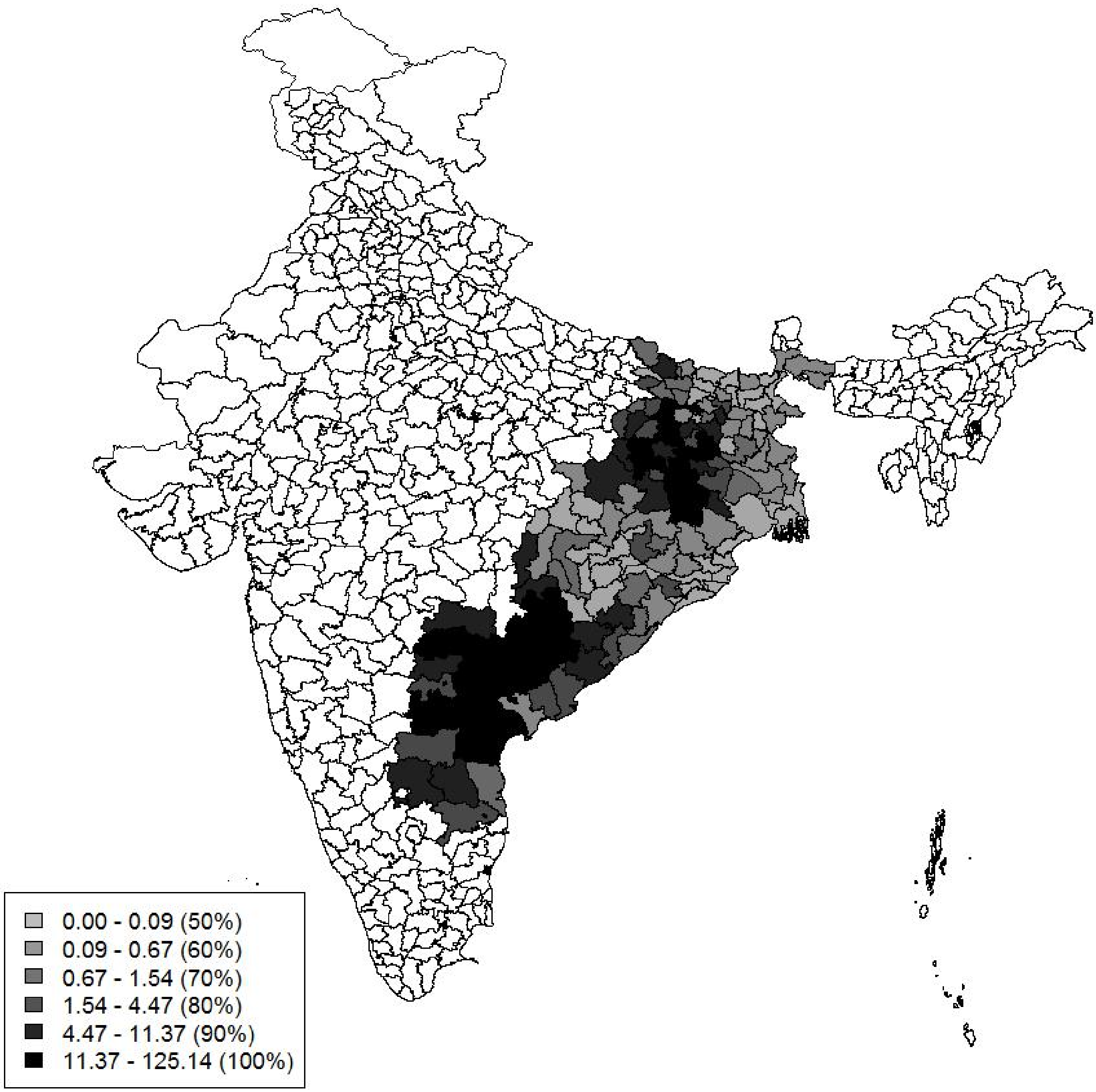
7 Tables and Figures

Figure 1: Maoist Conflict Violence Over Time, 1999-2009



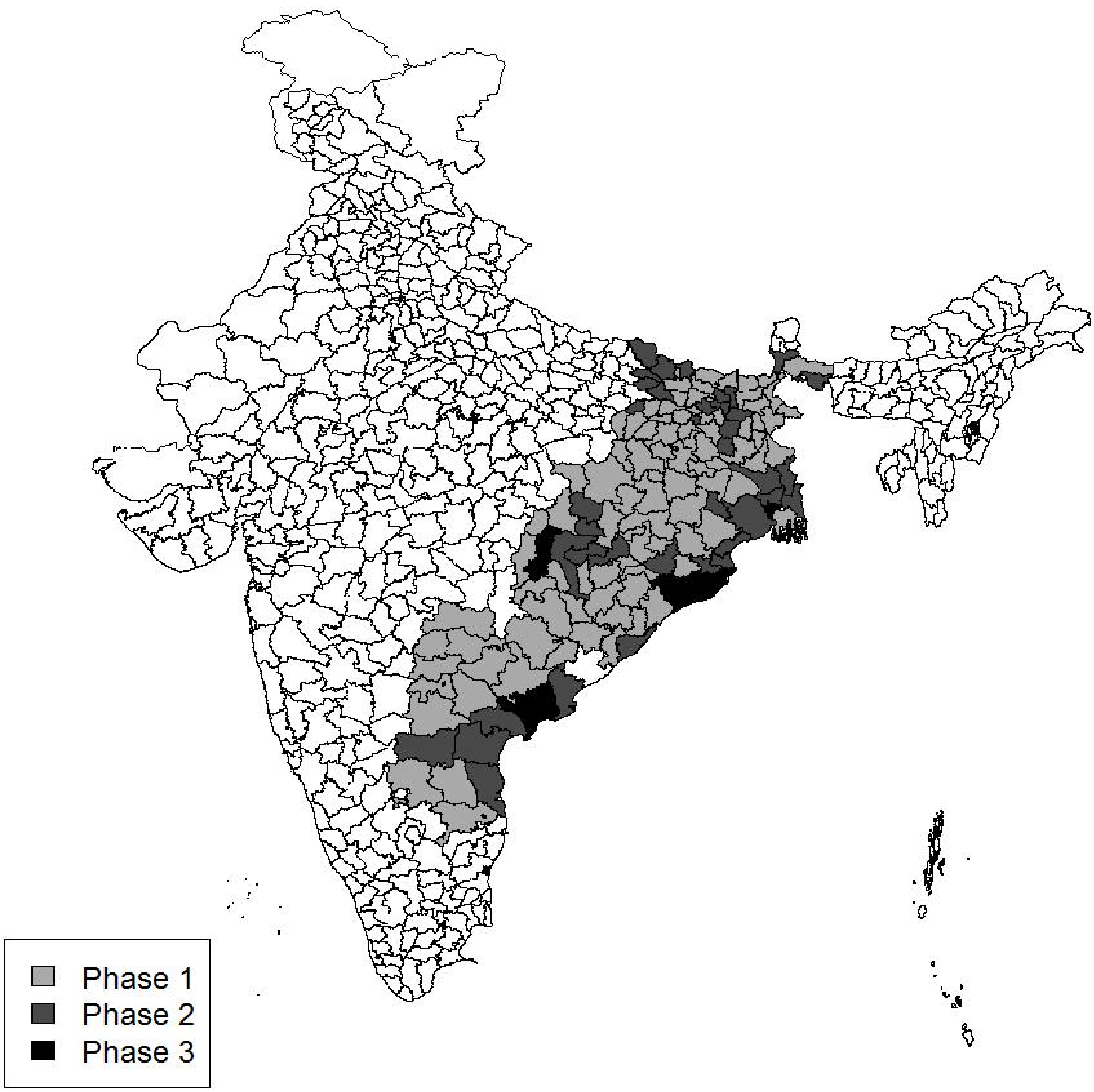
Notes: Incidents represents sum of recorded violent incidents, including those not resulting in deaths. Deaths sums civilian, Maoist, and security personnel deaths. Dataset covers 144 districts in 'red belt' states – Andhra Pradesh, Bihar, Chhattisgarh, Jharkhand, Orissa, and West Bengal – between 1999 and 2009.

Figure 2: Average Annual Deaths from Maoist Conflict by District, 1999-2009



Notes: Districts shaded by decile of average annual number of total deaths resulting from Maoist conflict, 1999-2009. Dataset covers 144 districts in 'red belt' states – Andhra Pradesh, Bihar, Chhattisgarh, Jharkhand, Orissa, and West Bengal – between 1999 and 2009. Unshaded districts not in analysis.

Figure 3: Districts by NREGS Adoption Phase



Notes: Districts shaded by NREGS adoption phase. Phase 1 districts received NREGS in 2006, phase 2 districts in 2007, and phase 3 districts in 2008. Dataset covers 'red belt' states – Andhra Pradesh, Bihar, Chhattisgarh, Jharkhand, Orissa, and West Bengal – between 1999 and 2009. Unshaded districts not in analysis.

Table 1: Balance on Development Indicators Before and After Control for Backwardness Index

	Dependent variable:					
	2004-05 Real Wage		SC/ST% Pop		Villages with School	
	(1)	(2)	(3)	(4)	(5)	(6)
Phase 2	6.153*	3.579	−11.030***	2.151	0.026	0.030
	(3.272)	(3.393)	(3.331)	(2.014)	(0.033)	(0.035)
Phase 3	3.233	−0.111	−14.704**	0.516	0.058	0.062
	(4.718)	(4.842)	(5.805)	(3.367)	(0.057)	(0.059)
BI		16.347**		−76.914***		−0.022
		(7.133)		(4.419)		(0.078)
Constant	50.401***	36.724***	38.004***	102.871***	0.709***	0.728***
	(1.734)	(6.205)	(1.818)	(3.864)	(0.018)	(0.068)
Observations	99	99	141	141	141	141

Notes: Unit of observation district. Phase 2 and Phase 3 are dummy variables indicating the second and third phases of NREGS implementation. The reference category is phase 1. BI is a control variable representing a district's score on the backwardness index. 2004-05 real wage is the average daily real wage of male agricultural field laborers between 2004-2005(source: Agricultural Wages of India series). SC/ST% Pop is the percentage of the population comprised by scheduled caste and scheduled tribal groups (source: 2001 census). Villages with School represents the share of villages in a district with a primary school (source: 2001 census). Analysis estimated by OLS. *p<0.1; **p<0.05; ***p<0.01

Table 2: Descriptive Statistics

	Mean	SD	Within-SD	5%	25%	50%	75%	95%
Panel A: Main Analysis								
Incidents	4.44	11.62	8.12	0.00	0.00	0.00	4.00	22.05
Total Deaths	5.05	20.73	15.92	0.00	0.00	0.00	2.00	26.05
Civilian Deaths	1.86	10.19	8.77	0.00	0.00	0.00	1.00	8.00
Maoist Deaths	1.87	7.58	5.75	0.00	0.00	0.00	0.00	11.00
Security Deaths	1.32	7.01	5.69	0.00	0.00	0.00	0.00	7.00
Backwardness Index	0.90	0.20	0.00	0.43	0.80	0.96	1.05	1.14
Rainfall	-0.01	0.35	0.25	-0.61	-0.20	0.01	0.24	0.51
NREGS	0.36	0.48	0.48	0.00	0.00	0.00	1.00	1.00
Panel B: Program Indicators								
Employment	51.27	50.17	25.72	6.43	18.40	34.30	61.06	163.40
Expenditure	625.25	556.79	317.75	115.98	259.94	428.75	799.95	1888.09
Works	9.06	13.61	6.20	0.90	2.35	4.27	9.62	33.74
Female Employment	21.63	29.65	15.09	0.85	4.54	10.34	22.81	91.82
SC Employment	14.23	14.74	6.72	1.34	4.14	8.73	18.89	46.22
ST Employment	11.33	15.38	6.71	0.00	0.78	5.38	15.47	42.85

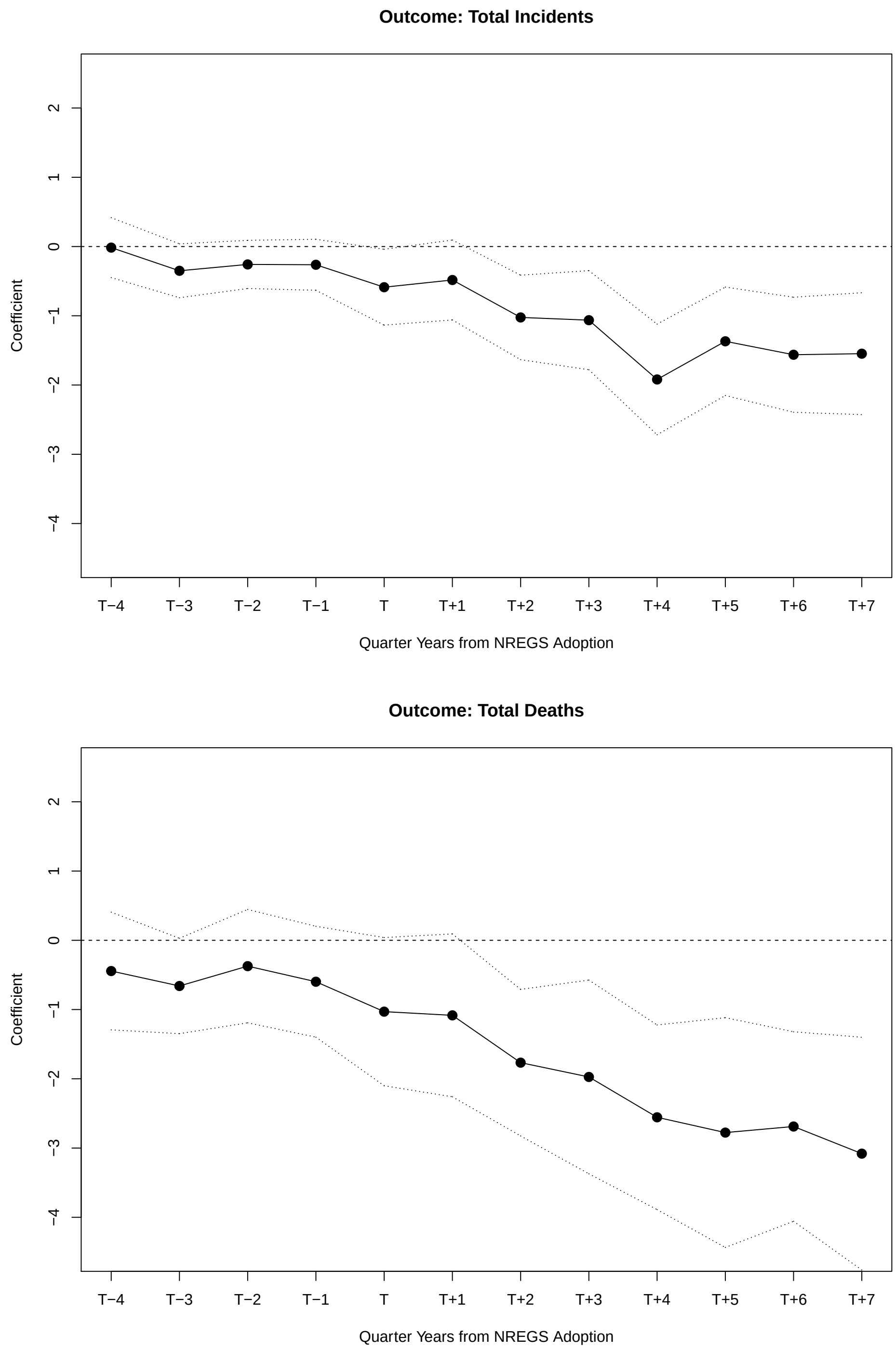
Notes: Unit of observation district-year, 2000-2009. Within-SD is within-district standard deviation of variable after partialing out district averages. Incidents variable sums recorded violent incidents, including those not resulting deaths. Total deaths variable sums civilian, Maoist, and security personnel deaths. Backwardness index variable does not vary over time. Rainfall variable is the log of annual mm, standardized by subtracting the overall sample mean. NREGS is a binary indicator that takes a value of 1 for program adoption and 0 otherwise. Panel B restricts sample to district-years post-NREGS adoption. Employment is in 100,000 person-days. Employment figures are also dis-aggregated by female, scheduled caste, and scheduled tribe recipients. Expenditure is in million rupees. Works is in 1,000 projects.

Table 3: Poisson Regression Estimates of Effect of NREGS Adoption on Maoist conflict violence

	Dependent variable:					
	Total Incidents			Total Deaths		
	(1)	(2)	(3)	(4)	(5)	(6)
% Change	−82	−81	−78	−87	−86	−78
	[−92,−59]	[−92,−57]	[−91,−46]	[−97,−43]	[−97,−36]	[−95,−3]
Long-run	−1.707***	−1.655***	−1.506***	−2.059***	−1.932**	−1.525**
	(0.413)	(0.411)	(0.455)	(0.766)	(0.754)	(0.761)
NREGS	−0.693***	−0.687***	−0.451	−0.551	−0.529	−0.309
	(0.238)	(0.237)	(0.275)	(0.507)	(0.499)	(0.502)
NREGS_{t-1}	−1.014***	−0.967***	−1.055***	−1.508**	−1.399**	−1.216**
	(0.281)	(0.284)	(0.271)	(0.610)	(0.631)	(0.492)
Y_{t-1}		0.001			0.002	
		(0.001)			(0.001)	
State × Year FE			Y			Y
Year FE	Y	Y		Y		
District FE	Y	Y	Y	Y	Y	Y
BI × Year	Y	Y	Y	Y	Y	Y
Observations	1,410	1,410	1,410	1,410	1,410	1,410
Clusters	141	141	141	141	141	141

Notes: Unit of observation is district-year, 2000-2009. Total incidents represents total incidents of Maoist violence. Total deaths represents sum of civilian, Maoist, and security force deaths. Long-run is sum of coefficient on contemporaneous treatment variable *NREGS* and on lagged treatment variable NREGS_{t-1}, divided by 1− coefficient on Y_{t-1} in the case of specifications controlling for a lagged value of dependent variable. We exponentiate this quantity, and subtract 1, to obtain the estimated percentage change in violence caused by two or more years of NREGS adoption. A 95% confidence interval is reported in brackets. Analysis estimated by Poisson quasi-maximum likelihood model. Standard errors in parentheses adjusted for clustering within districts. See Table A1 for parallel OLS results. *p<0.1; **p<0.05; ***p<0.01

Figure 4: Changes in Violence Before and After NREGS Adoption by Quarter



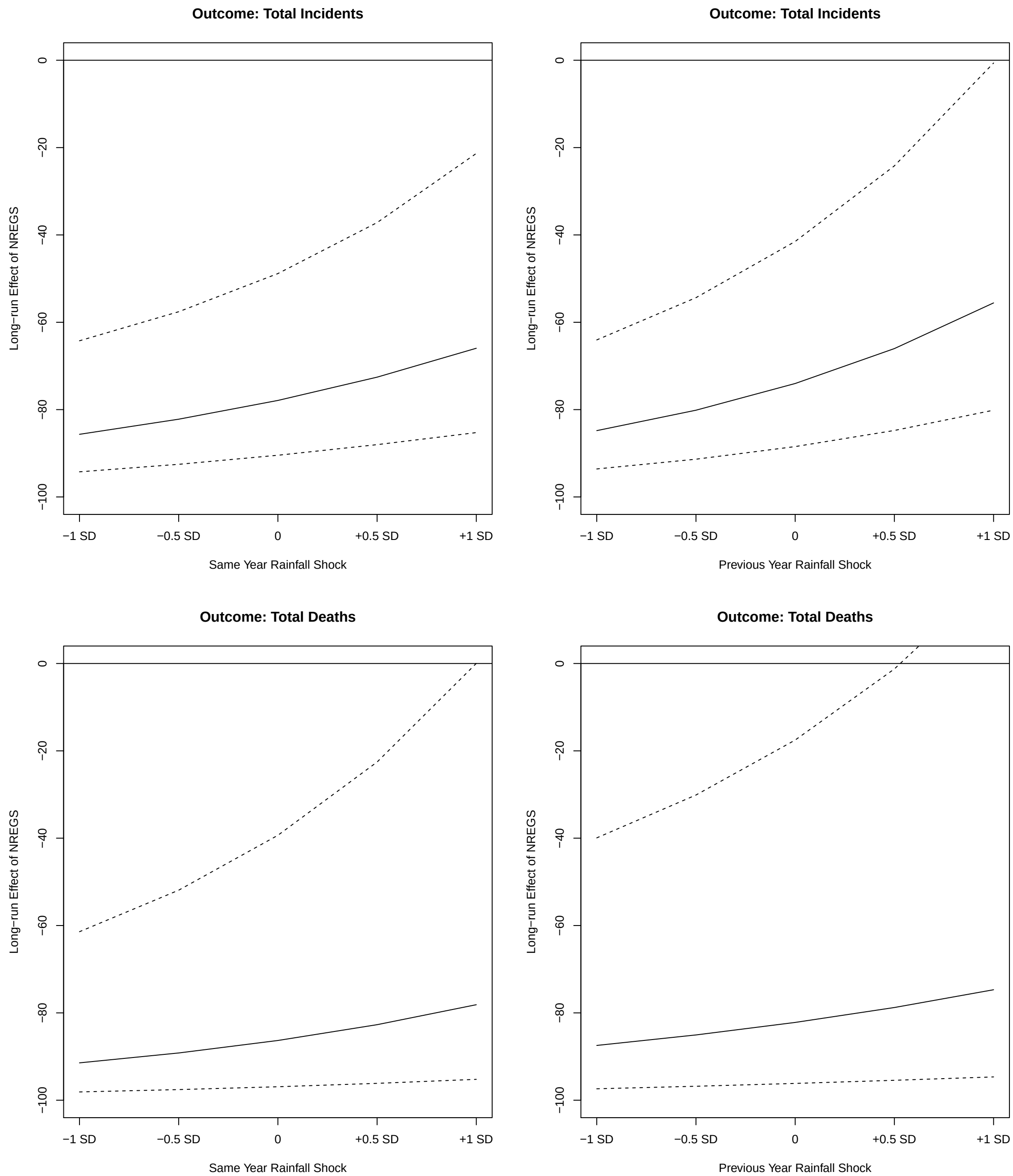
Notes: Unit of observation district-quarter year. Points represent coefficient on indicator of time period, relative to the adoption of NREGS at time T. Vertical bands represent 95% confidence intervals. Final time period indicator represents the eighth quarter of program adoption onward. Poisson QML regression model controls for district and year fixed effects, and backwardness index score variable interacted with year dummy variables. Standard errors adjusted for clustering within districts.

Table 4: Dis-aggregating the Effects of NREGS Adoption on Deaths by Target of Violence

	Dependent variable:								
	Civilian Deaths			Maoist Deaths			Security Force Deaths		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
% Change	−86	−85	−81	−90	−90	−84	−74	−66	−74
	[−97, −26]	[−97, −20]	[−96, −3]	[−99, 0]	[−99, 1]	[−99, 143]	[−98, 235]	[−97, 285]	[−98, 266]
Long-run	-2.000^{**}	-1.901^{**}	-1.664^{**}	-2.300^{**}	-2.327^{*}	−1.804	−1.359	−1.074	−1.333
	(0.863)	(0.856)	(0.833)	(1.172)	(1.194)	(1.372)	(1.310)	(1.236)	(1.342)
NREGS	-0.955^{*}	-0.943^{*}	−0.657	−0.774	−0.780	−0.560	0.893	0.959	1.085
	(0.550)	(0.546)	(0.502)	(0.635)	(0.637)	(0.775)	(0.705)	(0.693)	(0.932)
NREGS_{t-1}	−1.035	−0.956	-1.007^{*}	-1.526^{*}	-1.548^{*}	−1.244	-2.252^{**}	-2.030^{**}	-2.418^{***}
	(0.646)	(0.664)	(0.574)	(0.895)	(0.912)	(0.838)	(0.986)	(0.967)	(0.900)
Y_{t-1}		0.001			−0.0003			0.003^{**}	
		(0.001)			(0.002)			(0.002)	
State × Year FE			Y			Y			Y
Year FE	Y	Y		Y	Y		Y	Y	
District FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
BI × Year	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,410	1,410	1,410	1,410	1,410	1,410	1,410	1,410	1,410
Clusters	141	141	141	141	141	141	141	141	141

Notes: Unit of observation is district-year, 2000-2009. Long-run is sum of coefficient on contemporaneous treatment variable *NREGS* and on lagged treatment variable NREGS_{t-1}, divided by 1− coefficient on Y_{t-1} in the case of specifications controlling for a lagged value of dependent variable. We exponentiate this quantity, and subtract 1, to obtain the estimated percentage change in violence caused by two or more years of NREGS adoption. A 95% confidence interval is reported in brackets. Analysis estimated by Poisson quasi-maximum likelihood model. Standard errors in parentheses adjusted for clustering within districts. *p<0.1; **p<0.05; ***p<0.01

Figure 5: Violence Reducing Effects of NREGS as a Function of Rainfall Shocks



Notes: Unit of observation district-year. Plots represent estimated percentage change in violence caused by two or more years of NREGS adoption, as a function of rainfall. Rainfall variable is the log of annual mm, with standardized sample mean 0. In sample a within-district standard deviation in the natural log of rainfall was 0.25. All regressions include district fixed effects (as well as year fixed effects and controls for backwardness index score in interaction with year dummy variables). Vertical bands represent 95% confidence interval. Analysis estimated by Poisson QML model. Standard errors adjusted for clustering within districts. See table A2 for full regression results in table form.

Table 5: OLS Estimates of Effects of Rainfall Shocks on NREGS Program Intensity

	Dependent variable:					
	Employment	Expenditure	Works	Female Employment	SC Employment	ST Employment
	(1)	(2)	(3)	(4)	(5)	(6)
Rainfall$_t$	−16.917	−203.975*	−2.173	−12.786*	−3.697	−4.056
	(11.070)	(116.185)	(2.403)	(6.664)	(2.678)	(2.758)
Rainfall$_{t-1}$	−30.604***	−336.593***	−7.032***	−18.609***	−5.669**	−5.362**
	(11.706)	(119.658)	(2.656)	(6.953)	(2.759)	(2.564)
District FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Observations	505	505	505	505	505	505
Clusters	141	141	141	141	141	141

Notes: Unit of observation district-year, with sample restricted to district-years post-NREGA adoption. Indicators of program are intensity are total employment (in 100,000 person-days; mean 51.27), total expenditure (in million rupees; mean 625.25), total public works projects (1,000 projects; mean 9.06), and employment for female (mean 21.63), scheduled caste (mean 14.23), and scheduled tribe recipients(mean 11.33), respectively. Rainfall variable is the log of annual mm, with standardized sample mean 0. In sample a within-district standard deviation in the natural log of rainfall was 0.25. Analysis estimated by OLS. Standard errors in parentheses adjusted for clustering within districts. *p<0.1; **p<0.05; ***p<0.01

Appendix

1. Database on Maoist Conflict Violence

Sources: To construct a database on Maoist conflict violence, we utilize multiple press sources: 1) two national English dailies: The Indian Express and The Hindu; 2) two regional English language newspapers: Times of India (Patna edition) and The Telegraph; 3) six regional language press sources: Eenadu, Hindustan, Prabhat Khabar, Deshbandhu, Harit Pradesh, Navbhara;, and 4) two wire services: PTI and IANS.

Coding: Over two years for each state a team of researchers examined archives of daily editions of the media sources above for mentions of Maoist conflict incidents. A copy of each story was sent to a core team for coding to ensure consistency. For each incident the following event details were recorded: 1) date (month and year), 2) location (district and village), 3) type of incident (bomb explosion, kidnapping, etc.), 4) civilian casualties, 5) Maoist casualties, 6) security personnel casualties.

A frequently used phrase in reportage is 'suspected Maoists' and 'Maoist sympathisers'. We have coded these as civilians. Another issue relates to the Salwa Judum, an anti-Maoist militia supported by the government of Chhattisgarh. Since members of this militia had the status of Special Police Officers (SPOs) and received training and wages from the Chhattisgarh state government we have coded them as security forces.

Comparison to SATP: Our dataset goes back farther in time, to 1999, than does the widely used South Asia Terrorism Portal dataset on Maoist conflict violence, which begins in 2005. Because we utilize multiple press sources, including those in local languages, we also measure a significantly higher number of casualties than does the SATP dataset, which is based upon national English-language press sources. Between 2005 and 2009, we measure 4783 total deaths, compared to just 3509 in the SATP dataset in the six red belt states under analysis. We measure 36% more civilian deaths, 38% more security personnel deaths, and 36% more Maoist deaths.

Table A1: OLS Estimates of Effect of NREGS Adoption on Maoist conflict violence

	Dependent variable:					
	Total Incidents			Total Deaths		
	(1)	(2)	(3)	(4)	(5)	(6)
Long-run	-3.673^{**}	-3.894^{**}	-4.274^{**}	-6.756^{*}	-10.149^{**}	-6.969
	(1.630)	(1.626)	(1.919)	(3.601)	(4.837)	(4.673)
$NREGS_t$	-1.143	-1.275	-1.321	-2.409	-2.864	-2.619
	(0.950)	(0.913)	(1.241)	(2.489)	(2.364)	(3.330)
NREGS_{t-1}	-2.531^{***}	-2.142^{***}	-2.953^{***}	-4.347^{***}	-3.011^{**}	-4.350^{**}
	(0.898)	(0.821)	(0.918)	(1.655)	(1.485)	(1.779)
Y_{t-1}		0.123^{***}			0.421^{***}	
		(0.009)			(0.058)	
State × Year FE			Y			Y
Year FE	Y	Y		Y		
District FE	Y	Y	Y	Y	Y	Y
BI × Year	Y	Y	Y	Y	Y	Y
Observations	1,410	1,410	1,410	1,410	1,410	1,410
Clusters	141	141	141	141	141	141

Notes: Unit of observation is district-year, 2000-2009. Total incidents represents total incidents of Maoist violence. Total deaths represents sum of civilian, Maoist, and security force deaths. Long-run is sum of coefficient on contemporaneous treatment variable $NREGS$ and on lagged treatment variable NREGS_{t-1}, divided by $1-$ coefficient on Y_{t-1} in the case of specifications controlling for a lagged value of dependent variable. Analysis estimated by OLS. Standard errors in parentheses adjusted for clustering within districts. See table 3 in text for parallel Poisson QML results. *p<0.1; **p<0.05; ***p<0.01

Table A2: The Effects of NREGS Adoption in Interaction with Rainfall Shocks

	Dependent variable:			
	Total Incidents		Total Deaths	
	(1)	(2)	(3)	(4)
NREGS	-0.653^{***}	-0.615^{**}	-0.532	-0.388
	(0.243)	(0.269)	(0.491)	(0.518)
NREGS_{t-1}	-0.856^{***}	-0.763^{***}	-1.457^{**}	-1.357^{**}
	(0.301)	(0.278)	(0.646)	(0.583)
Rain_t	-0.107		-0.258	
	(0.157)		(0.190)	
$\text{Rain}_t \times \text{NREGS}_t$	0.360		0.485	
	(0.418)		(0.845)	
$\text{Rain}_t \times \text{NREGS}_{t-1}$	1.369^{***}		1.392^{*}	
	(0.530)		(0.815)	
Rain_{t-1}		-0.415^{**}		-0.237
		(0.209)		(0.320)
$\text{Rain}_{t-1} \times \text{NREGS}_t$		0.552^{**}		1.155^{**}
		(0.274)		(0.550)
$\text{Rain}_{t-1} \times \text{NREGS}_{t-1}$		1.595^{***}		0.248
		(0.402)		(0.704)
District FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
BI × Year	Y	Y	Y	Y
Observations	1,410	1,410	1,410	1,410
Clusters	141	141	141	141
Rainfall Interactions F-stat	36.6^{***}	55.4^{***}	12.9^{***}	11.7^{***}

Notes: Unit of observation district-year, 2000-2009. Total incidents represents total incidents of Maoist violence. Total deaths represents sum of civilian, Maoist, and security force deaths. Rainfall variable is the log of annual mm, with standardized sample mean 0. In sample a within-district standard deviation in the natural log of rainfall was 0.25. See Figure 5 in text for plots of long-run effects of NREGS adoption as a function of same year and previous year rainfall shocks. Analysis estimated by Poisson QML model. Standard errors in parentheses adjusted for clustering within districts. *p<0.1; **p<0.05; ***p<0.01