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Renewable Natural Resource Shocks and Conflict Intensity: Findings from India's Ongoing Maoist Insurgency

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Abstract
An interesting stream of the civil conflict literature has identified an important subset of civil conflicts with disastrous consequences, that is, those that emerge as a consequence of shocks to renewable natural resources like land and water. This literature is, however, reliant on qualitative case studies when claiming a causal relationship leading from renewable resource shocks to conflict. In this article, we seek to advance the literature by drawing out the implications of a well-known formal model of the renewable resources–conflict relationship and then conducting rigorous statistical tests of its implications in the case of a serious ongoing civil conflict in India. We find that a one standard deviation decrease in our measure of renewable resources increases killings by nearly 60 percent over the long run.

Keywords
vegetation shocks, Maoists, conflict, instrumental variables

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The disastrous consequences of civil wars have by now been well documented in the academic literature. Civil conflict has severe consequences for economic welfare, whether expressed in lost growth or adverse impact on poverty or health. Cerra and Saxena (2008) have found in a cross-country study that output declines 6 percent in aftermath of civil war. Murdoch and Sandler (2002) demonstrate significant spillover effects for civil war on economic growth in neighboring countries. In a single country setting, Abadie and Gardeazabal (2003) show that civil conflict in the Basque region of Spain significantly depressed growth relative to neighboring regions. On poverty, Justino and Verwimp (2006) have found that 20 percent of Rwanda's population moved into poverty as a consequence of civil war in the 1990s. And focusing on health, Ghobarah, Huth, and Russett (2003) show how civil conflicts have adverse long-term consequences.

An interesting stream of the civil conflict literature has identified an important subset of civil conflicts with disastrous consequences, that is, those that emerge as a consequence of shocks to renewable natural resources like land and water.[1] The claim of this literature is that a decline in renewable resources that are essential for populations to survive generates intense economic incentives for conflict.[2] Such conflicts often result in large numbers of deaths as well as damage to the environment (Kahl 2006), which makes the relationship between renewable resource shocks and conflict worthy of systematic analysis. This literature is, however, reliant on qualitative case studies when claiming a relationship between renewable resource shocks and conflict. The empirical work is also not driven by a rigorous theoretical framework. In this article, we seek to advance the literature by drawing out the implications of a well-known formal model of conflict and then conducting rigorous statistical tests of the implications of this model for the relationship between renewable resources and conflict.

Our study generates its main empirical prediction from a highly regarded formal model of civil conflict by Chassang and Padró-i-Miquel (2009), which explicitly focuses on land productivity shocks. As we show subsequently, the econometric strategy that we adopt is closely tailored to match the environment of the model. To test the implications of the model, we use a proxy for land productivity shocks based on satellite images of land, one that is widely used by agricultural economists. In order to be able to conduct a thorough microanalysis, we create a new district-level data set of killings in a belt of four states in East Central India (Bihar, Jharkhand, Chhattisgarh, and Andhra Pradesh), where a large proportion of the population is dependent on land productivity for its livelihood and where there is an active Maoist insurgency in progress. We find that adverse shocks to land productivity cause substantial increases in the intensity of conflict, and a shock of one standard deviation to the productivity of land increases total conflict deaths by nearly 60 percent over the long run. Our findings thus not only serve to verify the claims of the case study literature but additionally allow us to use the implications of a formal model to generate quantitative estimates of the extent to which renewable resource shocks can contribute to killing. An important policy implication of our article is that insurance schemes that smooth the income flows of vulnerable populations from year to year may yield society the benefit of less conflict. We see these as the main contributions of the article.

Our focus on the Maoist conflict in East Central India offers the benefit of being potentially generalizable to the many areas in the developing world where the survival of citizens is severely threatened by shocks to renewable resources. The presence of substantial tribal populations, which rely heavily on forest products for survival, as well as the dominant presence of a rain-fed agricultural sector, which leaves the population extremely vulnerable to year-to-year shocks in land productivity, makes this region resemble many conflict-prone regions of Africa and Latin America in terms of potential vulnerability to conflict driven by renewable resource shocks. Our focus on one part of the world permits us to collect detailed data at an extremely micro level. Consequently, we are able to implement a rigorous microeconometric strategy that is closely tied to an explicit microtheoretical framework. Nonetheless, it is certainly the case that the external validity of our study is not the same as for a cross-country study. In effect, we are trading off external validity to a degree, in return for gaining greater internal validity. We see the civil conflict literature as already being rich in the opposite trade-off and much less so in the trade-off that we are making. We thus see our article as offering a useful complement to the long dominant cross-country orientation of the conflict literature.

Our research design has another implication driven by the expense of data collection. The Maoist conflict in East Central India has its origin in the 1970s. The prohibitive expense associated with micro-data collection served as a barrier to going back by decades when collecting data. This means that we cannot interpret our results as providing insights into the onset of civil conflicts. Rather our article is about the effect of renewable resource shocks on the intensity of an ongoing conflict (which we feel is an interesting subject in its own right).

Finally, it is important to emphasize up front that the focus of our project is on renewable resources, forest and agricultural land, and thus does not overlap with the concerns of studies that focus on nonrenewable resources such as oil. While this limits the breath of our findings, it once again contributes to the tightness of the research design.

The plan of the article is as follows. We first summarize the relevant literature. We then discuss the theory that motivates our article. The data—a novel aspect of our article—and the econometric strategy are the subject of the next section. We subsequently present our results and examine their robustness and follow-up with the results of a cross validation (CV) designed to demonstrate the study's external validity. We then conclude.

Literature Review

The Literature on the Resource Shocks, Opportunity Cost, and Conflict

Our article contributes to two broad streams of empirical literature on civil conflict. The first stream lies at the intersection of political science and economics by virtue of its use of sophisticated econometric techniques (in particular its careful concern with causal inference). This particular stream has been well summarized by Blattman and Miguel (2009). We refer readers to this article for a comprehensive review and only highlight some of the articles from this review that are most relevant to our article.

Since the channel through which land productivity shocks foment conflict is via an adverse shock to peoples' livelihoods, our article shares a close connection with political economy articles highlighting the relationship between income and conflict. Well-known articles that highlight the income–conflict relationship include Collier and Hoeffler (1998, 2004) and Fearon and Laitin (2003) who show that per capita gross domestic product (GDP) is correlated with conflict at the cross-country level. Our methods and approach differ from these articles. They derive their finding from cross-sectional variations in income where heterogeneity across entities renders the findings less credible than if the heterogeneity were controlled. Our subnational context does that to a great extent, since policy at the national level is naturally controlled. More fundamentally, our study focuses on the timing of conflict, which allows us to clearly demonstrate the causal connection between degradation of vegetation on the land and ebb and flow in the intensity of a continuing conflict. Our article is more similar to Miguel, Satyanath, and Sergenti (2004) who show that year-to-year changes in GDP are associated with changes in conflict. Our article differs from this article in directing attention to shocks to land productivity as a basic cause of the Indian insurgency, rather than GDP. In many parts of the country where the conflict continues, subsistence farming dominates livelihoods and a modern economy has yet to take shape. Hence, threats to land productivity are a more powerful reason for taking up arms than shocks to money income.

Our article is related to an emerging political economy literature that finds adverse international commodity price shocks to be a driver of conflict by exacerbating poverty of the population whose livelihoods depend on the consumption and/or production of these commodities. In this category, Besley and Persson (2008) examine the effects of changes in world market prices of exported and imported commodities. They find that these (plausibly exogenous) price changes are significantly associated with conflict. Dube and Vargas (2013) show that, in Columbia, decreases in the international price of coffee—a labor-intensive good, and therefore whose cultivation employs large numbers of rural labor—are associated with more conflict, while decreases in the international price of oil—a capital-intensive good and therefore whose production is not labor sensitive—are associated with less conflict. They interpret these results in the spirit of the theoretical model of Dal Bó and Dal Bó (2011). Shocks to international commodity prices, in turn, get reflected in the incomes and livelihoods of those employed in the production of those commodities. In the study by Dube and Vargas, adverse coffee price shocks decreased the *opportunity cost* of fighting and therefore provoked conflict. The decrease in the price of oil, on the other hand, lowered the value of an appropriable good (i.e., oil) and reduced the incentive to fight over it. Also related are the findings of Verwimp

(2003, 2005) and Humphries and Weinstein (2008), who demonstrate poverty as a driver of the Rwandan and Sierra Leone conflicts, respectively. We note that, differently from these articles, we capture the relationship between shocks to land productivity and conflict.

The second relevant stream is the one referred to in the first section. We highlight the contributions that are most proximate to our article from the insightful review of this literature by Mildner, Lauster, and Wodny (2011). They divide the political science literature on natural resource shocks and conflict into two broad categories. One category focuses on the effects of resource deprivation on conflict—largely in the realm of renewable resources like agricultural land, while the other focuses on resource curse effects—largely in the realm of nonrenewable resources like oil. Our article falls in the former category. Qualitative scholars who have focused on renewable natural resources, including Homer-Dixon (1994, 1999) and Kahl (2006), emphasize the threats presented by depletion of croplands, forests, water, and fish stocks to peoples' livelihoods. They argue that such adverse resource shocks force people to fight for survival, resulting in huge human and economic costs. Whereas authors of this school primarily rely on case studies to justify their claims, their broad claims have also found support in some statistical studies, for example, Hauge and Ellingsen (1998) and Theisen (2008). However, in sharp contrast to the political economy literature mentioned earlier in this section, these articles lack compelling strategies for addressing endogeneity concerns.

In sum, we have one stream of literature that is econometrically sophisticated but does not address land productivity shocks, while another addresses land productivity shocks but is not econometrically sophisticated. Our article fills this gap in the literature by applying state-of-the-art econometric techniques to study the relationship between land productivity shocks and conflict.

The Literature on the Conflict in East Central India

While India has had a history of radical peasant movements (SinghaRoy 2004), most observers trace the roots of the current conflict in East Central India to the Maoist "Naxalite" movement, which originated in 1967 as an anti-landlord peasant uprising in Naxalbari, a village in the state of West Bengal. It was put down by the government, but over the next two decades the Maoists re-grouped in other states in East Central India with significant populations of economically deprived tribal peoples. The goal was to re-start the struggle to topple the government from an environment where Maoist ideology might have relatively high appeal. The conflict began intensifying in the early 2000s—the period of our study—after the merger of several extremist groups that shared a broadly similar ideology.[3] The apparent impulse for unification came from the Nepalese Maoists who were successful in gaining national office in their country with a coordinated strategy.[4] According to official Indian government data, the conflict has resulted in 7,862 deaths in the period 2000–2009.[5] The Online Appendix contains details on this conflict.

There is a growing academic literature on the abovementioned conflict. Borooah (2008) examines which socioeconomic variables explain the existence of Maoist activity in some districts of India but not others using data from the Indian Planning Commission and South Asian Intelligence Review. The dependent variable is the likelihood of violence. The main findings are that the probability of a district being Maoist affected rises with an increase in its poverty rate and falls with a rise in its literacy rate and that Maoist activity in a district reduces the overall level of violent crime and crimes against women. These results are from cross-sectional ordinary least square (OLS) regressions at the district level and pool across heterogeneous regions. Iyer (2009) examines terrorist incidents in general, where terrorist activity includes separatist movements, communal violence, and the Maoist insurgency. She too finds a cross-sectional relationship between violence and poverty. Sen and Teitelbaum (2010) examine the effects of mining on conflict and conclude that the geographical spread of Maoist movement is simply too wide to be accounted for by mining activity. Hoelscher, Miklian, and Vadlamannati (2011) analyze cross-sectional data from six Indian states, finding that conflict increases with forest cover, prevalence of conflict in neighboring districts, and the population share of scheduled castes (SCs) and scheduled tribes (STs). Gomes (2011) looks at landholdings and historical land institutions and finds a strong effect of land inequality on Maoist violence. Vanden Eynde (2011) examines the strategic choices of targets and the intensity of violence of Maoist insurgents and finds a reduced form effect for rainfall on conflict.

None of these articles directly addresses land productivity shocks that are the focus of this article. Further, they use data sets that are strongly urban in their bias, which is at odds with a conflict that is primarily rural. As we describe subsequently, our new data set was born out of this concern and addresses it seriously. Finally, except for Vanden Eynde (2011), these articles fail to address questions of econometric identification that are central to establishing a causal relationship.

Theory

Chassang and Padró-i-Miquel (2009) offer a rigorous theoretical exposition of the relationship between shocks to the productivity of land and civil conflict, and we use this article to motivate our hypotheses as well as econometric strategy. In the Chassang and Padró-i-Miquel model, control over a piece of land is divided between two groups. Both groups can choose to either grow crops or devote their resources to fighting for the other group's land. Land productivity varies from period to period. The groups decide whether to bargain or fight after observing shocks to the productivity of land. The decision to fight or not fight is based on a comparison of the expected value of fighting and not fighting, both of which are a summation of the return from the first period (which in the case of fighting takes into account the cost of fighting and the probability of winning) and the returns from the land in the future. The main implication of the model is that "war occurs if current economic circumstances are bad enough, independently of expected future play" (p. 221). They describe their intuition as follows

> when t is small [i.e. in periods when the productivity of land is relatively low], the opportunity costs of fighting are small because returns from labor are meager. However, the future value of a victory remains constant because it depends on the expected future returns to land. If returns to labor – that is, the opportunity cost to workers – are low enough, groups want to unilaterally defect from peace, making war inevitable. Thus, in the presence of a drought, groups do not fight for the meager returns to land today. Rather, they fight today to capture land that will be valuable tomorrow, when the drought ends. They fight precisely because current returns to land (and therefore labor) are small enough that they make capturing additional land for the future very tempting. (p. 223)

Our main testable hypothesis drawn from the theory is:

> **Hypothesis 1:** Adverse shocks to the productivity of land are associated with more conflict.

Our econometric strategy takes into account several important points that are made in the Chassang and Padró-i-Miquel's article. First, they point out that the cross-country literature on civil war suffers from omitted variable bias, emanating from the exclusion of the repressive capacity of the state from regression specifications (Chassang and Padró-i-Miquel 2009, 220). No doubt, this is on account of the difficulty of measuring national repressive capacity. We are able to address this critical concern by conducting our analysis on subnational units (districts) within one country (India), in a context where the capacity to repress remains overwhelmingly in the hands of the national government. Second, they point out that a correct test for the effects of transitory shocks on conflict is inevitably based on examining *within variation* (Chassang and Padró-i-Miquel 2009, 214). In line with this, we employ district fixed effects in all our specifications. We also include time dummies in all our specifications to address secular changes in the repressive capacity of the national government that would apply to all districts.

Note that the causal mechanism of the Chassang and Padró-i-Miquel model passes from shocks to land productivity, through income, to conflict. In the absence of subnational GDP data, our analysis should then focus on the part of the country where income is closely linked to the productivity of land. Such a region is one where the population is primarily dependent on agricultural employment or on the sale of forest products for their livelihood. Accordingly, we focus on four contiguous states in East Central India that are heavily populated by tribal populations that rely on agriculture and the forest to survive[6] (see Online Appendix 1 for details). Because of this design of our study, we are confident that a negative association between land productivity shocks and conflict in districts of these states is plausibly interpreted as being consistent with Chassang and Padró-i-Miquel's causal mechanism, which emphasizes the opportunity cost of fighting.

Our decision to focus on conflict within a single country also has implications for what measure of conflict we place on the left-hand side of our regressions. Most of the statistical literature on civil war is cross country in nature. This literature uses threshold values of deaths (25 or 1,000) to generate dummy variables that indicate the *onset* or the incidence of civil war in any given year. However, there is no obvious justification for using the same thresholds when an analysis is conducted at the subnational level (since deaths per district are likely to be much lower than deaths at the national level). Rather than creating some arbitrary threshold of our own to capture the onset or incidence of conflict, we have created a more refined continuous measure of the number of conflict deaths in a district year. In effect, what we are trying to understand is not the determinants of the initiation of hostilities but rather the determinants of changes in the intensity of an ongoing conflict.

Econometric Strategy and Data

Econometric Strategy

A key aspect of our econometric strategy is the choice of a variable that captures variations in land productivity. Poorer crop yield and a less rich forest cover plausibly go with more sparse vegetation as a visual counterpart. Confirming this intuition, several studies by climate scientists, for example those by Labus et al. (2002) and Boschetti et al. (2009), show a strong relationship between satellite images of vegetation density and land productivity. Based on this foundation, such satellite measures, especially the normalized difference vegetation index (NDVI) measure that we describe in detail subsequently, have come to be widely used by agricultural economists to capture land productivity. In line with the practice of agricultural economists, we use the NDVI measure of the density of vegetation as our proxy for land productivity. The econometric model we adopt for our empirical investigations is:

$$\begin{aligned}\ln(\text{TotalDeaths})_{i,t} = \varphi\ \ln(\text{TotalDeaths})_{i,t-1} &+ \beta_1 \text{Vegetation}_{i,t} \\ &+ \beta_2 \text{Vegetation}_{i,t-1} + \beta_3 \text{Vegetation}_{i,t-2} \\ &+ \text{Year fixed effects} + \text{District fixed effects} + e_{i,t},\end{aligned} \tag{1}$$

where i indexes district and t, the year. After accounting for the year and district fixed effects, the error term $e_{i,t}$ is identically and independently distributed normally. All models are estimated with robust standard errors clustered at the district level. Cross-sectional regressions may capture the effect of unobserved institutions rather than the impact of a theoretically well-defined channel. With district dummies, the coefficient estimates from equation (1) are based on within-district variation in the data and control for a variety of unobserved differences across districts and states.

Year fixed effects account for macro-events impacting all districts in a given year, for example, inflation. The lagged dependent variable captures dynamics in the dependent variable. If the dependent variable is serially correlated, say as an AR(1) process, then ignoring the dynamics would violate the assumption of no serial correlation in the errors and incorrect standard errors on the coefficients. The lagged vegetation variables flexibly incorporate a distributed lagged effect of vegetation shocks on the intensity of violence.

While we would like to believe variation in vegetation to be exogenous, and the regression as specified in equation (1) to be capturing the causal effect of vegetation shocks on conflict, we must consider the possibility that vegetation is plausibly endogenous. If, in order to facilitate counterinsurgency operations, government security forces destroy forests in locations where conflict is anticipated, then the same relationship that we postulate holds, but due to reverse causality from conflict to lagged vegetation. Such scorched earth tactics are in evidence in conflicts around the world, and there may be other channels through which the error term (shocks to conflict) is correlated with shocks to vegetation. We employ an instrumental variables (IV) method for dealing with the endogeneity problem using rainfall to instrument for vegetation. Rainfall shocks are very likely exogenous to conflict. By separating the extent to which variation in vegetation is caused by rainfall but not other factors, we are able to use this exogenous source of variation in vegetation to establish a causal link from natural resource shocks to conflict intensity. The IV version of equation (1) is used to quantify the causal impact. In IV regressions, vegetation and its two lags are instrumented using rainfall and its three lags.

Data

Killings. We have mentioned that a principal contribution of the article is the creation of a new data set of killings—our measure of intensity of conflict—in the Maoist belt. Our CASI (Center for the Advanced Study of India) data set has been compiled from multiple media sources. Two national English dailies—the *Indian Express* and *The Hindu*—have covered the Maoist issue consistently, but they provide only partial coverage of specific incidents. We drew on another ten media sources, namely, two additional English-language newspapers that have a regional base: *Times of India* (Patna edition) and *The Telegraph*; six regional-language press sources: *Eenadu*, *Hindustan*, *Prabhat Khabar*, *Deshbandhu*, *Harit Pradesh*, and *Navbharat*; and two wire services: Press Trust of India and Indo-Asian News Service. Creating a reliable data set required substantive effort in overcoming barriers with regard to access, cost, and quality. We took the (costly) decision to have our team go through multiple sources of news for each state, ensuring as much overlapping coverage as possible. More details about the process and these efforts are described in Online Appendix 2.

In the CASI, we separately measure deaths of security forces, civilians, and Maoists due to conflict. The demarcation between civilians and Maoists is not always clear-cut, given the Maoist strategy of mixing in with the local population and government's tendency of sometimes classifying civilians killed by security forces as Maoists. Security forces personnel deaths are not subject to such issues. The data set also includes a measure of all these deaths combined, called TotalDeaths, which serves as our measure of overall conflict intensity.

The CASI improves on previously available data sets by not focusing exclusively on English-language sources. Measuring deaths in the largely rural Maoist conflict exclusively from an urban-oriented English-language press would err, sometimes grossly, on the side of underreporting. The official source on Maoist-related casualties is released by the Ministry of Home Affairs of the Government of India. However, this data set is available only at the state level. Other data sources are the Rand-MIPT Terrorism Incident database, data from the Worldwide Incidents Tracking System (WITS) from National Counter Terrorism Centre, and data from the South Asian Terrorism Portal (SATP). The Rand-MIPT and the WITS data sets are worldwide. Their Indian data are ad hoc and do a poor job at capturing Indian data reported by the non-English-language press. The SATP data set has been assembled by the Institute for Conflict Management, available at their Web site. These data are based primarily on reports in the major English press but does a considerably better job than the other two, especially in more recent years. However, this data set begins in 2005 and is not as comprehensive as ours, which goes back to 2000.

The improvement in the CASI data over other databases is depicted in Figure 1.[7] Other data underestimate the actual number of deaths. The oft-used SATP data, which are based on English-language sources, and therefore has an urban bias, underreport considerably compared to our data. That said, we subjected our analysis to SATP district-level data and verified that our results hold with SATP data (more on this subsequently).

Many studies of the Maoist movement use state-level data. The district, however, is the relevant unit of analysis because conflict is locally confined to regions where Maoists find the space to induce a deprived population to collectively organize. Because of their distinct identities, local populations do not usually migrate to other districts. Their identities are closely connected with their lands, which is why Maoists are able to convince tribals and local populations to join their insurgency.

Rainfall. Rainfall data are from the high-resolution ($1^\circ \times 1^\circ$ latitude/longitude) gridded daily rainfall data set for the Indian region reported by the India Meteorological Department (IMD). The daily rainfall data are archived at the National Data Centre, IMD, Pune. IMD operates about 537 observatories (Rajeevan et al. 2006). In addition, most state governments maintain rain gauges for real-time rainfall monitoring. IMD has rainfall records of 6,329 stations over varying periods. Of these, 537 are IMD observatory stations, 522 are in the hydrometeorology program, and 70 are agromet stations. The rest are rainfall-reporting stations maintained by state governments. Rajeevan et al. show that this data correlates well, both spatially and intertemporally, with the VASClimo data set, a global gridded rainfall data set constructed in Germany. We match district capitals in the sixty-eight regions in the

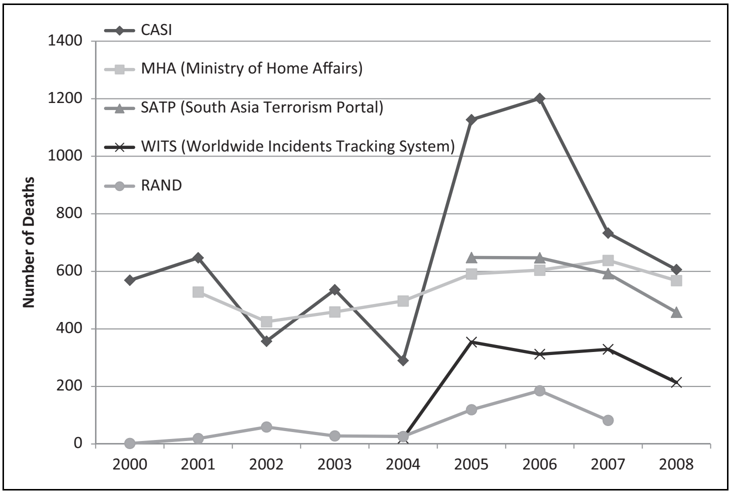

Figure 1. Deaths related to Maoist violence in major Maoist states (Andhra Pradesh, Bihar, Jharkhand, and Chhatisgarh): Comparison of CASI, MHA, SATP, WITS, and Rand data sets.
Note: CASI = Center for the Advanced Study of India; MHA = Ministry of Health Affairs; SATP = South Asian Terrorism Portal; WITS = Worldwide Incidents Tracking System.

four Maoist belt states to the nearest rain station and ascribe that rain data to the district. The daily rainfall data are then aggregated to annual data.

Vegetation. Satellite imagery is now widely used in the sciences to track changes in vegetation and forest cover (e.g., Myneni et al. 1998; Tucker et al. 2001; Nemani et al. 2003).[8] We use the NDVI to measure annual change in vegetation for Indian districts. The NDVI data are derived from visible infrared and near-infrared data acquired from the moderate resolution imaging spectroradiometer (MODIS) sensor on National Aeronautics and Space Administration (NASA) satellites. The NDVI index is computed as $\text{NDVI} = (\text{NIR} - \text{VIS})/(\text{NIR} + \text{VIS})$, where NIR is the near-infrared band value and VIS is the visible light or the red band value recorded by the satellite sensor. The NASA site explains the computation as follows: Healthy vegetation absorbs most of the visible light that hits it and reflects a large portion of the near-infrared light, while unhealthy or sparse vegetation reflects more visible light and less near-infrared light.[9] For a given grid, the NDVI ranges in value from -1 to $+1$. The fact that NDVI is a good measure of forest cover is affirmed by D'Arrigo et al. (2000) who show a high correlation between NDVI and direct measures of forest density from tree rings. We mapped the data from MODIS into $1° \times 1°$ latitude/longitude grids for India. In each grid, the monthly NDVI data are averaged to obtain the mean annual NDVI. They are then mapped into the sixty-eight districts as was done for the rainfall data. NASA images for India from 2007 to 2008 are depicted in Online Appendix Figures A1.1–A1.4. The contrast between sparse and dry vegetation in the premonsoon season and the fecund vegetation after the monsoon rains is clearly evident.

Consumption and control variables. For robustness, we also report results using consumption expenditures sourced from the annual National Sample Surveys (NSS) conducted over the 2000–2008 period. Monthly per capita expenditure (MPCE) at the household level collected in these surveys are averaged using sample-proportionate-to-population weights to calculate MPCE in each of the sixty-eight districts. Matching our figures with those in the NSS summary reports produced by the Government of India verified the accuracy of our MPCE calculations.

We could, as many studies have done, rely exclusively on MPCE to proxy poverty and draw our central conclusions from that measure. To do so would be on thinner ground for at least two reasons. First, MPCE can be a noisy measure at the district level where NSS samples are as small as .01 percent of the population of households. Second, debates about Indian poverty-line calculations from MPCE data have exposed the fact that poverty measures from the MPCE are less related to calorific intake which they purport to measure and more related to current prices (e.g., Deaton and Drèze 2009; Patnaik 2010). The within-district variation in MPCE (conditional on the fixed effects) can be small if, for example, sampling error variance as a proportion to total variation in MPCE is high, or if the variation in MPCE is due mainly to price variation but not variation in calorific intake. The consequence is downwardly biased estimates. We report results using MPCE as supporting evidence of our core results.

The literature has attempted to attribute the Maoist conflict to a variety of factors including mining activity in this mineral-rich region of India, the predominance of persons belonging to socially marginalized communities (SCs and STs), worsening inequality, and conflict spilling over from neighboring regions. We control for each of these influences. The value of bauxite and iron ore mined in each district is measured annually from Indiastat.com and compiled using the Indian Mineral Year Book annual district production figures published by the Indian Bureau of Mines. The proportion of each district's population that is SC and ST is from the NSS, and the district-level MPCE is used to calculate the Consumption GINI. Finally, the number of the two closest districts that have experienced Maoist violence in the past year captures spillover effects.

To sum up, a comprehensive data set has been assembled for the sixty-eight districts in the four Maoist belt states of Andhra Pradesh, Bihar, Chhattisgarh, and Jharkhand annually over 2001–2008. By incorporating dynamics and using up to three lags, model coefficients are estimated from 340 observations. They constitute a balanced panel of the sixty-eight districts over the period 2004–2008. Online Appendix Table A1 provides descriptive panel statistics for all variables used in the analysis. The data are available upon request from the authors. Finally, Figure A2 in the Online Appendix plots a time series of total killings aggregated across districts and the vegetation index NDVI. The plots suggest, even in the aggregate data, an inverse relationship—the number of killings is higher when vegetation is denuded. Online Appendix Figure A3 indicates that rainfall is a plausibly good instrument for vegetation due to the strong positive association of vegetation with rainfall and lagged rainfall. We will exploit these relationships in the econometric analysis.

Analysis

Core Results

We start by reporting the association between vegetation and total deaths (= Security + Maoist + Civilian Deaths) in Table 1 from three models: log-linear OLS, negative binomial (NB) since the dependent variable is a count variable,[10] and an error correction model (ECM) which makes the short- and long-term effects clear and obvious.[11] Column 1 shows that vegetation is negatively associated with total deaths and the relationship is significant at the 1 percent level. The ECM in equation (2) (see endnote 11) and the distributed lag model in equation (1) should provide the same inference about short- and long-run impacts of vegetation shocks. The third column in Table 1 reports estimates from the ECM. The immediate short-run impact of a negative vegetation shock of .01 is estimated to increase TotalDeaths by $-13.99 \times .01 = 14$ percent in both, the distributed lag model in equation (1) and the ECM in equation (2), see note 11. The *long-run* impact of the same shock is estimated to be $(.01) \times (-13.99 - 7.86 - 7.83)/(1 - .08)$ using estimates in the OLS column, and $(.01) \times (-29.67/(.920)$ using estimates in the ECM column. Both calculations lead to the same long-run impact, that is, a 32.3 percent increase in TotalDeaths. The count data NB model in the second column affirms the negative association, estimating the long-run impact at 47.3 percent.[12]

In columns 4–6, we add controls described in Theory section. Their inclusion improves the model fit without altering the negative association between vegetation and the intensity of conflict. Of the control variables, the spillover variable (Neighborhood2) and the proportion that is tribal (ST) are positively associated with total killings.

To strengthen support for a causal relationship between vegetation and total deaths, we now examine only that part of the variation in vegetation that is captured by shifts in rainfall. This instrumental variables strategy is an improvement in the results given in Table 1 because rainfall (unlike vegetation) is not subject to reverse causality from conflict. Properly instrumenting for vegetation with rainfall would be justified by a strong first-stage relationship between them. Table 2 empirically endorses the strong positive relationship between once lagged rainfall and vegetation, affirming the case for rainfall as a good instrument. The Kleibergen–Paap statistic of 10.19 indicates no weak instruments (WI) problem (Stock and Yogo 2004). Hansen's test cannot reject the joint null that rainfall and its lags are uncorrelated with the error term and are therefore valid instruments and that rainfall and its lags

Table 1. Renewable Resource Shocks and Killings

OLS and NB models (uninstrumented)

	No controls			With controls		
	Dependent variable			Dependent variable		
	ln(TotalDeaths)	TotalDeaths	Δln(TotalDeaths)	ln(TotalDeaths)	TotalDeaths	Δln(TotalDeaths)
	OLS	NB	ECM	OLS	NB	ECM
ln(TotalDeaths)$_{t-1}$	.0802	.0944	−.920***	.067	.086	−.933***
	[.0693]	[.115]	[.069]	[.067]	[.123]	[.067]
Vegetation$_t$	−13.99***	−21.87**		−12.38**	−17.74*	
	[4.732]	[8.923]		[4.823]	[9.445]	
Vegetation$_{t-1}$	−7.857*	−16.48*	−29.67***	−7.103	−12.57	−26.94***
	[4.565]	[9.481]	[10.01]	[5.014]	[10.26]	[10.03]
Vegetation$_{t-2}$	−7.827	−14.33		−7.449	−14.68	
	[5.567]	[12.35]		[5.477]	[13.14]	
ΔVegetation$_t$			−13.99***			−12.38**
			[4.731]			[4.823]
ΔVegetation$_{t-1}$			7.827			7.449
			[5.567]			[5.477]
Neighborhood2				.367***	.546***	.367***
				[.091]	[.168]	[.091]
Proportion SC				.765	1.854	.765
				[.763]	[1.984]	[.763]
Proportion ST				1.565**	2.719*	1.565**
				[.645]	[1.513]	[.645]
Consumption GINI				1.079	.866	1.079
				[.875]	[1.715]	[.875]

Value of mining output				.037*	.044	.0370*
				[.022]	[.034]	[.022]
N	340	340	340	335	335	335
R^2	.130	.205	.503	.189	.214	.543
K	77	77	77	82	82	82
α		1.098***			1.029***	
$z_{(\Sigma\ VEG)}$	−2.964***	−2.742***	—	−2.687***	−2.213**	—

Note: Robust standard errors clustered (by district). All models include district fixed effects and year dummies. NB models estimated with true fixed effects (in the model). Negative binomial (NB) models: Pseudo R^2 reported. α is the overdispersion parameter in NB model. $\alpha > 0$ indicates overdispersion and is NB appropriate; $\alpha = 0$ indicates Poisson is appropriate. Error correction model (ECM): Dependent variable is Δln(TotalDeaths). See equation (2) in note 11. Within-R^2 reported. All coefficients are to be interpreted as in a log-linear model. $z_{(\Sigma VEG)}$ tests the hypothesis: $\text{Vegetation}_t + \text{Vegetation}_{t-1} + \text{Vegetation}_{t-2} = 0$. OLS = ordinary least squares; SC = scheduled castes; ST = scheduled tribes.

***$p < .01$.

**$p < .05$.

*$p < .1$.

Table 2. First Stage for IV Results

Dependent variable: lags of vegetation index (NDVI)

	No controls			With controls		
	Vegetation$_t$	Vegetation$_{t-1}$	Vegetation$_{t-2}$	Vegetation$_t$	Vegetation$_{t-1}$	Vegetation$_{t-2}$
ln(TotalDeaths)$_{t-1}$	.0002	−.001	−.001	.00009	−.0008	−.001
	[.001]	[.001]	[.001]	[.0007]	[.0008]	[.0007]
Rain$_t$	−.104	−.0144	−.652***	−.097	−.033	−.599**
	[.243]	[.247]	[.247]	[.245]	[.252]	[.250]
Rain$_{t-1}$	1.393***	.0828	−.369*	1.370***	.0865	−.363*
	[.211]	[.238]	[.216]	[.212]	[.244]	[.217]
Rain$_{t-2}$	.272	1.585***	.0688	.267	1.575***	.0652
	[.225]	[.223]	[.254]	[.220]	[.225]	[.255]
Rain$_{t-3}$	1.189***	.419	1.349***	1.097***	.374	1.282***
	[.254]	[.256]	[.253]	[.261]	[.267]	[.258]
Neighborhood2				−.003**	−.001	−.001
				[.001]	[.001]	[.001]
Proportion SC				.003	.001	.007
				[.007]	[.008]	[.008]
Proportion ST				.008	.003	.006
				[.009]	[.009]	[.008]
Consumption GINI				−.025*	.003	.004
				[.015]	[.015]	[.014]
Value of mining output				.0001	−.0001	.0006**
				[.0003]	[.0004]	[.0003]
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
N	340	340	340	335	335	335
R^2	.218	.222	.206	.253	.229	.221
k	78	78	78	82	82	82
Weak instrument diagnosis						
Partial R^2	.14	.134	.158	.134	.13	.142
First-stage *F*	12.86	13.61	14.55	11.85	12.96	11.9
Kleibergen–Paap (WI)	10.19			9.41		

Note: Robust standard errors clustered (by district). Models include sixty-eight district fixed effects and five-year dummies. IV = instrumental variables; NDVI = normalized difference vegetation index; SC = scheduled caste; ST = scheduled tribe; WI = weak instruments.
***$p < .01$.
**$p < .05$.
*$p < .1$.

are correctly excluded in the estimated equation (exclusion restrictions and the Anderson–Rubin (A-R) test are further discussed subsequently).

Our core IV results are presented in Table 3. The first column confirms the negative relationship between vegetation and total deaths seen in the uninstrumented regressions, providing the basis for a causal claim relating less vegetation to more killings. In the IV model, twice lagged vegetation is statistically significant, revealing an effect that is not just contemporaneous. However, tests of equality of coefficients of contemporary, once lagged, and twice lagged vegetation fail to reject equality, implying that we cannot read into the results that the twice lagged effect is statistically larger. The main difference in the IV results is that the long-run impact of vegetation shocks is twice as large. A *negative* one standard deviation shock (= .011) to vegetation increases TotalDeaths by $[(-11.54 - 8.831 - 38.45)/(1 - .040)] \times (-.011)$ or 67 percent. With controls (column 2), the long-run impact of the shock on total killings is 58.2 percent. These are our core results, and they provide strongly supportive evidence for hypothesis 1.

It is worth mentioning that the causal impact of vegetation shocks on killings is strongly in evidence even if we use only contemporaneous vegetation, without lags. Therefore, the results are not driven by the use of the lags. Their inclusion helps us understand how the lagged effect may be distributed and to accurately calculate the long-term impact of vegetation shocks. Also notable in Table 3 is that the proportion of the population that is tribal is a statistically significant control variable, providing some support for an ethnic grievance argument.

In the remaining columns of Table 3, we examine the results when we break out Maoist, civilian, and security force deaths. This is useful in determining the category of deaths for which the vegetation–conflict link is most robust. The strongest results are for security force deaths inflicted by Maoist rebels: the long-run impact of a shock that denudes vegetation by one (within) standard deviation is to increase security force deaths by 40 percent. For civilian deaths, even though the individual vegetation coefficients are not statistically significant, their total impact—and hence the long-run impact of a vegetation shock—is statistically significant at 5 percent. Maoist deaths, inflicted by security forces and private militias, are also importantly determined by vegetation shocks.[13] We draw the implication that the impact of vegetation shocks is evident even for these types of deaths. The opportunity cost hypothesis is plainly evident, especially in the case of rebel-initiated killings of security forces.

Robustness

Count data models. Table 4 repeats these models, but using a two-stage NB model in which vegetation is instrumented in the first stage. The results are robust to this model change.[14] The same adverse one standard deviation vegetation shock that reduces vegetation by 0.011 increases total deaths by 65 percent, and Maoist inflicted deaths of security and militia forces by 75 percent. Overdispersion in the data is indicated by statistically significant estimates of the parameter α, which

Table 3. Renewable Resource Shocks and Killings: OLS-IV with Rainfall Instruments

Dependent variable	ln (Total Deaths)		ln (Civilian Deaths)	ln (Maoist Deaths)	ln (Security Deaths)
Second stage					
ln(*Dependent variable*)$_{t-1}$	.040	.0199	−.0163	−.0372	−.0814
	[.078]	[.078]	[.08]	[.077]	[.093]
Vegetation$_t$	−11.54	−7.445	−10.89	−.334	−14.13
	[14.28]	[14.72]	[10.50]	[12.74]	[9.314]
Vegetation$_{t-1}$	−8.831	−3.724	−11.88	−3.624	1.824
	[13.31]	[13.28]	[9.745]	[11.21]	[8.960]
Vegetation$_{t-2}$	−38.45***	−40.65***	−16.04	−26.78**	−21.70**
	[14.49]	[15.27]	[10.55]	[13.19]	[9.769]
Neighborhood2		.324**	.365***	.214*	−.067
		[.131]	[.119]	[.122]	[.106]
Proportion SC		1.022	.409	−.334	−14.13
		[.710]	[.541]	[12.74]	[9.314]
Proportion ST		1.606**	1.132*	−3.624	1.824
		[.796]	[.634]	[11.21]	[8.960]
Consumption GINI		1.409	.944	−26.78**	−21.70**
		[1.181]	[.967]	[13.19]	[9.769]
Value of mining output		.069	.021	.068**	.072*
		[.048]	[.045]	[.033]	[.039]
N	340	335	335	335	335
k	77	82	82	82	82
$z_{(\Sigma\ \mathrm{VEG})}$	−3.276***	−2.694***	−2.542**	−2.027**	−2.383**
p Value$_{(\Sigma\ \mathrm{VEG})}$	.001	.001	.011	.044	.018
First stage					
#Instruments	4	4	4	4	4
Kleibergen–Paap (WI)	10.19	9.409	8.914	9.642	10.04
Hansen's *J*	.060	.003	7.949	.508	.684
Hansen's *J* (*p*-value)	.807	.957	.005	.476	.408
A-R	3.547***	2.845**	3.141**	1.745	2.929**
A-R (*p* value)	.008	.025	.015	.141	.021

Note: Robust standard errors in brackets. Clustered (by district) in all models. $z_{(\Sigma \mathrm{VEG})}$ tests the hypothesis: Vegetation$_t$ + Vegetation$_{t-1}$ + Vegetation$_{t-2}$ = 0. A-R = Anderson–Rubin; IV = instrumental variables; OLS = ordinary least squares; SC = scheduled caste; ST = scheduled tribe; WI = weak instruments.
***p < .01.
**p < .05.
*p < .1.

Table 4. NB with Rainfall IV: Renewable Resource Shocks and Killings

	Dependent variable							
	ln(TotalDeaths)		ln(CivilianDeaths)		ln(MaoistDeaths)		ln(SecurityDeaths)	
ln(Dependent variable)$_{t-1}$	.104	.091	.0362	.0371	−.0824	−.0894	−.128	−.176
	[.105]	[.107]	[.173]	[.168]	[.137]	[.113]	[.232]	[.189]
Vegetation$_t$	−15.05	−8.313	−31.51	−33.69	8.53	16.21	−54.78	−42.9
	[24.11]	[26.12]	[38.07]	[36.27]	[29.32]	[29.01]	[52.01]	[59.06]
Vegetation$_{t-1}$	−25.17	−4.906	−53.31	−22.58	15.87	16.96	−25.39	−11.92
	[30.76]	[31.29]	[46.15]	[42.54]	[39.93]	[41.74]	[47.24]	[51.81]
Vegetation$_{t-2}$	−69.15**	−74.03**	−54.46*	−34.53	−72.94*	−82.21*	−75.83	−94.67**
	[31.68]	[31.35]	[29.55]	[36.57]	[43.41]	[42.77]	[51.50]	[47.67]
Neighborhood2		.514**		.647*		.601*		−.211
		[.215]		[.349]		[.329]		[.498]
Proportion SC		2.338		.88		2.327		5.456**
		[2.075]		[2.326]		[1.461]		[2.370]
Proportion ST		3.333**		3.693**		2.994		−.0201
		[1.557]		[1.682]		[2.540]		[2.625]
Consumption GINI		1.412		1.981		2.524		.294
		[1.993]		[2.621]		[2.169]		[2.852]
Value of mining output		.105***		.0503		.123***		.114**
		[.0392]		[.0440]		[.0449]		[.0505]
N	340	335	340	335	340	335	340	335
k	80	85	80	85	80	85	80	85
$z_{(\Sigma \text{ VEG})}$	−3.07***	−2.325**	−2.563***	−1.527	−1.06	−1.161	−2.373**	−1.97**
p Value$_{(\Sigma \text{ VEG})}$	.002	.012	.010	.127	.289	.246	.018	.049
α	1.107	1.027	1.252	1.058	1.332	1.225	1.76	1.445

Note: Robust standard errors in brackets. Clustered (by district) in all models. $z_{(\Sigma \text{ VEG})}$ tests the hypothesis: Vegetation$_t$ + Vegetation$_{t-1}$ + Vegetation$_{t-2}$ = 0. The (over-) dispersion parameter α tests the hypothesis of Poisson ($\alpha = 0$) over NB ($\alpha > 0$). IV = instrumental variable; NB = negative binomial; SC = scheduled caste; ST = scheduled tribe.
***$p < .01$.
**$p < .05$.
*$p < .1$.

rejects the Poisson model in favor of the NB model. Not reported here is our robustness check using the Arellano–Bond model in which vegetation and its lags are instrumented not by rainfall but by lagged shocks to vegetation. Those estimates also affirm these results, although the quantitative estimates vary. These results are available from us.

Consumption shocks. Despite conceptual issues about the link to poverty of consumption spending data (MPCE) from the Indian NSS, the variable is available and has been used in previous studies. The partial correlation, after accounting for fixed effects, between vegetation and consumption is around .15. The low correlation is possibly due to sampling error in consumption due to the small size of district samples. Despite its shortcomings, we want to see if consumption shocks have the same effect on killings as vegetation shocks. Table 5 reports the consumption spending counterpart of the OLS-IV models in Table 3. Even though the individual coefficients on consumption and its lags are statistically insignificant, the z statistic indicates that collectively they are statistically significant. A one (within) standard deviation consumption shock that reduces consumption spending by rupees (Rs.) 136 per month has a severe long-term repercussion on killing: it increases total deaths by 131 percent and Maoist-inflicted security deaths by 173 percent! Note that a decrease in consumption spending of Rs. 136 per month would consign many households to starvation, since the mean consumption spending in the sample is only Rs. 640 per month, or approximately a dollar a day at purchasing power parity. Such a shock would reduce the opportunity cost of rebellion drastically. These numbers supply a stark answer to the question why Maoists have succeeded in enlisting into their cadre in these regions.

By and large, the consumption results are an affirmation of the vegetation results, where causality in both cases is based on an opportunity cost of conflict argument. What remains unclear is whether rainfall solves the endogeneity of consumption data. The Kleibergen–Paap statistic does not pass the thumb-rule threshold of five and is in fact less than one. However, the statistically significant A-R statistic rescues the result. The A-R statistic tests the significance of the endogenous regressors and rejects the joint null hypothesis that the coefficients of the endogenous regressors in the structural equation (the three consumption variables) are jointly equal to zero.[15] This statistic is important in assessing the results when instruments are weak as is the case here. Perhaps consumption is closely correlated with its lags because consumption in most households in these regions is already at subsistence, so that there is limited year-to-year variation in consumption. Such correlation induces collinearity, so that sharp inference about individual coefficients is no longer possible. Whatever the reason, the A-R statistic indicates that despite the WI problem, the causal impact of consumption shocks on Maoist killings is "WI robust": the WI problem enlarges the confidence intervals around the mean long-run impact but does not affect its negative sign.

SATP data. An essential reason why we expended a two-year effort to collect data in Maoist areas was to represent the Maoist conflict as locally as possible. The SATP

Table 5. Consumption Shocks and Killings: OLS-IV with Rainfall Instruments

	Dependent variable			
	ln(TotalDeaths)	ln(CivilianDeaths)	ln(MaoistDeaths)	ln(SecurityDeaths)
Second stage				
ln(Dependent variables)$_{t-1}$	.0389	.0284	.0473	−.127
	[.103]	[.115]	[.108]	[.171]
Consumption$_t$	−5.636	−6.347	−1.812	−6.776
	[6.816]	[6.041]	[4.796]	[6.331]
Consumption$_{t-1}$	1.461	−3.398	.879	3.078
	[6.222]	[3.971]	[4.005]	[5.097]
Consumption$_{t-1}$	−8.576	−1.778	−6.035	−5.234
	[5.553]	[3.161]	[4.322]	[4.388]
Neighborhood2	.268	.15	.264	−.208
	[.286]	[.308]	[.183]	[.250]
Proportion SC	.118	−.0749	.188	−.0856
	[1.513]	[1.210]	[.938]	[1.284]
Proportion ST	−1.347	−.88	−.421	−3.233
	[2.669]	[2.254]	[1.800]	[2.498]
Consumption GINI	.915	1.6	.751	.356
	[2.011]	[1.495]	[1.605]	[1.659]
Value of mining output	−.0255	−.0335	.00744	.00926
	[.0465]	[.0412]	[.0330]	[.0398]
N	335	335	335	335
k	82	82	82	82
$z_{(\Sigma \text{ CONSUMP})}$	−1.78*	−2.262**	−1.48	−1.682*
p Value$_{(\Sigma \text{ CONSUMP})}$	.076	.025	.14	.094
First stage				
#Instruments	4	4	4	4
Kleibergen-Paap (WI)	.606	.426	.618	.513
Hansen's J	.0561	2.197	.457	.352
Hansen's J (p value)	.813	.138	.499	.553
A-R	2.845**	3.141**	1.745	2.929**
A-R (p value)	.0247	.0152	.141	.0215

Note: Robust standard errors in brackets. Clustered (by district) in all models. $z_{(\Sigma \text{ CONSUMP})}$ tests the hypothesis: Consumption$_t$ + Consumption$_{t-1}$ + Consumption$_{t-2}$ = 0. A-R = Anderson–Rubin; IV = instrumental variables; OLS = ordinary least squares; SC = scheduled castes; ST = scheduled tribes.
***p < .01.
**p < .05.
*p < .1.

data, being arm's length and urban in nature, are less representative. As we have described, SATP underrepresents the extent of violence in Maoist areas. Despite its biases, SATP data are oft used, and it is a relevant question whether our findings are robust to their use. SATP officially disseminates data only at the state level not at the district level. District-level data may be assembled manually from SATP source materials but mapping into districts requires many judgment calls. In this, we are grateful to an anonymous scholar whose work required manually creating district-level SATP killings, and who generously made the data available to us.

We find that our core results are affirmed in the SATP data as well. The effect of vegetation on killings is smaller than what we find, which is consistent with urban bias that we think affects the SATP data. However, the finding that adverse vegetation shocks cause killings to increase is clear in the SATP data as well. These results are available from the authors.

More states. It is informative to assess the effects of expanding the definition of the Maoist Belt beyond the hard core Maoist Belt states. A logical expansion is to Orissa because, while not traditionally considered to be part of the Maoist Belt, parts of Orissa were subject to Maoist conflict. We coded killings in Orissa and checked if our core results were affected by the state's inclusion, and found our core results to be neither qualitatively nor quantitatively very different. These results are available from the authors. Should states in which only one or two districts are affected by Maoist violence be included in the sample? An example is Maharashtra, a state that is among the most economically developed in the country, but whose easternmost districts border the Maoist region. Including these Maoist districts does not alter the results much.

Casual channel. The causal story—that adverse shocks to rainfall affect conflict via their effect on livelihoods in agriculture and in the forest sector—should be strengthened if the impact of vegetation shocks on killings of security forces by Maoists is weaker when a district's economy is more dependent on mining employment and, by implication, less dependent on agricultural and forest employment. In the model, interacting vegetation with mining quantifies this effect: a positive sign on the interaction implies that the adverse impact of a vegetation shock on conflict is alleviated by alternative employment possibilities in mining. We find the interaction terms to be positive and significant (Online Appendix Table A2), while the effect of vegetation is significantly negative at low levels of mining value, which strongly supports the claim.

Finally, we address the exclusion restriction inherent in using rainfall not as a regressor but as an instrument. First, we note that the range of controls and fixed effects address violations of the exclusion restriction. For instance, the possibility of a direct relationship between rainfall and conflict because tribals or lower castes migrate to high-rainfall regions is controlled by the proportion SC and proportion ST variables. Since members of these socially marginalized communities are the main prosecutors of this conflict, we believe this effectively captures an exclusion restriction violation that passes through out-migration and the associated disruption of societies. Similarly, the channel by which rainfall might affect conflict via greater inequality of income is accounted for by the Consumption GINI variable. Finally, any relationship between rainfall and conflict via mining employment is captured by mining output.[16]

If fighters do not want to fight when it is raining, rainfall may directly affect conflict. This violation of the exclusion restriction is hard to measure. Whether this affects our analysis remains unknown to us. However, we do not believe it biases the reported results significantly because according to our killings data, most deaths in Maoist incidents occur outside of the rainfall season regardless of whether it was a good or bad rainfall year.

Could our results be driven by states in which STs have not achieved representation in political parties that compete in state politics? Jharkhand and Chhattisgarh, which were carved out as separate states in November 2000 precisely to ensure greater representation of tribals, are states where there is such representation. Ironically, the Maoist insurgency gained greater momentum in these areas *after* the new states were created whereupon tribals became more politically represented. Further, the Jharkhand subsample shows a significant relationship between vegetation and conflict, suggesting that it is not political representation that is driving the results (Online Appendix Table A3).

Cross validation

How robust to different samples are our inferences about land productivity shocks as a driver of conflict? Equally importantly, how well is the model able to predict beyond the sample? That is, is the model externally valid? These are questions that must be asked about new models of conflict because they promise much, often failing to deliver outside of the confines of their study. Ward, Greenhill, and Bakke (2010) assess the Collier–Hoeffler and Fearon–Laitin models, with varying degrees of success for those models.

In this section, we perform two CV exercises, one inferential and one predictive. In the *inferential* CV, we demonstrate the robustness of our results using a *k*-fold CV technique (see e.g., Hastie, Tibshirani, and Friedman 2009, ch. 7). In our panel setting, this is achieved by dropping a cross section at a time. Hastie et al. indicate that the most robust form of *k*-fold CV is one where the dropped sample and the test sample both are closely matched on the regressors. With panel data, dropping a cross section at a time comes close to this ideal, ex ante. Due to exogeneity of the regressors (or instrumented regressors), the regressors in the two sets should match, on average. While *k*-fold CV has generally been used to search for the "best" model, our primary goal here is inferential, to see whether our inference about the long-run impact of vegetation on killings remains robust to dropping (matching) subsets of the data.

Table 6 shows the output from the inferential CVs. The first column shows the baseline results from the full sample. The difference between Table 3 (column 2) and this column is that the lagged dependent variable is omitted in this exercise. Since it is insignificant, its absence makes little difference to the coefficient estimates. The other columns reestimate the second stage of the OLS-IV model after dropping a cross section at a time.[17] The bottom two rows indicate the size and statistical significance of the long-term impact of vegetation on killings. It remains statistically significant at the 5 percent level for all subsamples except when the 2008 panel is dropped, when statistical significance is at 10 percent. Thus, the 2008 cross section is more influential in determining statistical significance. The magnitude of the long-term impact of vegetation on killings, however, remains similar across the CVs, ranging between −45.15 and −55.51.

Ward, Greenhill, and Bakke (2010) perform an illuminating sensitivity analysis of variables used in the Fearon–Laitin and the Collier–Hoeffler models. Table 6 similarly indicates which of our coefficients are sensitive to dropping cross sections. The 2008 panel is important for Neighborhood2: 2008 Killings are especially associated with killings in the most proximate districts. ST (proportion of ST) depends on the 2006 panel for its statistical significance. Mining output depends on the 2004 panel for its statistical significance, but it also appears that killings in 2008 were not strongly associated with mineral extraction (dropping 2008 makes the mining finding stronger).

Table A4 in the Online Appendix reports the inferential CV output from the second-stage IV-NB models, reestimated after dropping a cross section at a time. The long-term impact of vegetation on killings remains statistically significant at the 10 percent level for all subsamples except when the 2007 panel is dropped, indicating that panel's influence on the NB results. Once again, the magnitude of the long-term impact of vegetation on killings remains large across the CVs, ranging between −62.87 and −110.7. That is, a shock that denudes vegetation by one standard deviation (of .011) increases TotalDeaths by between 50 percent $\left(e^{(-62.87 \times .011)} - 1\right)$ and 70 percent. Across the subsamples, hypothesis 2 is well supported.

To test the *predictive* accuracy of the vegetation model, we returned to the field to collect 2009 killings data by district. This effort produced a new panel for 2009, which we use to test the predictive accuracy of the models we have reported. We believe that testing predictive accuracy is best done using new data from the same (quasi) experiment. Though costly to do, we have taken the effort to make this possible. The procedure we adopt is as follows. Killings are predicted both in sample and for the out-of-sample 2009 cross section, using the IV models whose estimates are reported in Tables 3 and 4.[18] The in-sample performance of two measures of fit are compared with the out-of-sample performance. The first measure of fit is the R^2 of the simple regression of actual killings on predicted killings. The second measure of fit is the simple correlation ρ of actual and predicted killings. These are the most interesting parameters in question because the strength of agreement between two variables is the issue in question.

An appropriate statistic for testing strength of association is "Fisher's z" chi-square statistic.[19] We use it to formally test whether the R^2 from the actual-on-predicted

Table 6. Fivefold CV: OLS Second Stage

Dependent variable: ln(TotalDeaths)

	Cross section dropped					
	None	2004	2005	2006	2007	2008
Second stage						
Vegetation_t	−7.325	−.915	−4.253	−23.27	−1.78	−11.34
	[14.06]	[19.59]	[16.71]	[19.39]	[13.80]	[17.76]
Vegetation_{t-1}	−3.766	−6.417	−1.697	2.732	−.256	−16.99
	[13.98]	[14.08]	[17.76]	[16.50]	[19.18]	[16.06]
Vegetation_{t-2}	−41.09**	−37.81*	−49.56**	−33.2	−48.57**	−23.06
	[16.71]	[19.16]	[18.87]	[20.23]	[18.47]	[18.73]
Neighborhood2	.324***	.393***	.273*	.350**	.410***	.154
	[.121]	[.141]	[.146]	[.146]	[.128]	[.173]
Proportion SC	1.017	.796	1.1	1.172	1.201	.82
	[.936]	[.959]	[.984]	[1.411]	[.993]	[1.059]
Proportion ST	1.583**	1.686**	1.462*	1.06	1.771*	2.423**
	[.726]	[.827]	[.739]	[.975]	[1.032]	[1.069]
Consumption GINI	1.387	1.85	1.202	.797	1.858	.948
	[1.149]	[1.460]	[1.474]	[1.513]	[1.150]	[1.411]
Value of mining output	.071***	.028	.074***	.061***	.073***	.205***
	[.021]	[.025]	[.022]	[.022]	[.022]	[.025]
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N	335	268	268	268	268	268
k	82	82	82	82	82	82
ΣVegetation	−52.18**	−45.15**	−55.51**	−53.73**	−50.61**	−51.4*
$z_{(\Sigma\ \text{VEG})}$	−2.522	−1.999	−1.985	−2.351	−2.303	−1.717

Note: Robust standard errors in brackets. Clustered (by district) in all models. Procedure is as follows. The first stage regression is estimated for the full sample, and the three vegetation variables predicted. Ordinary least squares (OLS) is used to estimate the second stage with these instrumented variables (IV) and reported earlier. The first column is for the full sample. The other columns drop an annual cross section each. The first stage regression include district and time fixed effects plus other variables except vegetation. Lag-dependent variable is not included in the analysis (since cross sections are dropped). R^2 from second stage OLS are all above .70. ΣVegetation is the sum: $\text{Vegetation}_t + \text{Vegetation}_{t-1} + \text{Vegetation}_{t-2}$. $z_{(\Sigma\ \text{VEG})}$ is the *z* statistic for the hypothesis: ΣVEGETATION = 0. CV = cross validation; SC = scheduled caste; ST = scheduled tribe.

***$p < .01$.

**$p < .05$.

*$p < .1$.

Table 7. Prediction CV: OLS Regression of Actual Killings on Predicted Killings Predicted Killings based on OLS-IV estimates in Table 3, Column 2

Dependent variable: TotalDeaths						
	Cross section					
	2004	2005	2006	2007	2008	2009
Predicted total deaths	.237***	.877***	5.320***	2.340***	.411***	.411***
	[.067]	[.065]	[.982]	[.148]	[.014]	[.003]
Constant	2.464***	4.264***	−21.15***	−7.091***	4.147***	3.832***
	[.740]	[1.530]	[5.415]	[1.404]	[1.193]	[1.271]
N	67	67	67	67	67	67
R^2	.214	.837	.845	.942	.83	**.924**
"Fisher's z" χ^2test	62.57***	5.240**	4.556**	.625	5.850**	—
Ho: 2009 sample $R^2 \geq$ within-sample R^2	Fail to reject	Fail to reject	Fail to reject	Fail to reject	Fail to reject	—
ρ (Correlation)	.462	.915	.919	.970	.911	**.961**
"Fisher's z" χ^2 test:	68.1***	5.156**	4.531**	.571	5.792**	—
2009 sample $\rho \geq$ within-sample ρ	Fail to reject	Fail to reject	Fail to reject	Fail to reject	Fail to reject	—

Note: Robust standard errors in brackets. Clustered (by district) in all models. Procedure is as follows. Using the estimates in Table 3 Column 2, log killings are predicted for the in-sample (up to 2008) and out-of-sample (2009) years. The table reports the separate simple regressions of actual-on-predicted killings for each in-sample and the out-of-sample cross section. Fisher's z χ^2 test is described in the article and in Cox (2008). It tests the hypothesis that the fit, measured by the regression R^2 and the simple correlation coefficient, respectively, is higher in the out-of-sample 2009 cross section than in any in-sample cross section. Failure to reject implies that the 2009 killings are at least as well predicted by the vegetation model as killings in any other cross section. Data for out-of-sample cross section were collected and assembled by the authors. See the article for details. Boldface values are the baseline R^2 values against which all other R^2 in the row are compared using a formal test. The results of that test are in boldface. CV = cross validation; IV = instrumental variables; OLS = ordinary least squares.
***$p < .01$.
**$p < .05$.
*$p < .1$.

killings regression using the out-of-sample cross section is greater than the R^2 from the actual-on-predicted killings regression using similarly sized in-sample cross sections. Since the panels are balanced across years, this comparison is across equal sample sizes.[20] For simple regressions, comparing R^2 is equivalent to using Akaike's information criteria. If the actual-on-predicted R^2 from the 2009 cross section is no smaller than the actual-on-predicted R^2 from other subsample cross sections, we take it to be an affirmation of the vegetation model's ability to predict killings. We follow the same procedure for testing the equality of the correlations ρ. In the simple regression setting, the results are almost identical because ρ and R^2 are similarly constructed.

Table 7 shows the results of the Fisher's *z* chi-square test when predictions about TotalDeaths are made from the OLS-IV model in Table 3. The vegetation model does surprisingly well in predicting total killings in 2009. The actual-on-predicted regression R^2 for the postestimation sample of .924 exceeds all other in-sample cross sections except 2007. Fisher's z indicates that even for the 2007 panel, the hypothesis that the fit in the postestimation cross section is better than the fit in the 2007 cross section cannot be rejected (χ^2 statistic = .625). The results are very similar if we compare simple correlations of in-sample cross sections with the out-of-sample cross section.

In Table A5 in the Online Appendix, we report the Fisher's z chi-square test results where predictions about TotalDeaths are made using the IV-NB model estimated in Table 4. The notable difference between these and the OLS results in Table 7 is that while the vegetation model does well in predicting total killings in 2009 ($R^2 = .920$), the actual-on-predicted regression using the 2006 panel has a better fit ($R^2 = .961$). This does not in itself raise doubts about the vegetation model's predictive ability. If anything, our prior belief was that the model's postsample performance would be lesser than in any in-sample cross section. Its actual performance exceeded our expectations. While this is heartening for the vegetation model, its resilience requires tests with newer cross sections in the Maoist context as well as its demonstrated success in predicting casualties in the context of other continuing internal conflicts.

Conclusion

We demonstrate a strong and substantively large causal relationship between adverse renewable resource shocks and the intensity of conflict in India's Maoist belt. In doing so, we make two contributions to the conflict literature. First, we provide rigorous econometric support for claims in the renewable resource shocks literature for a causal link between such shocks and conflict. Second, we advance the civil conflict literature in international relations, international political economy, comparative politics, and development economics. By holding the state's capacity constant—inherently in our single country design and further facilitated with the use of fixed effects—we are able to disentangle livelihood effects from the effects of lower state capacity to repress.

Further research on the relationship between renewable resource shocks and conflict could focus on generalizing these findings beyond a single country context. This would require developing instruments for renewable resource shocks in a cross-country context and in developing better measures of state capacity for repression than those that are currently available.

Conflicts like the Maoist insurgency are not only theoretically likely in the process of development but which are being observed with some regularity around the emerging world. Getting to the source of these conflicts is the essential first step in attempting to address them. That threats to livelihood are a fundamental cause of the violence appears to provide a solution, such as providing insurance against shocks that threaten livelihoods. The institutional barriers that obstruct such solutions should be addressed with greater focus in policy and academic work.

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Notes

1. This is distinct from the literature on nonrenewables such as oil.
2. Homer-Dixon (1994, 1999) and Kahl (2006) are prominent contributions to this literature.
3. The terms "Maoist" and "Naxalite" are often used interchangeably in the media. Maoism can be seen as a radical brand of Naxalism. The 2004 merger of the People's War Group and the Maoist Communist Centre into the Communist Party of India (CPI; Maoist), along with the CPI (Marxist-Leninist) Liberation's increasing focus on parliamentary politics, has rendered "Maoist" an appropriate term for today's conflict. Unlike Naxalism, which has no single doctrine, the specific ideology of Maoism can be found in the official documents of the CPI (Maoist). The Indian government officially terms these groups as "Left-Wing Extremists". Detailed accounts of the origins of the Naxalite movement are Banerjee (1980) and Singh (1995). Careful ethnographic studies of the Maoist movement are Bhatia (2005) for Bihar and Hariss (2010) reviews ethnographic studies.
4. Bohara, Nepal, and Gawande (2011) analyze Nepal's Maoist insurgency using a cross section of villages.
5. Data collated from the Annual Reports of Ministry of Home Affairs, Government of India.

6. In 2004, 73.6 percent of Andhra Pradesh's, 80.1 percent of Bihar's, 90.9 percent of Chhattisgarh's, and 64.4 percent of Jharkhand's workforce were employed in agriculture.
7. We found considerable underreporting of the Maoist problem by any individual newspaper. The coverage of national newspapers was stronger in some regions than others depending on the strengths of their local bureaus. In some years, the extent of overlap in coverage of incidents in two leading national English-language newspapers—*The Hindu* and *Indian Express*—was less than a fifth! Even after they were combined, it amounted to a small fraction of the total number of incidents in our final authors' original data set database, since it was built up from multiple sources. It is not surprising to see databases that rely on national papers significantly underreporting the actual incidents.
8. In the Indian context, for example, Panigrahy et al. (2010) find dense forests denuding at .72 percent per year in the Western Ghats, while Prabhakar, Somanathan, and Mehta (2006) find deforestation in the Himalayas.
9. Accessed December 13, 2013, http://earthobservatory.nasa.gov/Features/MeasuringVegetation/measuring_vegetation_4.php.
10. Our data reject the Poisson model whose conditional mean equals conditional standard deviation.
11. Equation (1) is easily translated into the error correction model (ECM) form (e.g. De Boef and Keele 2008) as:

$$\begin{aligned}\Delta \ln(\text{TotalDeaths})i,t = {} & (\varphi - 1)\ln(\text{TotalDeaths})_{i,t-1} + (\beta_1 + \beta_2 + \beta_3)\text{Vegetation}_{i,t-1} \\ & + \beta_1\,\Delta\text{Vegetation}_{i,t} - \beta_3\,\Delta\text{Vegetation}_{i,t-1} \\ & + \text{Year fixed effects} + \text{District fixed effects} + e_{i,t},\end{aligned} \tag{2}$$

where Δ is the difference operator: $\Delta X_{i,t} = X_{i,t} - X_{i,t-1}$. In the ECM, we regress the first difference of Maoist-related deaths on one lag of Maoist-related deaths, one lag of vegetation, the first difference of vegetation, the first difference of lagged vegetation, and district and year fixed effects. In equation (2), the immediate impact of a one unit vegetation shock is a β_1 percent change in Maoist-related deaths. The *long-run* impact of a one unit shock is a $[100 \times (\beta1 + \beta2 + \beta3)/(1 - \varphi)]$ percent change in Maoist-related deaths.
12. The impact of a negative one standard deviation shock to killings from the NB model is computed by exponentiating $[(-.011) \times (\beta1 + \beta2 + \beta3)/(1 - \varphi)]$ and subtracting one. Explicitly using the negative binomial (NB) estimates in Table 1, this is $e^{(-.011)\times(-21.87-16.48-14.33)/(1-.0944)} - 1 = .473$, or 47.3 percent.
13. The Anderson–Rubin (A-R) test fails to achieve 5 percent significance, indicating weak instruments (WI), despite the opposite inference provided by the Kleibergen–Paap statistic. This is discussed further later.
14. The NB link function is log linear, making the log-linear ordinary least squares model a close cousin.
15. The A-R statistic actually tests the *joint* null hypothesis that (1) the coefficients of the endogenous regressors in the structural equation are each equal to zero and (2) the overidentifying restrictions are valid. Thus, the A-R statistic can reject either because the endogenous regressors are significant (here $\beta_1 = 0$, $\beta_2 = 0$, and $\beta_3 = 0$) or because the

orthogonality of the instruments with the error term fails. Tests have been proposed conditional on instrument orthogonality. Moreira (2003) shows that the A-R test is optimal when the equation is just identified (number of excluded instruments equals the number of endogenous variables), which is the case here. A caveat is that with WI and strong endogeneity of regressors, the ability of the A-R test to not falsely reject (i.e., its power) may be poor.

16. A potential exclusion restriction violation highlighted by Sarsons (2011) in the context of riots in India lies in the need to account for the building of large dams. This may be a relevant consideration for an analysis that includes western India (where two-thirds of India's large dams exits and where most of India's dam building occurs). However, in our analysis there is no overtime variation in our regions in the building of large dams. Using the standard definition of fifty meters, no new large dams were built in our region after 2000.
17. The first stage, in which vegetation and its lags are instrumented, is estimated for the full sample and these predicted values used in the second-stage estimations. This is necessary because the first stage is not possible to estimate after dropping cross sections. Since the endogenous variables and instruments contain lags, dropping cross sections would also drop those lags.
18. Some regressors for the 2009 cross section are new data. For example, the crucial vegetation and rainfall instruments are measured for 2009. So is Neighborhood2. The remaining regressors (Scheduled Caste, Scheduled Tribe, GINI, and Mining) are replicated from 2008.
19. The sampling distribution of an estimated correlation is skewed since correlations must lie in $[-1, 1]$. Fisher's atanh transform (inverse hyperbolic tangent) termed Fisher's z makes the sampling distribution of the transformed variable approximately normally distributed (Fisher 1990). We use the method outlined in Cox (2008, 45) to compute Fisher's z. The use of Fisher's z to compare measures of fit assumes that samples are independently drawn. The small and statistically insignificant coefficients on the lagged dependent variables in Tables 3 and 4 support the assumption that the killings data in each cross section are not autocorrelated (i.e., are drawn from a dependent process), so independence of cross sections is weakly satisfied.
20. Different sample sizes are easily accommodated Fisher's z.

Supplemental Material

The online appendix is available at http://jcr.sagepub.com/supplemental.

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