Online Appendix

1. Violence Data

Construction of Local Language Violence Data Set

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, Navbharat;, 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 data set goes back farther in time, to 1999, than does the widely used South Asia Terrorism Portal data set 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 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.

Validation of Local Language Violence Data Set

Recent research shows that violence data are not always a good measure of the intensity of conflict [1], highlighting the importance of proper conceptualization and validation of violence data [2]. We examine the correlation between the Indian government's qualitative measure of *insurgent strength* with our quantitative measure of pre-treatment violence levels. The qualitative indicator of insurgent strength that we utilize is the Indian government's 2004 classification of 32 districts as most impacted by "left-wing extremism" (LWE). Twenty-nine of these appear in the sample analyzed in this study.[3] Figure A1 shows the share of districts with an LWE classification by quartile of pre-treatment violence levels – average quarter-yearly insurgency-related deaths between 1999 and 2005 – as measured in our data set. The probability of a LWE classification rises monotonically and sharply with pre-treatment violence, showing that our quantitative violence measure is highly, though not perfectly, correlated with qualitative measures of insurgency strength.

Comparison of Local Language and English Language Data Sets

A central challenge for our identification strategy is that is requires violence data that is accurately measured within districts *over time*, which motivated us to build a new and more accurate data set on Maoist conflict violence based on local language data sources. By contrast, a significant source of bias in two concurrent papers on the Maoist conflict[4] is that both rely

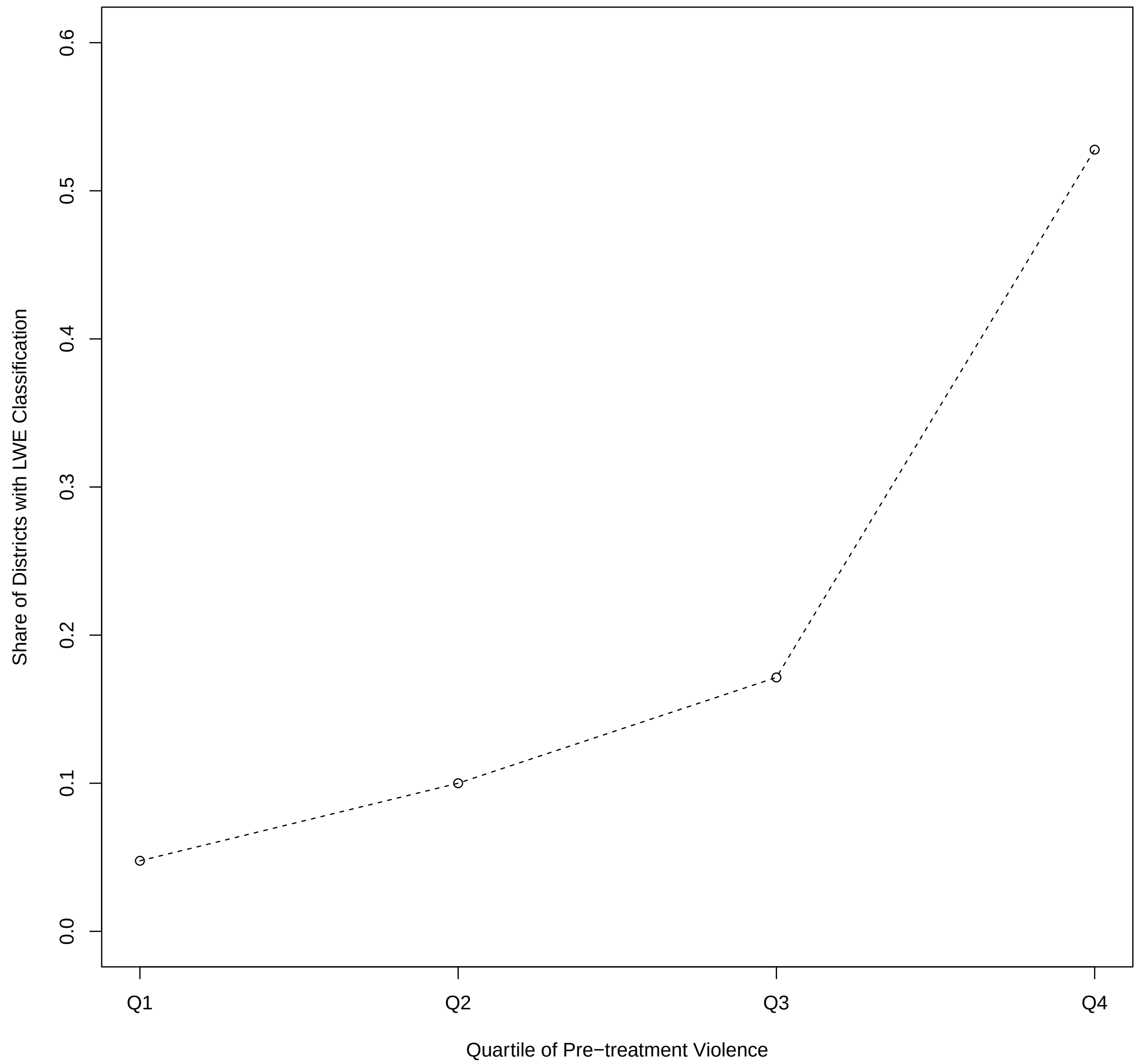
[1] Kalyvas, 2006

[2] Weinstein, 2006

[3] This classification was developed by India's Home Ministry, utilizing reports of insurgent strength from local administrators and police stations.

[4] Fetzer, 2014; Khanna and Zimmermann, 2014

Figure A1: Pre-treatment Violence Levels and LWE Classification



Notes: Horizontal axis indicates quartile of average quarter-yearly deaths from the Maoist conflict between 1999 and 2005. The vertical axis indicates share of districts in each quartile with a "left-wing extremism" (LWE) designation from Indian government. Data set covers 144 districts in six 'red belt' states, with 29 in sample with a LWE designation.

upon database of English language media press clippings compiled by the South Asian Terrorism Portal (SATP). English language media sources in India, such as the *Times of India*, are biased in their reporting toward urban areas. Due to the national scope of their coverage, they also only began to cover the Maoist conflict in depth in recent years when the conflict began to grab national headlines. This is problematic for our identification strategy, because i) overall violence tends to be under-reported, since the Maoist conflict is concentrated in rural areas, and ii) over-time variation in violence is obscured by over-time bias in coverage of the conflict. By contrast, local language media source picked up on the Maoist conflict before it grabbed national headlines and provide more balanced coverage of rural areas since a significant portion of their readership is rural and local

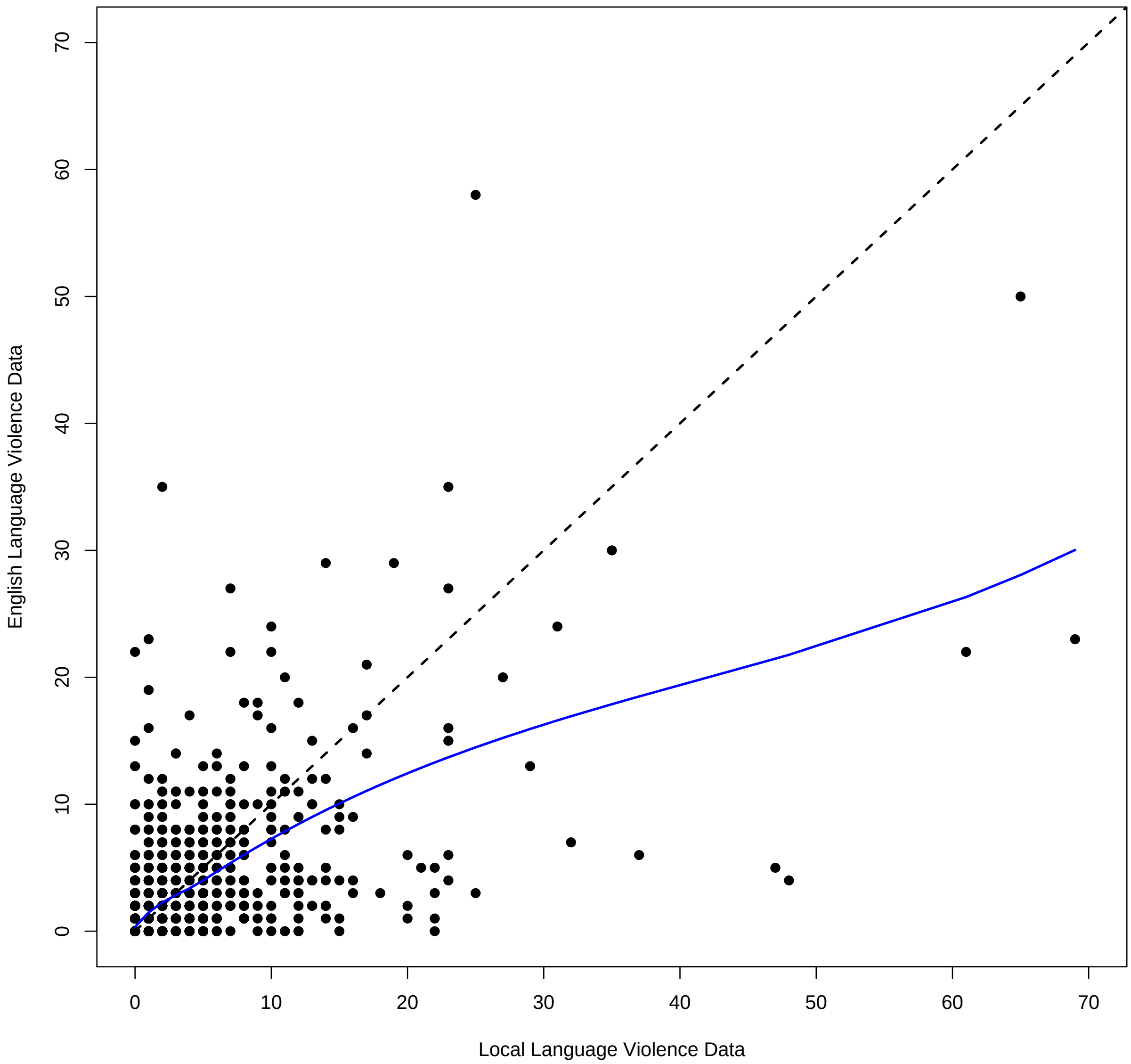
We can analyze the type of reporting bias that is mitigated by utilizing a dataset that draws on local language sources instead of just English language sources. As our English language data benchmark, we take the widely utilized South Asia Terrorism Portal Database on Maoist conflict violent incidents.[5] We take the SATP timeline of events, and utilize natural language processing to geo-code each event to 2001 district boundaries and quarter-year, generously counting each incident that spanned multiple districts as a separate incident in each district-quarter year.[6] We can then compare the English language data to the local language data for the years, 2005-2009, that the two data sets overlap. Our first observation is that the English language data set systematically under-reports violence, especially in district-quarter years with high violence intensity levels. This is apparent in a scatter plot of English language violence reports versus against reports in the local language data set (Figure A2); while the correlation is strong and positive, the fitted loess regression line falls systematically below the 45 degree line, with the

[5]For comparison, we focus on violent incidents, instead of number of casualties, because the latter is often ambiguous in the event descriptions provided on the SATP website.

[6]We also utilize natural language processing to include only *violent* incidents (and not non-violent incidents, such as policy decisions or speeches, which are included in the timeline as events). We do this by subsetting the list of incidents to those which include at least one of an extensive list of violent verb and noun stems, e.g. "kill", "explo", "abduct", "injur", etc. 84 percent of all the incidents are coded as violent.

gap becoming larger at higher violence levels.

Figure A2: Under-reporting in English Language Data set Versus Local Language Data set



Notes: Each point represents a district-quarter year, 2005-2009. Horizontal axis measure number of violent incidents in the local language (Bengali, Hindi, Oriya, and Telugu) dataset utilized in this paper. Vertical axis measure number of violent incidents in the widely used South Asia Terrorism Database based upon English language press sources. Dashed line represents 45 degree line. Blue line represents fitted loess regression line.

Our second observation is that reporting in the English language data set is systematically biased towards reporting violence in more urban districts and in more recent years. We show this by regressing the difference in violent incident counts between the English language and our local language data set by district-quarter year on different predictors (Table A1). Reporting in the English language data set becomes larger in more recent years, when the Maoist conflict began to attract the attention of the urban English media in India, and is larger in more urban areas and less areas with more disadvantaged minorities. The magnitude of the reporting bias is large. Moving forward in time by one year improves reporting of violence in the SATP data set by .71 incidents per district-quarter-year, or nearly 70% of the mean of 1.10 incidents per district-quarter year that is the average in our local language data set. A one standard deviation increase in urbanization is estimated to increase reporting of violence in the SATP data set by 13.3% of the mean in our local language data set. A one standard deviation increase in the percentage of disadvantaged minorities in the population reduces reporting of violence in the SATP data set by roughly 10% of the mean in our local language data set.

Since our empirical strategy utilizes *over time* variation, the massive reporting bias over time makes the English language data set virtually unusable. Since the Maoist conflict is concentrated in and NREGS serves poor and rural areas, the urban bias of reporting in the English language data set is also problematic. By under-measuring deaths in rural areas, where the effects of NREGS are concentrated, as well producing a great deal of over-time measurement error, the SATP dataset attenuates estimates of the impact of NREGS on the Maoist conflict. In Table A2 we replicate our main analyses using the biased SATP violence data. The coefficient estimates are negative but, predictably, much smaller than those based on our more accurate local language violence data. This comparison of data sets partly explains why Khanna and Zimmerman[7] conclude that NREGS caused no lasting impact upon violence. When we replicate our analyses using the same biased English-language data, we predictably under-estimate the pacifying effects of NREGS as well. This comparison of results is fruitful in highlighting the

[7]Khanna and Zimmermann, 2014

Table A1: Determinants of Reporting Bias in English Language Data

	Dependent variable:
	English Violent Reports - Local Language Reports
Year	0.710^{***}
	(0.042)
Urbanization	0.012^{**}
	(0.005)
SC/ST%	-0.006^{*}
	(0.003)
Constant	-1.542^{***}
	(0.103)
Observations	2,840
Adjusted R^2	0.093

Notes: Observations are district-quarter year, 2005-2009. Outcome variable is number of violent incidents recorded in SATP database based on English language press sources, minus number of violent incidents recorded in our data set which utilizes local language data sources. Year begins at 0 for 2005. Urbanization is percentage of population living in urban areas, normalized to have mean 0 (sd: 12.2). SC/ST% is percentage of population comprised of scheduled caste and scheduled tribe groups,normalized to have mean 0 (sd: 18.1). Urbanization and disadvantaged minority population data from 2001 census.

methodological importance of using local language data sources to study civil conflict accurately.

Table A2: Replication of Analysis Using Biased English Language Data set

	Dependent variable:				
	SATP Total Incidents				
	(1)	(2)	(3)	(4)	(5)
NREGS	−0.154	−0.245	−0.126	−0.191	0.174
	(0.280)	(0.272)	(0.256)	(0.297)	(0.238)
NREGS$_{t-1}$		0.116			
		(0.147)			
Y$_{t-1}$			0.033***		
			(0.010)		
Spatial				0.289	
				(0.448)	
State-Year FE	N	N	N	N	Y
Year FE	Y	Y	Y	Y	N
District FE	Y	Y	Y	Y	Y
BI × Year FE	Y	Y	Y	Y	Y
LWE × Year FE	Y	Y	Y	Y	Y
Observations	2,820	2,820	2,820	2,820	2,820
Clusters	141	141	141	141	141

Notes: Unit of observation is district quarter-year, 2005-2009. Total incidents represents total incidents of Maoist violence, based on South Asia Terrorism Portal Database using English language press sources. 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

2. Measuring Local State Capacity

To measure local state capacity, we use two district-level indicators: i) a measure of program intensity based on average annual days of NREGS employment per rural person, computed by dividing administrative data on total days of NREGS employment divided by the rural population as measured in the 2001 census; ii) a district level ranking of local state capacity constructed by averaging a district's rank across four basic state services as measured in the 2001 census. The four basic state services we measure for each district are share of villages with: i) a paved road; ii) a primary school; iii) a primary health center; iv) and an agricultural credit cooperative (the lowest tier of the Indian government's agricultural credit network).

In Table A3 we examine the correlates of our local state capacity ranking. We show that our local state capacity ranking strongly predicts NREGS employment intensity. It is relatively uncorrelated with pre-treament violence levels, pre-treament wage levels, and pre-treatment irrigation levels. It is somewhat correlated with pre-treatment presence of minority populations. The evidence suggests that our ranking of local state capacity captures some of the essential ingredients of state capacity as a concept. It captures a district's ability to implement policies, measured prior to and independent of the specific program under study. It is 'sticky' over time, predicting actual NREGS employment provision nearly a decade later. It also appears to be relatively uncorrelated with many other variables that may independently shape the effects of NREGS adoption on violent civil conflict. In any case, in all empirical specifications we additionally control for potentially omitted variables to ensure that they do not drive the heterogeneity in pacifying effects that we attribute to local state capacity.

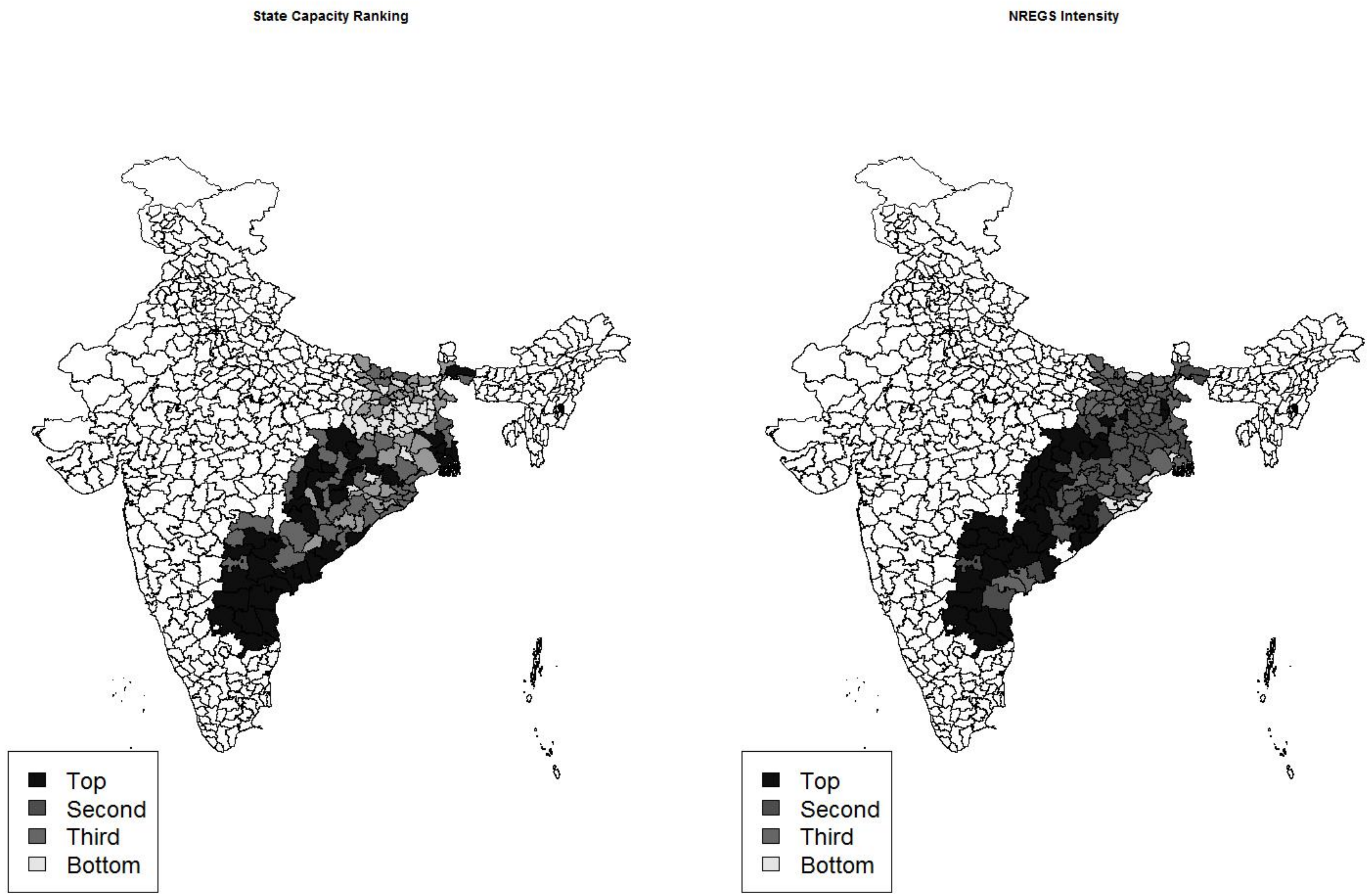
In Figure A3, we provide side-by-side maps of our local state capacity ranking indicator and measure of NREGS program intensity. Consistent with qualitative information, we see that local state capacity is concentrated in the southern states of Andhra Pradesh of Chhattisgarh. There exists substantial heterogeneity in local state capacity *within* regions as well.

Table A3: Correlates of Local State Capacity Ranking Measure

	Dependent variable:				
	Intensity	Pre-violence	Pre-wage	Pre-irrigation	SC/ST%
	(1)	(2)	(3)	(4)	(5)
Top Quartile Ranking	1.746^{***}	0.819	1.996	−5.562	10.080^{**}
	(0.596)	(0.533)	(3.203)	(4.240)	(4.259)
Second Quartile Ranking	1.029^{*}	−0.256	2.042	-9.963^{**}	12.243^{***}
	(0.592)	(0.533)	(3.239)	(4.240)	(4.259)
Third Quartile Ranking	−0.004	−0.486	5.964^{*}	1.282	3.384
	(0.588)	(0.529)	(3.467)	(4.212)	(4.231)
Constant	2.467^{***}	1.042^{***}	−2.461	3.602	-6.541^{**}
	(0.427)	(0.380)	(2.502)	(3.063)	(3.076)
Observations	141	144	118	142	142
Adjusted R^2	0.065	0.027	0.002	0.043	0.053

Notes: Unit of observation district. Explanatory variables are indicators for the different quartiles of our local state capacity ranking measure. Intensity is post-adoption average annual days of NREGS employment provided per rural capita based on 2001 census (mean: 3.2, sd: 2.5). Pre-violence is average pre-treatment quarter-annual deaths (mean: 1.06, sd: 2.27). Pre-wage (mean: 52.19, sd: 12.0) is average wage for a male daily laborer, 2004-2005, from Agricultural Wages of India data series. Irrigated is share of rural land under irrigation in 2001 (mean: 23.7, sd: 18.0 . SC/ST% is percentage of population comprised of scheduled caste and tribes as of 2001 census (mean: 33.9, sd: 18.2). In all regressions, these variables are centered at their means. Analysis estimated by OLS.$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Figure A3: Local State Capacity Across Districts in the Red Belt



Notes: District shaded according to quartile of respective measures of local state capacity. See text for details.

3. Robustness Tests

We investigate the robustness of our estimates of the effects of NREGS adoption on Maoist conflict violence in an array of different specifications which explore different meaningful subsamples of the data. All of the results are broadly consistent with the main analyses in the paper. These analyses are reported in Table A4.

The specifications reported in columns (1) and (5) prune the data to exclude the LWE districts, an alternative to controlling for time trends specific to the LWE districts. Columns (2) and (6) prune the LWE districts and additionally prune districts by BI score to ensure overlap across phase groups in BI score, thereby obviating concerns about common support. Specifically, we limit the sample to districts with a BI score smaller than the 90th percentile of BI scores among phase1 districts (which had the lowest BI scores) and greater than the 10th percentile BI scores among phase 3 districts (which had the highest BI scores). This mitigates the potential parametric sensitivity of our covariate adjustment strategy (King and Zeng, 2006), which controls for BI score and LWE classification in interaction with time dummy variables.

Columns (3) and (7) include the full sample of districts but limit the analysis to a narrower span of years, 2005-09, around the adoption of NREGS. This provides a check that our results are not driven by the number of pre-treatment years that we include in the analysis. Columns (4) and (8) include a separate indicator for each phase of NREGS adoption instead of pooling across phases with a single NREGS adoption indicator. Though we have less statistical power to identify phase-by-phase effects, the coefficient estimates are negative for each phase of NREGS adoption.

Table A4: Robustness Tests: Poisson Regression Estimates of NREGS Effect on Maoist Conflict Violence

	Dependent variable:							
	Total Incidents				Total Deaths			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
NREGS	−0.757***	−0.773**	−0.576***		−0.955	−2.322	−0.758	
	(0.210)	(0.334)	(0.191)		(0.652)	(1.702)	(0.609)	
NREGS Phase 1				−1.038***				−1.100*
				(0.316)				(0.653)
NREGS Phase 2				−0.468				−0.696
				(0.446)				(0.770)
NREGS Phase 3				−0.045				−2.066
				(0.831)				(1.579)
Year FE	Y	Y	Y		Y	Y	Y	
District FE	Y	Y	Y	Y	Y	Y	Y	Y
BI × Year FE	Y	Y	Y	Y	Y	Y	Y	Y
LWE × Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Sample	No LWE	LWE/BI Pruned	2005−09	Full	No LWE	LWE/BI Pruned	2005−09	Full
Observations	4,480	1,520	2,536	5,640	4,480	1,520	2,536	5,640
Clusters	112	38	141	141	112	38	141	141

Notes: Unit of observation is district quarter-year, 2000-2009. Total incidents represents total incidents of Maoist violence. Total deaths represents sum of civilian, Maoist, and security force deaths. Columns (1) and (5) prune LWE districts from sample. Columns (2) and (6) prune LWE districts as well as districts with a BI score less than the 10th percentile of the BI score of Phase 3 districts or greater than the 90th percentile of of the BI score of Phase 1 districts. Columns (3) and (7) prune years prior to 2005. Columns (4) and (8) disaggregate the effects of NREGS by phase of adoption. 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

4. Timing of Effects

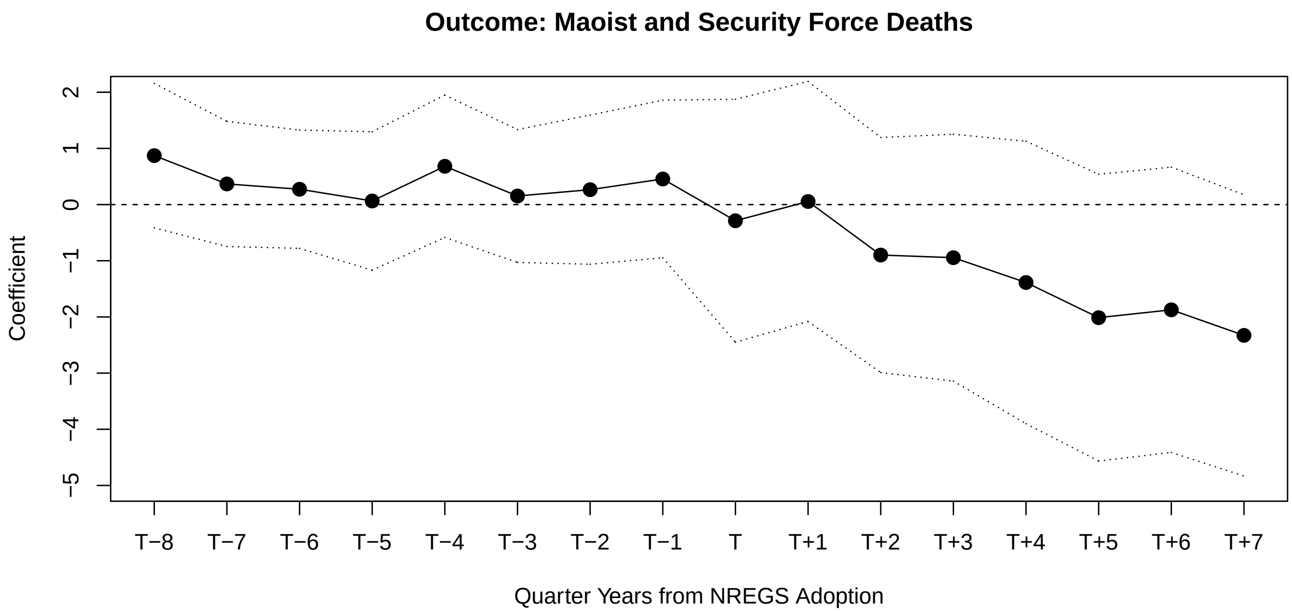
No Evidence of Counter-insurgency Effects

One concern about the estimated decline in violence that we observe following NREGS adoption is that perhaps this decline is driven by counter-insurgency campaigns timed to coincide with NREGS's roll-out across districts. If this were the case, however, we should expect to see a short-run spike in violence around the roll-out of NREGS. According to qualitative information, the Indian government's large-scale counter-insurgency operation ('Operation Green Hunt') launched in 2010, for example, resulted in a large short-run increase in Maoist and security force deaths before violence declined again. However, we do not find this pattern around the roll-out of NREGS. Figure A4 provides a time period-by period analysis of changes in Maoist and security force deaths in the quarter years around NREGS adoption. Consistent with the overall impact of NREGS on violence, we see no increase in violence in the period leading up to NREGS adoption, followed by a steady decline with the adoption of NREGS.

No Evidence of Policy Bundling

There is no qualitative evidence to suggest that the Indian government formally bundled other policies to coincide with the timing of NREGS's roll-out across districts. The Indian government's attempts to coordinate counter-insurgency operations and development programs did not begin until in 2010 with the introduction of the "Integrated Action Plan", which provides some scope for coordination between police officers and bureacrats in districts classified as affected by "left-wing extremism". In the time period of our analysis, NREGS was coordinated exclusively by the Ministry of Rural Development, which possesses national jursidiction over the program's implementation, while counter-insurgency operations are conducted by the Ministry of Home Affairs and by state-level governments. NREGS was also the only new major rural development program to be introduced in the period 2006-08.

Figure A4: Changes in Security Force and Maoist Deaths Before and After NREGS Adoption by Quarter-Year



Notes: Unit of observation district-quarter year. Outcome variable is security force deaths plus Maoist deaths. Points represent coefficient on indicator of time period, relative to the adoption of NREGS at time T. Dashed 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 and LWE designation interacted with year dummy variables. Standard errors adjusted for clustering within districts.

Nonetheless, in spite an absence of *formal* policy bundling in the period under analysis, it is possible that other *informal* rule of law or development initiatives were timed to coincide with NREGS's roll-out. To assess this, we collect state-level panel data on state police numbers and district-level data on kilometers of road built under PMGSY, the second largest rural development program operated by the Ministry of Rural Development. It is then possible to estimate panel fixed effects regressions, at the state and district level, to see if there is any evidence of a correlation between state police strength or road building and the roll-out of NREGS. These analyses are reported in Table A5.

The results show that there is no evidence of informal policy bundling. Controlling for state and year fixed effects, within states over time there was no correlation between state-level police numbers per capita and the share of districts adopting NREGS. Controlling for district and year fixed effects, within districts over time there was no correlation between road building under the second largest rural development program and the probability of adopting NREGS. Together with qualitative information, these results suggest that other interventions did not coincide with NREGS's roll-out and do not drive our estimates.

No Evidence of Violence Trends Related to State Capacity on Its Own

An important result in our analysis is that NREGS's effects were concentrated in districts with high levels of local state capacity. This could raise the concern that secular changes in violence over time related to local state capacity could be driving the results. For this to bias the results, these trends would have to be correlated with the roll-out of NREGS across districts in some fashion without being an outcome of program adoption itself. To investigate this possibility, we examine fine-grained time patterns in changes in violence around the adoption of NREGS in high- and low-state capacity districts. We do this by including time period indicators for the periods preceding and following NREGS adoption, in interaction with dummy variables representing the different quartiles of local state capacity. In Figure A5, we provide a comparison

Table A5: Tests for Policy Bundling: State Police and Roads

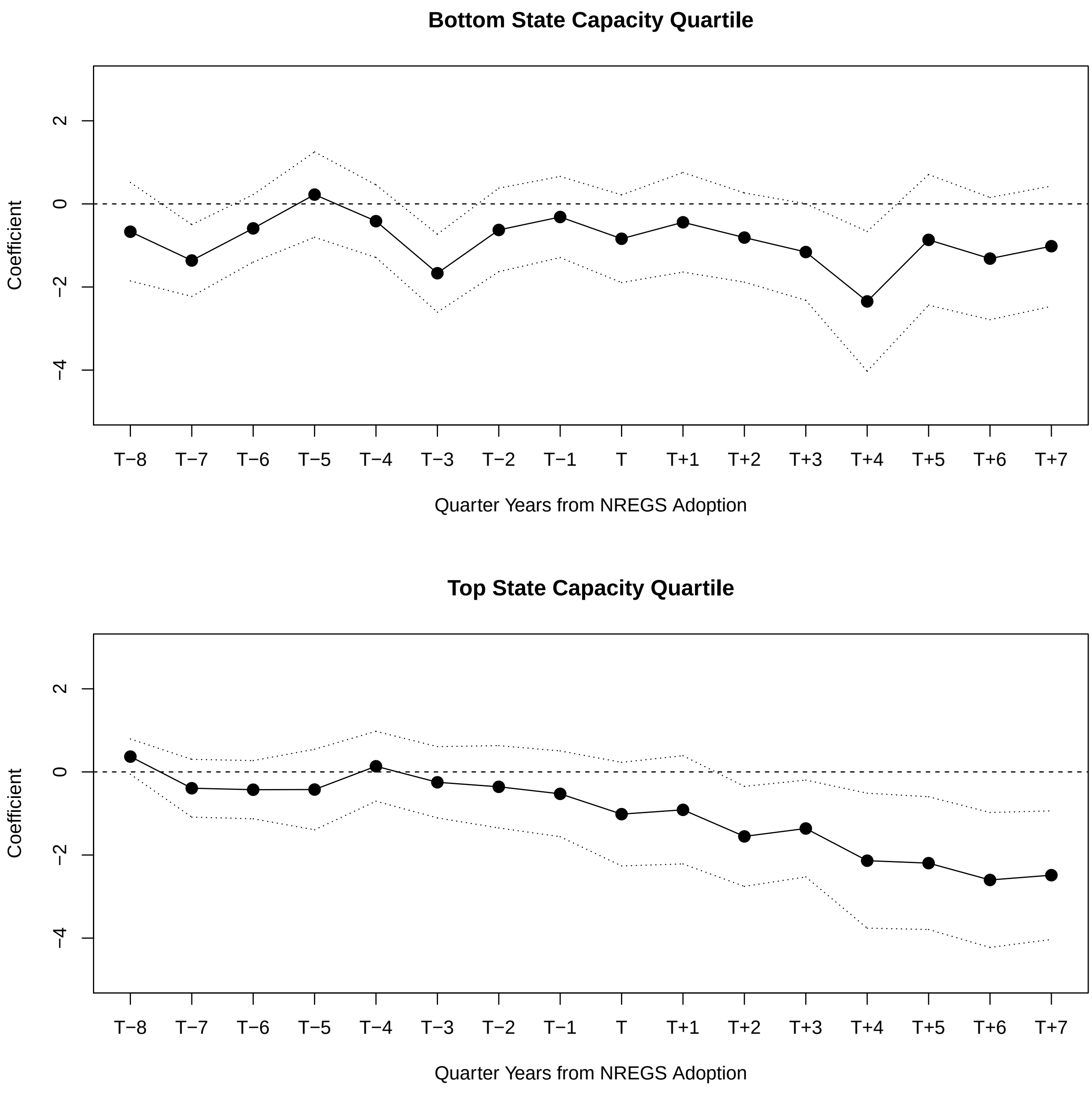
	Dependent variable:	
	NREGS Adoption	
	(1)	(2)
State Police (per 1000 pop)	0.042	
	(0.070)	
PMGSY Roads (100 KMs)		0.003
		(0.005)
Unit FE	Y	Y
Year FE	Y	Y
Unit	State	District
Time Period	2004-09	2001-09
Observations	451	1,269
R^2	0.973	0.914

Notes: Unit of observation is state-year in column (1) and district-year in column (2). Outcome variable is an indicator of share of districts in state adopting NREGS in column (1) and a binary indicator of adopting NREGS at the district level in column (2). State Police is a measure of total number of police officers in the state per 1000 citizens (Mean: 0.26, SD: 0.16). PMGSY Roads is a measure of total roads completed (in 100 KMs) under PMGSY in a district-year (Mean: 0.59, SD: 1.06. Analysis estimated by OLS. *p<0.1; **p<0.05; ***p<0.01

of changes in violence around the adoption of NREGS in the bottom- and top-state capacity quartile districts.

The results show that in high-local state capacity areas, violence fell precisely following the adoption of NREGS and not prior — suggesting that it is the adoption of the anti-poverty program *in a high-state capacity setting* which drives the pacifying effects we observe rather than secular changes in violence related to state capacity on its own. Consistent with the theoretical argument, in low-local state capacity settings, by contrast, NREGS adoption made little impact on levels of violence.

Figure A5: Changes in Violence Before and After NREGS Adoption in High- and Low-State Capacity Districts



Notes: Unit of observation district-quarter year. Points represent coefficient on indicator of time period, relative to the adoption of NREGS at time T. These time indicators are interacted with dummy variables representing the different top and bottom quartiles of local state capacity. Dashed 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 and LWE designation interacted with year dummy variables. Standard errors adjusted for clustering within districts.

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