NBER WORKING PAPER SERIES

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EVIDENCE FROM INDIA

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Working Paper 27395
http://www.nber.org/papers/w27395

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 2020

Shah gratefully acknowledges funding from NSF grant #1658852. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 27395
June 2020
JEL No. O11,O38,O47

ABSTRACT

This paper explores the aggregate economic effects from India's National Rural Employment Guarantee Scheme (NREGS), which provides up to 100 days of labor to rural laborers at the mandated minimum wage. We examine the within-district change to night-time lights and banking deposits using the staggered program rollout for identification. We find consistent and robust evidence that NREGS increased aggregate economic output by 1-2% per capita measured by night-time lights. This effect, however, is not equal across districts. We observe no positive effect of the program in poorer districts, illuminating an important source of heterogeneity.

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1 Introduction

There is much debate about the best way to transfer money to the poor in developing countries, and public works programs are increasingly being used to promote welfare among the poor. In South Asia the largest share of spending on social protection programs goes to public works, though this share is high across most lower and middle income countries. In fact there are approximately 90 public works programs across lower and middle income countries compared to 57 conditional cash transfer programs and 91 unconditional cash transfer programs in the world today (World Bank, 2018).

These programs, which provide rural employment at a fixed wage, are popular because the work requirements entail that such schemes will be "self-targeting" in that non-poor individuals will not sign up to do such work. In developing countries, governments do not observe income for the vast majority of the population so targeting can be a challenge (Hanna and Olken, 2018). Workfare may also prevent dependency in that the poor will turn away from such schemes once better labor market opportunities arise, making them an attractive alternative to unconditional and conditional cash transfers where there is uncertainty about how to phase households off such programs. India's National Rural Employment Guarantee Scheme (NREGS) is the largest such program in the world, with 600 million rural residents eligible to participate and a fiscal allocation of 0.5 percent of India's GDP.

At the household-level, recent research suggests that NREGS increases rural income through wage increases (Imbert and Papp, 2015; Sukhtankar, 2017; Zimmermann, 2020), and Muralidharan et al. (2017) argue that the majority of these income gains are attributable to indirect market effects rather than direct increases in NREGS income, at least in Andhra Pradesh. Most of the research in this area has been micro-founded. Little is known, however, about any aggregate macro effects of these programs, and whether there are any long-term benefits to output.

In this paper we test for aggregate effects of the Mahatma Gandhi National Rural Employment Guarantee Act passed in 2005, exploiting the phased rollout of the program. The intention of NREGS is to help poor rural farmers by providing up to 100 days of paid labor for public works projects (e.g, irrigation projects, road work, etc.). The rollout of the program happened at the district level between 2006 and 2008 (Ministry of Rural Development, 2010). On average poorer, or "backwards" districts received the program first, which was a goal of the federal government. Details are given in Appendix Table 1.

Our estimation strategy uses this staggered district-level roll out of NREGS in a dynamic panel to estimate the within-district impact of the program on aggregate output. To measure aggregate output at the district level within India, we use two proxies: a yearly index of night-time lights and a quarterly measure of total banking deposits recently released by the Indian Government. As first shown by Henderson et al. (2012), night-time lights are strongly correlated with overall economic activity and serve as a valid proxy for measuring output or GDP in a setting where comprehensive income data is not available. District deposits serve as a secondary measure that also intend to measure overall economic activity. While not separable, the use of aggregate output accounts for local general labor market effects, direct effects from infrastructure projects, and potential benefits from the presence of a social safety net.[1]

We find consistent results across both measures. NREGS has a positive, statistically significant, and robust effect on night-time lights and banking deposits of approximately 0.05 standard deviations for both outcome measures, leading to back-of-the-envelope increases of 1-2% in output per capita (from night-time lights). We also show that the impact of NREGS is not identical for all districts. Districts that were targeted to receive the program first are shown to have no relative benefit from the program while the aggregate beneficial effects of NREGS are only evident for later wave, better-off districts. The impact of NREGS in wave three districts is roughly three times as large as the base estimates for night-time lights ($\approx 3-4\%$ increases in output per capita) and twice as large for banking deposits. Therefore the program appears to only be increasing output for the relatively better off districts.

A causal interpretation for these results is based on the assumption that districts were not trending differently prior to implementation. We show parallel trends across the waves for both outcomes of interest. We show the empirical results are robust to controls for district criteria used in the ranking of "backwards" districts. Lastly our results are also robust to state-by-year fixed effects which account for state-level differences and the effectiveness of each state in implementation. Another potential concern is the roll out of a rural electrification plan in 2005, the year before NREGS implementation. The Rajiv Gandhi Grameen Vidyutikaran Yojana (hereafter RGGVY; Prime Minister's Rural Electrification Program) allocated funds for the general improvement of electricity networks in rural India. How-

[1]One benefit to output from the presence of a public safety net is the pursuit of a higher mean and higher variance strategy, which is supported by the work of Raghunathan and Hari (2015) who find that farmers pursue a riskier set of crops following the enactment of NREGS.

ever, Burlig and Preonas (2016) show that while the program did increase electrical use, it had limited economic impacts. We also show that the effect of NREGS is independent of RGGVY.

Our contribution to this literature is threefold. First, we offer an aggregate macro impact of this program which is relatively uncommon in this literature. Second, we document interesting spatial heterogeneity in the treatment effects that could be useful in thinking about how to better target anti-poverty programs more generally. Lastly the use of night-time lights could be used to test for aggregate impacts of other large social welfare programs across the developing world to contribute to the debate on which types of programs we should be recommending to decrease poverty, given the lack of comprehensive income data in most low income countries.

A summary of prior micro studies have used survey data to show wage gains to agricultural or casual laborers and substantial spatial heterogeneity in the effectiveness of the program across India. Merfeld (2019) shows that wage changes due to the program are spatially heterogeneous. Imbert and Papp (2015) show wage increases for the poor following the roll out of NREGS, and this increase is primarily located in 7 "star" states. Our results, which account for state-level trend differences and the effectiveness of each state in implementation, suggest that the positive impacts are concentrated in the richer districts. Dutta et al. (2012) show that demand for NREGS is greatest in poorer states but access to the program is weakest in these states. Zimmermann (2020) shows small increases in overall employment and wages that correspond to NREGS use as a safety net program following negative rainfall shocks; this finding is also echoed in the state of Andhra Pradesh (Johnson, 2009). Other survey based studies also find small effects on unskilled wages with mixed statistical significance (Azam, 2012; Berg et al., 2018; Deininger and Liu, 2013). In addition to testing whether these micro-level findings scale up, our use of output accounts for potential productivity differences between the public and private sectors. In other words, NREGS could increase employment while reducing overall output; our approach tests for this.

2 Background and Data

2.1 NREGS

NREGS is the largest public employment program in the world, providing up to 100 days of work to any rural household whose adults are willing to do unskilled manual labor at the statutory minimum wage. This program was created in 2005 as a way to fight rural poverty. The most direct way is by providing extra employment opportunities, thus income to the poorest households in rural areas.

NREGS claims that work is to be made available to anyone who demands it within 15 days of receiving an application to work, otherwise the state government is liable to pay an unemployment allowance. Open village meetings are supposed to identify suitable projects and local government institutions (gram panchayats) are given a central role in planning and implementation. The stated goals of this policy are: 1) social protection; 2) the creation of durable assets (such as water security, soil conservation, higher land productivity) through the manual labor conducted by the workers; 3) employment of disadvantaged workers such as women, SC and ST;[2] and 4) inclusive growth in rural India through the policy's impact on livelihood security and democratic empowerment (Breitkreuz et al., 2017).

In 2009-10, approximately 53 million households across India were beneficiaries of NREGS, though it has been argued there is excess demand for this program (Dutta et al., 2012). Because the program is so large, the government decided to phase it in across the country over three years (2006-2008). We use the timing of the staggered roll out of the program for identification. The roll out is not random; it is based on a pre-period poverty ranking of districts. Poorer or more "backwards" districts were supposed to receive the program first according to government priorities. However, it was also guaranteed that each state would receive at least one district in the first wave of the program. Two hundred districts were given access to the program in February 2006, the next 130 in April 2007, and the program became available to the remaining 270 districts in April 2008. We will refer to these three groups of districts as wave one, wave two, and wave three districts. In Appendix Table 1 we show summary statistics by wave. On average, wave one and two districts are poorer than wave three districts, meaning earlier districts are poorer. This is also seen in Figure 1, which separately plots the simple average of the two outcomes of interest over time by wave.

[2] The Scheduled Castes (SCs) and Scheduled Tribes (STs) are officially designated groups of historically disadvantaged people in India.

NREGS being targeted to poorer districts earlier is only problematic for the identification strategy, however, if wave one, two, and three districts are trending differently. We explore this further in Section 3.1.

2.2 Data

Annual data for district-level output within India is mostly missing and of questionable accuracy when available. To get around this issue we use two proxies for district output: annual night-time lights and quarterly banking deposits. Spurred by the findings of Henderson et al. (2012), a number of papers have begun to use night-time lights as a proxy for missing sub-national measures output (Asher and Novosad, 2017, 2020; Bleakley and Lin, 2012; Henderson et al., 2018; Hodler and Raschky, 2014; Michalopoulos and Papaioannou, 2013, 2014; Lee, 2018). We follow this strategy to measure changes in district-level output within India. We also use a newly released time series of quarterly district deposit data from the Reserve Bank of India to validate night-time lights and to serve as another proxy for aggregate economic activity.

Night-time lights data. Night-time lights data are global and annual beginning in 1992 from the National Oceanic and Atmospheric Administration. Night-time lights data are reported for 30 arc second latitude-by-longitude grids (roughly a square kilometer at the equator) and are indexed to an integer range from 0 to 63.[3] To get a district level measure within India, we take the average of all grids within district borders using a district map from the census year of 2011. Urban districts that are not reported in NREGS, districts with no variation in NREGS spending (i.e., 0 throughout), and states composed of a singular district are omitted from our base analysis, bringing our sample of districts to 619 out of the 640 districts from the 2011 Census. Summary statistics for night-time lights are given in Appendix Table 1, and a plot of annual means by wave is given in the top panel of Figure 1. The figure shows that average lights are higher in wave three districts prior to the program and that the average increases more rapidly in wave three districts post roll out.

Bank deposit data. Quarterly deposit data is obtained from a recently released district-level data set from the Reserve Bank of India (RBI). The district-level data is available

[3] Averages are used when data is reported for two satellites in a year. Raw data can be found at `https://ngdc.noaa.gov/eog/dmsp/downloadV4composites.html`.

from 2004 onward and is comprised of aggregate deposits from commercial banks, including regional rural banks and small finance banks.[4] The RBI does not report deposits for all districts in 2004q1, resulting in a sample of districts that is smaller than when night-time lights is the outcome of interest. We also omit 3 additional districts–Ahmedabad, Bangalore, and Pune–that are outliers in deposits (10 standard deviations from the mean). However, the inclusion of these districts does not alter our findings—it just makes them larger (results available upon request).

India is unique in that even rural villages have access to banking services either through village banks or local agents known as business correspondents that enable savings and credit transactions. In fact, payments from NREGS were scheduled to be directly deposited into a workers savings account, creating a direct relationship between NREGS and deposits (Kochar, 2018). However, spending on NREGS represents a small fraction of overall deposits, roughly 0.02 percent for 2013. We know of no past studies that have used deposits as a proxy for output, but even in a cash-heavy economy such as India's, within district changes to deposits (when accounting for trends with time fixed effects) will measure changes to aggregate economic activity. For the purposes of this paper, the use of district-level deposits is simply meant to capture the potential flow of funds following NREGS and is seen as a complementary measure to the use of night-time lights.[5] A plot of quarterly means of bank deposits by wave is given in the bottom panel of Figure 1.

NREGS, Backward District Criteria, and RGGVY. The primary regressor is a time indicator for the beginning of NREGS roll out within a district. This variable is constructed from the official statistics (Ministry of Rural Development, 2017) and turns on in the year of wave assignment. A continuous count of NREGS enrollees is tied to uptake, poverty, implementation of the program, agricultural conditions, and available projects; all of which are likely associated with trends in output. Our use of the simple indicator is done to avoid these unobserved variables and their association with output, which is the outcome of interest. The use of an indicator is similar to the identification strategy in Shah and Steinberg (2019) and Imbert and Papp (2015). However, due to the timing of their NSS data, they are only able to exploit variation between waves one/two and wave three. Since

[4]The data can be found under the Time-Series Publications/Quarterly Statistics on Deposits and Credit of Scheduled Commercial Banks tab at `https://dbie.rbi.org.in/DBIE/dbie.rbi?site=home`.

[5]Prior to NREGS, the cross-district correlation between night-time lights and deposits is 0.56 in 2004 and 0.57 in 2005.

we have annual light and banking data, we will be able to exploit variation from all three waves of the roll out.

We also control for criteria associated with the ranking of "backwards" districts, which was used by the government in selecting districts for each wave. The district rankings are constructed from the following criteria: (1) the ST/SC fraction from the 1991 Census, (2) agricultural wages for 1996/7, and (3) output per agricultural worker c.1990-1993 (Planning Commission, 2003).[6] Unfortunately these variables are only available for 445 (out of the 619) districts so sample sizes get slightly smaller in specifications where we control for backwards district criteria. These variables are time invariant; therefore, we interact each with year for night-time lights (or year-quarter for bank deposits) to further control for potential selection bias in the estimation.

In addition to these direct controls for selection, we also include controls for the mean pre-period growth in either night-time lights (annual, 2000-2005) or deposits (quarterly, 2004-2005). Again, the inclusion of pre-period growth rates intends to control for potential differential trends and selection associated with the roll out of NREGS.

The roll out of NREGS coincides with the RGGVY, a rural electrification program. Due to timing of spending, we are concerned about districts that received RGGVY funding from the 10^{th} Plan (c.2005-2010). We control for differential effects of night-time lights in districts that received RGGVY by interacting an indicator for whether a district received RGGVY during the 10^{th} Plan and time fixed effects.[7] As a further step, we omit districts that received RGGVY funding from the regression analysis and the results remain similar (see Section 4.3).

3 Estimation Framework

We estimate difference-in-differences (DD) regressions to measure the impact of NREGS on night-time lights and banking deposits using all three years of the program roll out. For ease in interpretation, we standardize each output measure (i.e., lights and deposits) to a mean of zero and standard deviation of one, such that the effect of NREGS measures the change in standard deviations of the respective outcome variable.

[6]Data can be found at `http://nrega.nic.in/Planning_Commision.pdf`

[7]District level data for RGGVY funding can be found here: `http://www.ddugjy.gov.in/portal/statewisesummary.jsp`.

Specifically, we estimate:

$$std\, y_{it} = \beta_1 \text{NREGS}_{it} + D_i + (Y_t \times S_i) + \mathbf{X_i'Y_t} + \epsilon_{it} \quad (1)$$

where $std\, y_{it}$ is night-time lights or bank deposits in the district (i) and year/quarter (t) level. NREGS is an indicator of whether district i has NREGS in year/quarter t. The DD coefficient of interest is β_1. District and state-by-year/quarter fixed effects are respectively denoted by D and $Y \times S$ and are included in all estimations. Pre-period trends, RGGVY implementation, and criteria for backwards district selection are time invariant, so we interact each with year/quarter indicators; this is denoted by $\mathbf{X_i'Y_t}$. In Table 1 we report three standard errors: district clustered, district/spatial for 30 km, and district/spatial for 200 kilometers (Fetzer, 2014; Hsiang, 2010) since we are concerned that standard errors may be biased from spatial correlation of the night-time lights.

3.1 Identification Assumptions

The identification assumption for the DD is that in the absence of the program, the underlying trends in the outcome variables would have been the same in wave one, two, and three districts. We test this assumption for both dependent variables of interest in various ways, and the results suggest we do not have to worry about underlying trends in the outcome variables biasing the main results.

To test for differential trends across districts we estimate the following equation:

$$std\, y_{it} = \sum_{t=2000}^{2013} \beta_{1t}(wave1_i \times Y_t) + \sum_{t=2000}^{2013} \beta_{2t}(wave2_i \times Y_t) + D_i + (Y_t \times S_i) + \epsilon_{it} \quad (2)$$

where $std\, y_{it}$ is the variable whose trend we are examining; $wave1_i$ and $wave2_i$ are indicators for being in the district wave in which NREGS rolls out; Y_t is a vector of indicators for each year from 2000-2013 (2004-2013 for bank deposits). The coefficients of interest, β_{1t} and β_{2t} take on a unique value for each year and pick up the differences between waves one, two, and three in each year. The base year is 2005, the year prior to implementation. All other variables are as defined previously in equation 1. Subfigure (a) of Figures 2 and 3 plots the estimates of β_{1t} and β_{2t} and confidence intervals of the difference between wave one and two relative to wave three respectively for night-time lights and deposits. The figures show that there are no significant differences in trends across districts pre-rollout for both dependent variables.

We also show the same effects for wave three in subfigure (b) for night-time lights in Figure 2 and banking deposits in Figure 3. Specifically, we plot β_{3t} from the following equation above:

$$std\, y_{it} = \sum_{t=2000}^{2013} \beta_{3t}(wave3_i \times Y_t) + D_i + (Y_t \times S_i) + \epsilon_{it} \tag{3}$$

The coefficient for wave three districts by year from equation (3) is plotted in subfigure (b) of Figures 2 and 3 for both outcomes of interest. This plot simply changes the omitted category from equation (2) to show differential trends between wave three and the earlier (combined) waves 1 and 2. Again it is clear there are no pre-trends prior to 2006, the initiation year of NREGS. But following 2006, we see a relative decline in each proxy of output for early waves (subfigure (a) of Figures 2 and 3) compared to a relative increase for wave three districts (subfigure (b) of Figures 2 and 3).

In Appendix Table 2 we compare late vs. early NREGS rollout districts using both the pre-period mean level and the pre-period growth rate of night-time lights and deposits. As shown, districts that receive NREGS in wave three (2008) have significantly higher levels for each proxy of output in the pre-period compared to districts that received the program in 2006 and 2007, implying level differences. When looking at growth rates, however, no significant differences are seen across either indicator. This null finding is further amplified by the point estimates being roughly zero for all estimations. Including waves one and two of NREGS separately instead of jointly does not alter the findings. The evidence in Figures 2 and 3 and Appendix Table 2 suggests that the DD estimations do not violate the parallel trends assumptions in outcomes.

4 Results

4.1 Effect of NREGS on Aggregate Output

The results from the estimation of equation (1) are given in Table 1 where panel A displays results from the dependent variable night-time lights (2000-2013) and panel B for quarterly banking deposits (2004-2013). Three versions of the standard errors for the coefficient of interest are reported in Table 1; these include district clustered in parentheses, spatially adjusted for 30km in square brackets, and spatially adjusted for 200km in braces. The analysis is yearly for night-time lights in Panel A from 2000-2013, and quarterly for banking deposits from 2004-2013 in Panel B. NREGS rollout starts in wave one districts in 2006 and continues until 2008. In columns (1)-(2) we include the entire sample of districts, while a restricted sample of districts for which we have backward district characteristics is considered in columns (3)-(5). All models include district and state-by-time (i.e., year or quarter-year) fixed effects.[8]

Column (1) of Panel A in Table 1 shows that the NREGS rollout is associated with a 0.05 standard deviation increase in the average light index. Column (2) controls for the average district-level growth rate in night-time lights for the pre-period 2000-2005 and whether the district received RGGVY. The estimated effect is practically identical to that of column (1), suggesting again that pre-period differences and/or RGGVY funding are not driving the coefficient on NREGS. Columns (3) and (4) repeat the analysis of columns (1) and (2) using a restricted sample for which we have data on the three criteria used in formulating the roll-out of NREGS. Column (5) introduces controls for the pre-period selection criteria (backward district characteristics) interacted with time fixed effects. The inclusion of these controls reduces the coefficient on NREGS slightly, but the estimated effect remains similar in magnitude and statistically significant at the 1% level.

Panel B of Table 1 mirrors Panel A, replacing night-time lights with quarterly district banking deposits, and in general, the estimated effect of NREGS in Panel B is similar to that of Panel A. For the unrestricted sample, NREGS has a significant positive effect on deposits within a district, being associated with a 10% of a standard deviation increase in banking deposits following the roll out of NREGS. The smaller sample in columns (3)-(4) leads to a smaller point estimate, but the effect of NREGS remains positive and is very similar in magnitude to the estimate on lights (1/20th of a standard deviation).[9]

While explicit direct comparisons to other studies is difficult, the effect of NREGS on night-time lights is similar in magnitude to the estimated effect of Special Economic Zones

[8]The effect of NREGS remains positive and statistically significant when using year or quarterly fixed effects in place of the state specific time fixed effects.

[9]Appendix Table 3, panel A re-estimates Table 1 using this smaller sample size of districts in Panel B, and we show that this does not substantially change estimated coefficients when considering night-time lights.

in China (Alder et al., 2016).[10] As stated above, we know of no other papers that consider district-level deposits as an outcome as this is newly released data by the Government of India. That said, the relative consistency in magnitude in the effect of NREGS across night-time lights and deposits suggests the effect of NREGS is not spurious.

4.2 Heterogeneity Analysis: Do poorer districts fare worse?

By design and as shown in the summary statistics of Appendix Table 1, wave one and two districts with limited agricultural output and higher fractions of disadvantaged castes were targeted to receive the program earlier. These districts, however, may be poorer due to unobserved factors such as corruption or other factors that might hinder full realization of NREGS benefits.

Table 2 tests for heterogeneous responses to NREGS by estimating the interaction between an indicator for wave three rollout (the better off districts) and the NREGS indicator. Again, night-time lights are the outcome in Panel A, and deposits in Panel B.[11] The marginal effects reported in Table 2 suggest that wave three districts have positive and statistically significant (at the 1% level) effects that are roughly 2-3 times larger than the base estimates of Table 1, whereas the effect of NREGS for wave one and two districts is statistically indistinguishable from zero. The estimates are remarkably similar for both proxies of output denoted by each panel. Appendix Table 2 shows that wave three districts have a significantly higher measure of night-time lights and deposits in periods prior to the enactment of the program. These wave three districts seem better able to leverage the program and the findings in Table 2 suggest that the positive effect of NREGS observed in Table 1 is driven solely by the more well-off wave three districts. Earlier wave one and two districts are estimated to have no beneficial effects from the enactment of NREGS.[12]

[10] From Alder et al.'s column (1) of Table 9, the estimated 5 percent increase in night-time lights from the enactment of a state-level Special Economic Zone is approximately equal to an increase of 0.05 s.d. at the mean.

[11] In order to conserve space, spatially adjusted standard errors are omitted from Table 2. As in Table 1, statistical significance is not altered when accounting for spatial adjustments.

[12] Similar results are found in Appendix Table 4, which explores heterogeneity from the pre-period agricultural wage.

4.3 Robustness Analysis

Identification is based on the roll-out (2006–08) and we extend the time series to 2013. There is some concern that we might be violating identification assumptions of parallel trends pre-rollout by extending the time series so far out. As a robustness test, we limit the estimation data time period to two years before and after the roll out or 2004-2010. Therefore we are able to further limit concerns of bias from pre-existing trends and further establish that the estimated effect is a result of NREGS. Estimates are given in Appendix Tables 5 and 6. The reduced sample period produces estimates that are slightly smaller in magnitude and statistically significant at conventional levels. The smaller magnitudes are not surprising given there are likely issues with the initial district-level implementation of NREGS and that cumulative benefits to output are omitted with the shorter time period—e.g., completion of public works projects, optimization of crops, etc.

We perform an additional robustness test to further verify the estimated effect is not driven by the RGGVY program. We re-estimate Tables 1 and 2 omitting all districts that received any funding for the RGGVY program. These results are respectively given in Appendix Tables 7 and 8.[13] As shown, the omission of a selective one-third of our sample does not significantly alter the estimated relationship between NREGS and night-time lights or deposits, providing further proof that our baseline effect is not the result of the coinciding RGGVY program.

Given the staggered rollout of NREGS and new work by Goodman-Bacon (2019), we also explore which treatment effect is driving the positive coefficient of NREGS.[14] In short, the nature of our lights panel–i.e., balanced pre and post periods to the NREGS rollout and roughly equal districts in each rollout wave–generates similar weights for each 2x2 DD effect. For the deposit sample, weights are less balanced due to lesser available data in the pre-period, so the later 2008 wave is given more weight in comparison. Decompositions further show that the positive coefficient is from late-vs-early treatment effects rather than from early-vs-late effects; a finding consistent with the estimates of Table 2. These decompositions are shown in Appendix Figures 1 - 4.

[13]From Appendix Table 1, 35 percent of districts received funding from RGGVY, and given the nature of the RGGVY program, there is a greater overlap between RGGVY and wave one districts. But as shown in Section 4.2, the positive estimated effect of NREGS on night-time lights is driven by wave three districts, further suggesting that our results are not from the RGGVY program.

[14]Other recent papers in this area include Borusyak and Jaravel (2018), Callaway and Sant'Anna (2019) and de Chaisemartin and D'Haultfoeuille (forthcoming)

In summary, it appears the results are robust to restricting the sample over time (decreasing number of years) and space (omitting RGGVY districts), and the results are not biased due to the staggered rollout.[15]

4.4 Converting Lights to Income

In order to better understand these estimates and to place them within the existing literature, we perform a back-of-the-envelope exercise to convert the estimated effect on night-time lights into income. To do so, we regress a state-level measure of GDP per capita on state-aggregated night-time lights for 2005.[16] Then using the coefficient from this regression, we convert our estimated effect on lights into an estimated effect on income. Specifically, a one unit increase in average state-level lights is associated with a ₹1785 increase ($p<0.01$) in per capita net state domestic product, using 2005 prices.

Taking this state-level relationship to our district-level estimates from Table 1 implies that NREGS led to an increase in annual income for all individuals of ₹459 using the unrestricted sample (column (2) of Table 1) and an increase of ₹234 for the restricted sample with controls (column (5) of Table 1), compared to average per capita (2011 census population) expenditures from 2006-2013 of ₹253 (Ministry of Rural Development, 2016).[17] These increases are respectively 2 percent and 1 percent of the mean 2004-2005 net state domestic product per capita and are in line with Azam (2012) who finds an increase in wages for casual labor of 1 percent increase for men and 8 percent for women.[18]

When considering the larger effect for the wave three districts, the per capita increase in annual income increases to ₹1162 for the unrestricted sample and ₹890 for the restricted, controlled estimate, which correspond respectively to 6 percent and 4 percent of mean 2005

[15] While not shown, our results are also robust to controlling for expenditures from a precursor to NREGS–the Sampoorna Grameen Rozgar Yojana program (Bahal, 2020).

[16] To aggregate our district measure of lights to the state, we simply use the state average. We then weight the state-level regressions by the number of districts used in calculating this average. State-level GDP per capita are found at `http://www.mospi.gov.in/sites/default/files/press_releases_statements/statewise_sdp1999_2000_9sep10.pdf`

[17] Calculated using the state-level coefficient that converts lights to income, the standard deviation of lights, and the coefficients from the respective columns of Table 2. Specifically, the increase in ₹429 is from $1784.716 \times 4.5965 \times 0.0559$, which is the coefficient from the state-level lights-to-gdp regression $\times$ the standard deviation of lights from the unrestricted sample (Appendix Table 1) $\times$ the coefficient from column (2) of Table 2. A similar calculation is used for the restricted sample conversion of ₹234 and for other reported measures of income.

[18] Assuming hours worked to be constant, a percentage change in wage will be equal to the percentage change in income. If instead hours worked increase, then the percentage change in wage will be associated with a larger change in income.

income. This percentage change is similar to estimates from Berg et al. (2018) who find a ≈5 percent increase in agricultural wages in well implemented NREGS districts. The increase for wave three districts is also about one-third of the 13 percent increase in earnings from low-income households from the improved implementation of NREGS found in Muralidharan et al. (2017).

It is important to note a few caveats with this approach of converting lights to income. Prior research finds positive effects on wages (income) for those only in the lower part of the income distribution (Berg et al., 2018). Our aggregate approach mutes these distributional effects by considering only the district-level change for all individuals. It very well could be the case that the positive increase in earnings is driven by large effects for low-income households with no-to-negative income changes for higher-income households; however, we are not able to measure this heterogeneity. Additionally, our measure of income is per capita, not per household. This too leads to an understatement of the effects relative to the literature, since we are measuring income gains for those not in the labor force to a greater degree.

5 Conclusion

This paper shows that a large public works program in India led to an increase in economic activity by leveraging the differential timing in the roll out of NREGS. While we acknowledge that the NREGS roll out is based on level-differences in pre-period economic well-being, we provide evidence that there is no significant difference in pre-trends. Our primary estimates given by Table 1 provide evidence that the program did increase economic activity measured by both night-time lights and banking deposits.

We employ a secondary test to measure the impact of the program by richer vs. poorer districts. The estimates suggest that poorer districts receive no beneficial effects from NREGS and that the gains from the NREGS program are found only in the wave three, relatively well-off districts (Table 2). This finding has important policy implications, especially for poverty alleviation programs within India, but also for the world at large. Unobserved or hard to measure factors, such as institutional quality, corruption, etc., that drag down economic output may also counteract programs designed to stimulate economic activity.

Despite clear evidence of the heterogeneity in the effect of NREGS, we remain uncertain about the mechanisms driving the null effect for wave one and two districts. While there is a growing literature on corruption and leakage related to NREGS as well as potential interventions that might help improve these institutional problems (see for example Niehaus and Sukhtankar (2013); Muralidharan et al. (2016); Banerjee et al. (forthcoming)), more work needs to be done to pin down the precise mechanisms that led to "backwards" districts remaining behind.

More generally, the approach of this paper can be used to estimate the aggregate effects of other social welfare programs in developing countries. The use of night-time lights as a proxy for aggregate output allows us to account for both direct effects and local general equilibria effects from NREGS. The net effect of changes to night-time lights account for productivity effects from the movement of labor from private to public work, effects from public works projects themselves, and potentially higher mean, higher variance crop allocations. Of interest would be to test if other social welfare programs in developing countries (e.g., Progresa in Mexico or GiveDirectly in East Africa), exhibit similar increases in output (or night-time lights) as shown by NREGS in India.

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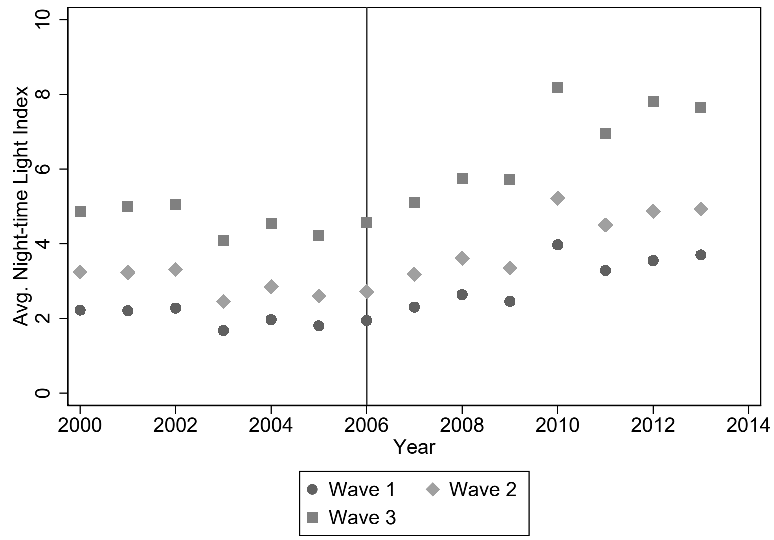
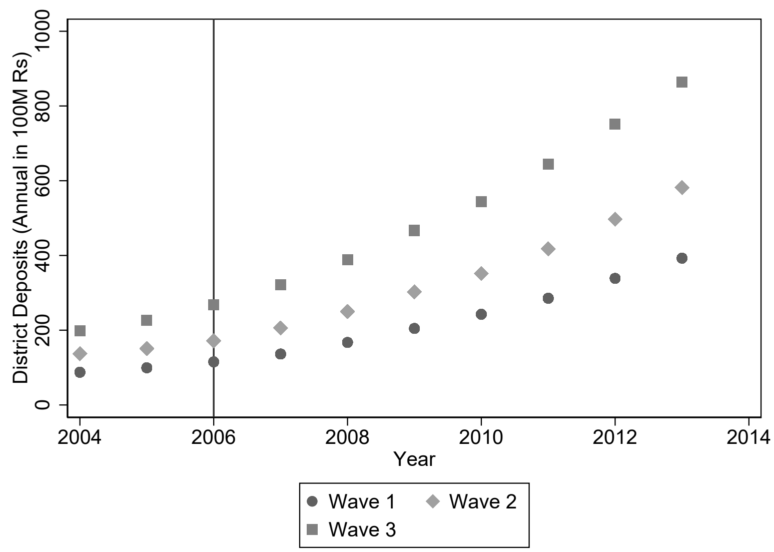
7 Tables and Figures



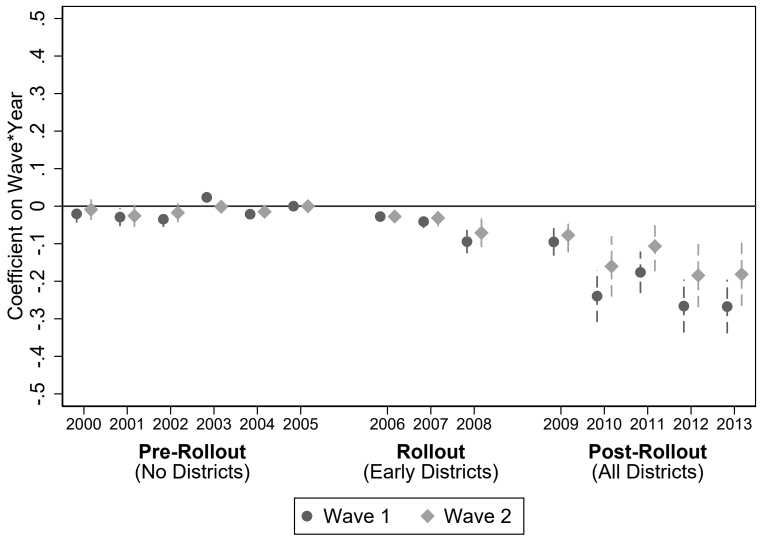
(a) Night-Time Lights



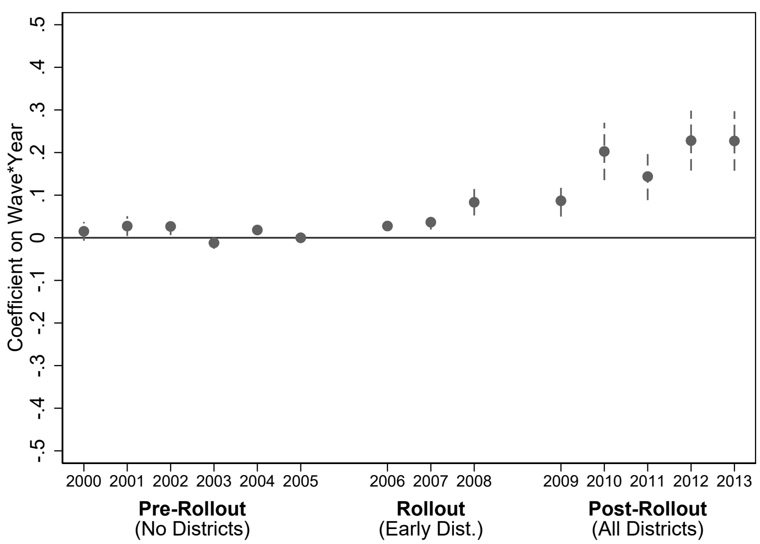
(b) Deposits

Figure 1. Average Output Measures Over Time

Notes:
Sub-figure (a) plots the yearly average of the night-time lights index for each NREGS wave for the sample period, 2000-2013. As discussed in Sec. 2.2, night-time lights are given by 1 sqr. km grids and denoted by an integer from 0-63, for which we take the within-district average. Sub-figure (b) plots the yearly average of banking deposits for each NREGS wave for available data, 2004-2013. Sources and further descriptions of banking deposits are given in Sec. 2.2.



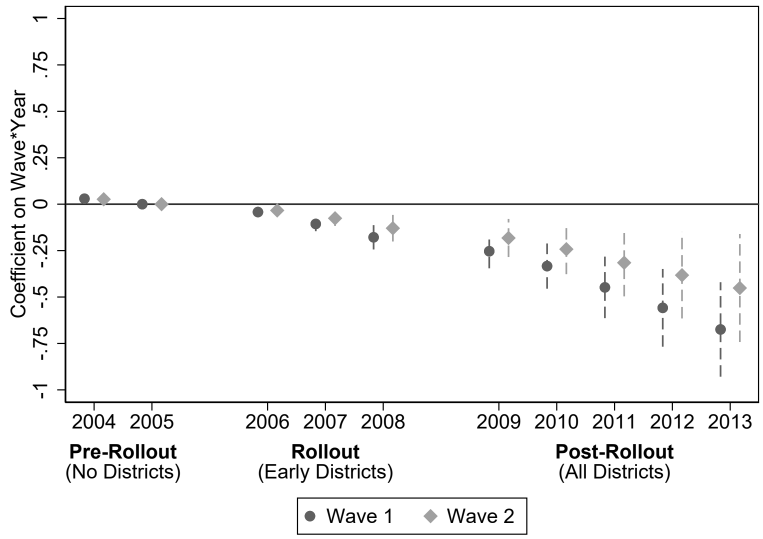
(a) Waves 1 and 2



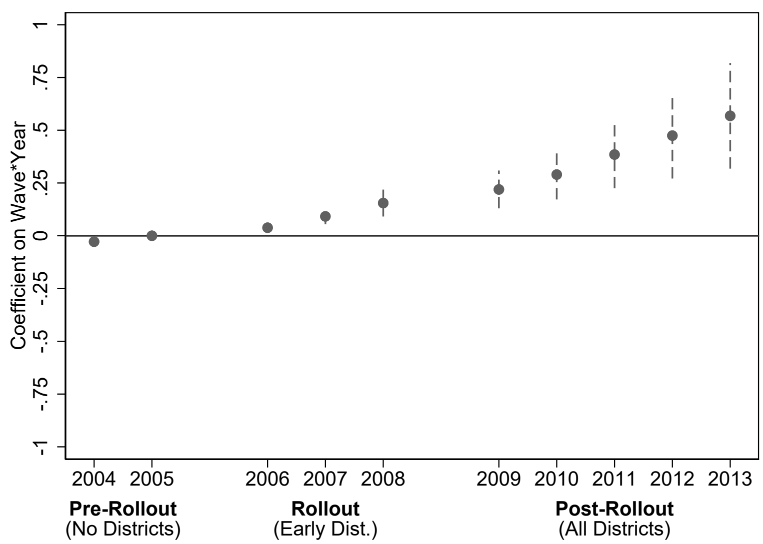
(b) Wave 3

Figure 2. Effect of NREGS on Night-time Light by Wave

Notes:
Sub-figure (a) plots β_{1t} and β_{2t} from estimating equation (2) and sub-figure (b) plots β_{3t} from estimating equation (3) for night-time lights, showing a relative decrease in night-time lights for waves 1 and 2 (a) compared to a relative increase for wave three (b) following full enactment of NREGS in 2008.



(a) Waves 1 and 2



(b) Wave 3

Figure 3. Effect of NREGS on Deposits by Wave

Notes:
Sub-figure (a) plots β_{1t} and β_{2t} from estimating equation (2) and sub-figure (b) plots β_{3t} from estimating equation (3) for aggregate district-level deposits. As with lights, a relative decrease is seen in deposits for waves one and two (a) compared to a relative increase for wave three (b).

Table 1. Effect of NREGS on Aggregate Output

	(1)	(2)	(3)	(4)	(5)
	Panel A. Night-time lights, 2000-2013 (yearly)				
NREGS	0.0558***	0.0559***	0.0503***	0.0531***	0.0330***
	[0.0087]	[0.0083]	[0.0095]	[0.0088]	[0.0098]
	(0.0099)	(0.0093)	(0.0114)	(0.0101)	(0.0107)
	{0.0111}	{0.0103}	{0.0129}	{0.0113}	{0.0119}
Observations	8666	8666	6230	6230	6230
Districts	619	619	445	445	445
	Panel B. Banking deposits, 2004-2013 (quarterly)				
NREGS	0.0931***	0.0950***	0.0574***	0.0632***	0.0487***
	(0.0180)	(0.0184)	(0.0159)	(0.0168)	(0.0149)
	[0.0121]	[0.0124]	[0.0109]	[0.0117]	[0.0113]
	{0.0125}	{0.0128}	{0.0110}	{0.0118}	{0.0114}
Observations	22,280	22,280	17,080	17,080	17,080
Districts	557	557	427	427	427
Pre-period growth rate×time FE		Y		Y	Y
RGGVY Indicator×time FE		Y		Y	Y
Backwards district criteria×time FE					Y
District and State×time FE	Y	Y	Y	Y	Y

Notes:
This table presents results for β_1 from equation 1 for the dependent variables night-time lights (panel A) and bank deposits (panel B). Night-time lights and deposits have been converted to a standard distribution. All regressions include district and state×time fixed effects. Time is a year dummy for night-time lights and a year-quarter dummy for bank deposits. Columns 1-2 show results for a maximum sample of districts, and columns 3-5 show results for a restricted set of districts for which we have backwards criteria (fraction ST/SC from the 1991 Census, agricultural wages for 1996/7, and output per agricultural worker 1990-1993) interacted with time. RGGVY Indicator×time is an indicator for whether a district received RGGVY funding during the 10th plan interacted with time. The pre-period growth rate is the average annual growth rate in night-time lights in the pre-period (2000-2005) or the average quarterly growth rate in deposits in the pre-period (2004q1-2005q4). Three standard errors are reported: district clustered standard errors are given in parentheses–i.e., "( )"; district clustered spatially adjusted (30km) standard errors are given by brackets–i.e., "[ ]"; and district clustered spatially adjusted (200km) standard errors are given by braces–i.e., "{ }". Statistical significance is denoted by *, **, and *** for the 10%, 5%, and 1% levels, respectively.

Table 2. Heterogeneity by Wave of NREGS

	(1)	(2)	(3)	(4)	(5)
	Panel A. Night-time lights, 2000-2013 (yearly)				
NREGS	-0.0054	0.0018	-0.0118	0.0019	-0.0087
	(0.0088)	(0.0086)	(0.0089)	(0.0085)	(0.0097)
NREGS × Wave 3	0.1573***	0.1400***	0.1714***	0.1433***	0.1345***
	(0.0282)	(0.0268)	(0.0281)	(0.0259)	(0.0300)
Marginal Effect Wave 3	0.1519***	0.1417***	0.1594***	0.1451***	0.1257***
	(0.0235)	(0.0224)	(0.0246)	(0.0227)	(0.0269)
p-value, β_{NREGS} = Marg. Eff. Wave 3	0.0000	0.0000	0.0000	0.0000	0.0000
Observations	8666	8666	6230	6230	6230
Districts	619	619	445	445	445
	Panel B. Banking deposits, 2004-2013 (quarterly)				
NREGS	-0.0126	-0.0082	0.0035	0.0112	0.0176
	(0.0142)	(0.0139)	(0.0142)	(0.0143)	(0.0160)
NREGS × Wave 3	0.1965***	0.1897***	0.1054**	0.1005**	0.0719*
	(0.0449)	(0.0435)	(0.0429)	(0.0403)	(0.0415)
Marginal Effect Wave 3	0.1839***	0.1815***	0.1089***	0.1117***	0.0895***
	(0.0367)	(0.0361)	(0.0346)	(0.0337)	(0.0338)
p-value, β_{NREGS} = Marg. Eff. Wave 3	0.0000	0.0000	0.0023	0.0028	0.0335
Observations	22,280	22,280	17,080	17,080	17,080
Districts	557	557	427	427	427
Pre-period growth rate×time FE		Y		Y	Y
RGGVY Indicator×time FE		Y		Y	Y
Backwards-district criteria×time FE					Y
District and State×time FE	Y	Y	Y	Y	Y

Notes:
This table presents results for β_1 from equation 1 interacted with an indicator for wave 3 districts for the dependent variables night-time lights (panel A) and bank deposits (panel B). Night-time lights and deposits have been converted to a standard distribution. All regressions include district and state×time fixed effects. Time is a year dummy for night-time lights and a year-quarter dummy for bank deposits. Columns 1-2 show results for a maximum sample of districts, and columns 3-5 show results for a restricted set of districts for which we have backwards criteria (fraction ST/SC from the 1991 Census, agricultural wages for 1996/7, and output per agricultural worker 1990-1993) interacted with time. RGGVY Indicator×time is an indicator for whether a district received RGGVY funding during the 10th plan interacted with time. The pre-period growth rate is the average annual growth rate in night-time lights in the pre-period (2000-2005) or the average quarterly growth rate in deposits in the pre-period (2004q1-2005q4). Standard errors are clustered by district, and statistical significance is denoted by *, **, and *** for the 10%, 5%, and 1% levels, respectively.

Appendix Tables and Figures

Online Only

Appendix Table 1. Summary Statistics

Variable	Obs.	Mean	Std Deviation	Min	Max
Night-time lights (Annual)					
Full sample	8666	4.2601	4.5965	0	44.2152
Pre-NREGS (2000-2005)	3714	3.4573	3.5968	0	29.3199
Post-NREGS (2006-2013)	4952	4.8623	5.1408	0	44.2152
NREGS rollout (full sample)					
Wave 1	619	0.3102	0.4629	0	1
Wave 2	619	0.2165	0.4122	0	1
Wave 3	619	0.4733	0.4997	0	1
Restricted sample	6230	4.2989	3.9682	0.0464	36.1565
Pre-NREGS (2000-2005)	2670	3.5185	3.0699	0.0464	21.0920
Post-NREGS (2006-2013)	3560	4.8841	4.4377	0.04790	36.1565
NREGS rollout (restricted sample)					
Wave 1	445	0.3843	0.4870	0	1
Wave 2	445	0.2449	0.4305	0	1
Wave 3	445	0.3708	0.4836	0	1
Banking Deposits (Quarterly; in ₹100M)					
Full sample	22280	348.135	616.662	0.17	9497.18
Pre-NREGS (2004-2005)	4456	159.151	229.613	0.17	2030.94
Post-NREGS (2006-2013)	17824	395.381	671.570	0.44	9497.18
NREGS rollout (full sample)					
Wave 1	557	0.3160	0.4653	0	1
Wave 2	557	0.2298	0.4211	0	1
Wave 3	557	0.4542	0.4983	0	1
Restricted sample	17080	305.339	496.324	4.98	9497.18
Pre-NREGS (2000-2005)	3416	141.060	181.531	4.98	1878.25
Post-NREGS (2006-2013)	13664	346.408	539.681	9.2	9497.18
NREGS rollout (restricted sample)					
Wave 1	427	0.3653	0.4821	0	1
Wave 2	427	0.2506	0.4339	0	1
Wave 3	427	0.3841	0.4869	0	1
Controls (Night-time lights sample)					
RGGVY, 10th plan funding	619	0.3586	0.4800	0	1
Wave 1	192	0.4844	0.501	0	1
Wave 2	134	0.3657	0.4834	0	1
Wave 3	293	0.2730	0.4463	0	1
Agr. Wage, 1996-97 (₹per day)	445	39.1611	15.4187	15	117
Wave 1	171	32.4947	9.3286	15	110
Wave 2	109	36.1477	9.3253	16	61
Wave 3	165	48.0606	19.0813	20	117
Output / Agr. Worker, 1990-93 (₹/Worker)	445	8177.809	7422.807	914	78424
Wave 1	171	5247.175	3463.79	914	31951
Wave 2	109	6751.45	5075.64	1707	39861
Wave 3	165	12157.27	9670.87	3443	78424
Percent SC/ST Pop., 1991	445	27.3942	14.934	2.8	94.7
Wave 1	171	36.283	17.6559	10.1	94.7
Wave 2	109	25.6982	10.7766	10.3	59.9
Wave 3	165	19.3024	7.3493	2.8	44.5

Notes:
This table displays summary statistics. For outcome variables, which are time-variant, observations are given by district×time. For time-invariant controls and district rollout, the unit of observations is the district. Summary statistics for district controls are also given by wave.

Appendix Table 2. Selection on level, not trend

Dependent Variable:	std. Pre-period mean		std. Pre-period growth rate	
	(1)	(2)	(3)	(4)
	Panel A. Night-time lights, 2000-2005 (yearly)			
Indicator for wave 3 NREGS	0.4266***	0.4220***	0.2056	0.3204
	(0.0999)	(0.0931)	(0.1685)	(0.2141)
State FE	Y	Y	Y	Y
Observations	619	445	619	445
	Panel B. Banking deposits, 2004-2005 (quarterly)			
Indicator for wave 3 NREGS	0.4469***	0.2426*	0.0488	-0.0018
	(0.1191)	(0.1157)	(0.0787)	(0.0721)
State FE	Y	Y	Y	Y
Observations	557	427	557	427

Notes:
This table estimates the cross-district association between wave three NREGS rollout and the pre-period mean and growth rate of night-time lights and deposits. The dependent variable is night-time lights in Panel A and banking deposits in Panel B; both outcomes have been converted to a standard distribution. As shown, wave 3 districts have higher average night-time lights and deposits (columns 1 and 2), but this level difference is not seen in pre-period growth rates (columns 3 and 4), implying no difference in pre-treatment trends. Standard errors are clustered by district, and statistical significance is denoted by *, **, and *** for the 10%, 5%, and 1% levels, respectively.

Appendix Table 3. Night-lights Estimation with Bank Deposit Sample

Dependent Variable: Std. mean district light-index (2000-2013)					
	(1)	(2)	(3)	(4)	(5)
	Panel A. Base				
NREGS	0.0608***	0.0620***	0.0550***	0.0579***	0.0388***
	(0.0084)	(0.0080)	(0.0095)	(0.0088)	(0.0099)
Observations	7798	7798	5978	5978	5978
Districts	557	557	427	427	427
	Panel B. Heterogeneity: Wave three				
NREGS	-0.0018	0.0071	-0.0087	0.0044	-0.0042
	(0.0091)	(0.0091)	(0.0090)	(0.0087)	(0.0099)
NREGS × Wave 3	0.1632***	0.1439***	0.1739***	0.1479***	0.1376***
	(0.0276)	(0.0269)	(0.0282)	(0.0260)	(0.0304)
Observations	7798	7798	5978	5978	5978
Districts	557	557	427	427	427
	Panel C. Heterogeneity: Pre-period Agr. wage				
NREGS			-0.0500	-0.0168	0.0041
			(0.0367)	(0.0335)	(0.0231)
NREGS × Agr. wage (Rs, 1996-7)			0.0028***	0.0020**	0.0009*
			(0.0010)	(0.0009)	(0.0005)
Observations			5978	5978	5978
Districts			427	427	427
Pre-period growth rate×time FE		Y		Y	Y
RGGVY Indicator×time FE		Y		Y	Y
Backwards-district criteria×time FE					Y
District and State×time FE	Y	Y	Y	Y	Y

Notes:
This table omits districts for which we do not have banking deposit data. Using the smaller sample of districts does not alter the main findings. Standard errors are clustered by district, and statistical significance is denoted by *, **, and *** for the 10%, 5%, and 1% levels, respectively.

Appendix Table 4. Heterogeneity by Pre-period agricultural wage

	(1)	(2)	(3)
	Panel A. Night-time lights		
NREGS	-0.0454	-0.0121	-0.0040
	(0.0338)	(0.0316)	(0.0233)
NREGS × Agr. wage (Rs, 1996-7)	0.0024**	0.0016*	0.0010*
	(0.0009)	(0.0009)	(0.0005)
Marginal Effect			
10th percentile of agr. wage	0.0150	0.0281**	0.0190
	(0.0125)	(0.0120)	(0.0116)
p-value, β_{NREGS} = Marg. Eff. 10^{th} perc.	0.0000	0.0007	0.0545
90th percentile of agr. wage	0.0875***	0.0764***	0.04963***
	(0.0206)	(0.0186)	(0.0111)
p-value, β_{NREGS} = Marg. Eff. 90^{th} perc.	0.0000	0.0000	0.0000
Observations	6230	6230	6230
Districts	445	445	445
	Panel B. Banking deposits		
NREGS	-0.0154	-0.0113	-0.0347
	(0.0460)	(0.0442)	(0.0468)
NREGS × Agr. wage (Rs, 1996-7)	0.0018	0.0019	0.0022*
	(0.0013)	(0.0012)	(0.0012)
Marginal Effect			
10th percentile of agr. wage	0.0306*	0.0372**	0.0208
	(0.0185)	(0.0185)	(0.0210)
p-value, β_{NREGS} = Marg. Eff. 10^{th} perc.	0.0127	0.0205	0.0083
90th percentile of agr. wage	0.0904**	0.1003***	0.0928***
	(0.0331)	(0.0340)	(0.0270)
p-value, β_{NREGS} = Marg. Eff. 90^{th} perc.	0.0014	0.0023	0.0000
Observations	17,080	17,080	17,080
Districts	427	427	427
Pre-period growth rate×time FE		Y	Y
RGGVY Indicator×time FE		Y	Y
Backwards-district criteria×time FE			Y
District and State×time FE	Y	Y	Y

Notes:
This table replicates Table 2 with a different measure of district well-being: mean agricultural wage in 1996-7. Standard errors are clustered by district, and statistical significance is denoted by *, **, and *** for the 10%, 5%, and 1% levels, respectively.

Appendix Table 5. Effect of NREGS, Restricting Data to 2004-2010

	(1)	(2)	(3)	(4)	(5)
			Panel A. Night-time Lights		
NREGS	0.0442***	0.0437***	0.0385***	0.0386***	0.0240***
	(0.0074)	(0.0071)	(0.0080)	(0.0074)	(0.0081)
Observations	4333	4333	3115	3115	3115
Districts	619	619	445	445	445
			Panel B. Banking Deposits		
NREGS	0.0235***	0.0245***	0.0162***	0.0183***	0.0144***
	(0.0050)	(0.0052)	(0.0045)	(0.0049)	(0.0048)
Observations	15,596	15,596	11,956	11,956	11,956
Districts	557	557	427	427	427
Pre-period growth rate×time FE		Y		Y	Y
RGGVY Indicator×time FE		Y		Y	Y
Backwards-district criteria×time FE					Y
District and State×time FE	Y	Y	Y	Y	Y

Notes:
This table replicates Table 1, limiting the sample range to two years before and after the NREGS roll out using data from 2004-2010. Standard errors are clustered by district, and statistical significance is denoted by *, **, and *** for the 10%, 5%, and 1% levels, respectively.

Appendix Table 6. Heterogeneity by Wave, Restricting Data to 2004-2010

	(1)	(2)	(3)	(4)	(5)
	Panel A. Night-time Lights				
NREGS	-0.0068	-0.0045	-0.0118	-0.0093	-0.0147*
	(0.0077)	(0.0077)	(0.0074)	(0.0074)	(0.0084)
NREGS × wave 3	0.1147***	0.1089***	0.1217***	0.1176***	0.1115***
	(0.0227)	(0.0220)	(0.0225)	(0.0207)	(0.0243)
Marginal Effect Wave 3	0.1079***	0.1044***	0.1100***	0.1083***	0.0969***
	(0.0183)	(0.0176)	(0.0192)	(0.0175)	(0.0211)
p-value, β_{NREGS} = Marg. Eff. Wave 3	0.0000	0.0000	0.0000	0.0000	0.0000
Observations	4333	4333	3115	3115	3115
Districts	619	619	445	445	445
	Panel B. Banking Deposits				
NREGS	-0.0278***	-0.0258***	-0.0111	-0.0085	-0.0018
	(0.0093)	(0.0088)	(0.0090)	(0.0084)	(0.0086)
NREGS × wave 3	0.1268***	0.1230***	0.0711***	0.0689***	0.0505**
	(0.0273)	(0.0263)	(0.0265)	(0.0248)	(0.0249)
Marginal Effect Wave 3	0.0990***	0.0972***	0.0600***	0.0604***	0.0487***
	(0.0194)	(0.0191)	(0.0188)	(0.0181)	(0.0184)
p-value, β_{NREGS} = Marg. Eff. Wave 3	0.0000	0.0000	0.0002	0.0001	0.0059
Observations	15,596	15,596	11,956	11,956	11,956
Districts	557	557	427	427	427
Pre-period growth rate×time FE		Y		Y	Y
RGGVY Indicator×time FE		Y		Y	Y
Backwards-district criteria×time FE					Y
District and State×time FE	Y	Y	Y	Y	Y

Notes:
This table replicates Table 2, limiting the sample range to two years before and after the NREGS roll out using data from 2004-2010. Standard errors are clustered by district, and statistical significance is denoted by *, **, and *** for the 10%, 5%, and 1% levels, respectively.

Appendix Table 7. Effect of NREGS: Omitting RGGVY Districts

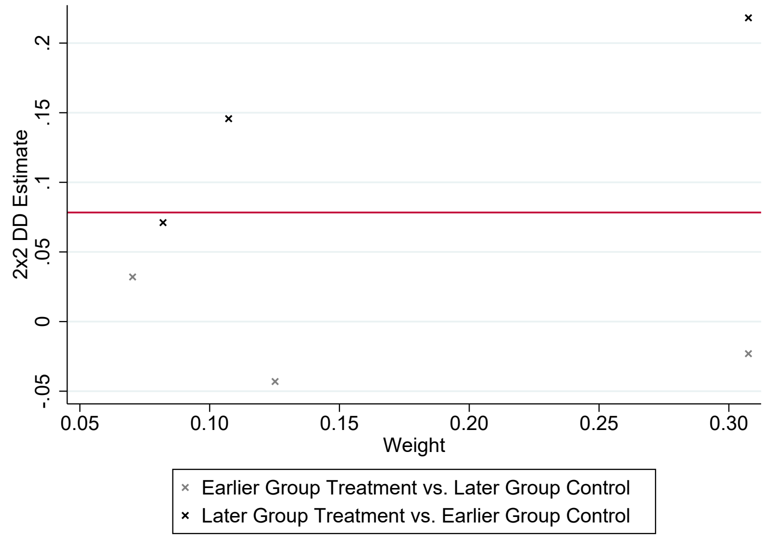
	(1)	(2)	(3)	(4)	(5)
	Panel A. Night-time lights, 2000-2013 (yearly)				
NREGS	0.0544***	0.0545***	0.0534***	0.0535***	0.0230*
	(0.0113)	(0.0112)	(0.0119)	(0.0119)	(0.0125)
Observations	5558	5558	3682	3682	3682
Districts	397	397	263	263	263
	Panel B. Banking deposits, 2004-2013 (quarterly)				
NREGS	0.0957***	0.1012***	0.0612***	0.0750***	0.0518**
	(0.0245)	(0.0252)	(0.0229)	(0.0247)	(0.0224)
Observations	13,880	13,880	10,240	10,240	10,240
Districts	347	347	256	256	256
Pre-period growth rate×time FE		Y		Y	Y
Backwards-district criteria×time FE					Y
District and State×time FE	Y	Y	Y	Y	Y

Notes:
This table replicates Table 1 while omitting districts that received funding for the RGGVY program during the 10th plan. Omitting these districts does not significantly alter the estimated effect of NREGS, further suggesting confounding from RGGVY is not driving the results. Standard errors are clustered by district, and statistical significance is denoted by *, **, and *** for the 10%, 5%, and 1% levels, respectively.

Appendix Table 8. Heterogeneity by Wave of NREGS: Omitting RGGVY districts

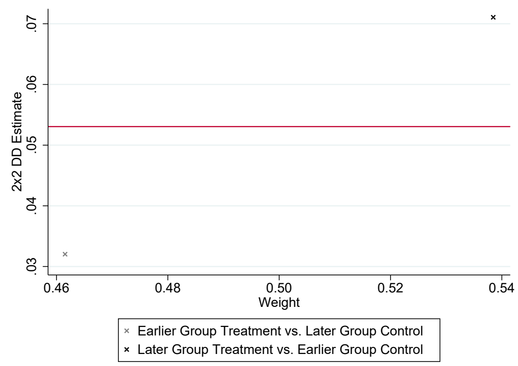
	(1)	(2)	(3)	(4)	(5)
	Panel A. Night-time lights, 2000-2013 (yearly)				
NREGS	0.0043	0.0062	0.0113	0.0141	-0.0023
	(0.0112)	(0.0108)	(0.0124)	(0.0115)	(0.0131)
NREGS × Wave 3	0.1228***	0.1184***	0.1107***	0.1036***	0.0774**
	(0.0335)	(0.0328)	(0.0312)	(0.0303)	(0.0362)
Marginal Effect Wave 3	0.1271***	0.1245***	0.1220***	0.1177***	0.0751**
	(0.0282)	(0.0288)	(0.0269)	(0.0269)	(0.0317)
p-value, β_{NREGS} = Marg. Eff. Wave 3	0.0000	0.0000	0.0000	0.0001	0.0146
Observations	5558	5558	3682	3682	3682
Districts	397	397	263	263	263
	Panel B. Banking deposits, 2004-2013 (quarterly)				
NREGS	-0.0021	0.0061	0.0037	0.0164	0.0228
	(0.0205)	(0.0205)	(0.0221)	(0.0224)	(0.0249)
NREGS × Wave 3	0.1732***	0.1673***	0.1063	0.1074*	0.0636
	(0.0620)	(0.0610)	(0.0648)	(0.0622)	(0.0666)
Marginal Effect Wave 3	0.1711***	0.1734***	0.1100**	0.1238**	0.0864*
	(0.0489)	(0.0364)	(0.0496)	(0.0493)	(0.0523)
p-value, β_{NREGS} = Marg. Eff. Wave 3	0.0003	0.0006	0.0319	0.0292	0.2239
Observations	13,880	13,880	10,240	10,240	10,240
Districts	347	347	256	256	256
Pre-period growth rate×time FE		Y		Y	Y
RGGVY Indicator×time FE		Y		Y	Y
Backwards-district criteria×time FE					Y
District and State×time FE	Y	Y	Y	Y	Y

Notes:
This table replicates Table 2 while omitting districts that received funding for the RGGVY program during the 10th plan. Omitting these districts does not significantly alter our estimated effect of NREGS, further suggesting confounding from RGGVY is not driving our results. Standard errors are clustered by district, and statistical significance is denoted by *, **, and *** for the 10%, 5%, and 1% levels, respectively.

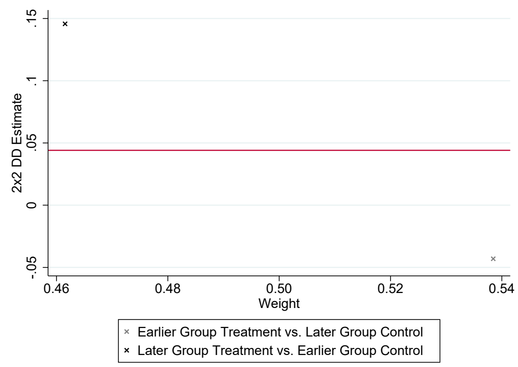


Appendix Figure 1. Goodman-Bacon Decomposition: Night-time Lights

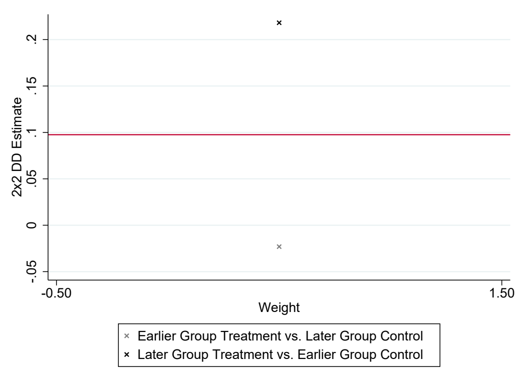
Notes:
This figure plots weights and point estimates for each 2x2 DD comparison. Estimates are from regressing std. night-time lights on the NREGS indicator with district and year FE. As seen, the positive effect of NREGS is driven by the later-vs-early effect; a finding synonymous with Table 2.



2006 vs 2007



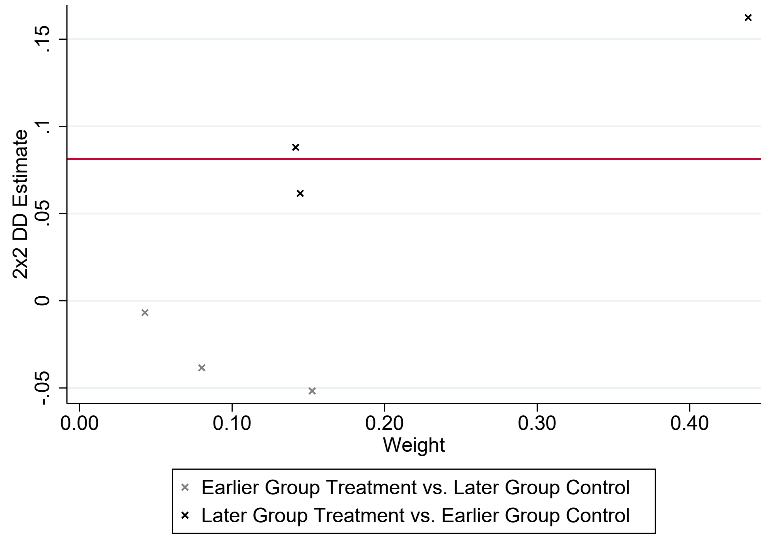
2007 vs 2008



2006 vs 2008

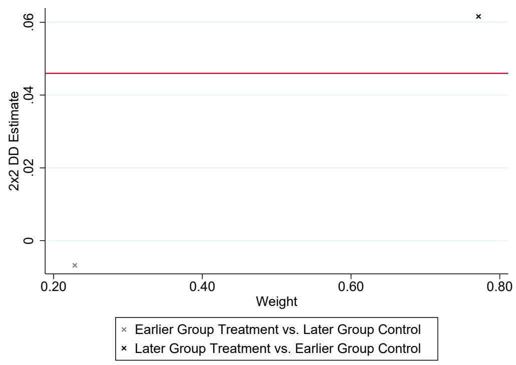
Appendix Figure 2. Goodman-Bacon Decomposition Omitting Rollout Years: Night-time Lights

Notes:
This figure estimates Goodman-Bacon decompositions while omitting a single rollout year. This omission allows for more direct comparisons of the 2x2 DD effects. As seen, each rollout is given roughly equal weight and later rollouts are driving the positive effect in night-time lights

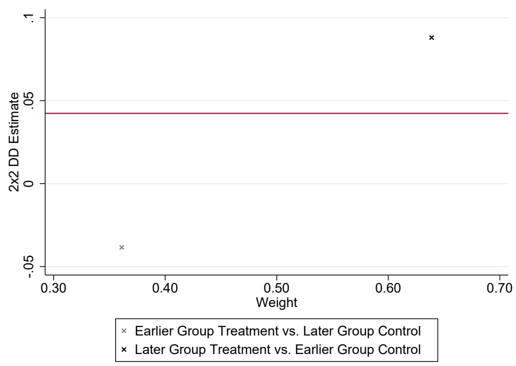


Appendix Figure 3. Goodman-Bacon Decomposition: Bank Deposits

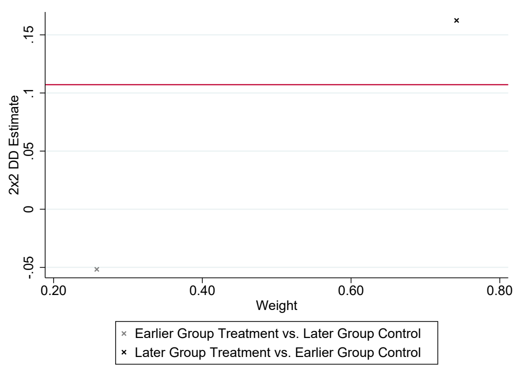
Notes:
This figure plots weights and point estimates for each 2x2 DD comparison. Estimates are from regressing std. deposits on the NREGS indicator with district and year FE. As seen, the positive effect of NREGS is driven by the later-vs-early effect; a finding synonymous with Table 2.



2006 vs 2007



2007 vs 2008



2006 vs 2008

Appendix Figure 4. Goodman-Bacon Decomposition Omitting Rollout Years: Bank Deposits

Notes:
This figure estimates Goodman-Bacon decompositions while omitting a single rollout year. This omission allows for more direct comparisons of the 2x2 DD effects. As seen, each rollout is given roughly equal weight and later rollouts are driving the positive effect in deposits.