Out of the Darkness and Into the Light? 
Development Effects of Rural Electrification

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Abstract

Over 1 billion people lack electricity access. Developing countries are investing billions of dollars in rural electrification, targeting economic growth and poverty reduction, despite limited empirical evidence. We estimate the effects of rural electrification on economic development in the context of India’s national electrification program, which reached over 400,000 villages. We use a regression discontinuity design and high-resolution geospatial data to identify medium-run economic impacts of electrification. We find a substantial increase in electricity use, but reject effects larger than 0.26 standard deviations across numerous measures of economic development, suggesting that rural electrification may be less beneficial than previously thought.

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1 Introduction

Approximately 1.1 billion people around the world still lack access to electricity. These people are overwhelmingly rural, and live almost exclusively in Sub-Saharan Africa and Asia. In recent years, developing countries have made large investments to extend the electricity grid to the rural poor. The International Energy Agency estimates that approximately $9 billion was spent on electrification in 2009, which it expects to rise to $14 billion per year by 2030 (International Energy Agency (2011)). This is not surprising, given that electrification is widely touted as an essential tool to help alleviate poverty and spur economic progress; universal energy access is one of the UN’s Sustainable Development Goals (UNDP (2015), World Bank (2015)). While access to electricity is highly correlated with GDP at the national level, there exists limited evidence on the causal effects of electricity access on rural economies.

Recovering causal estimates of the effects of electrification is challenging, since energy infrastructure projects target relatively wealthy or quickly-growing regions. Selection of this kind would bias econometric estimates of treatment effects toward finding large economic impacts. Previous work has relied on instrumental variables strategies to circumvent this problem, and has tended to find large positive effects of electrification. Posited mechanisms for these gains include structural transformation, which in turn changes employment opportunities (Rud (2012)); female empowerment (Dinkelman (2011)); increased agricultural productivity (Chakravorty, Emerick, and Ravago (2016)); health improvements as households switch from kerosene and coal to electricity (Barron and Torero (2016)); and greater educational attainment (Lipscomb, Mobarak, and Barham (2013)).
This paper documents that while large-scale rural electrification causes a substantial increase in energy access and power consumption, it leads at best to small changes in economic outcomes in the medium term. We exploit quasi-experimental variation in electrification generated by a population-based eligibility cutoff in India’s massive national rural electrification program, Rajiv Gandhi Grameen Vidyutikaran Yojana (RGGVY). The “Prime Minister’s Rural Electrification Program” was launched in 2005 to expand electricity access in over 400,000 rural Indian villages across 27 states. In order to cap program costs, the Central Government introduced a population-based eligibility cutoff based on the size of village neighborhoods (“habitations”). When the program was introduced, only villages with constituent habitations larger than 300 people were eligible for electrification under RGGVY.

We pair detailed geospatial information with rich administrative data on the universe of Indian villages and use a regression discontinuity (RD) design to test for the village-level effects of RGGVY eligibility on employment, asset ownership, household wealth, village-wide outcomes, and education. This design relies on relatively weak identifying assumptions, and we provide evidence that these assumptions are satisfied below. We estimate effects using a main sample of nearly 30,000 villages across 22 states. We demonstrate that RGGVY led to statistically significant and economically meaningful increases in electric power availability and consumption that is visible from space. We then show that despite these gains, electrification led to at most modest changes in economic outcomes. More specifically, we are able to reject even small changes, of 0.26 of a standard deviation, across a range of outcomes, including employment, asset ownership, the housing stock, village-wide outcomes, household

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1. In the 2001 Indian Census, the village was the lowest-level administrative unit. Villages are composed of habitations (or “hamlets”), which correspond to the inhabited areas of a village. South Asian villages typically have one or more inhabited regions surrounded by agricultural land. India’s 600,000 villages contain approximately 1.6 million unique habitations.
wealth, and school enrollment. Taken together, these results suggest that the causal impact of large-scale rural electrification on economic development may be substantially smaller than previously thought.

We show that these small effects do not simply reflect issues with the timing or quality of RGGVY project implementation. Our results are quantitatively similar for villages electrified near the beginning and near the end of our sample period, meaning that any confounding rollout effects are unlikely. Likewise, we find quantitatively similar results for the subset of states with above-average power supply reliability, which suggests that even in places with relatively infrequent power outages, the economic impacts remain quite small. We also employ an alternative identification strategy, difference-in-differences (DD), which reveals that our RD results appear to generalize to villages far from our 300-person population cutoff. Using this DD approach, we find treatment effects that are broadly consistent with our RD strategy, across the full support of Indian village populations. Our main RD results also stand up to a battery of placebo tests, falsification exercises, and robustness checks.

This paper makes three key contributions to the existing literature. First, our results contrast starkly with the large economic impacts of electrification found in earlier work. They apply directly to rural villages across 27 states in India, representing the world’s largest un-electrified population. Perhaps more importantly, we use a regression discontinuity design to quantify the effects of electrification; this necessitates substantially weaker identifying assumptions than the instrumental variables approaches of the prior literature. Second, we add to the knowledge on the economic effects of infrastructure in developing countries. Existing
work in this area has tended to find large positive impacts of infrastructure investments. We provide evidence that electricity infrastructure may not necessarily spur large-scale economic growth. Third, our results contribute to a small but growing literature on energy use in the developing world. We demonstrate that while electrified villages are consuming power, this energy use does not appear to be transforming rural economies.

The remainder of the paper proceeds as follows: Sections 2, 3, and 4 describe rural electrification in India, our empirical strategy, and the data used in our analysis. Section 5 presents our main empirical results, which we discuss and interpret in Section 6. Section 7 concludes.

2 RGGVY

At the time of its independence in 1947, only 1,500 of India’s villages had access to electricity (Tsujita (2014)). By March 2014, that number had risen to 576,554 out of 597,464 total villages. This massive technological achievement is largely attributable to a series of national electrification programs, the first of which began in the 1950s. The flagship program of India’s modern electrification efforts was Rajiv Gandhi Gramin Vidyutikaran Yojana (RGGVY), or the Prime Minister’s Rural Electrification Plan. Prior to RGGVY, over 125,000 (21 percent) of rural villages had no access to power whatsoever. Many of the remaining villages had extremely limited power access; 57 percent of all rural households lacked access to electricity,

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2. See, for example, Donaldson (forthcoming) on the effects of railroads on trade costs and welfare in India and Banerjee, Duflo, and Qian (2012) and Faber (2014) on roads in China.

with the majority of unelectrified households falling below the poverty line. Substantial opportunities remained to expand electricity access in rural communities.

RGGVY was launched in 2005 with the goal of extending power access to over 100,000 unelectrified rural villages in 27 Indian states. The program also set out to provide more intensive electrification to over 300,000 “under-electrified” villages. RGGVY’s primary mandate was to install and upgrade electricity infrastructure — specifically transmission lines, distribution lines, and transformers — in order to support electric irrigation pumps, small-to-medium industries, cold chains, healthcare, schooling, and information technology applications. Such infrastructure investments aimed to “facilitate overall rural development,  
employment generation, and poverty alleviation” (Ministry of Power (2005)). RGGVY also extended electric connections to public places, including schools, health clinics, and local government offices. While the program focused on providing electricity infrastructure to support growing village economies, RGGVY was also charged with extending household electricity access by offering free grid connections to all households below the poverty line. RGGVY investments occurred primarily on the intensive margin, upgrading existing infrastructure to have the capability to power growing rural economies. The majority of RGGVY works, including new grid connections, occurred in villages with some degree of household electrification prior to 2005.

In order for a village to be electrified under RGGVY, its state government had to submit an implementation proposal to the Rural Electrification Corporation (REC), a public-private financial institution overseen by the national government’s Ministry of Power. These

4. Above poverty line households were able to purchase connections. All households were required to pay for their own power consumption. The program did not subsidize the consumption of electricity for any household, but Indian retail electricity tariffs are heavily subsidized, and average 2.4 rupees (4 U.S. cents) per kilowatt-hour.
district-specific proposals, or Detailed Project Reports (DPRs), were based on village-level surveys carried out by local electric utilities, covering both unelectrified villages and partially electrified villages in need of “intensive electrification.” Each DPR proposed a village-by-village implementation plan, which included details on new electricity infrastructure to be installed and the number of households and public places to be connected. The REC reviewed DPR proposals, approved projects, and disbursed funds to states.

Funding for RGGVY came from India’s 10th (2002–2007) and 11th (2007–2012) Five-Year Plans. Districts were sorted into Plans on a first-come, first-serve basis: the first group of approved DPRs were allocated funding under the 10th Plan, and the next group were allocated funding under the 11th Plan. Under the 10th Plan, all villages with habitation populations above 300 were eligible for RGGVY electrification. Under the 11th Plan, this threshold was decreased to 100. Approximately 164,000 (267,000) villages in 229 (331) districts in 25 (25) states were slated for electrification under the 10th (11th) Plan, which also targeted 7.5 million (14.6 million) below-poverty-line households for free connections. Funding for the 10th Plan was disbursed between 2005 and 2010, with over 95 percent of funds released before 2008. The 11th Plan distributed funds between 2008 and 2011.

![Figure 1 about here](http://www.rggvy.gov.in)

Figure 1 shows the spatial distribution of RGGVY districts covered by the 10th and 11th Plans, highlighting the program’s broad scope. The vast majority of eligible districts

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5. Midway through the 12th Plan, RGGVY was subsumed into Deendayal Upadhyaya Gram Jyoti Yojana (DDUGJY); the remaining projects are slated to be finished by the end of the 13th Plan. As of 2016, all villages are eligible for electrification under DDUGJY, regardless of size.

6. We downloaded data on RGGVY implementation from [http://www.rggvy.gov.in](http://www.rggvy.gov.in) since replaced with [http://www.ddugjy.gov.in](http://www.ddugjy.gov.in). Appendix C describes the RGGVY program in greater detail, along with additional background on the history of rural electrification in India.
received RGGVY funding under exactly one Five-Year Plan, and 23 out of 27 states contain both 10th- and 11th-Plan districts. We focus our empirical analysis on the districts that received RGGVY funding under the 10th Plan, because electrification in these districts was completed earlier, giving us a longer post-electrification sample period.

3 Empirical approach

3.A Regression discontinuity design

In this paper, we aim to estimate the causal effect of rural electrification on development. Because energy infrastructure programs are large-scale investments, and because governments allocate funds to specific regions or groups of people in ways that are likely correlated with economic outcomes of interest, it can be challenging to disentangle the impact of electrification from other observed and unobserved factors that affect development. Furthermore, since the electricity grid is spatially integrated, a national-scale rollout of electrification is likely to have different effects than can be observed by a randomized controlled trial that impacts a few hundred rural villages. To overcome these challenges, we implement a regression discontinuity design, allowing us to identify the causal effect of electrification at scale.

Under the RGGVY program rules, villages in 10th-Plan districts were eligible for treatment if they contained habitations with populations of 300 or above. Our RD analysis includes only villages whose districts received funding under the 10th Plan, and we restrict our sample to villages with exactly one habitation. This allows us to use an RD to esti-

7. Lee, Miguel, and Wolfram (2016) are implementing a randomized controlled trial of household electrification in 150 rural communities in Western Kenya.
mate local average treatment effects for villages with habitation populations close to this 300-person cutoff. In this sharp RD design, eligibility for treatment changes discontinuously from 0 to 1 as village population (our running variable) crosses the 300-person threshold, allowing us to identify the effects of eligibility for RGGVY on both observed changes in electrification and on village-level economic outcomes.\(^8\)

This design necessitates two main identifying assumptions. First, we must assume continuity across the RD threshold for all village covariates and unobservables that might be correlated with our outcome variables. While this assumption is fundamentally untestable, we support it with evidence from several key village characteristics.\(^9\) We know of no other Indian social program with a 300-person eligibility threshold. Second, we assume that our running variable, 2001 Census population, is not manipulable around the threshold. Because our running variable predates the announcement of RGGVY in 2005, we are confident that our population data were not influenced by the future existence of RGGVY. Figure 3 shows no evidence of bunching of villages around this 300-person population cutoff.

Given these assumptions, our RD design provides a consistent estimate of the effect of eligibility for treatment on outcomes of interest for the set of single-habitation villages located in districts that received RGGVY funding under the 10th Plan. Formally, we estimate:

\[
Y_{vs}^{2011} = \beta_0 + \beta_1 Z_{vs} + \beta_2 (P_{vs} - 300) + \beta_3 (P_{vs} - 300) \cdot Z_{vs} + \beta_4 Y_{vs}^{2001} + \eta_s + \epsilon_{vs}
\]

for \(300 - h \leq P_{vs} \leq 300 + h\), where \(Z_{vs} \equiv 1[P_{vs} \geq 300]\).

\(^8\) See Imbens and Lemieux (2008) and Lee and Lemieux (2010) for further detail about the formal assumptions underlying RD analysis, and practical issues in applying RD designs.

\(^9\) We find no evidence to suggest that pre-period covariates change discontinuously across the 300-person cutoff. These results are available in Appendix B.4.5 as well as in Figure 5 below.
$Y_{vs}^{2011}$ represents the outcome of interest in village $v$ in state $s$ in 2011, $P_{vs}$ is the 2001 village population, $Z_{vs}$ is the RD indicator equal to one for villages above the cutoff, $h$ is the RD bandwidth, $Y_{vs}^{2001}$ is the 2001 value of the outcome variable, $\eta_s$ is a state fixed effect, and $\varepsilon_{vs}$ is an idiosyncratic error term. We cluster our standard errors at the district level to allow for arbitrary dependence between the errors of villages within the same district. This accommodates both implementer-specific correlations within a district’s DPR (RGGVY’s unit of project implementation) and natural spatial autocorrelation between nearby villages. We use a preferred RD bandwidth of 150 people on either side of the 300-person cutoff; this allows us to include a large sample of villages, while remaining confident that villages away from the discontinuity are similar to those at the 300-person cutoff.

3.B Economic Outcomes

Economic theory suggests that electrification could impact village economies through several channels. First, as electricity becomes available, we should expect small firms to invest in new capital equipment that uses power. This in turn would raise the marginal product of labor in the non-agricultural sector, drawing workers to new employment opportunities (Rud (2012)). On the other hand, electrification could spur agricultural mechanization, which would improve farm productivity (Chakravorty, Emerick, and Ravago (2016)).

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10. Neither the 2001 value of the outcome variable nor the fixed effects are necessary for identification, but they improve the precision of our estimates (see Lee and Lemieux (2010)).

11. We perform bandwidth sensitivity checks in Appendix B.1.2, including calculating the Imbens and Kalyanaraman (2012) optimal bandwidth; our results are not sensitive to bandwidth choice.

12. In the Indian context, one potential use of electricity in agricultural production is to power irrigation tubewells.
could either increase or decrease employment in agriculture.\footnote{The potential for changes in agricultural employment depends on several factors, including the excess supply of labor, the excess supply of farmland, the degree to which farm mechanization and agricultural labor are complements or substitutes, and the effect of electricity access on agricultural commodity prices.} However, because the marginal product of labor would unambiguously increase in both the agricultural and non-agricultural sectors, this should increase wages, incomes, and expenditures.

Next, electricity access may lead to gains for women. New employment opportunities, like those described above, could enable more women to work outside the home. Alternatively, newly-electrified households could invest in labor-saving devices, which could decrease the time required for women to complete household duties. This could also lead to increased female employment, either outside the home or in microenterprises. Dinkelman (2011) uses an instrumental variables approach in South Africa, and finds that electrification substantially raises female employment through this latter channel.

Rural electrification may also bring substantial health benefits. Kerosene is widely used throughout the developing world as a fuel for both lighting and cooking, and Indian households also commonly cook with coal and biomass. Combustion of these fuels produces harmful indoor air pollution, which is especially detrimental to young children and infants in utero. Access to electricity may foster investment in electric lights and electric cookstoves, which would likely reduce indoor air pollution and improve child health outcomes (Barron and Torero (2016)). Electrification may also indirectly improve health outcomes, through higher incomes and improved access to health care.

Finally, electrification could impact educational attainment through several channels. On the extensive margin, total school enrollment may increase if electrification leads to income gains, making households less reliant on child labor earnings. On the other hand,
rising wages may draw students out of school and into the labor force. Alternatively, we might expect electricity access to change the education production function. Lighting or computing facilities in schools may improve learning in the classroom, and children in homes with electric lighting will likely develop more effective study habits. If electrification improves student performance, it could affect the intensive margin of schooling as students tend to stay in school longer, causing enrollment in upper grades to increase. Using instrumental variables strategies, Lipscomb, Mobarak, and Barham (2013) find that rural electrification increases the number of years that students attend school.

4 Data

Our empirical analysis uses data from four main sources. First, we link satellite images of nighttime brightness to village boundary shapefiles, yielding a panel of village brightness. Next, we use several large administrative datasets published by three different Indian government entities, which contain village populations and a broad set of economic indicators. Armed with a wealth of data on Indian villages, we can test the channels through which we expect electrification under RGGVY to impact economic development.

4.A Nighttime lights data

In order to understand the economic effects of electrification resulting from RGGVY, we must first demonstrate that RGGVY led to a meaningful increase in electricity access and consumption in rural Indian villages. A binary indicator of the presence of electricity infrastructure would be insufficient, since it would mask heterogeneity in power quality, electricity
consumption, and connection density. There exists no comprehensive dataset of power consumption at the village level across India, but we are able to construct a measure of electricity consumption using remotely-sensed data.

As an indicator of electrification under RGGVY, we use changes in nighttime brightness as observed from space. The National Oceanic and Atmospheric Administration’s Defense Meteorological Satellite Program–Operational Line Scan (DMSP-OLS) program collects images from U.S. Air Force satellites, which photograph the earth daily between 8:30pm and 10:00pm local time. After cleaning and processing these images, NOAA averages them across each year and distributes annual composite images online. Each yearly dataset reports light intensity for each 30 arc-second pixel (approximately 1 km$^2$ at the equator) on a 0–63 scale, which is proportional to average observed luminosity. Figure 2 shows nighttime brightness in India in 2001 and 2011.

Economists frequently use these nighttime lights data as proxies for economic activity, as popularized by Chen and Nordhaus (2011) and Henderson, Storeygard, and Weil (2012). Existing work demonstrates that nighttime brightness can also be used to detect electrification, even at small spatial scales: Min et al. (2013) find evidence of a statistically detectable relationship between NOAA DMSP-OLS brightness and the electrification status of rural villages in Senegal and Mali. Min and Gaba (2014) show that a similar correlation between

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14. This cleaning removes any sunlit hours, glare, cloud cover, forest fires, the aurora phenomena, and other irregularities. Nighttime lights data are available for download at [http://ngdc.noaa.gov/eog/dmsp/downloadV4composites.html](http://ngdc.noaa.gov/eog/dmsp/downloadV4composites.html). We use the average lights product in our main analysis. See Appendix A.3 for further discussion.

15. Chen and Nordhaus (2011) detail the relationship between physical luminosity and brightness in the nighttime lights images.
electrification and nighttime brightness also exists in rural Vietnam. Chand et al. (2009) show a direct relationship between nighttime lights and electric power consumption in India, while Min (2011) finds a strong correlation between brightness and district-level electricity consumption in Uttar Pradesh. We build on this research by using nighttime brightness to demonstrate that RGGVY successfully increased village electricity access, where nighttime lights serve an objective measure of realized energy consumption in these villages.

Importantly, these satellite images represent a lower bound on electricity consumption. While nighttime brightness data record light output (including lighting from houses, public spaces, and outdoor streetlights), they do not directly measure electricity consumed for other purposes. Because all electricity end-uses rely on the same power grid, we treat increases in nighttime brightness as necessary indicators of investments in electricity infrastructure. Likewise, if total electricity consumption increases, we should expect nighttime brightness to increase as well, as more power reaches rural villages. A potential concern with using nighttime lights to proxy for total electricity consumption is that we could mistake new sources of outdoor lighting for increases in electricity access. However, RGGVY’s primary mandate was to expand and improve electricity infrastructure, and there is no mention of streetlight installation in 10th-Plan program documentation. Hence, an observed increase in nighttime brightness as a result of RGGVY would very likely be driven not by new streetlights alone, but rather by village-wide increases in access to energy services.

16. RGGVY 11th-Plan documentation did discuss streetlights in the context of a small carve-out for microgrids targeted at extremely remote villages. Because this carve-out did not exist under the 10th Plan, the 300-person eligibility cutoff did not apply for these villages.
We construct a village-level panel of nighttime brightness by overlaying annual NOAA DMSP-OLS images with 2001 village shapefiles. Our preferred measure of a village’s lighting is the maximum brightness of any pixel whose centroid lies within its borders. We use the brightest pixel because Indian villages are typically organized such that there are centralized populated areas surrounded by fields. This targets our electrification measure at the populated parts of villages, while avoiding measurement error from brightness averaged across unlit agricultural land. In performing this calculation, we are forced to drop 10 states from our sample. We are missing shapefiles for five states, which represent fewer than 3 percent of the total villages covered by RGGVY. We also exclude five states because we believe these shapefiles to be of extremely low quality: the correlation between the village area implied by the shapefiles and village area recorded by the Indian Census, the entity in charge of defining village boundaries, is below 0.35. We are left with a nighttime lights sample of 370,689 villages across 15 states. We do not impose these sample restrictions for any other outcome variables.

17. Indian villages have official boundaries, which are recorded by the Census Organization of India. Every square meter in India (excluding bodies of water and forests) is contained in a city, town, or village. We use shapefiles of village boundaries published by ML InfoMap, Ltd.

18. We calculate this level in ArcGIS, using the standard Zonal Statistics as Table operation. For villages too small to contain a pixel’s centroid, we assign the brightness value of the pixel at the village centroid.

19. Our results remain largely unchanged if we use the mean lights value rather than the maximum value. We also undertake a procedure to remove measurement error from the nightlights data via linear projection. See Appendix A.3 for details.

20. The five states with missing shapefiles are Arunachal Pradesh, Meghalaya, Mizoram, Nagaland, and Sikkim. The five states with low-quality shapefiles and village areas are Assam, Himachal Pradesh, Jammu and Kashmir, Uttar Pradesh, and Uttarakhand. The remaining states in the sample all have correlations between datasets above 0.6. See Appendix A.2 for further discussion.
4.B Census of India

We combine several village-level datasets published by the Census of India from the 2001 and 2011 decennial Censuses. The Primary Census Abstract (PCA) contains village population data, and a detailed breakdown of labor allocation by gender and job type. In particular, the PCA reports the number of men and women that are working in agriculture; “household industry workers” (engaged in informal production of goods within the home); and “other workers” that engage in all other types of work. Examples of “other workers” include government servants, municipal employees, teachers, factory workers, and those engaged in trade, commerce, or business. These data allow us to test for sectoral shifts in employment due to RGGVY electrification, either away from agriculture (consistent with structural transformation) or into agriculture (consistent with increased agricultural productivity). We also test for effects on female employment. Because we observe the share of women engaged in economic activity both outside and within the home, these data are well-suited to capture potential impacts of electrification on female labor.

The Houselisting Primary Census Abstract (HPCA) provides extensive data on living conditions, household size, physical household characteristics, and asset ownership. These data report the fraction of households that own a variety of assets, including radios, mobile phones, bicycles, motorcycles, and televisions. RGGVY may have contributed both directly and indirectly to asset ownership, if households purchased electric appliances to take advan-

21. These data are all publicly available at http://www.censusindia.gov.in. Because our research design relies on observing a large number of villages with populations around 300, we are unable to use additional Indian survey datasets such as the NSS or ASI. These datasets do not include a sufficient number of small villages to support our RD analysis, and are not designed to be representative below the district level.

22. The agriculture category is decomposed further into “cultivators” (on their own land) and “agricultural laborers” (on others’ land).
tage of improved power availability, or if potential income games from electrification enabled increased household expenditures on durable goods. Physical housing characteristics such as floor and roof materials are indicators of household wealth. If RGGVY spurred increases in household expenditures, we expect to observe medium-run investments to improve the housing stock. The HPCA also allows us to examine the health channel, as this dataset reports the fraction of households that cook with electricity and that use kerosene as a main source of lighting.

Finally, the Village Directory (VD), another Census dataset, contains detailed information on village amenities. In particular, the VD includes data on the presence of education and medical facilities; banking facilities and agricultural credit societies; the existence and quality of road network connections and the presence of bus services; and communications access, including postal services and mobile phone networks. We use these data to test for the effects of RGGVY on village amenities. The VD also includes information on village electrification, in the form of binary indicators of electric power availability in each village, separately for the agricultural, domestic, and commercial sectors. These indicators are coded as “1” if any electric power was available for a given end use anywhere in the village, and as “0” otherwise. Two-thirds of RGGVY 10th-Plan villages met this criterion at baseline (i.e. were coded as “1” for electric power availability), making these variables particularly poorly suited to analyze the effects of RGGVY. The main goals of RGGVY were to upgrade energy infrastructure and increase the penetration of electricity access within each village. The VD data contain no information on the intensity of electrification within a village, and therefore

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23. In 2001, the VD was a separate Census product. In 2011, it was bundled into the District Census Handbook (DCHB).
do not reflect the vast majority of RGGVY works. We instead turn to the nighttime lights data, which allow us to track intensive-margin changes in energy consumption.

We combine the PCA, HPCA, and VD data into a two-wave village-level panel. The 2001 PCA also reports the official 2001 population of each village, which was the population of record for the RGGVY program, and which we use as our RD running variable. However, RGGVY implementing agencies were instructed to determine eligibility based on 2001 habitation (sub-village neighborhood) populations. To the best of our knowledge, the only nation-wide habitation census in existence was conducted by the National Rural Drinking Water Program. We use a fuzzy matching algorithm, modified from Asher and Novosad (2016), to link this habitation census to our village panel and identify the 50 percent of villages with exactly one habitation. For these single-habitation villages, habitation populations are equivalent to village populations—meaning that 2001 village population should exactly correspond to the population that determined RGGVY eligibility for these villages.

The main dataset for our analysis contains the 2001–2011 Census, nighttime brightness, RGGVY program implementation details, and the number of habitations in each village. The subsample of single-habitation, 10th-Plan villages comprises 20 percent of Indian villages. After restricting this 20 percent sample to our preferred RD bandwidth of 150 people above

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24. The 2011 Village Directory also reports the average hours of electricity available per day, by sector. Because electricity is distributed over an integrated grid, it is unlikely that RGGVY’s infrastructure upgrades would have any effect on these measures of electricity access.

25. RGGVY ledgers we observed in Rajasthan were pre-printed with 2001 Census populations.

26. Administered by the Ministry of Drinking Water and Sanitation, this census of habitations was collected in 2003 and 2009, and is available at [http://indiawater.gov.in](http://indiawater.gov.in).

27. We thank the authors for sharing their code. Appendix A.5 details our matching algorithm.

28. 50 percent of villages are in districts eligible under RGGVY’s 10th Plan, 86 percent of villages match to the habitation census, and 52 percent of matched villages in 10th-Plan districts have one habitation. Our analysis excludes villages that match to the habitation census but have populations that disagree by over 20 percent across datasets, as these matches are likely erroneous. In Appendix B, we show that including these villages slightly attenuates our RD point estimates as expected, yet they remain statistically significant.
and below the 300-person threshold, we are left with 29,765 10th-Plan single-habitation villages from 22 states.\footnote{Three small states with 10th-Plan districts (Manipur, Kerala, and Tripura) are excluded from our final regression because they have no villages that meet these criteria.} The left panel of Figure\ref{fig:population} displays a histogram of village populations, showing that the modal village lies within our RD window of 150–450 people. The right panel demonstrates how our two sample restrictions reduce the size of our RD sample, and shows that our running variable, 2001 village population, is smooth across the RD threshold.

Table\ref{table:summary} reports 2001 summary statistics for three sets of villages with populations between 150 and 450: all Indian villages, all villages in 10th-Plan districts, and all villages in 10th-Plan districts that have only one habitation. On average, villages in 10th-Plan districts are geographically smaller and less electrified than the national average, but similar across a range of other covariates. 10th-Plan villages with only one habitation are very similar on observables to average 10th-Plan villages.

4.C Socioeconomic and Caste Census

We draw on individual-level microdata from the Socioeconomic and Caste Census (SECC) for measures of income and alternative employment data. The SECC was collected between 2011 and 2012, with the goal of enumerating the full population of India. We obtained a subset of these data from the Ministry of Petroleum and Natural Gas, whose liquid petroleum gas subsidy program, Pradhan Mantri Ujjwala Yojana, uses SECC data to determine eligibility.\footnote{The Ministry of Rural Development, who collected the SECC, are in the process of making the full dataset publicly available. As of now, only district-level aggregates are posted at \url{http://secc.gov.in/welcome}. We downloaded our data in Excel format from \url{http://lpgdedupe.nic.in/secc/secc_data.html}.}
As a result, we observe the universe of rural individuals that are eligible for this fuel subsidy program. This includes all individuals living in households that satisfied at least one of seven poverty indicators, and that did not meet any of fourteen affluence criteria. This yields a dataset of data of 332 million individuals from 81 million households, representing roughly half of all households in rural India.

For this selected sample, we observe individual-level data on age, gender, employment, caste, and marital status; and household-level data on the housing stock, land ownership, asset ownership, and income sources. We use the SECC to test for the effects of RGGVY on wealth, using three main indicators. First, we test for the fraction of households with at least one poverty indicator (and no affluence indicators), as measured by the fraction of 2011 Census households that appear in our SECC dataset. Next, the SECC contains an indicator for whether the main income earner in each household earns at least 5,000 rupees per month. This represents the highest-resolution measure of household income in a large-scale Indian dataset, enabling us to directly, albeit coarsely, test the effect of electrification on income. We also use SECC data to test for the effects of RGGVY on the fraction of households that own land or have at least one salaried laborer, two additional wealth indicators. Finally, we construct SECC employment variables that are analogous to the Census’s village-wide measures, allowing us to test for distributional employment effects among the subset of households with poverty indicators.

31. The sample also excludes the less than 1 percent of the population that met one of five destitution indicators. See Appendix A.6 for more details on the inclusion and exclusion criteria. We are missing data from six rural districts, which represent less than 1 percent of Indian villages.

32. All households whose primary earner made over 10,000 rupees per month were ineligible for the fuel subsidy program, and are not included in our SECC dataset.
4.D District Information System on Education

In order to estimate the effects of electrification on education, we include data on the universe of Indian primary and upper primary schools from the 2005–2006 school year through the 2014–2015 school year.\[^{33}\] These data come from the District Information System on Education (DISE), which reports annual school-level snapshots on a variety of student, teacher, and school building characteristics. We collected these data at the school level and construct a 10-year panel dataset containing information from 1.68 million unique schools.\[^{34}\] This panel is strongly unbalanced, and the average school appears in 7 out of a possible 10 years. Given that the reporting of school characteristics varies considerably across years, we focus our analysis on village-wide enrollment counts, which are consistently reported by gender and grade level. We test for effects of RGGVY on total enrollment, enrollment by gender, and enrollment by grade level, which allows us to measure how electrification impacted both the extensive and intensive margins of schooling.

5 Regression discontinuity results

5.A Electrification

In order to demonstrate that RGGVY had a meaningful effect on electrification in eligible villages, we examine the effects of eligibility for RGGVY on nighttime brightness. Specifically, we use Equation (1) to estimate the effect of having a 2001 population above the RGGVY

\[^{33}\] While we use the full time series to match DISE schools to Census villages, we restrict our analysis to the 2010–11 school year, for consistency with our other outcome variables.

\[^{34}\] We downloaded these data from [http://schoolreportcards.in/SRC-New/](http://schoolreportcards.in/SRC-New/) See Appendix A.7 for details.
cutoff on village brightness in 2011. After removing states with low-quality or missing shape-files, we are left with a sample of 18,686 single-habitation villages, in RGGVY 10th-Plan districts across 12 states, with populations in our RD bandwidth of 150–450 people.

Figure 4 presents the results from our preferred RD specification graphically, while Table 2 reports the corresponding numerical results. We find that 2011 nighttime brightness increased discontinuously at the 300-person threshold by 0.15 units of brightness. This jump is statistically significant at the 5 percent level, with a \( p \)-value of 0.015. Appendix B.1 demonstrates that this is robust to a range of alternative bandwidths, functional forms, and specifications.

Though this point estimate might seem small, these results in fact demonstrate that RGGVY eligibility led to a substantial increase in brightness for barely-eligible villages as compared to barely-ineligible villages. To interpret these effects, we turn to the remote sensing literature. The magnitude of the effect we observe is consistent with ground-truthed estimates by Min et al. (2013), who find that electrification is associated with a 0.36-unit increase in nighttime brightness in rural villages in Senegal\(^{36}\). Our point estimate of 0.15 is on the same order of magnitude but smaller, which is to be expected, given that villages in

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35. These results include a control for 2001 nighttime brightness. Due to substantial cross-sectional heterogeneity, conditioning on the pre-period level dramatically improves the signal-to-noise ratio. This is common practice with remote sensing data (see also Jayachandran et al. (2016)). If we restrict the RD sample to include only villages that had electric power availability, according to the 2001 Census, we recover a nearly identical result (\( \hat{\beta}_1 = 0.16 \) with a \( p \)-value of 0.046). This suggests that the Census’s 1/0 indicator variable for electric power availability masks substantial changes in electricity access under RGGVY, which we are able to detect using nighttime lights.

36. This result uses the same average annual DMSP–OLS product that we use, unlike many of the other results reported in the paper, which rely on monthly composites that are not publicly available. We exclude the Mali results described in Min et al. (2013) because the authors exclude them from their main regression estimates.
our RD bandwidth are significantly smaller than the villages studied in Senegal. In a similar exercise, Min and Gaba (2014) find that a 1-unit increase in brightness corresponds to 60 public streetlights or 240–270 electrified homes in Vietnamese villages.  

Extrapolating these results to the Indian context, our estimated 0.15-unit increase translates to roughly 9 additional streetlights per village. This represents a substantial increase in nighttime luminosity, especially considering that RGGVY did not install streetlights. Alternatively, if we extrapolate the (weaker) household relationship to our setting, a 0.15-unit increase would translate to roughly 38 newly electrified homes, or 68 percent of households in the average village in our RD sample. These estimates from Senegal and Vietnam suggest that our effect size in India is consistent with a substantial increase in village electrification under RGGVY, especially given that many electricity end-uses that RGGVY sought to enable are not captured by the nighttime brightness proxy.

We perform a series of validity tests in order to demonstrate that this increase in brightness is, in fact, attributable to the RGGVY program. First, we estimate Equation (1) using 2005 nighttime brightness as the dependent variable. Because RGGVY was announced in 2005 and nearly all project implementation began in subsequent years, we should not expect to find an immediate effect of program eligibility on brightness. The left panel of Figure 5 shows no visual evidence of a discontinuity in 2005 brightness at the 300-person threshold. The point estimate in this regression is 0.031, with a standard error of 0.020.

37. The relationship between nighttime brightness and streetlights is predictably stronger than the relationship between nighttime brightness and electrified homes.

38. While many factors could cause the relationship between household electrification and nighttime brightness to differ between India and West Africa or Vietnam, Min et al. (2013) and Min and Gaba (2014) provide evidence that the magnitude of our RD point estimate is consistent with what we might expect from a substantial increase in electricity access in these small villages.
and is not statistically significant at conventional level. This demonstrates that nighttime brightness was smooth at the 300-person cutoff prior to RGGVY.  

Next, we conduct a placebo test using 801 placebo RD “thresholds” between 151 and 1000. For each threshold, we re-estimate Equation (1) and save \( \hat{\beta}_1 \). We plot the distribution of these placebo coefficients in the center panel of Figure 5. We also perform a randomization inference exercise, by scrambling the relationship between nighttime brightness and village population 10,000 times. For each iteration, we estimate Equation (1), and the right panel of Figure 5 shows the resulting distribution of RD point estimates. The red lines indicate our estimate of \( \hat{\beta}_1 \), which falls above the 99th percentile of the placebo distribution and above the 98th percentile of the randomization distribution. This provides evidence that our RD estimates do not simply reflect spurious volatility in the relationship between nighttime lights and village population data.

We also perform a falsification exercise based on the implementation details of the RGGVY program. Our RD sample includes only villages that were eligible for RGGVY under the 10th Plan, for which the relevant eligibility cutoff was 300 people. It also includes only those villages confirmed to have exactly one habitation, for which 2001 village population is the appropriate running variable. We should not find effects at the 300-person cutoff on nighttime brightness for villages eligible under the 11th Plan, for which the relevant eligibility cutoff was moved from 300 to 100 people. Similarly, we should not find any RD

39. We perform a variety of additional pre-period covariate smoothness checks in Appendix B.4.5, and find no evidence of discontinuities prior to RGGVY. Appendix B.3 demonstrates that the discontinuity in brightness steadily increases from 2006 onward.

40. We test all 801 integer values in \([151, 275] \cup [325, 1000]\), which is asymptotically equivalent to simulating placebo draws across this discrete support. We omit thresholds between 275 and 325 to avoid possible contamination of the placebo results with the real threshold. We also avoid placebo thresholds below 151, to ensure positive values across the full 300-person RD window.

41. We assign lights values to each village by sampling \( \{Y_{v}^{2001}, Y_{v}^{2011}\} \) pairs without replacement.
effects for villages comprising multiple habitations, because these villages’ populations do not correspond to the habitation populations that determined RGGVY eligibility. Figure 5 presents RD results estimated using these alternative samples: as expected, none exhibits evidence of a discontinuity at the 300-person cutoff. This provides strong evidence that RGGVY, rather than spurious effects or other programs, is causing these effects.

5.B Economic outcomes

We now turn to the effects of RGGVY eligibility on village economies, and test for impacts of electrification via each of the potential channels discussed in Section 3.B. We estimate Equation (1) using outcome variables from six broad categories: employment, asset ownership, housing stock characteristics, village-wide outcomes, household income, and education. Each RD regression uses a dependent variable from 2011, while controlling for 2001 population as the running variable, state fixed effects, and the 2001 level of the dependent variable (unless otherwise noted).

First, we test for employment effects by estimating Equation (1) using the total number of male (female) workers in a given category divided by the total male (female) population of a village as the dependent variable. Figure 7 summarizes these workforce results graphically for each gender and sector, and Panel B of Table 3 reports them numerically. We find that eligibility for RGGVY caused a 0.7 percentage point decrease in the share of men working in agriculture, on a mean of 42 percent. In contrast, the percentage of men in

42. 2011 population does not change discontinuously at the 300-person threshold. See Panel A of Table 3 where we find that RGGVY caused no meaningful changes in village demographics.
non-agricultural, non-household labor increased by 0.5 percentage points, on a mean of 10 percent. While these sectoral shifts are statistically significant and in a direction consistent with the structural transformation hypothesis, these effect sizes are very small: we can reject changes in male labor allocation larger than 1.3 percentage points. We find no statistically significant effects of electrification on the share of women working in any sector, and similarly narrow confidence intervals allow us to reject changes in female employment larger than 1.3 percentage points.43

We next test for effects of RGGVY eligibility on the share of households with a variety of different assets and housing stock characteristics. Figure 8 depicts RD results for the percent of households that own a telephone, own a television, own a motorcycle, have kerosene lighting, have mud floors, and are categorized as “dilapidated” by the Census. We see no strong graphical evidence of discontinuous changes in any of these dependent variables at the 300-person cutoff. Table 3 presents these results numerically in Panels C and D, while also reporting on the share of households with radios, bicycles, and without assets; the share of households cooking with electricity or gas; and the share of households with thatched roofs. Consistent with the graphical evidence in Figure 8 these results show that RGGVY did not lead to economically meaningful investments in electricity-using assets, non-electricity-using assets, or the housing stock in the medium term. We can reject increases larger than 1 percentage point in all cases. This suggests that RGGVY is unlikely to have contributed to significant increases in household expenditures. The program is also unlikely to have

43. These results focus on the extensive margin of employment (i.e., number of workers). We also test for effects the intensive margin of employment in Appendix B.5 (i.e., share of workers working at least six months of the year). We find no evidence of statistically significant or economically meaningful changes on the intensive margin.
led to meaningful reductions in indoor air pollution, since we see no effects on the share of households with kerosene lighting or electric/gas cooking.

In Panel E of Table 3 we present RD results for village-level outcomes, including mobile phone coverage, the presence of agricultural credit societies, and the presence of irrigation tubewells, and the share of village area planted and irrigated. These results are not statistically significant, and even the upper bounds on the 95 percent confidence intervals represent economically insignificant changes (smaller than 2 percentage points) in these outcomes. Taken together, these result imply that if RGGVY did lead to increases in agricultural productivity, farmers did not respond by increasing either the scale of irrigation or total farmland.

44. Tubewells are deep wells used for groundwater extraction, which are a common means of irrigation throughout rural India. Electric pumps improve the efficiency of tubewells.

Next, we test for effects of RGGVY eligibility on economic outcomes among households with at least one poverty indicator. We estimate Equation (1) using the fraction of households with at least one poverty indicator (and zero affluence indicators) as the dependent variable. We also test for effects on the fraction of this subset of households for which the main income earner earns at least 5,000 rupees per month. The top row of Figure 9 presents these results graphically, revealing no evidence that RGGVY led to changes in these outcomes. Panel A of Table 4 reports the corresponding regression results, along with RD estimates for the fraction of households that report salaried employment and that own land. For each outcome, we can reject increases larger than 1.6 percentage points, at 95 percent confidence, suggesting that
eligibility for RGGVY did not have economically meaningful effects on household poverty or wealth.

Using the SECC dataset, we also can test whether RGGVY eligibility had different employment impacts among individuals of lower socioeconomic status. We construct sector-specific labor shares that are analogous to Panel B of Table 3 except that they include only adults living in households with at least one poverty indicator. We report these results graphically in the bottom row of Figure 9 and numerically in Panel B of Table 4. While the SECC sample differs notably from the village averages in the PCA, these results are broadly consistent with our main labor results, and visual evidence suggests a small decrease (increase) in agricultural (other) employment for adult men. We can reject 2 percentage point shifts across all six labor categories, which suggests that the average employment effects of RGGVY were similar to the effects on less wealthy households.

Finally, we test for the effects of RGGVY eligibility on education. We estimate Equation (1) using village-wide enrollment for grades 1–8, both pooled and separately by gender, as the dependent variable. We also test for separate effects for primary (grades 1–5) and upper primary (grades 6–8) enrollment, where the latter reflects changes on the intensive margin of schooling. We report these results in Figure 10 and Table 5, which show no statistically significant changes in enrollment at the 300-person threshold. As with our other results,

45. See Appendix A.6 for further information on how we constructed these categories from the SECC data.
46. These regressions control for the 2005 level of the outcome variable, which is the earliest year of enrollment data available. Also, we note that because these village-level enrollment regressions aggregate enrollment across all schools in each village, they might confound changes in within-school attendance with changes in enrollment due to new school construction over time. Appendix B.3 repeats these same regressions using school-level enrollment observations, while conducting additional sensitivity analysis.
our 95 percent confidence intervals can reject even moderate changes in enrollment on either the intensive or extensive margins.

[Figure 10 about here; Table 5 about here]

Taking these results together, we conclude that while the provision and consumption of electricity substantially increased as a result of RGGVY eligibility, we detect no economically meaningful changes in labor outcomes, asset ownership, the housing stock, village-level outcomes, household income, or school attendance. Our RD results are precisely estimated, enabling us to rule out even modest effect sizes for these outcomes. This suggests that eligibility for RGGVY did not lead to structural transformation, increased agricultural productivity, female empowerment, reductions in indoor air pollution, improved education, or poverty reductions.

6 Interpretations and Extensions

6.A Scaling

The above regressions recover intent-to-treat estimates: they show the effect of being eligible for RGGVY on our outcomes of interest. In order to compute average treatment effects, we need to scale these estimates such that we recover the effect of electrification on development.

We propose several methods of scaling our estimates. First, we consider inflating our outcomes based on the proportion of villages within our bandwidth that RGGVY claims to

47 We do not scale via two-stage least squares because we do not have access to a binary “RGGVY electrification” variable, nor would this variable capture different levels of energy access and consumption across villages treated under RGGVY, as discussed above.
have treated. This is akin to the scale factor we would apply with a traditional instrumental variables estimator. RGGVY’s district-level aggregate data suggest that between 56 and 82 percent of eligible villages were treated by the program.\footnote{RGGVY’s aggregate village counts in 10th-Plan districts sum to 56 percent of the total number of villages in these districts, and 82 percent of villages with 2001 populations over 300.} This implies that our estimates should be inflated by approximately a factor of 1.5 in order to recover the causal effects of treatment under RGGVY.

Alternatively, we can calibrate a scaling factor to the magnitude of the increase in nighttime brightness, which we estimate to be 0.15 units of brightness. Min et al. (2013) suggest that when villages in Senegal were electrified, they experienced increases of approximately 0.4 nighttime brightness points. If, alternatively, we apply Min and Gaba (2014)’s estimates of a 1-unit increase in brightness corresponding to 240–270 electrified households, then full electrification of the average village in our RD sample with 56 households would imply an increase of 0.2 brightness points.\footnote{These increases of 0.4 and 0.2 are internally consistent; the average villages in Min et al. (2013) and Min and Gaba (2014) are larger than the villages in our RD bandwidth.} This suggests that our RD estimates should be inflated by a factor of between 1.3 and 3 to recover the average effect of RGGVY electrification.\footnote{We do not propose a scale factor based on Min and Gaba (2014)’s streetlights estimate, since we do not have data on the number of streetlights per village, and because RGGVY did not install streetlights.}

Scaling the point estimates reported in Tables \ref{tab:table3}--\ref{tab:table5} by a factor of 3 does not yield adjusted estimates that are economically meaningful. For the vast majority of outcomes, we see no visual evidence of a discontinuity, suggesting that these upper bounds are quite conservative. Even after inflating the 95 percent confidence intervals by these factors, we can still reject 4 percentage point changes in labor outcomes, 4 percentage point changes in asset ownership, 5 percentage point changes in the housing stock, and 8 percentage point changes in village-level outcomes. We can also reject 6 percentage point changes in outcomes
in Table 4 as well as 21 student (30 percent) increases in total school enrollment. Scaling by a factor of 3, we can rule out effects larger than 0.26 of one standard deviation in all outcomes presented in Tables 3–5.

Even if we were to scale our estimates by an extremely conservative factor of 10, we can still reject effect sizes consistent with the previous literature.\(^51\) Dinkelman (2011) finds that electrification caused 9–9.5 percentage point increases in female employment; we can reject 2 percentage point increases in total female employment.\(^52\) Lipscomb, Mobarak, and Barham (2013) likewise find large effects of electrification on total employment rates; we can reject 1 percentage point increases in the village-wide employment rate even after applying a conservative scaling factor of 10.\(^53\) Chakravorty, Emerick, and Ravago (2016) find that rural electrification leads to a 56 percent decrease in a deprivation index, and a 38 percent increase in household expenditures; scaling our Table 4 results by 10, we can reject an 11 percentage point decrease in the share of households with at least one poverty indicator, and a 13 percentage point increase in the share of households (with at least one poverty indicator) with monthly incomes greater than 5,000 rupees.

51. In order to arrive at factor of 10, which we believe to be the most conservative interpretation of our results, we assume that RGGVY only impacted household electricity end-uses. Our nighttime brightness effect of 0.15 is comparable to the change in brightness associated with a 10 percentage point increase in the share of households with electric lighting, a proxy for household power consumption, at the mean of our RD sample. This suggests a scaling factor of 10 to translate this into an increase from 0 to 100 percent of households.

52. We estimate Equation (1) pooling female employment across all three sectors, resulting in an RD point estimate of \(-0.0067\) with the upper end of our 95 percent confidence interval of 0.0015, which we multiply by 10. We can similarly reject increases of 3 percentage points in female agricultural employment, 1 percentage point in female household employment, and 4 percentage points in female other employment.

53. If we pool all six labor outcomes in Panel B of Table 3, the resulting RD point estimate is \(-0.0053\) with an upper 95 percent confidence interval of 0.0002.
6.B Heterogeneous effects

It is possible that our results mask heterogeneity in the quality of energy services experienced by RGGVY villages. In particular, India faces major electricity shortages, which vary across locations (Allcott, Collard-Wexler, and O’Connell (2016)). If half of the villages in our sample experienced frequent power outages while the other half received consistent power, our average intent-to-treat estimate across both groups would be small even if RGGVY led to large economic effects in places with high-quality energy supply. We test for this by re-estimating all of our RD results using the subset of states with above-average power availability (Central Electricity Authority (2011)). In this subsample, our estimated RD coefficient on nighttime brightness increases from 0.15 to 0.25, statistically significant at the 1 percent level. However, the results for labor, asset ownership, the housing stock, village-level outcomes, and household wealth are quantitatively similar to those estimated using the full RD sample.

This suggests that poor power quality in a subset of states is not attenuating our estimate of the average effect across the full sample. Moreover, our main RD results reflect the realized implementation of a large-scale national rural electrification program in the developing world. Even if we had found substantial positive effects for a subset of states, the overall treatment effect would be indicative of the degree to which future rural electrification programs might be limited by the supply reliability.

54. These seven states are (in decreasing order of 2011 power quality): Chhattisgarh, Orissa, Karnataka, West Bengal, Gujarat, Haryana, and Rajasthan.

55. Appendix B.10 reports regression results for both split-sample exercises discussed in this section. The schooling results are qualitatively similar, but somewhat less robust.
It is also possible that we do not detect large effects because the benefits of electrification take many years to accrue. While we cannot rule this possibility out completely, our 2011 outcome data were collected between three and five years after 95 percent of villages in our sample received RGGVY funding. Even if there were significant delays in implementation, this is much longer than the time span over which development interventions are typically studied. Nevertheless, we recover quantitatively similar RD point estimates when we restrict our RD sample to districts with early RGGVY funding. Therefore, it is unlikely that our small results are driven by villages that failed to take advantage of the full set of possible medium-run benefits of electric power before being surveyed by the 2011 Census.

6.C Difference-in-differences

Finally, we might be concerned that villages close to the 300-person RD threshold stand little to gain from electrification. Perhaps these small villages are simply too poor, too credit-constrained, or too economically isolated to translate increased electricity access into new employment or income-generating opportunities. We employ a second identification strategy, difference-in-differences (DD), to test for the effects of RGGVY eligibility on larger villages far from our RD threshold. Recall that there were two major phases of RGGVY implementation: the 10th-Plan phase and the 11th-Plan phase. The majority of 11th-Plan electrification projects had not been completed before the 2011 Census. We can therefore use 10th-Plan districts as a “treated” group and 11th-Plan districts as a “control” group in a DD framework. We estimate the following fixed effects specification on our two-decade

56. Over 70 percent of villages in our RD sample are in districts that received RGGVY funding before the end of 2006. See Appendix Table 42.

57. Selection into the different plans was non-random. It is plausible that 10th-Plan districts were more administratively capable than 11th-Plan districts, likely biasing our DD estimates upward.
village panel:

\[ Y_{vt} = \gamma_0 + \sum_b \gamma^b \cdot 1_{[10th \times Post]}_{vt} \cdot 1_{P_v \in \text{Bin}_b} + \delta_t + \eta_v + \varepsilon_{vt} \]

where \( 1_{[10th \times Post]}_{vt} \) is an indicator equal to one if village \( v \) was eligible for RGGVY under the 10th Plan and the year \( t \) is 2011, \( 1_{P_v \in \text{Bin}_b} \) are 2001 village population bins along the full support of populations (shown in Figure 3), \( \delta_t \) are year fixed effects, and \( \eta_v \) are village fixed effects. This necessitates stronger identifying assumptions than our RD specification, namely that villages in 10th-Plan districts were trending in parallel to 11th-Plan villages prior to RGGVY. Village-level data are not available for the 1991 Census, therefore we are unable to directly test this assumption.\(^{58}\)

Figure 11 compares our main RD results with DD results from estimating Equation (2) with 300-person population bins, for nighttime brightness and male agricultural workers. For both outcomes, the RD point estimates lie within the DD confidence intervals. Moreover, the DD effect of RGGVY on nighttime lights increases nearly monotonically in population, while the DD effect for male agricultural labor is close to constant as population increases. This suggests that our small RD results for male agricultural employment are likely to be externally valid outside of our RD bandwidth. Other economic outcome variables show similarly constant DD coefficients across small and large villages.\(^{59}\)

These DD results are broadly consistent with our RD results, despite using a much larger population of villages (from 10th- and 11th-Plan districts, including multi-habitation

\(^{58}\) In Appendix B.11 we test for differential pre-trends using district-level data. These trends are not statistically zero, suggesting that our DD results should be interpreted with some caution.

\(^{59}\) We report additional DD results in Appendix B.11
villages) and using 11th-Plan villages as counterfactuals (as opposed to barely ineligible 10th-Plan villages). Beyond allowing us to extend our RD results to larger villages, the DD results are encouragingly similar to the RD. Relying on alternative identifying assumptions on a different sample of villages, we again demonstrate that RGGVY caused nighttime brightness to increase, but has not meaningfully improved the economic outcomes that we observe.

[Figure 11 about here]

6.D Costs and benefits

We do not have direct estimates of village-level program costs, incomes, or expenditures. However, we perform several back-of-the-envelope calculations based on our RD results. This enables us to better understand the overall economics of RGGVY, while also quantifying the costs and benefits of electrification.

First, we consider the per-village costs of RGGVY implementation. In 2005, RGGVY was expected to cost 634.2 billion rupees, or approximately $17.2 billion. Given the stated scope of the program detailed in Section 2, this suggests a cost per village of approximately 1,470,000 rupees, or $36,000 in 2015 USD.

We can apply average Indian rural wage rates to estimate the income differential that might have resulted from the (small) sectoral shift from agricultural to non-agricultural

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60. The SECC income data indicate whether households’ main income earners earned more or less than 5,000 rupees per month, and comes from a selected subset of households. Hence, we exclude these data from the subsequent cost-benefit analysis.

61. We use the 2005 exchange rate of 44 rupees per dollar, and convert to 2015 USD.

62. This is comparable to Chakravorty, Emerick, and Ravago (2016), who report average electrification costs of $42,000 per village in the Philippines.
employment we observe under RGGVY. According to India’s National Sample Survey Office, the average 2011 wage for male (female) non-agricultural workers was 196 (116) rupees per day, which was 26 (0.9) percent higher than the average agricultural wage of 155 (115) rupees per day. To compute the average increase in village-level income, we scale the lower bound of our confidence interval for male (female) agricultural labor from Table 3, \(-0.013 (-0.013)\), by a factor of 3. This converts our intent-to-treat estimate into an average treatment effect, and it implies a maximum shift out of male and female agricultural employment of 3.9 percentage points.\(^{63}\) If all of these men (women) shifted from agriculture into non-agriculture employment, then total daily male (female) village wage earnings would have increased by approximately 293 (7) rupees. If each employed person worked 365 days per year, this would translate into a total annual village income increase of approximately 109,000 rupees, or an upper bound of 1.4 percent.

Alternatively, we can use our RD estimates on household asset ownership to infer changes in expenditures resulting from electrification. Scaling the upper confidence intervals in Panel C of Table 3 by 3, we can reject increases in asset ownership of greater than 3.9 percent for mobile phones, 3.0 percent for televisions, 2.1 percent for bicycles, and 1.2 percent for motorcycles. Monetizing these upper bounds using asset prices from ICRISAT’s Village Dynamics in South Asia dataset, this implies a maximum average household expenditure of 572 rupees.\(^{64}\) Supposing that only 10 percent of RGGVY-driven expenditure increases were spent on these four durable goods implies a maximum increase in per-household expenditure of 5,720 rupees, or a total village-wide increase of around 398,000 rupees. These asset pur-

\(^{63}\) In keeping with Section 6.A, we apply a scaling factor of 3 throughout this section.

\(^{64}\) The average prices for durables commonly purchased after electrification are: Rs 2,796 for cell phones, Rs 4,166 for televisions, Rs 1,259 for bicycles, and Rs 25,922 for motorcycles.
chases occurred during the 3–6 year period after electrification; if we conservatively assume that they all occurred within 3 years of electrification, this would represent at best a 2.1 percent increase in annual village expenditures.65

Our back-of-the-envelope estimates suggest that annual village income increased by a maximum of 109,000 rupees, that annual village expenditures increased by a maximum of 133,000 rupees, and that RGGVY electrification came at a cost of approximately 1,470,000 rupees per village. These results are quite conservative: though we do not measure all possible benefits from electrification, the benefits we do use in performing this calculation come almost entirely from regression estimates where we cannot reject zero; our assumptions in performing this calculation also make it biased towards finding large effects. Using the larger expenditure estimate and applying a conservative 3 percent discount rate, this translates into a payback period of approximately 12 years.66

At best, we find that RGGVY increased annual incomes by 1.4 percent and annual expenditure by 2.1 percent, despite causing a substantial shift in nighttime lights. This suggests exercising caution when using nighttime brightness as a proxy for income or expenditures. The DMSP-OLS dataset measures light emissions. Because brightness relates directly to energy consumption through lighting, it serves as a useful indicator of electrification. Since electrification should lead to increased brightness even absent a corresponding increase in incomes, we do not use the DMSP-OLS data as a proxy for income/expenditures, and caution others against doing so when evaluating programs that directly increase light emissions.

65. India’s average rural monthly per capita expenditures were 1,430 rupees for 2011–2012.
66. This starkly contrasts with Chakravorty, Emerick, and Ravago (2016), who find a payback period of approximately 1 year; however, it is corroborated by evidence from Lee, Miguel, and Wolfram (2016), who use revealed preference results to suggest that the costs of electrification are much larger than the benefits.
Importantly, our results do not speak directly to the effects of RGGVY on welfare. It is quite possible that electrification has dramatically increased average quality of life for rural Indians. Indeed, since villagers are using more power as a result of RGGVY, revealed preference suggests that they benefit from the program. Even though we measure a wide range of outcome variables which are typical of large-scale administrative datasets, there may be important utility benefits that we cannot measure. Our results highlight the need to incorporate additional non-market measures into future administrative data collection efforts.

7 Conclusion

In this paper, we evaluate the medium-run effects of electrification on development using a regression discontinuity (RD) design which exploits a population eligibility threshold in India’s national rural electrification program, RGGVY. We find that eligibility for RGGVY led to substantial changes in nighttime brightness and power availability. Despite this increase in energy access, we find that electrification did not have economically meaningful impacts on a range of development outcomes.

These results hold when we rescale our reduced form estimates to account for the proportion of eligible villages that underwent treatment. We see similar effects on development among states with high and low average reliability of electricity supply. We also find similar effects when we restrict our analysis to the earliest districts to obtain RGGVY funding, suggesting that our results do not depend on the timing of our post-intervention data. Finally, we apply a difference-in-differences strategy, which relies on alternative identifying assump-
tions and includes a larger sample of villages well outside our RD bandwidth. These results support the main conclusions from our RD analysis that while nighttime lights, and therefore power consumption, increased substantially with RGGVY electrification, other development outcomes that we observe did not. Our cost-benefit calculations suggests a much longer payback period than previously estimated.

These results are the first to suggest that electrifying rural villages may not cause sizable economic gains in the medium term. Our regression discontinuity strategy relies on much less stringent identifying assumptions than the instrumental variables approaches of previous work, allowing us to measure effects of a natural rollout of rural electrification, at scale. In contrast to the existing literature, we find that electrification did not yield even modest changes in labor, income, household wealth, asset ownership and expenditures, village-level outcomes, and education. These null results come from the world’s largest unelectrified population, and appear to generalize to over 400,000 villages across rural India.

Nevertheless, electrification may lead to large economic benefits in certain contexts, and may have important positive effects on human well-being that we are unable to quantify. An important direction for future work will be to understand when, where, and after how long electricity access and power availability have the greatest economic impact. For example, electrification may lead to substantial gains in economic productivity in urban settings, or in regions with budding local industries. There may also be substantial long-run effects of electrification, and more research is necessary to identify these benefits. Finally, we encourage future research on quantifying the non-market benefits from electrification that frequently go unmeasured.
References


## Tables

Table 1: Summary Statistics – Villages with Populations Between 150 and 450

<table>
<thead>
<tr>
<th>2001 Village Characteristics</th>
<th>All Districts</th>
<th>10th-Plan Districts</th>
<th>10th-Plan Districts Single-Habitation</th>
</tr>
</thead>
<tbody>
<tr>
<td>Village area (hectares)</td>
<td>199.74</td>
<td>177.98</td>
<td>173.53</td>
</tr>
<tr>
<td></td>
<td>(462.39)</td>
<td>(561.29)</td>
<td>(661.57)</td>
</tr>
<tr>
<td>Share of area irrigated</td>
<td>0.23</td>
<td>0.30</td>
<td>0.35</td>
</tr>
<tr>
<td></td>
<td>(0.30)</td>
<td>(0.33)</td>
<td>(0.34)</td>
</tr>
<tr>
<td>Agricultural workers / all workers</td>
<td>0.39</td>
<td>0.37</td>
<td>0.37</td>
</tr>
<tr>
<td></td>
<td>(0.16)</td>
<td>(0.16)</td>
<td>(0.15)</td>
</tr>
<tr>
<td>Other workers / all workers</td>
<td>0.06</td>
<td>0.06</td>
<td>0.06</td>
</tr>
<tr>
<td></td>
<td>(0.08)</td>
<td>(0.08)</td>
<td>(0.08)</td>
</tr>
<tr>
<td>Employment rate</td>
<td>0.46</td>
<td>0.44</td>
<td>0.44</td>
</tr>
<tr>
<td></td>
<td>(0.14)</td>
<td>(0.14)</td>
<td>(0.14)</td>
</tr>
<tr>
<td>Literacy rate</td>
<td>0.45</td>
<td>0.44</td>
<td>0.45</td>
</tr>
<tr>
<td></td>
<td>(0.18)</td>
<td>(0.17)</td>
<td>(0.17)</td>
</tr>
<tr>
<td>Education facilities (0/1)</td>
<td>0.66</td>
<td>0.58</td>
<td>0.58</td>
</tr>
<tr>
<td></td>
<td>(0.47)</td>
<td>(0.49)</td>
<td>(0.49)</td>
</tr>
<tr>
<td>Medical facilities (0/1)</td>
<td>0.13</td>
<td>0.12</td>
<td>0.12</td>
</tr>
<tr>
<td></td>
<td>(0.34)</td>
<td>(0.32)</td>
<td>(0.32)</td>
</tr>
<tr>
<td>Banking facilities (0/1)</td>
<td>0.01</td>
<td>0.01</td>
<td>0.01</td>
</tr>
<tr>
<td></td>
<td>(0.11)</td>
<td>(0.11)</td>
<td>(0.10)</td>
</tr>
<tr>
<td>Agricultural credit societies (0/1)</td>
<td>0.03</td>
<td>0.03</td>
<td>0.03</td>
</tr>
<tr>
<td></td>
<td>(0.18)</td>
<td>(0.16)</td>
<td>(0.16)</td>
</tr>
<tr>
<td>Electric power (0/1)</td>
<td>0.68</td>
<td>0.62</td>
<td>0.64</td>
</tr>
<tr>
<td></td>
<td>(0.46)</td>
<td>(0.49)</td>
<td>(0.48)</td>
</tr>
<tr>
<td>Share households with indoor water</td>
<td>0.21</td>
<td>0.21</td>
<td>0.25</td>
</tr>
<tr>
<td></td>
<td>(0.17)</td>
<td>(0.17)</td>
<td>(0.19)</td>
</tr>
<tr>
<td>Share households with thatched roofs</td>
<td>0.27</td>
<td>0.27</td>
<td>0.28</td>
</tr>
<tr>
<td></td>
<td>(0.27)</td>
<td>(0.24)</td>
<td>(0.24)</td>
</tr>
<tr>
<td>Share households with mud floors</td>
<td>0.78</td>
<td>0.79</td>
<td>0.77</td>
</tr>
<tr>
<td></td>
<td>(0.17)</td>
<td>(0.16)</td>
<td>(0.17)</td>
</tr>
<tr>
<td>Average household size</td>
<td>5.36</td>
<td>5.53</td>
<td>5.56</td>
</tr>
<tr>
<td></td>
<td>(0.58)</td>
<td>(0.61)</td>
<td>(0.60)</td>
</tr>
<tr>
<td>Number of villages</td>
<td>129,438</td>
<td>62,638</td>
<td>29,765</td>
</tr>
</tbody>
</table>

Note. — This table shows village-level summary statistics from the 2001 Census, for three sets of villages with 2001 populations between 150 and 450: all villages, villages in 10th-Plan districts, and single-habitation villages in 10th-Plan districts. This third group corresponds to the sample of villages used in our RD analysis. We present workers by sector as the share of total workers in the village; “other” workers are classified as non-agricultural, non-household workers. The employment rate divides the number of workers by village population. Binary variables are labeled (0/1). Standard deviations in parentheses.
Table 2: RD – Nighttime Brightness

<table>
<thead>
<tr>
<th></th>
<th>2011 village brightness</th>
</tr>
</thead>
<tbody>
<tr>
<td>$1[2001\text{ pop} \geq 300]$</td>
<td>0.1493**</td>
</tr>
<tr>
<td></td>
<td>(0.0603)</td>
</tr>
<tr>
<td>2001 population</td>
<td>-0.0008</td>
</tr>
<tr>
<td></td>
<td>(0.0007)</td>
</tr>
<tr>
<td>$1[2001\text{ pop} \geq 300] \times 2001\text{ pop}$</td>
<td>0.0008</td>
</tr>
<tr>
<td></td>
<td>(0.0008)</td>
</tr>
</tbody>
</table>

2001 Control  Yes
State FEs  Yes
RD bandwidth  150
Number of observations  18,686
Number of districts  130
Mean of dependent variable  6.370
$R^2$  0.766

Note. — This table shows results from estimating Equation (1), which corresponds to Figure 4. We define village brightness based on the brightest pixel contained within the village boundary. This regression includes all single-habitation villages in 10th-Plan districts with 2001 populations in the RD bandwidth (a 150-person bandwidth includes villages with 2001 populations between 150 and 450), for the 12 states with available village shapefiles that match to Census village areas with a correlation above 0.35. Standard errors are clustered at the district level. Significance: ***, $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. 
Table 3: RD – Census Outcomes

<table>
<thead>
<tr>
<th>2011 Outcome Variable</th>
<th>RD Coefficient</th>
<th>Standard Error</th>
<th>95 Percent Confidence</th>
<th>Mean of Outcome</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>A. Demographic outcomes</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Total population</td>
<td>−0.8647 (2.528)</td>
<td>−5.820, 4.091</td>
<td>271.09</td>
<td></td>
</tr>
<tr>
<td>0-6 cohort / total population</td>
<td>0.0009 (0.001)</td>
<td>−0.001, 0.002</td>
<td>0.14</td>
<td></td>
</tr>
<tr>
<td>Average household size</td>
<td>−0.0051 (0.013)</td>
<td>−0.030, 0.020</td>
<td>5.13</td>
<td></td>
</tr>
<tr>
<td>Literacy rate</td>
<td>−0.0025 (0.002)</td>
<td>−0.007, 0.002</td>
<td>0.57</td>
<td></td>
</tr>
<tr>
<td><strong>B. Labor outcomes</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Male agricultural workers / male pop</td>
<td>−0.0071** (0.003)</td>
<td>−0.013, −0.002</td>
<td>0.42</td>
<td></td>
</tr>
<tr>
<td>Female agricultural workers / female pop</td>
<td>−0.0049 (0.004)</td>
<td>−0.013, 0.003</td>
<td>0.29</td>
<td></td>
</tr>
<tr>
<td>Male household workers / male pop</td>
<td>−0.0009 (0.001)</td>
<td>−0.002, 0.000</td>
<td>0.01</td>
<td></td>
</tr>
<tr>
<td>Female household workers / female pop</td>
<td>−0.0014 (0.001)</td>
<td>−0.004, 0.001</td>
<td>0.01</td>
<td></td>
</tr>
<tr>
<td>Male other workers / male pop</td>
<td>0.0046** (0.002)</td>
<td>0.001, 0.008</td>
<td>0.10</td>
<td></td>
</tr>
<tr>
<td>Female other workers / female pop</td>
<td>−0.0004 (0.002)</td>
<td>−0.004, 0.004</td>
<td>0.05</td>
<td></td>
</tr>
<tr>
<td><strong>C. Asset ownership</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Share of households with telephone</td>
<td>0.0025 (0.006)</td>
<td>−0.008, 0.013</td>
<td>0.54</td>
<td></td>
</tr>
<tr>
<td>Share of households with TV</td>
<td>0.0026 (0.004)</td>
<td>−0.005, 0.010</td>
<td>0.26</td>
<td></td>
</tr>
<tr>
<td>Share of households with bicycle</td>
<td>−0.0015 (0.004)</td>
<td>−0.010, 0.007</td>
<td>0.50</td>
<td></td>
</tr>
<tr>
<td>Share of households with motorcycle</td>
<td>−0.0008 (0.003)</td>
<td>−0.006, 0.004</td>
<td>0.13</td>
<td></td>
</tr>
<tr>
<td>Share of households without assets</td>
<td>0.0039 (0.004)</td>
<td>−0.004, 0.012</td>
<td>0.22</td>
<td></td>
</tr>
<tr>
<td><strong>D. Housing stock</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Share of households with elec/gas cooking</td>
<td>0.0005 (0.003)</td>
<td>−0.005, 0.006</td>
<td>0.07</td>
<td></td>
</tr>
<tr>
<td>Share of households with kerosene lighting</td>
<td>0.0029 (0.006)</td>
<td>−0.009, 0.015</td>
<td>0.48</td>
<td></td>
</tr>
<tr>
<td>Share of households with mud floors</td>
<td>0.0043 (0.004)</td>
<td>−0.003, 0.012</td>
<td>0.73</td>
<td></td>
</tr>
<tr>
<td>Share of households with thatched roof</td>
<td>−0.0034 (0.005)</td>
<td>−0.013, 0.007</td>
<td>0.23</td>
<td></td>
</tr>
<tr>
<td>Share of households dilapidated</td>
<td>−0.0031 (0.003)</td>
<td>−0.009, 0.002</td>
<td>0.07</td>
<td></td>
</tr>
<tr>
<td><strong>E. Village-wide outcomes</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1/0 Mobile phone coverage in village</td>
<td>−0.0008 (0.011)</td>
<td>−0.023, 0.021</td>
<td>0.75</td>
<td></td>
</tr>
<tr>
<td>1/0 Post office in village</td>
<td>0.0018 (0.004)</td>
<td>−0.005, 0.009</td>
<td>0.03</td>
<td></td>
</tr>
<tr>
<td>1/0 Ag credit societies in village</td>
<td>0.0013 (0.004)</td>
<td>−0.006, 0.009</td>
<td>0.02</td>
<td></td>
</tr>
<tr>
<td>1/0 Water from tubewell in village</td>
<td>−0.0023 (0.011)</td>
<td>−0.024, 0.019</td>
<td>0.44</td>
<td></td>
</tr>
<tr>
<td>Share of village area irrigated</td>
<td>−0.0057 (0.005)</td>
<td>−0.016, 0.004</td>
<td>0.35</td>
<td></td>
</tr>
<tr>
<td>Share of village area planted</td>
<td>0.0015 (0.006)</td>
<td>−0.010, 0.013</td>
<td>0.58</td>
<td></td>
</tr>
</tbody>
</table>

Note. — Each row represents a separate regression estimating Equation (1) on the outcome variable. The RD bandwidth includes 29,765 villages with 2001 populations between 150 and 450, across 225 districts. The second column shows the RD point estimate (\( \hat{\beta} \)) for each regression. All specifications control for the 2001 level of the outcome variable, except for share of village area planted (where 2001 values are not available) and 1/0 indicator variables. All specifications also include state fixed effects. Standard errors are clustered at the district level, which we use to calculate 95 percent confidence intervals in the fourth column. The fifth column reports the mean of the dependent variable for each RD regression. Significance: *** \( p < 0.01 \), ** \( p < 0.05 \), * \( p < 0.10 \).
### Table 4: RD – SECC Village-Level Outcomes

<table>
<thead>
<tr>
<th>2011 Outcome</th>
<th>RD Coefficient</th>
<th>Standard Error</th>
<th>95 Percent Confidence</th>
<th>Mean of Outcome</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>A. Share of households</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>At least one poverty indicator</td>
<td>0.0006</td>
<td>(0.006)</td>
<td>[−0.011, 0.012]</td>
<td>0.48</td>
</tr>
<tr>
<td>Monthly income greater than Rs 5,000</td>
<td>0.0043</td>
<td>(0.004)</td>
<td>[−0.004, 0.013]</td>
<td>0.08</td>
</tr>
<tr>
<td>One member holding salaried job</td>
<td>0.0030</td>
<td>(0.002)</td>
<td>[−0.002, 0.008]</td>
<td>0.02</td>
</tr>
<tr>
<td>Owning any land</td>
<td>−0.0005</td>
<td>(0.008)</td>
<td>[−0.017, 0.016]</td>
<td>0.44</td>
</tr>
</tbody>
</table>

**B. Adult employment**

<table>
<thead>
<tr>
<th></th>
<th>RD Coefficient</th>
<th>Standard Error</th>
<th>95 Percent Confidence</th>
<th>Mean of Outcome</th>
</tr>
</thead>
<tbody>
<tr>
<td>Male agricultural workers / adult men</td>
<td>−0.0091*</td>
<td>(0.005)</td>
<td>[−0.019, 0.001]</td>
<td>0.29</td>
</tr>
<tr>
<td>Female agricultural workers / adult women</td>
<td>−0.0039</td>
<td>(0.005)</td>
<td>[−0.013, 0.006]</td>
<td>0.08</td>
</tr>
<tr>
<td>Male household workers / adult men</td>
<td>0.0008</td>
<td>(0.001)</td>
<td>[−0.002, 0.004]</td>
<td>0.01</td>
</tr>
<tr>
<td>Female household workers / adult women</td>
<td>−0.0015</td>
<td>(0.008)</td>
<td>[−0.016, 0.013]</td>
<td>0.51</td>
</tr>
<tr>
<td>Male other workers / adult men</td>
<td>0.0052</td>
<td>(0.006)</td>
<td>[−0.007, 0.017]</td>
<td>0.42</td>
</tr>
<tr>
<td>Female other workers / adult women</td>
<td>0.0054</td>
<td>(0.005)</td>
<td>[−0.005, 0.016]</td>
<td>0.16</td>
</tr>
</tbody>
</table>

Note. — Each row represents a separate regression estimating Equation (1) on a different SECC village-level outcome. The first row of Panel A is coded as the share of total households in the village with at least one poverty indicator. Other outcomes in Panel A are coded as the proportion of this subset of households (with poverty indicators) that meet each criterion. Panel B outcomes are coded as the share of adult men (women) with an occupation in each subcategory, for the sample of adults in households with at least one poverty indicator. (We treat all individuals over 16 years of age as adults.) The second column shows the RD point estimate (\( \hat{\beta}_1 \)) for each regression. All specifications include state fixed effects, but they do not include any additional baseline control variables. The RD bandwidth includes 25,942 villages with 2001 populations between 150 and 450. These regressions contain fewer villages than regressions in Table 3 because only 87 percent of 10th-Plan, single-habitation, 150–450 person villages match to the SECC dataset. Standard errors are clustered at the district level with 222 clusters, which we use to calculate 95 percent confidence intervals in the fourth column. The fifth column reports the mean of the dependent variable for each RD regression. Significance: *** \( p < 0.01 \), ** \( p < 0.05 \), * \( p < 0.10 \).
Figure 1: Indian Districts by RGGVY Implementation Phase

Note. — This map shows 2001 district boundaries, shaded by RGGVY coverage status. Navy districts are covered under the 10th Plan, light blue districts are covered under the 11th Plan, cross-hatched districts were covered under both the 10th and 11th Plans, and white districts are not covered by RGGVY. As of 2001, India had 584 districts across its 28 states and 7 Union Territories. RGGVY covered 530 total districts in 27 states (neither Goa nor the Union Territories were eligible), with 30 districts split between the 10th and 11th Plans.
Figure 2: Nighttime Lights in India, 2001 and 2011

Note. — This figure shows the DMSP-OLS nighttime brightness data for India. The left panel shows nighttime lights in 2001, and the right panel shows nighttime lights for 2011. The $\approx 1\text{km}^2$ pixels in this image range in brightness from 0 to 63, covering the full range of the DMSP-OLS data.

Figure 3: Density of RD Running Variable

Note. — This figure summarizes the distribution of Indian village populations. The left panel shows the population distribution of villages in India in 2001 (solid navy) and 2011 (hollow blue). The right panel zooms in on the set of villages used in our RD analysis, within a 150–450 population window around the 300-person cutoff. Our RD sample of single-habitation 10th-Plan villages is shown in navy, relative to all Indian villages (white) and all villages in 10th-Plan districts (light blue).
Figure 4: RD – 2011 Nighttime Brightness

Note. — This figure shows RD results using maximum 2011 nighttime brightness as a dependent variable, as reported in Table 2. Blue dots show average residuals from regressing the 2011 maximum nighttime brightness on 2001 maximum nighttime brightness and state fixed effects. Each dot contains approximately 1,600 villages, averaged in 25-person population bins. Lines are estimated separately on each side of the 300-person threshold, for 18,686 single-habitation villages between 150–450 people, in 10th-Plan districts. The point estimate on the level shift is 0.149, with a p-value of 0.015. Neither slope coefficient is significant at conventional levels.
Figure 5: Nighttime Brightness – Validity Tests

Note. — This figure presents results from three RD validity checks. The left panel displays results from estimating our main specification using 2005 brightness as the dependent variable; the point estimate is 0.031 with a standard error of 0.020. The center panel was generated by estimating Equation (1) on 801 placebo RD thresholds, representing all integer values in $[151, 275] \cup [325, 1000]$. We omit placebo thresholds within 25 people of the true 300-person threshold to ensure that placebo RDs do not detect the true effects of RGGVY eligibility, and we exclude thresholds below 151 due to our 150-person bandwidth. The right panel was generated by scrambling village brightness 10,000 times and re-estimating Equation (1). The red lines represent the RD coefficient from the actual data at the correct 300-person threshold. Our RD point estimate falls above the 99th percentile of the placebo distribution and above the 98th percentile of the randomization distribution.

Figure 6: Nighttime Brightness – Falsification Tests

Note. — This figure presents three falsification tests for our RD on nighttime brightness. The left and right panels include only villages with multiple habitations, for which the running variable of village population did not determine village eligibility. The center and right panels include only villages in districts that became eligible for RGGVY under the 11th Plan, for which the appropriate eligibility cutoff was lowered from 300 to 100 people. Blue dots show average residuals from regressing 2011 nighttime brightness on 2001 brightness and state fixed effects. Each dot contains approximately 900–1,600 villages, averaged in 25-person population bins. Lines are estimated separately on each side of the 300-person threshold, for villages within the 150–450 population bandwidth. Supplementary Table B10 reports the regression results that correspond to these figures.
Figure 7: RD – Labor Outcomes

Note. — This figure shows the results from our preferred RD specification (Equation (1)), as reported numerically in Panel B of Table 3. Blue dots show average residuals from regressing the 2011 percentage of the male/female population classified in each labor category on the corresponding 2001 percentage and state fixed effects. Each dot contains approximately 1,500 villages, averaged in 15-person population bins. Lines are estimated separately on each side of the 300-person threshold, for all 29,765 single-habitation villages between 150 and 450 people, in 10th-Plan districts.
Figure 8: RD – Housing and Asset Ownership

Note. — This figure shows the results from our preferred RD specification (Equation (1)), as reported numerically in Panels C and D of Table 3. Blue dots show average residuals from regressing the 2011 percentage of households owning each asset (or with each characteristic) on the corresponding 2001 percentage and state fixed effects. Each dot contains approximately 1,500 villages, averaged in 15-person population bins. Lines are estimated separately on each side of the 300-person threshold, for all 29,765 single-habitation villages between 150 and 450 people, in 10th-Plan districts.
Figure 9: RD – SECC Village-Level Outcomes

Note. — This figure shows the results from our preferred RD specification (Equation (1)), as reported numerically in the first two rows of Table 4. The upper-left panel reports the proportion of total village households with at least one poverty indicator in 2011, while the upper-right panel reports the proportion of households with a poverty indicator that had a maximum monthly income over Rs 5,000 in 2011. The lower panels report the share of adult men in households with a poverty indicator with occupations in each category. Blue dots show average residuals from regressing the 2011 share of households on state fixed effects. Each dot contains approximately 1,600 villages, averaged in 20-person population bins. Lines are estimated separately on each side of the 300-person threshold for 25,942 villages, i.e. all 10th-Plan single-habitation villages within our 150-450 population RD bandwidth, that match to the SECC dataset.
Figure 10: RD – School Enrollment

Note. — This figure shows the results from our preferred RD specification (Equation (1)), as reported numerically in the first and last rows of Table 5. Blue dots show average residuals from regressing the 2011 number of (total, grades 6–8 only) students on the corresponding 2005 enrollment counts and state fixed effects. Each dot contains approximately 1,000 villages, averaged in 25-person population bins. Lines are estimated separately on each side of the 300-person threshold, for 12,251 single-habitation villages between 150 and 450 people, in 10th-Plan districts, with school-village matches and nonmissing 2005 and 2011 enrollment data.

Figure 11: Difference-in-Differences Results

Note. — This figure compares the reduced form effects from our preferred RD specification (Equation (1)) to the results from our DD specification (Equation (2)), using 300-person population bins. Navy blue dots show the RD coefficients, with whiskers indicating 95 percent confidence intervals. Light blue dots and dashed lines show the binned DD point estimates and 95 percent confidence intervals. The left panel shows the effects for nighttime lights, as measured by maximum village brightness. The right panel shows the effects for male agricultural workers. The RD results are statistically significant at the 5 percent level and the 1 percent level, respectively. The pooled DD point estimates are 0.45 and −0.008; both are statistically significant at the 10 percent level (Appendix B.11 reports these results in a regression table). DD regressions for lights and labor include 629,778 and 994,802 village-year observations, respectively.