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Out of the darkness and into the light? Development effects of rural electrification*

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Abstract

Nearly 1 billion people still lack electricity access. In response, developing countries are investing billions of dollars in "last-mile" electrification, although evidence on its economic impacts is mixed. We estimate the development effects of rural electrification in the context of India's national electrification program, RGGVY, which reached over 400,000 villages. Using regression discontinuity and difference-in-differences designs, we estimate that RGGVY meaningfully expanded electricity access. However, the program generated limited economic impacts after 3–5 years. Scaling our intent-to-treat estimates using instrumental variables, we find that while "fully electrifying" small villages reduces welfare, electrification likely increases welfare for larger villages.

JEL Codes: O13, O18, Q40

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1 Introduction

Nearly 1 billion people still lack access to electricity, despite substantial investments to extend the power grid across the developing world.[1] The International Energy Agency projects that achieving universal electrification will cost $49 billion per year between 2019 and 2030. The vast majority of remaining unconnected households live in rural South Asia and sub-Saharan Africa (International Energy Agency (2019)).

While access to electricity is highly correlated with GDP at the national level, prior research on the causal effects of electrification has produced mixed results. Seminal early work by Dinkelman (2011), Rud (2012), and Lipscomb, Mobarak, and Barham (2013) finds large positive impacts of electrification on development outcomes. In contrast, recent experimental evidence finds rural electrification to be welfare reducing, with negligible benefits and large costs (Lee, Miguel, and Wolfram (2020b)); and estimates only modest welfare gains from expanding access to grid power (Burgess et al. (2020a)). Importantly, such discrepancies may reflect differences in scale: while estimates of large economic impacts have tended to come from electrifying large populations (e.g. entire Indian states in Rud (2012), and whole Brazilian counties in Lipscomb, Mobarak, and Barham (2013)), studies finding less favorable welfare impacts have been conducted at the village level—a scale that is more representative of today's electrification efforts.

In this paper, we estimate the economic impacts of electrification in the context of India's massive national rural electrification program, Rajiv Gandhi Grameen Vidyutikaran Yojana (RGGVY). The "Prime Minister's Rural Electrification Program" was launched in

1. Roughly 800 million people remained unelectrified in 2018, down from 1.2 billion in 2010 (IEA, IRENA, UNSD, World Bank, WHO (2020)). Universal energy access is UN Sustainable Development Goal #7 (UNDP (2015)), and electrification is a key piece of the World Bank's investment strategy (The World Bank (2015)).

2005 to expand both domestic and commercial electricity access in over 400,000 rural villages across 27 Indian states. India is a useful setting for studying ongoing rural electrification efforts, as it contributed over 80 percent of global gains in new household grid connections between 2000 and 2016 (International Energy Agency (2017)). Moreover, India's per-capita income during the RGGVY period was similar to income levels in countries with significant unelectrified populations today (see Figure 1). The program's scope also provides a unique opportunity to address the divergent results from the existing literature.

We use two key features of RGGVY's implementation to identify the program's causal impacts on both electricity access and economic outcomes. First, villages were eligible for electrification under RGGVY only if they contained at least one neighborhood ("habitation") larger than 300 people.[2] This allows us to estimate a regression discontinuity (RD) design, using this population-based eligibility threshold. Second, RGGVY had a staggered rollout, treating districts in two waves corresponding to India's 10th and 11th Five-Year Plans. This facilitates a difference-in-differences (DD) design comparing first- vs. second-wave districts. Using a range of administrative and geospatial data sources, we apply our RD strategy to a sample of over 10,000 villages, and our DD strategy to nearly all of rural India.

We first show that RGGVY led to substantial increases in electricity access. We find that RGGVY provided commercial power to 1 in 13 barely-eligible villages that previously lacked access, while increasing average commercial power supply by 0.56 hours per day.[3] We also find that RGGVY electrified 1 in 7 previously unconnected rural households in first-

2. The village was the lowest-level administrative unit in the 2001 Census of India. Villages are composed of "habitations" (or "hamlets"), which correspond to distinct inhabited areas within 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.

3. These two estimates are internally consistent, as the average electrified village in this setting received under 11 hours per day of commercial power supply.

wave districts, and increased average household electricity consumption by 4 kilowatt-hours (kWh) per month. Consistent with these direct measures of electrification, we estimate 4–5 percentage point increases in household electric lighting adoption. We detect corresponding increases in satellite-derived nighttime brightness at the village level, using both our cross-sectional RD design and a DD event-study model. These results tell a consistent story: while RGGVY fell short of achieving "full electrification," it succeeded in meaningfully expanding electricity access and consumption in rural India.[4] However, despite these gains in electrification, we find that RGGVY led to at most modest changes in economic outcomes. We can reject intent-to-treat effects on our preferred outcome, per-capita consumption expenditure, greater than 2 percent of the mean using our RD strategy and 8 percent of the mean using our DD approach.

Next, we rescale our estimates of RGGVY's impacts via instrumental variables, in order to estimate the effects of rural electrification more broadly. Using a fuzzy RD design, which estimates local average treatment effects for villages with close to 300 people, we can reject per-capita expenditure increases greater than 30 percent as a result of "full electrification." Using a DD-IV design, we similarly reject per-capita expenditure increases greater than 30 percent in smaller villages (median of 1,043 people); in larger villages (median of 2,076 people), we cannot reject a tripling of per-capita expenditure due to "full electrification." These estimates reveal substantial heterogeneity in the economic impacts of electrification.

Finally, we use our estimates to conduct a benefit-cost analysis. We compute the 20-year return on investment (ROI) from rural electrification using two strategies for quantifying

4. These findings align with Indian policy reports on RGGVY (e.g., Sreekumar and Dixit (2011); Programme Evaluation Organisation, Planning Commission (2014); Josey and Sreekumar (2015)).

benefits: (i) our fuzzy RD and DD-IV per-capita expenditure estimates, and (ii) consumer surplus from household electricity consumption. Both strategies imply that "full electrification" of small (300-person) villages is benefit-cost negative, of medium (1,000-person) villages is likely benefit-cost negative, and of large (2,000-person) villages is benefit-cost positive. This difference reflects both greater per-capita benefits in larger villages and economies of scale in costs, and helps to reconcile divergent estimates from the prior literature.

This paper makes three key contributions. First, we provide new evidence on the economic impacts of rural electrification from across India—home to the world's largest unelectrified population during our sample period. Our estimates leverage policy variation from a flagship rural electrification program; they come from thousands of villages and hundreds of districts, in a country with income levels comparable to today's electrification frontier. Second, we add to the knowledge on the causal effects of infrastructure in developing countries. Whereas existing research has tended to find large positive impacts of infrastructure investments, our results indicate that "last-mile" infrastructure projects may be less likely to spur economic growth.[5] Third, we contribute to a growing literature on electricity in the developing world, with our finding of heterogeneous impacts of multi-sector (residential and non-residential) electrification.[6] Our results speak to two hypotheses proposed by Lee, Miguel, and Wolfram (2020a): (i) that the benefits of electrification vary across local eco-

5. For example, Donaldson (2018) finds that early railroad investments increased real incomes in India, and Faber (2014) finds that highway infrastructure investments led to potentially large aggregate efficiency improvements in China. Conversely, Asher and Novosad (2020) do not detect meaningful economic impacts from PMGSY, India's "last-mile" rural road construction program.

6. Several papers highlight heterogeneity at the intersection of electricity and economic development. For example, Gertler et al. (2016) find that income levels are a key driver of energy demand in Mexico; Allcott, Collard-Wexler, and O'Connell (2016) shows that generator ownership determines firms' responses to power outages in India; and Mahadevan (2020) and Ryan (2020) present evidence on disparate treatment of politically connected firms and individuals in energy contracts and power supply, respectively.

nomic settings, which we characterize in terms of heterogeneous village size; and (ii) that household electrification alone may be insufficient for economic gains. Even though RGGVY expanded access beyond the residential sector, we find that electrifying small villages reduces welfare.[7] We do see evidence of positive welfare impacts in larger villages.

The paper proceeds as follows: Sections 2, 3, and 4 describe the RGGVY program, our empirical strategies, and our data. Section 5 presents impacts of the RGGVY program on both electrification and economic outcomes. Section 6 presents rescaled estimates of the impacts of "full electrification". Section 7 concludes.

2 The RGGVY electrification program

Upon its independence in 1947, only 1,500 of India's villages had access to electricity (Tsujita (2014)). By 2004, after decades of electrification efforts, over 125,000 rural villages still lacked power access. In the remaining 467,000 villages, electrification was often extremely limited, with 57 percent of all rural households lacking grid connections. In 2005, the national government launched the flagship Rajiv Gandhi Grameen Vidyutikaran Yojana program (RGGVY), which sought to (i) connect over 100,000 unelectrified rural villages, and (ii) more intensively electrify over 300,000 "under-electrified" villages.[8]

RGGVY had a dual mandate to install electricity infrastructure to support village economies and connect unelectrified households. Infrastructure investments—transmission

7. Throughout this paper, we use the term "welfare" to refer to the benefits of electrification minus its costs. If a reader prefers not to take a stand on a particular social welfare function, they may interpret our results in terms of the "social surplus" of electrification.

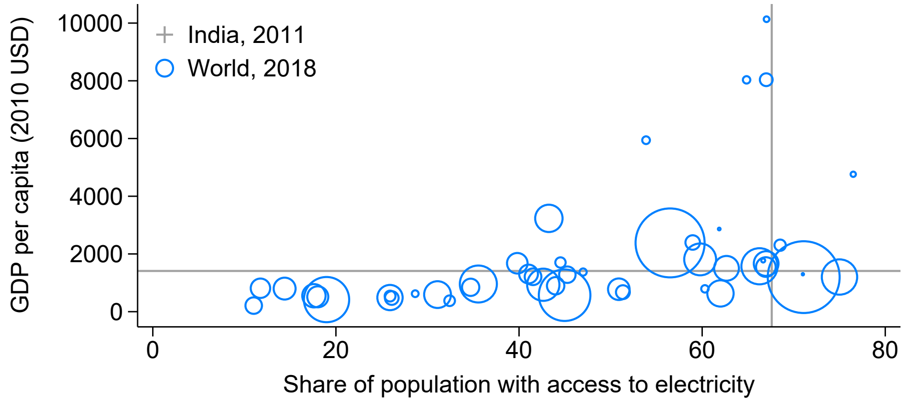
8. RGGVY translates to "The Prime Minister's Rural Electrification Plan." It was subsequently subsumed into Deendayal Upadhyaya Gram Jyoti Yojana (DDUGJY), the scheme launched in 2015 with the goal of providing continuous 24×7 power to all of rural India.

lines, distribution lines, and transformers—aimed to "facilitate overall rural development, employment generation, and poverty alleviation" by supporting electric irrigation pumps, microenterprises, and small-to-medium industries (Ministry of Power (2005)). New infrastructure also extended the grid to public places such as schools, health clinics, and local government offices. To increase residential power access, RGGVY was charged with providing free grid connections to below-poverty-line households.[9] RGGVY targeted both the extensive and intensive margins, connecting new villages to the grid while also upgrading existing infrastructure and connecting additional households in villages with some degree of electrification prior to 2005.

In order to receive RGGVY funding, states submitted Detailed Project Reports (DPRs) to the central government, based on village-level surveys conducted by local electricity utilities. Each DPR proposed a village-by-village implementation plan for a particular district, including specific infrastructure to be installed and the number of households to be connected. The Rural Electrification Corporation (overseen by the Ministry of Power) reviewed DPRs, approved projects, and disbursed funds to states. By 2011, RGGVY had provided over 253 billion rupees (5.45 billion US dollars) in funds and connected 17.5 million households to the grid—roughly 1 in 5 previously unelectrified rural households in India (Sreekumar and Dixit (2011)). RGGVY's setting is also instructive about ongoing electrification efforts: Figure 1 shows that India's 2011 GDP per capita is comparable to 2018 GDP per capita in countries where a substantial share of the population remains unelectrified today.

9. Above-poverty-line households were able to purchase connections. RGGVY did not alter electricity prices, but Indian retail electricity tariffs are already heavily subsidized (Burgess et al. (2020b)). In 2010, the median rural tariff was 2.64 (5 U.S. cents) rupees per kWh.

Figure 1: Electricity access and per-capita GDP – India vs. the world

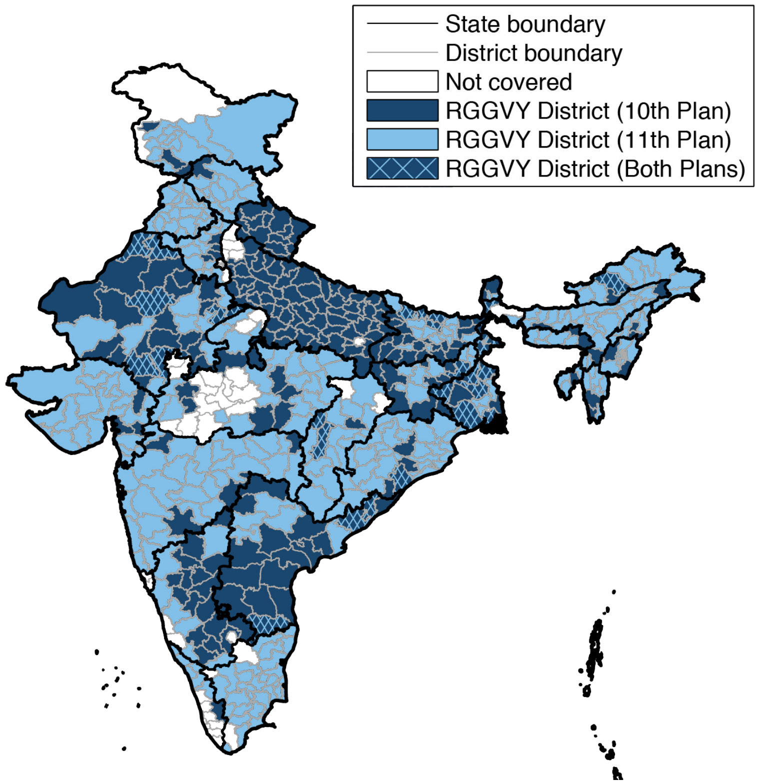


Note. — This figure plots per-capita GDP in constant 2010 USD vs. the share of the population with access to electricity across countries. Grey lines indicate India in 2011, at the end of our study period. Blue circles show all 46 countries with below 80 percent electricity access in 2018, with a total population of 1.28 billion people. Circles are scaled by population size. Of these 46 countries, 28, containing 66 percent of the people, had 2018 per-capita GDPs lower than India's 2011 per-capita GDP. An additional 5 countries, containing 11 percent of the people, had per-capita GDPs within 20 percent of 2011 India. Data are from the World Bank Open Data catalogue (World Bank (2021)).

Two features of RGGVY's implementation facilitate our empirical analysis. First, DPRs were funded under India's Five-Year Plans, and districts were sorted first-come-first-serve into two waves. The first wave (229 districts) were authorized under the 10th Plan, and received funding between 2005 and 2008. The second group (331 districts) were authorized under the 11th Plan, and received funds between 2008 and 2011. Approximately 164,000 (267,000) villages and 7.5 million (14.6 million) below-poverty-line households were slated for electrification under the 10th (11th) Plan. Figure 2 maps districts according to their Five-Year Plan; 23 of 27 states contain both 10th- and 11th-Plan districts. We leverage this staggered rollout to identify causal impacts of electrification, comparing 10th- vs. 11th-Plan districts in a difference-in-differences (DD) design.

Second, RGGVY determined village eligibility using the populations of sub-village "habitations" (i.e. neighborhoods). Under the 10th Plan, only villages with constituent

Figure 2: Indian districts by RGGVY implementation phase



Note. — This map shades 2001 districts by RGGVY coverage status. Navy districts were covered under the 10th Plan (RGGVY's first wave), light blue districts were covered under the 11th Plan (RGGVY's second wave), cross-hatched districts were covered under both 10th and 11th Plans, and white districts were not covered by RGGVY. In 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). 30 districts were split between the 10th and 11th Plans; 23 states contain both 10th- and 11th-Plan districts.

habitations larger than 300 people were eligible for electrification. We use a regression discontinuity (RD) design to estimate causal impacts of electrification at this 300-person cutoff, comparing barely-ineligible to barely-eligible villages in 10th-Plan districts.[10] The RD and DD designs are complementary: our RD analysis uses weaker identifying assumptions and village-level variation, but estimates effects that are local to 300-person villages; our DD analysis requires stronger identifying assumptions using district-level variation, but can estimate effects across the village size distribution, inclusive of within-district spillovers.

10. Under the 11th Plan, this threshold was decreased to 100 people. Due to limited density of sub-100-person villages, we restrict our RD analysis to the 300-person cutoff in 10th-Plan districts. Focusing on RGGVY's earlier wave also lets us estimate economic impacts over a longer time scale of 3–5 years.

3 Empirical strategy

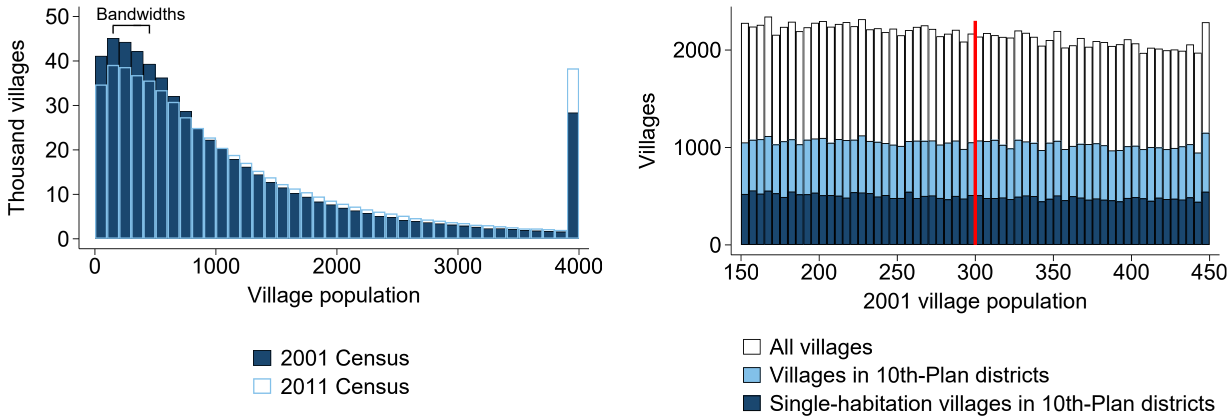
3.A Regression discontinuity design

We estimate a sharp RD design using RGGVY's 300-person cutoff, where a village's eligibility for treatment switches from 0 to 1 as the running variable crosses 300. This running variable is technically the *habitation* population, since the 300-person cutoff applied at the sub-village level. In the absence of official sub-village population counts, we restrict our RD sample to villages comprising exactly one habitation. For this 50-percent subset of villages, the village population is equivalent to the habitation population. This lets us use village population as a running variable to estimate local average treatment effects of RGGVY eligibility, for single-habitation villages in 10th-Plan districts with populations close to 300.

Our RD design necessitates two key 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, pre-RGGVY village-level covariates appear to be smooth across the 300-person cutoff (see Appendix B.3). We are also unaware of any other Indian social program that uses 300 people as a salient criterion.[11] Second, we assume that our running variable is not manipulable around the threshold. This assumption almost certainly holds, as program eligibility was contingent on populations from the 2001 Census, enumerated four years before RGGVY's announcement in 2005. Figure 3 shows no evidence of bunching at the 300-person cutoff.

11. Other Indian programs use population-based eligibility thresholds, including the PMGSY road construction program studied in Asher and Novosad (2020). PMGSY used 1000- and 500-person cutoffs.

Figure 3: Density of RD running variable



Note. — The left histogram shows Indian village populations for 2001 (solid navy) and 2011 (hollow blue), top-coding the right tail of each distribution at 4000. The right histogram zooms in on villages close to RGGVY's 300-person population cutoff, with 2001 populations between 150 and 450 (slightly wider than our optimal RD bandwidths). Navy bars show the sample of single-habitation 10th-Plan villages used in our RD analysis, relative to all Indian villages (white) and all villages in 10th-Plan districts (light blue).

Under these assumptions, the following RD specification estimates the causal impact of RGGVY eligibility for villages with close to 300 people:

$$Y_v \;=\; \beta_0 \;+\; \beta_1 Z_v \;+\; \beta_2(P_v - 300) \;+\; \beta_3(P_v - 300)\cdot Z_v \;+\; \theta \mathbf{X}_v \;+\; \eta_s \;+\; \varepsilon_v \tag{1}$$

$$\text{for} \;\; P_v \in [300 - h\,,\, 300 + h]\,, \qquad \text{where} \;\; Z_v \equiv \mathbf{1}[P_v \geq 300]\,.$$

Y_v represents the outcome of interest in village v. P_v is the 2001 village population (the RD running variable), while Z_v is an indicator of RGGVY eligibility. To increase precision, we control for pre-RGGVY village-level covariates in $\mathbf{X}_v$, including the lagged outcome variable (where possible). We also include state fixed effects η_s.[12] We implement Equation (1) using the `rdrobust` framework developed by Calonico, Cattaneo, and Titiunik (2014). Following the standard `rdrobust` procedures, we apply a triangular weighting kernel in distance from

12. Village-level controls and fixed effects are not necessary for identification, but they improve the precision of our RD estimates (see Lee and Lemieux (2010)).

the RD cutoff, calculate MSE-optimal RD bandwidths h, and use heteroskedasticity-robust nearest-neighbor standard errors.[13]

This sharp RD design estimates the impacts of *eligibility for the RGGVY program*. We use an analogous fuzzy RD design to estimate the impacts of *electrification*, instrumenting for village-level electricity access using the 300-person eligibility indicator. This transforms our intent-to-treat RD estimates into local average treatment effects (LATEs) that enable us to approximate a "full electrification" program.[14]

3.B Difference-in-differences design

We complement our RD design with a difference-in-differences (DD) analysis which leverages RGGVY's phased rollout. Our DD compares "treated" 10th-Plan districts (RGGVY's first wave) to a "control group" comprising both 11th-Plan districts (RGGVY's second wave) and non-RGGVY districts.[15] Using district-level variation, this DD design lets us study villages of all sizes, incorporate district-level outcome data, and capture within-district spillovers and general equilibrium effects.

Our main DD specification is:

$$Y_{dt} \;=\; \gamma\,\mathbf{1}[\text{10th Plan}]_d \times \mathbf{1}[\text{Post-2005}]_t \;+\; \eta_d \;+\; \delta_t \;+\; \theta_{dt} \;+\; \varepsilon_{dt} \tag{2}$$

13. The MSE-optimal bandwidth procedure computes a separate h for each outcome variable Y_v, following Calonico, Cattaneo, and Farrell (2018). We present `rdrobust` sensitivity analysis for alternative kernels, bandwidths, functional forms, and standard errors in Appendix Figures B1–B2 and Tables B1–B3.

14. Scaling up our intent-to-treat RD estimates also accounts for potential non-compliance with the 300-person eligibility rule. We implement fuzzy RD using the same `rdrobust` framework.

15. 37 districts in our control group had no RGGVY projects. We assign the 30 districts with RGGVY projects in both Plans to the "treated" group. Our DD results are robust to dropping all 67 of these districts (see Appendix Table B12).

where Y_{dt} is an outcome variable for district d in year t. γ captures the differential impact of RGGVY eligibility after 10th-Plan districts began receiving treatment in 2005, controlling for district fixed effects η_d and year fixed effects δ_t. To account for potential selection of districts into RGGVY's first wave, θ_{dt} includes three sets of linear trends: (i) trends grouping districts within each state by quartiles of 2005 household expenditures, in case states prioritized electrifying poorer districts; (ii) trends grouping districts by national deciles of 2005 household expenditures, in case such selection existed in absolute terms across states; and (iii) state-specific trends. Our DD identifying assumption is that, after controlling for these trends, 10th- and 11th-Plan districts would have continued on parallel counterfactual trajectories absent RGGVY. In support of this assumption, we fail to reject differential trends prior to RGGVY.[16]

Like our sharp RD design, Equation (2) estimates the causal impacts of the *RGGVY program* on outcomes of interest. To estimate the causal impacts of *electrification*, we instrument for electricity access using the $\mathbf{1}[\text{10th Plan}]_d \times \mathbf{1}[\text{Post-2005}]_t$ interaction in the analogous instrumental variables (DD-IV) model. This scales our ITT effects to account for the fact that RGGVY did not bring new electricity connections to every household. Since RGGVY prohibited hiring local workers, and since our endline data were collected several years after most 10th-Plan projects, we are confident in the exclusion restriction that RGGVY eligibility only impacted economic outcomes through RGGVY itself.[17]

16. See Table 1, Figure 6, and Appendix Tables B7–B9. After including state-specific trends, our pre-trend estimates are all statistically indistinguishable from zero except for TV ownership (which, if anything, suggests that 10th-Plan districts were trending *away* from TV ownership relative to 11th-Plan districts). For outcome variables with village-level granularity or annual frequency, we modify Equation (2) to include village fixed effects or multiple event-study γ coefficients.

17. Any exclusion restriction violation would involve a time-varying unobservable correlated with both economic outcomes and our instrument, but not captured by household electricity access.

4 Data

We use village-level data from the 2001 and 2011 Census of India. The 2001 village population serves as our RD running variable, and we isolate the subset of single-habitation villages by matching villages to a separate census of habitations.[18] We observe village-level electricity access in the 2011 Census, which reports: (i) dummies for the village's first grid connection in the domestic, agricultural, and commercial sectors; and (ii) average hours per day of power supplied to each sector. The Census also provides a range of village-level economic outcome variables, including demographics, employment by gender and sector, household characteristics, asset ownership, and community-wide amenities.

We also incorporate data from a low-income subset of the 2011 Socioeconomic and Case Census (SECC). We observe income and wealth variables for this subset of households, and we also use the subset to reconstruct the share of poor households in each village.[19] Using the full (unrestricted) 2011 SECC, the Socioeconomic High-resolution Rural-Urban Geographic Dataset for India (SHRUG) estimates consumption expenditure per capita at the village level (Asher et al. (2021); Asher and Novosad (2020)).[20] We use SHRUG expenditure per capita as our preferred village-level outcome variable for quantifying the impacts of electrification on economic well-being.

18. Official RGGVY ledgers we observed in Rajasthan were pre-printed with 2001 Census populations. The National Rural Drinking Water Programme conducted habitation censuses in 2003 and 2009. We link these data to Census villages by modifying a fuzzy matching algorithm from Asher and Novosad (2020). Appendix C.5 describes this matching algorithm; Appendix C.4 discusses the Census data in further detail.

19. Though the SECC enumerated the full population, we only observe households that met at least one of seven poverty indicators, and zero of fourteen affluence indicators. See Appendix C.7 for further details.

20. SHRUG constructs village-level expenditure by combining 2011 SECC microdata with data from the Indian Human Development Survey (2011–12). To our knowledge, this is only dataset of per-capita expenditure that covers all Indian villages. See Appendix C.6 for further details.

We use administrative microdata from two additional sources. The Economic Census surveys all non-farm establishments, which we use to construct counts of firms and firm employees in each village in 1990, 1998, 2005, and 2013.[21] The District Information System on Education (DISE) provides student enrollment and pass rates for all Indian primary and upper primary schools from 2005 to 2014.[22] Panel A of Table 1 reports pre-RGGVY summary statistics at the village level; our RD sample of single-habitation 10th-Plan villages appears quite similar to the universe of villages with populations between 150 and 450.

While Census variables capture the extensive margin of village electricity access, we use satellite images of nighttime brightness to captures intensive-margin gains in electrification. The National Oceanic and Atmospheric Administration (NOAA) publishes annual composite images that report light intensity on a 0–63 scale, at approximately 1 km^2 resolution. We construct a yearly brightness panel by assigning each village the maximum brightness over all pixels in its shapefile polygon.[23] We are unable to construct village-level brightness in 10 states, for which 2001 village shapefiles are unavailable or unreliable.[24] We exclude these states from our RD analysis in order to control for pre-RGGVY brightness at the village level, which is important for statistical power; our DD analysis includes these 10 states, since fixed effects subsume baseline controls (see Equation (2)).

21. This includes informal firms and public sector employers, as we discuss in Appendix C.8.

22. DISE data include 1.68 million unique schools, and previous researched has used them to measure student achievement (Adukia, Asher, and Novosad (2020)). Appendix C.9 describes these data in detail.

23. Indian villages are organized into central clusters of households surrounded by agricultural fields. Assigning villages their maximum brightness targets our electricity proxy on populated areas, rather than unlit cropland. We remove year-specific measurement error in brightness via linear projection, while also dropping a few extreme outliers. Appendix C.3 provides further detail on our nighttime brightness data.

24. Shapefiles are unavailable for Arunachal Pradesh, Meghalaya, Mizoram, Nagaland, and Sikkim. For Assam, Himachal Pradesh, Jammu and Kashmir, Uttar Pradesh, and Uttarakhand, available shapefile polygon areas are uncorrelated with village areas reported in the 2001 Census. Our remaining sample states contain 60 percent of 10th-Plan RGGVY villages. See Appendix C.2 for further discussion.

Table 1: Summary statistics prior to RGGVY

A. Village-level covariates, 150–450 population	All Districts	10th-Plan Districts	RD Sample
Agricultural workers / population (2001)	0.39	0.37	0.40
	(0.16)	(0.16)	(0.15)
Non-agricultural workers / population (2001)	0.06	0.06	0.06
	(0.08)	(0.08)	(0.08)
Number of firms in village (2005)	7.32	6.81	7.60
	(11.30)	(10.78)	(12.23)
Literacy rate (2001)	0.45	0.44	0.45
	(0.18)	(0.17)	(0.17)
School enrollment (2005–06 headcount)	87.71	91.61	71.25
	(188.18)	(140.95)	(115.25)
Electric access anywhere in village (2001)	0.68	0.62	0.67
	(0.46)	(0.49)	(0.47)
Distance to nearest town (km)	27.72	24.69	23.81
	(27.67)	(25.98)	(22.99)
Number of villages	129,438	62,638	18,686

B. District-level covariates, 2005 NSS	10th-Plan Districts	11th-Plan Districts	Pre-trend Estimates
Expenditure per capita (rupees/month)	869.87	988.00	14.577
	(231.73)	(371.21)	[20.719]
Share households consuming any electricity	0.46	0.66	−0.015
	(0.31)	(0.28)	[0.016]
Share households with electric lighting	0.46	0.67	−0.018
	(0.32)	(0.28)	[0.016]
Share households with electric fan	0.28	0.42	−0.019
	(0.22)	(0.29)	[0.017]
Share households with TV	0.20	0.30	−0.025**
	(0.14)	(0.20)	[0.012]
Share households with refrigerator	0.02	0.07	−0.004
	(0.05)	(0.11)	[0.004]
Share households with air conditioning	0.03	0.04	−0.006
	(0.05)	(0.09)	[0.005]
Number of districts	229	332	

Note. — Panel A reports means and standard deviations of village-level covariates from the 2001 Census, the 2005 Economic Census, and 2005–06 DISE school data. All three columns include only villages with 2001 populations between 150 and 450, which is slightly wider than our optimal RD bandwidths. The middle column includes districts in the first wave of RGGVY implementation. The right column further restricts the sample to single-habitation villages in 10th-Plan districts, in states with reliable village shapefiles. Panel B reports district-level means and standard deviations for 10th- vs. 11th-Plan districts using the 2005 NSS (representative at the household level). The right column reports district-level pre-trend estimates using 2000 and 2005 NSS data, comparing 10th vs. 11th Plans, 2005 vs. 2000 (including state-specific linear trends; standard errors in brackets). Appendix Tables B7–B9 report these regression results in full. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Satellite-derived brightness is a useful proxy for electrification, as it is proportional to observed luminosity from electric lighting (Chen and Nordhaus (2011)). Several previous studies have used nighttime brightness to detect rural electrification of small villages.[25] Since other electricity end-uses rely on the same integrated power grid, nighttime brightness likely understates total electricity consumption. One caveat is that these data do not directly measure electricity use, and they cannot distinguish between changes in street lighting vs. broader gains in electricity access. However, RGGVY did not install streetlights in the 10th Plan. Therefore, any RGGVY-driven increases in nighttime brightness are likely to reflect village-wide expansions in access to energy services.

Finally, we construct a repeated cross-section from the "thick" 2000, 2005, and 2010 waves of India's National Sample Survey (NSS). Each wave surveyed 60,000–80,000 rural households, with sampling weights that are representative at the district level. While NSS data lack the village identifiers required for our RD analysis, they directly report household-level electricity consumption, appliance ownership, and total expenditure. We collapse these data into a three-wave district-level panel which we use in our DD analysis. We use two NSS measures of rural electricity access: (i) the share of households consuming any electricity, and (ii) monthly kWh consumed by the representative household. Our primary NSS outcome variable is monthly expenditure per capita, which aligns with SHRUG's village-level expenditure variable. This follows a tradition of using consumption spending to approximate

25. Ground-truthed evidence comes from villages in India (Min (2011)), South Africa (Machemedze et al. (2017)), Vietnam (Min and Gaba (2014)), and Senegal and Mali (Min et al. (2013)).

well-being in development economics.[26] Panel B of Table 1 reports NSS summary statistics prior to RGGVY, comparing 10th- vs. 11th-Plan districts.

5 Impacts of RGGVY

5.A First stage: Electricity access and consumption

Village-level electricity access Using our RD strategy, we estimate RGGVY's impact on two sets of electricity outcomes from the 2011 Census: (i) dummies for a village's first grid connection to an end-use sector, and (ii) average hours per day of power supply by sector. Table 2 reports these results numerically, and Figure 4 presents the corresponding RD plots.[27] We find that RGGVY eligibility caused a 3.8 percentage point increase in the share of villages with electricity access in all sectors (domestic, agricultural, and commercial)—a 9 percent increase over the baseline mean, statistically significant at the 5 percent level. This is driven by a 4.3 pp (10 percent) increase in commercial power access, statistically significant at the 1 percent level. We find no impacts on the extensive margin of domestic (-0.4 pp) or agricultural (-0.1 pp) power access. This is consistent with RGGVY's emphasis on more intensively electrifying villages where the *first* household already had a grid connection. By contrast, the program's goal to support microenterprises appears to have connected 1 in 13 barely-eligible villages that previously lacked any commercial power access.[28]

26. For example, see Banerjee et al. (2015); Haushofer and Shapiro (2016); or Topalova (2010) and Atkin et al. (2020), which both use this same NSS expenditure variable. Appendix C.11 provides more information on how we use NSS data.

27. Appendix Figure A4 shows RD plots for the domestic and agricultural sectors, omitted here for brevity. Appendix Table A1 presents "difference-in-discontinuities" results, which are quantitatively similar.

28. With 44 percent of barely-ineligible villages having commercial power access, the maximum increase we could estimate would be 56 pp. Our null effects for domestic access are unsurprising given that 91 percent of barely-ineligible villages had domestic power.

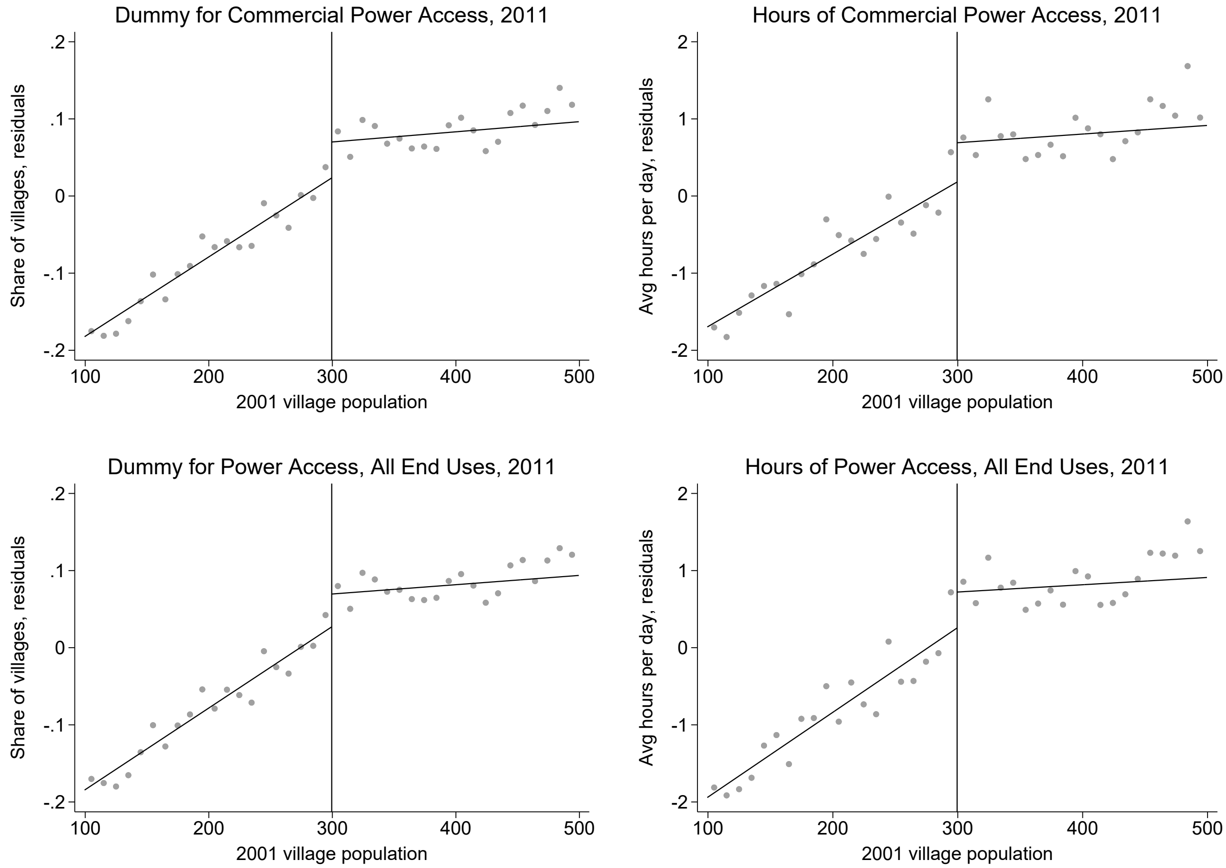
Table 2: Village-level RD in 2011 electricity access, by sector

	Outcome: Village-level electricity access			
	Domestic (1)	Agricultural (2)	Commercial (3)	All 3 sectors (4)
A. Dummy for any power access				
$\mathbf{1}$[2001 pop $\geq$ 300]	-0.004	-0.001	0.043***	0.038**
	(0.010)	(0.020)	(0.017)	(0.016)
Mean of dep var (< 300)	0.906	0.669	0.436	0.417
Optimal bandwidth	108	78	136	150
Village observations	13,517	9,836	16,900	18,574
B. Hours/day of power supply				
$\mathbf{1}$[2001 pop $\geq$ 300]	-0.042	-0.252	0.555**	0.283
	(0.207)	(0.257)	(0.220)	(0.240)
Mean of dep var (< 300)	11.386	5.382	3.957	5.050
Optimal bandwidth	88	82	126	117
Village observations	9,284	8,575	12,897	14,336

Note. — `rdrobust` estimates use linear polynomials, triangular kernels, MSE-optimal bandwidth, and nearest-neighbor standard errors. Regression samples include within-bandwidth single-habitation villages in RGGVY 10th-Plan districts (i.e. the first wave of RGGVY project implementation, for which 300 people is the relevant 2001 population-based eligibility cutoff). Each regression controls for pre-2005 nighttime brightness at the village level, and state fixed effects. Optimal bandwidths are symmetric above and below the 300-person cutoff, and we report means of the dependent variable for villages below the cutoff. In Panel A, outcomes are dummy variables for electricity access at the village level. In Panel B, outcomes are the average hours of power available per day in the village. Results are robust to alternative controls, kernels, bandwidth algorithms, and standard errors. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

We find that RGGVY eligibility increased commercial power supply by 0.56 hours per day (14 percent), statistically significant at the 1 percent level. This intensive-margin increase implies that newly-connected villages received 13 hours per day of commercial power supply, above the median of 10 hours per day for electrified villages in our RD sample. We do not detect a similar increase in hours of all-sector power supply, which likely reflects non-coincident consumption patterns across electricity end-uses (Burgess et al. (2020a)). Appendix Table A2 uses our DD strategy to estimate impacts on village-level power access beyond the RD bandwidth. This reveals a near-identical effect (3.7 pp) on all-sector power

Figure 4: Village-level RDs in 2011 electricity access



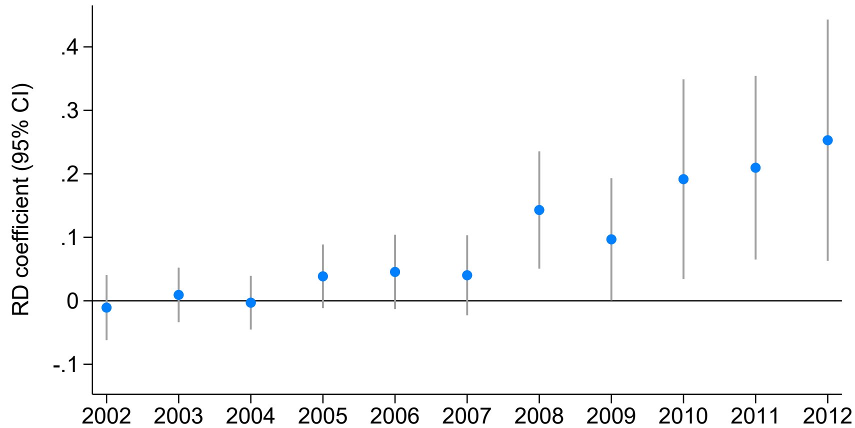
Note. — The top RD plots correspond to Column (3) of Table 2. The bottom RD plots correspond to Column (4) of Table 2. See table notes for details. Appendix Figure A4 displays RD plots corresponding to the other four regressions in Table 2.

access, with larger increases for the domestic (8.1 pp) and agricultural (5.2 pp) sectors. As further evidence of RGGVY's impact on households, we estimate a 3.7 pp increase in the share of households using electricity as a main source of lighting, applying our RD strategy to 10th-Plan districts with high RGGVY treatment intensity (see Appendix Table A12).[29]

Nighttime brightness Figure 5 presents RD estimates of the effect of RGGVY eligibility on nighttime brightness. We find null results prior to RGGVY's 2005 announcement,

29. We discuss this analysis of heterogeneous treatment intensity in Section 6.B below.

Figure 5: Village-level RD estimates in nighttime brightness, by year



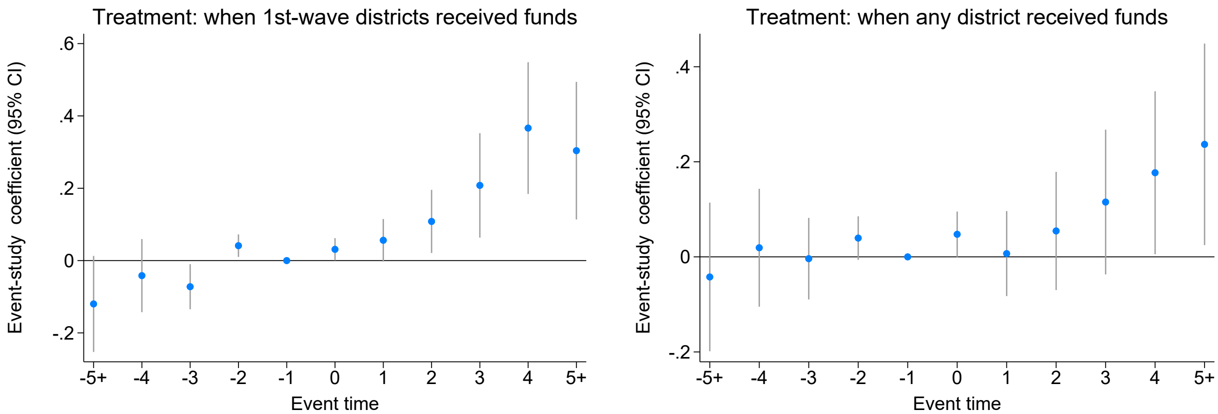
Note. — This figure plots RD coefficients for maximum nighttime brightness at the village level. We estimate a separate regression for each year, with `rdrobust` specifications identical to those in Table 2. Each regression controls for pre-2005 brightness at the village level; 2002–2005 regressions control for brightness in years preceding the outcome variable. Optimal bandwidths for these regressions range from 69 to 143. Results are robust to alternative controls, kernels, bandwidth algorithms, and standard errors. See notes under Table 2 for details. Appendix Tables A10 and B5 report these results numerically. Whiskers display 95 percent confidence intervals.

consistent with our RD assumption of baseline covariate smoothness.[30] Our RD estimates increase almost monotonically each year thereafter, and are statistically different from zero by 2008. By 2012, 4–6 years after 10th-Plan districts received funding, RGGVY eligibility had increased brightness by 0.25 units at the 300-person cutoff (statistically significant at the 1 percent level). Yearly brightness data also allow us to estimate DD event studies, where treatment turns on in the year district d first received RGGVY funds. Figure 6 reports these results, which align with our RD findings: 4–5 years after RGGVY project funding, average village brightness had differentially increased by 0.18–0.37 units.[31] As with our RD results, our event studies find no evidence of differential brightness prior to RGGVY treatment.

30. Appendix Figures A5 and B6 present the corresponding RD plots. Appendix Tables A10 and B5 report these results numerically. We find similar results if we estimate a single "difference-in-discontinuities" regression, rather than separate regressions for each year (see Appendix Figure A1).

31. This leverages both RGGVY's staggered rollout across 10th vs. 11th Plans and the staggered timing of DPR funding within each Plan, letting us capture RGGVY's delayed impacts in 11th-Plan "control" districts (right panel of Figure 6). Appendix Table A4 reports analogous pooled DD estimates.

Figure 6: Village-level DD event studies in nighttime brightness



Note. — Village-level DD event studies using annual nighttime brightness from 1998 to 2013. The outcome variable is maximum nighttime brightness in each year, for each village polygon. In the left panel, treatment (RGGVY eligibility) turns on in the year when each 10th-Plan district first received RGGVY project funds, using 11th-Plan districts as controls. In the right panel, treatment turns on for both 10th-Plan and 11th-Plan districts, in the first year the district received RGGVY project funds. 10th-Plan districts first received funds in 2005–2007, while 11th-Plan districts first received funds in 2008–2011. In both panels, the omitted year is the last year prior to a district's first receipt of funds. Regressions includes village fixed effects, state-by-year fixed effects, and village-specific linear time trends. Estimation samples include 10th- and 11th-Plan districts in states with reliable shapefiles, without restricting village size. Whiskers display 95 percent confidence intervals, with standard errors clustered by Census block.

To interpret these effect sizes, we use estimates from the remote sensing literature that ground-truth the relationship between rural electrification and nighttime brightness. Min et al. (2013) find that electrification is associated with a 0.36-unit increase in brightness in rural villages in Senegal. 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. Finally, Machemedze et al. (2017) find that connecting 50 South African homes to the grid is associated with an 0.35-unit brightness increase. In the context of these estimates, our results suggest that RGGVY caused meaningful on-the-ground increases in electricity use. Within our RD sample, a 0.25-unit brightness increase is also associated with a roughly 10 pp increase in the share of households with electric lighting (see Appendix Figure A16).

Household electrification Using our DD design with NSS data, we can directly estimate RGGVY's impacts on households. Table 3 presents these results; our preferred estimates, which control for state-specific time trends, are in Columns (2), (4), and (6). We find a 5.6 pp increase on the extensive margin of household electricity consumption (statistically significant at the 1 percent level); this intent-to-treat effect implies a 9 percent increase in household grid connections. We also find increases on the intensive margin of household electricity consumption, with ITTs of 4 kWh per month (13 percent) or 17 log points (both statistically significant at the 5 percent level). In Appendix Table A5, we report corresponding increases of 4.9 pp (8 percent) in electric lighting and 4.5 pp (11 percent) in electric fan adoption.[32]

These NSS estimates corroborate our village-level RD results, and demonstrate that RGGVY had a meaningful impact on household electricity consumption. Our 5.6 pp extensive-margin estimate implies that RGGVY connected 14 percent of previously unelectrified households in 10th-Plan districts.[33] Our 4 kWh intensive-margin estimate is consistent with moving all newly-connected households from 0 kWh to (above) the mean consumption level of electrified households. Our results also confirm that RGGVY fell short of full electrification. Since our first-stage estimates are robust and statistically precise, we can scale up RGGVY program impacts to estimate the causal effects of electrification on development outcomes.

First stage robustness Appendix Figures B1–B2 show that our first-stage RD results are broadly robust to alternative weighting kernels, bandwidth algorithms, standard errors, fixed effects, polynomials, and sample criteria. While a few sensitivities reduce statistical precision

32. Appendix Table A6 repeats this DD analysis using the analogous Census variables, and finds a similar 3.4 pp increase in the share of households with electric lighting.

33. The 95 percent confidence interval for Column (2) of Table 3 includes an 8.2 pp increase, which is consistent with RGGVY having electrified 1 in 5 previously unconnected households (as reported by Sreekumar and Dixit (2011)).

Table 3: District-level DD of household electricity access and usage

	$\mathbf{1}[\text{kWh} > 0]$		kWh/month		log (kWh/month)	
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbf{1}$[10th-Plan dist] $\times$ $\mathbf{1}$[2010]	0.048***	0.056***	2.468*	3.951**	0.306***	0.171**
	(0.014)	(0.014)	(1.319)	(1.751)	(0.068)	(0.075)
District FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Linear trends by:						
State exp. quartiles	Yes	Yes	Yes	Yes	Yes	Yes
Nat'l exp. deciles	Yes	Yes	Yes	Yes	Yes	Yes
State		Yes		Yes		Yes
Mean of dep var	0.590	0.590	31.446	31.446	3.038	3.038
District clusters	552	552	552	552	550	550
District-year observations	1670	1670	1670	1670	1661	1661

Note. — District-level DD with three NSS years (2000, 2005, 2010). We aggregate household-level data up to the district using sampling weights, for rural households only. Outcome variables are a dummy variable for whether a household consumed any electricity (Columns (1)–(2)), and monthly household electricity consumption in levels (Columns (3)–(4)) and in natural logs (Columns (5)–(6)). Difference-in-differences treatment is assigned at the district level, for 10th-Plan districts. Linear trends in quartiles of 2005 household expenditures per capita control for within-state selection in RGGVY implementation based on relative differences in districts' household consumption (e.g. states prioritizing electrification in their poorest districts). Linear trends in deciles of 2005 household expenditures per capita control for such selection in absolute terms. Standard errors clustered at the district level, collapsing to a single cluster for (i) districts that split in the 2001 Census, but which the 2000 NSS sampled based on 1991 district definitions; and (ii) the few cases where an RGGVY DPR included more than a single district. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

(for all-sector power access, in particular), our RD point estimates are stable across these variants. We also conduct RD falsification tests using samples of (i) 11th-Plan villages, for which the 300-person cutoff did not determine RGGVY eligibility; and (ii) multi-habitation villages, for which total village population is the wrong running variable. Appendix Figures B3–B4 reveal null effects for all but the correct RD sample of 10th-Plan single-habitation villages. Finally, our RD results pass placebo tests that compare the correct 300-person cutoff to a distribution of randomly generated cutoffs (see Appendix Figure B5). Our NSS results pass an analogous randomization test that scrambles the assignment of districts into the 10th-Plan "treated" group (see Appendix Figure B9).

5.B Reduced form: Economic outcomes

Having established that RGGVY increased rural electrification, we perform a "program evaluation" by estimating reduced-form intent-to-treat effects on a range of development outcomes. Table 4 reports these village-level RD results numerically, while Figure 7 presents the corresponding RD plots for key outcomes.[34] We estimate precise null effects for our preferred economic outcome, SHRUG expenditure per capita (Table 4, Panel A). We can reject increases greater than 29 rupees per month (2 percent of mean expenditure) or 1.5 log points. We can also reject over 3 pp decreases in the share of households with a poverty indicator and the share of households relying on cultivation income; over 2 pp increases in the share of low-income households earning at least 5000 rupees per month; and over 1 pp increases in the share of low-income households with a salaried job. In Panel B, we test for the Tiebout (1956) "vote with their feet" mechanism, by using 2011 population as an RD outcome. While we find a positive point estimate of 6.2 people, we can reject increases greater than 14 people (5 percent).[35]

Panels B–G of Table 4 present additional reduced-form results for village demographics, employment, firms, asset ownership, house characteristics, and education. We find no statistical evidence that RGGVY eligibility caused robust non-zero impacts in any of these outcomes. While shifts from agricultural to non-agricultural labor become statistically significant under alternative kernels and bandwidths, we can still reject changes greater than 2

34. Appendix Figures A11–A14 present RD plots for other outcomes in Table 4, omitted here for brevity. We report reduced-form RD sensitivities in Appendix Tables B1–B4; our results are similar under alternative kernels and RD bandwidths. Appendix Table A22 reports RD results for additional village-level outcomes. Panel M presents indexed and aggregated outcomes, to address any multiple testing concerns.

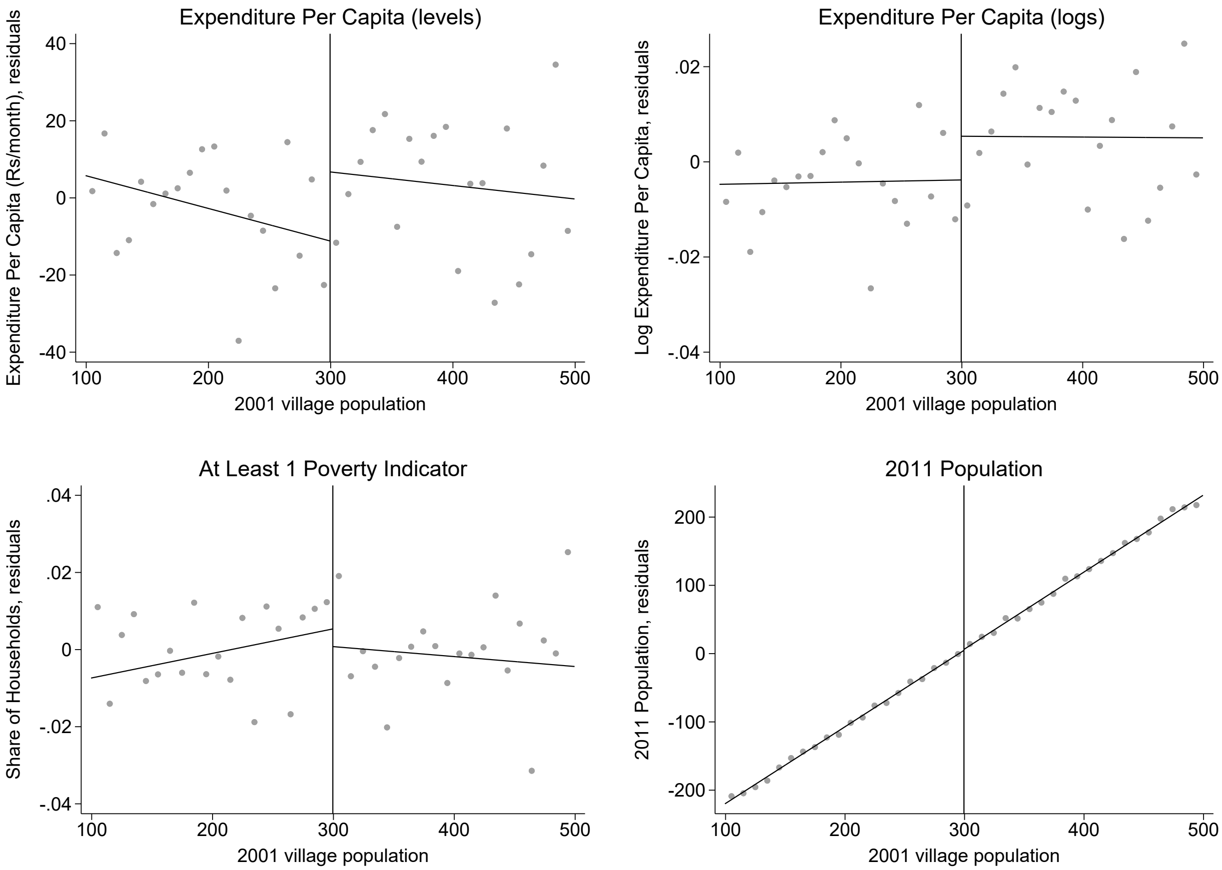
35. Because many of our outcomes are denominated as shares of village households or population, the fact that RGGVY did not impact population means that any changes in these outcomes should be driven by their numerators rather than their denominators.

Table 4: Village-level RD – reduced-form outcomes

	RD estimate	Std error	95% CI	Mean Y_v
A. Consumption and income (2011)				
Expenditure per capita (Rs/month)	−5.222	(17.565)	[−39.649, 29.206]	1365.353
Expenditure per capita (logged)	−0.010	(0.013)	[−0.034, 0.015]	9.668
Share HH with poverty indicator	−0.004	(0.013)	[−0.030, 0.022]	0.547
Share HH rely on cultivation income	−0.007	(0.012)	[−0.030, 0.016]	0.421
Share HH earning > Rs 5k/mth	0.002	(0.009)	[−0.016, 0.020]	0.070
Share HH with salaried job	0.004	(0.003)	[−0.003, 0.010]	0.012
B. Village demographics (2011)				
Population	6.213	(3.874)	[−1.379, 13.805]	296.447
Share population age 0–6	0.001	(0.002)	[−0.002, 0.004]	0.141
Average household size	0.024	(0.024)	[−0.023, 0.072]	4.908
C. Workers as share of population (2011)				
Ag workers, total	−0.006	(0.007)	[−0.019, 0.007]	0.399
Ag workers, male	−0.007	(0.006)	[−0.018, 0.004]	0.465
Ag workers, female	−0.005	(0.009)	[−0.024, 0.013]	0.329
Non-ag workers, total	0.004	(0.003)	[−0.002, 0.011]	0.075
Non-ag workers, male	0.004	(0.004)	[−0.005, 0.013]	0.096
Non-ag workers, female	0.005	(0.004)	[−0.003, 0.013]	0.053
D. Firm outcomes (2013)				
Number of firms	0.812	(0.716)	[−0.591, 2.214]	8.125
Number of firm employees	−2.173	(4.620)	[−11.228, 6.882]	15.969
E. Household asset ownership (2011)				
Share HH with telephone	−0.042***	(0.014)	[−0.069, −0.015]	0.480
Share HH with TV	0.003	(0.007)	[−0.011, 0.016]	0.225
Share HH with bicycle	0.000	(0.008)	[−0.016, 0.017]	0.473
Share HH with motorcycle	0.002	(0.004)	[−0.007, 0.010]	0.121
F. Household characteristics (2011)				
Share HH with elec/gas cooking	−0.003	(0.004)	[−0.010, 0.004]	0.033
Share HH with kerosene lighting	−0.005	(0.013)	[−0.030, 0.020]	0.437
Share HH with mud floors	0.014*	(0.008)	[−0.003, 0.030]	0.719
Share HH with thatched roofs	−0.003	(0.010)	[−0.023, 0.017]	0.235
G. School outcomes (2014–15 school year)				
# students enrolled, grades 1–5	3.086	(3.681)	[−4.128, 10.301]	46.417
# students enrolled, grades 6–8	−1.949	(2.394)	[−6.642, 2.744]	10.314
# students passed, grades 4–5	−0.438	(0.510)	[−1.437, 0.561]	5.150
# students passed, grades 7–8	−0.558	(0.418)	[−1.378, 0.261]	1.469

Note. — Each row reports results from a separate RD regression. In Panels B–C and E–F, we control for the 2001 level of the outcome variable. In Panels D and G, we control for the 2005 (or 2005–06) level of the outcome variable. RD robust regressions are otherwise identical to those in Table 2. Optimal bandwidths range from 51 to 136 above/below 300 people. Results are broadly robust to alternative controls, kernels, bandwidth algorithms, and standard errors. Estimates for telephones and mud floors are not robust to alternative weighting kernels and bandwidth algorithms. The right column reports means of the outcome variable for villages below the 300-person cutoff. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Figure 7: Village-level RDs in expenditure, poverty, and population



Note. — RD plots correspond to four regressions in Table 4: rows 1–3 of Panel A, and row 1 of Panel B. See table notes for details. All four RDs are statistically imprecise, such that the estimated discontinuities in expenditure change signs when we increase the RD bandwidth to 200 (from the optimal bandwidths of 74 and 72 in rows 1–2 of Panel A). Appendix Figures A11–A14 display RD plots corresponding to the remaining regressions in Table 4.

pp (i.e. roughly 1–2 workers).[36] Null results for firm and education outcomes have the least precision, but these point estimates still indicate relatively small effects.[37]

36. Appendix Tables B1–B4 report these reduced-form RD sensitivity analyses. Statistically significant results for telephone ownership and mud floors are not robust to alternative kernels or bandwidth algorithms.

37. For outcomes with available pre-RGGVY data, we report the analogous DD estimates in Appendix Table A23. They are broadly consistent with our RD results in Table 4, except for statistically significant increases in household size (but not fertility), decreases in kerosene lighting, and increases in school enrollment (not robust to a DD analysis that includes RGGVY's 11th-Plan rollout).

Table 5: District-level DD of household consumption expenditures

	Expenditure per capita (rupees/month)			
	Levels		Logs	
	(1)	(2)	(3)	(4)
$\mathbf{1}$[10th-Plan district] × $\mathbf{1}$[2010]	15.149	27.467	0.017	0.029
	(25.422)	(24.051)	(0.020)	(0.022)
District FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Linear trends by:				
State exp. quartiles	Yes	Yes	Yes	Yes
Nat'l exp. deciles	Yes	Yes	Yes	Yes
State		Yes		Yes
Mean of dep var	978.150	978.150	6.833	6.833
District clusters	552	552	552	552
District-year observations	1670	1670	1670	1670

Note. — District-level DD with three NSS years (2000, 2005, 2010). The outcome variable is total household expenditures per capita (net of electricity spending per capita), over the 30-day period period prior to survey enumeration, in 2010 rupees per month (Columns (1)–(2)) and log-transformed (Columns (3)–(4)). Regressions are otherwise identical to those in Table 3; see notes under Table 3 for further details. Standard errors clustered at the district level, collapsing to a single cluster for (i) districts that split in the 2001 Census, but which the 2000 NSS sampled based on 1991 district definitions; and (ii) the few cases where an RGGVY DPR included more than a single district. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 5 presents reduced-form DD results for NSS per-capita expenditure.[38] As with our SHRUG expenditure RD, we find precise null results. For our preferred specification with state-specific time trends, we recover statistically imprecise point estimates of 27 rupees per month (3 percent of mean expenditure), and 2.9 log points; we can reject RGGVY-induced increases greater than 75 rupees per month (8 percent), and 7.1 log points. Taken together, our RD and DD results imply that while the RGGVY program meaningfully increased electricity access and consumption, these increases did not translate into substantial short-to-medium-run improvements in economic development.

38. Before aggregating total expenditures to the district level (using NSS sampling weights), we subtract spending on electricity. Net expenditures are a more appropriate welfare proxy, since they account for the benefits *and* costs of consuming electricity.

6 Economic impacts of electrification

Next, we move beyond the RGGVY program to estimate the development impacts of rural electrification more broadly. Since RGGVY did not electrify all villages and rural households, it is possible that a more expansive "full electrification" program would have yielded more pronounced economic impacts. To bridge this gap, we scale our reduced-form estimates by our first-stage estimates of RGGVY's impacts on electricity access and consumption.

6.A Rescaled treatment effects

We implement a village-level fuzzy RD using two endogenous treatment variables: daily hours of commercial power supply and nighttime brightness. For each treatment variable, we apply an additional scaling factor in order to interpret the resulting estimates relative to a "full electrification" benchmark. We scale commercial power supply by 10 hours per day—the median 2011 supply in villages with nonzero commercial access.[39] This places a realistic limit on "full electrification" in a context where less than 5 percent of electrified villages received 24-hour power supply. We scale nighttime brightness by 3 units, corresponding to a shift from the 25th to the 75th percentile of 2011 brightness in our RD sample; 3 brightness units is also correlated with a 100 pp increase in the share of households with electric lighting.[40]

39. A 10-hour increase corresponds with providing *new* commercial connections at the median power quality, or with shifting a previously connected village from the 25th to the 75th percentile of commercial power quality. Appendix Figure A15 presents this distribution (mean of 10.9 hours, interquartile range of 9 hours).

40. Appendix Figure A15 presents this highly skewed distribution of 2011 nighttime brightness for our RD sample (mean of 6.2, interquartile range of 2.6). Appendix Figure A16 plots median brightness against household penetration of electric lighting; median brightness was 2.8 units higher for villages with 90–100 percent electric lighting penetration than for villages with 0–10 percent electric lighting penetration. This provides an internally consistent benchmark for "full electrification", given the observed brightness levels of the small villages in our sample.

Table 6: Fuzzy RD in expenditure per capita, using two endogenous variables

	Expenditure per capita (Rs/month)			
	Levels	Logs	Levels	Logs
	(1)	(2)	(3)	(4)
Hours/day of commercial power	−22.160	−0.020		
	(29.019)	(0.021)		
95% CI for 10-hour increase	[−790.4, 347.2]	[−0.613, 0.208]		
Units of nighttime brightness			−36.900	−0.054
			(89.315)	(0.066)
95% CI for 3-unit increase			[−635.9, 414.5]	[−0.549, 0.226]
Mean of dep var (< 300)	1366.7	9.668	1364.3	9.667
Optimal bandwidth	106	115	99	102
Village observations	10,402	11,240	11,716	12,045

Note. — Fuzzy RD robust estimates using two endogenous village-level "treatment" variables: 2011 average hours per day of commercial power in Columns (1)–(2), and 2011 nighttime brightness in Columns (3)–(4). For hours of commercial power, we report 95 percent confidence intervals scaled up by a factor of 10, the median of the 2011 distribution of non-zero hours of commercial power in the RD bandwidth (the interquartile range is 9). For nighttime brightness, we use a scaling factor of 3, which is slightly greater than the interquartile range of the 2011 distribution of village-level brightness in the RD bandwidth. Both scaling factors denominate local average treatment effects in terms of a village moving from the 25th to the 75th percentile of the "treatment" variable. Outcomes variables are 2011 SHRUG consumption expenditures per capita in levels (rupees/month) and logs. Each fuzzy RD uses a linear polynomial, triangular kernel, MSE-optimal bandwidth, and nearest-neighbor standard errors. Regression samples include within-bandwidth single-habitation villages in RGGVY 10th-Plan districts (i.e. the first wave of RGGVY project implementation, for which 300 people is the relevant 2001 population-based eligibility cutoff). Each regression controls for pre-2005 nighttime brightness at the village level, and state fixed effects. Optimal bandwidths are symmetric above and below the 300-person cutoff, and we report means of the dependent variable for villages below the cutoff. Results are robust to alternative controls, kernels and bandwidth algorithms. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 6 presents fuzzy RD results for our preferred village-level outcome, expenditure per capita. Columns (1)–(2) use hours of commercial power as an endogenous variable, and we find that 1 additional hour caused a statistically insignificant decrease of 22 rupees per month, or 2 log points. Scaling up to a 10-hour increase in commercial power supply, we can reject a local average treatment effect (LATE) greater than 347 rupees per month (25 percent), or 21 log points. Columns (3)–(4) use nighttime brightness as the "treatment" variable, where we find statistically insignificant decreases of 37 rupees per month, or 5 log points. Scaling up to a 3-unit brightness increase yields similar upper confidence intervals: we can reject a LATE greater than 415 rupees per month (30 percent), or 23 log points. Using either benchmark for "full electrification," these results imply that rural electrification is unlikely to create large welfare gains in 300-person villages in the short-to-medium term.[41]

Next, we scale up our reduced-form DD estimates using two-stage least squares, instrumenting with whether each district belonged to RGGVY's 10th Plan. Here, our endogenous NSS "treatment" variable is the share of households consuming any electricity; this requires no additional rescaling, since a 100 pp increase in household connections is already a relevant "full electrification" benchmark. We report these results in Columns (1) and (4) of Table 7. The statistically insignificant point estimates suggest increases in expenditure per capita of 316 rupees per month (a 32 percent increase in mean expenditures), or 36 log points. However, these estimates are noisy (likely due to a small first-stage F-statistic), and we cannot reject that "full electrification" would cause expenditure per capita or to double.[42]

These DD-IV estimates differ from our fuzzy RD estimates, likely due to the fact that they average over villages of all sizes: the 50th (90th) percentile NSS village had a population of 1,913 (6,854). To make our DD-IV and fuzzy RD estimates more comparable, while also reflecting the reality of last-mile electrification, we need to exclude very large villages from the NSS sample when scaling up to "full electrification."[43] We construct separate district-

41. Appendix Tables A26–A27 report the analogous fuzzy RD results for the remaining outcomes in Table 4, all of which are statistically indistinguishable from zero.

42. While this F-statistic of 11.9 meets the $F \geq 10$ "rule of thumb", it does not exceed the Stock and Yogo (2005) critical values. A weak instrument should induce upward bias on our expenditure estimates, given the positive correlation between household electricity connections and expenditure (see Appendix Table B11).

43. Compared to sub-500-person villages, villages with over 4,000 people were twice as likely to have all-sector power access prior to RGGVY. Given greater pre-existing power access, large villages are also less externally valid for considering a 0-to-100 pp increase in household connections (i.e. "full electrification").

Table 7: District-level DD-IV of household consumption expenditures

	Expenditure per capita (rupees/month)					
	Levels			Logs		
	(1)	(2)	(3)	(4)	(5)	(6)
1[HH consumes any elec]	315.9	−680.4	887.2	0.361	−0.229	0.629
	(529.8)	(518.7)	(646.7)	(0.422)	(0.327)	(0.518)
95% confidence	[−724.8, 1356.5]	[−1704.8, 343.9]	[−383.5, 2157.9]	[−0.469, 1.190]	[−0.875, 0.417]	[−0.389, 1.646]
Village weight quintiles	Pooled	1	2–5	Pooled	1	2–5
50th pctile of 2001 pop	1913	1043	2076	1913	1043	2076
90th pctile of 2001 pop	6854	4875	7291	6854	4875	7291
District FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Linear trends by:						
State exp. quartiles	Yes	Yes	Yes	Yes	Yes	Yes
Nat'l exp. deciles	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep var	978.2	1128.2	948.2	6.833	6.957	6.804
District clusters	552	162	494	552	162	494
District-year obs	1670	418	1488	1670	418	1488
First-stage F-statistic	11.91	16.74	8.84	11.91	16.74	8.84

Note. — District-level DD with three NSS years (2000, 2005, 2010), estimated via two-stage least squares, and instrumenting for household electricity access with the interaction **1**[10th-Plan district] × **1**[2010]. Columns (1) and (4) are analogous to Columns (1) and (3) of Table 5, respectively; as in Table 5, the outcome variable is net of per capita spending on electricity. Columns (2)–(3) and (5)–(6) split the sample on within-year quintiles of NSS village weights before collapsing to the district level using sampling weights. We do not observe village populations for the 2000 NSS wave, meaning that we cannot estimate a 3-period panel splitting directly on village size. However, isolating the first quintile of NSS sampling weights shifts the distribution of 2001 village populations (as observed in 2005 and 2010 waves) towards smaller villages. Standard errors clustered at the district level, collapsing to a single cluster for (i) districts that split in the 2001 Census, but which the 2000 NSS sampled based on 1991 district definitions; and (ii) the few cases where an RGGVY DPR included more than a single district. The bottom row reports Kleibergen-Paap first-stage F-statistics. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

level panels for villages in the first quintile (Q1) vs. quintiles 2–5 (Q25) of village sizes, proxying for population using village-level NSS sampling weights. Ideally, we would split the sample using village populations; however, since we only observe village populations for the 2005 and 2010 NSS waves, we prefer to split using sampling weights and retain the 2000 NSS wave, increasing statistical power. This approach follows from the NSS's sampling strategy, which is explicitly proportional to village size. Isolating Q1 villages (with small sampling weights) removes the vast majority of villages over 4000 people, shifting the NSS distribution towards smaller villages with a median population of 1,043.[44] Appendix Tables A8–A9 show statistically significant first-stage estimates for both Q1 and Q25 subsamples; we find larger Q1 effects on the extensive margin, consistent with smaller villages having had fewer household connections prior to RGGVY.[45]

Columns (2) and (5) of Table 7 present DD-IV results for the Q1 subsample, which has a relatively strong first stage (the F-statistic of 16.74 exceeds the Stock and Yogo (2005) critical values). While the point estimates indicate decreases in per-capita expenditure of 680 rupees per month, or 23 log points, neither estimate is statistically distinguishable from zero. We can reject average treatment effects of "full electrification" greater than 344 rupees per month (30 percent), or 42 log points. If we instead use a 50 pp increase in household connections as our "full electrification" policy experiment—since roughly half of rural Indian households were grid-connected at baseline—we can reject 15 percent expenditure increases.[46] The upper confidence intervals from our Q1 DD-IV estimates and our fuzzy RD estimates are very similar—despite using different consumption measures, identification strategies, and endogenous treatment variables. Together, these results demonstrate that last-mile electrification of small villages is unlikely to yield large increases in consumption expenditure.

44. Appendix Figure C6 plots village populations in the 2005 and 2010 NSS waves, split by Q1 vs. Q25, showing that splitting on sampling weights is an effective way of removing large villages from the sample.

45. Appendix Table A7 finds similar extensive-margin results for a 2-wave NSS panel, comparing villages larger vs. smaller than 2000 people (roughly the median NSS village size). The sub-2000-person subsample produces results similar to the Q1 subsample, but with a weaker first-stage F-statistic. Appendix Figure A2 replicates these DD population splits using a 2-year Census panel, for the extensive margin of village (rather than household) electricity access. This likewise reveals larger first-stage effects for smaller villages.

46. Appendix Table A25 reports DD-IV results for a 2-wave NSS panel of sub-2000-person villages; these estimates are very similar to our Q1 estimates in Table 7 (though splitting directly on village population shortens our NSS panel to two waves, weakening the first stage). Our DD-IV regressions also lose first-stage power if we include state-specific trends (see Appendix Table B10), yet the Q1 point estimates remain similar.

Our results for larger villages tell a different story. While our point estimates for the Q25 subsample are statistically imprecise, they are positive and large (Columns (3) and (6) of Table 7). We cannot reject a tripling (or 165 log point increase) of expenditures in these larger villages, albeit with a weak first-stage F-statistic of 8.8. When we further restrict the Q25 subsample to districts with at least 10 hours per day of rural electricity supply, this first-stage F-statistic rises to 15.8, and we estimate a per-capita expenditure increase of 1,428 rupees per month (139 percent), or 94 log points (statistically significant at the 10 percent level; see Appendix Table A20). By contrast, when we impose this same 10-hours-of-power-supply restriction on the Q1 subsample, we can reject 26 percent increases above mean expenditure per capita. These results uncover substantial heterogeneity in the benefits of rural electrification: in small-to-medium villages, we reject even modest increases in per-capita expenditure; in large villages, our results suggest sizable expenditure gains.

6.B Heterogeneous RGGVY implementation intensity

A potential concern with scaling up to "full electrification" treatment effects is that doing so extrapolates beyond the range of observed RGGVY policy outcomes. To address this concern, we leverage heterogeneity in RGGVY's implementation intensity across districts. We split 10th-Plan districts based on whether RGGVY treated at least 60 percent of villages, as reported by district-level administrative data. This isolates a subset of 90 high-intensity districts with a narrower gap between "full electrification" and RGGVY's actual impacts.[47]

47. There were 35 low-intensity 10th-Plan districts, where RGGVY treated less than 60 percent of eligible villages. We drop 5 10th-Plan districts from this heterogeneity analysis, due to implausibly large village counts in RGGVY implementation data. Appendix Figure A6 shows the distribution of RGGVY implementation intensity across 10th-Plan districts.

For this subsample of high-intensity districts, our first-stage RD coefficient for hours of commercial power nearly doubles to 0.973 hours per day (see Appendix Table A11). We also estimate increases of 0.636 hours for all-sector power supply (significant at the 5 percent level), and 0.502 hours for domestic power supply (significant at the 10 percent level). Our RD coefficients on the dummies for commercial and all-sector access increase from 4.3 and 3.8 pp to 5.6 and 5.6 pp, respectively. Isolating this subsample of RGGVY-intensive districts also nearly doubles our coefficients on nighttime brightness (see Appendix Figure A7). Finally, Appendix Table A13 estimates a heterogeneous DD model, where we find larger effects on household electricity consumption in high-intensity districts (5.4 kWh per month and 22 log points, compared to 4.0 kWh per month and 17 log points in Table 3).

Despite these much stronger first-stage effects, our reduced-form RD results for high-intensity districts are largely unchanged. Appendix Table A14 reports null effects across most RD outcomes, including SHRUG expenditure per capita. While we find suggestive evidence that RGGVY may have decreased the share of households with poverty indicators by 3.3 pp, or increased non-agricultural employment by 0.9 pp (both statistically significant at the 10 percent level), these magnitudes remain modest.[48] Appendix Table A15 shows that if we scale up to "full electrification" using only high-intensity districts, our fuzzy RD results still reject expenditure increases greater than 30 percent, or 24 log points—despite requiring much smaller scaling factors. While heterogeneous DD estimates show large statistically significant expenditure increases in high-intensity districts, these ITT expenditure effects become statistical zeros in the Q1 subsample that drops extremely large villages (see

48. The most statistically significant result in Appendix Table A14 is a 4 pp decrease in kerosene lighting, which mirrors the 3.7 pp increase in electric lighting in Appendix Table A12.

Appendix Table A16).[49] Together, these results demonstrate that even in districts where RGGVY came closest to the "full electrification" ideal, we can reject meaningful economic impacts in small-to-medium villages.

6.C Welfare analysis

Our scaled-up treatment effect estimates allow us to evaluate the welfare effects of "full electrification." We use two strategies for quantifying economic benefits, which we compare against the costs of electrification. First, we use our fuzzy RD and DD-IV estimates for expenditure per capita. This welfare proxy is well-suited to measure treatment effects from an electrification program which affects multiple sectors of the village economy, since consumption spending should capture electricity-induced changes in agricultural and firm productivity. To account for the uncertainty in our econometric estimates, we take 10,000 draws from the sampling distributions of our fuzzy RD coefficients in Columns (1) and (3) of Table 6, and our DD-IV coefficients in Columns (2)–(3) of Table 7.[50]

Our second strategy for quantifying benefits uses our econometric estimates to model households' consumer surplus from electricity use. To construct electricity consumption for newly-electrified households, we divide our intensive-margin kWh estimates by our extensive-margin household connections effects, using our first-stage NSS results for the Q1 and Q25 subsamples (see Appendix Tables A8–A9). This effectively scales RGGVY's impact on

49. Appendix Table A24 reports homogeneous DD estimates for the Q1 and Q25 subsamples, for comparison. We lack the statistical power to estimate a DD-IV model that splits on RGGVY implementation intensity.

50. Appendix Figure A17 plots these sampling distributions, which we convert to 2010 rupees per year. Our NSS expenditure variable subtracts spending on electricity, in order to account for the benefits *and* costs of electricity consumption. We lack the data to do the same for SHRUG expenditure per capita, meaning that our fuzzy RD estimates may slightly overstate net welfare benefits.

household electricity consumption to a "full electrification" benchmark where 100 percent of households receive new grid connections, while conservatively assuming that any changes in kWh consumed come exclusively from newly-connected households. Our results imply that the average newly-connected household consumed 53.9 kWh per month in Q1 villages, and 73.4 kWh per month in Q25 villages. We combine these kWh estimates with the median electricity price in the 2010 NSS of 2.64 rupees per kWh, and a demand elasticity of -0.62 from Burgess et al. (2020a), which is also set in rural India.[51] We calculate consumer surplus under the assumption of linear demand, imposing this elasticity at the above consumption levels (following Lee, Miguel, and Wolfram (2020b) and Barreca et al. (2016) in applying a Dubin and McFadden (1984) discrete/continuous model).[52]

We compute village surplus by multiplying per-capita expenditure by population size, to measure effects for 300- (aligning with our fuzzy RD LATEs), 1,000-, and 2,000-person (roughly the median village sizes in our Q1 and Q25 subsamples) villages. We do the same for our estimates of per-household consumer surplus, assuming 5 people per household (the NSS median). Then, we calculate the present discounted sum of village-wide benefits over 20 years. We subtract the up-front costs of electrification, using RGGVY's allowable cost norms: "low" (1.3 million rupees) or "high" (1.8 million rupees) fixed costs per village, plus 2,200 rupees in variable costs per household (Banerjee et al. 2014).[53] These numbers are on the low end of rural electrification costs: while Chakravorty, Emerick, and Ravago (2016) report

51. Burgess et al. (2020a) estimate this price elasticity of demand for grid power in rural Bihar. Mahadevan (2020) finds a very similar elasticity of -0.56 for rural households in West Bengal.

52. This consumer surplus approach has the advantage of capturing non-expenditure-related aspects of private utility. For example, it would capture students' ability to study at night using electric lighting.

53. Variable costs are based on the allowable costs for BPL household connections, which were fully subsidized under RGGVY. We inflate these cost norms from 2008 rupees to 2010 rupees.

Table 8: Return on investment from electrification, using consumption expenditure

	Pr(20-year ROI > 0) by village population			
	300	300	1000	2000
Low fixed cost				
$r = 0.05$	0.184	0.289	0.087	0.904
$r = 0.15$	0.150	0.244	0.083	0.901
High fixed cost				
$r = 0.05$	0.170	0.273	0.086	0.903
$r = 0.15$	0.130	0.213	0.081	0.900
Expenditure/capita	SHRUG	SHRUG	NSS	NSS
Endog. variable	Hours of power	Brightness	$\mathbf{1}$[HH elec > 0]	$\mathbf{1}$[HH elec > 0]
Instrument	300-person RD	300-person RD	1st-wave district	1st-wave district
Estimation sample	RD bandwidth	RD bandwidth	Quintile 1	Quintiles 2–5

Note. — We calculate benefits using our results from Columns (1) and (3) of Table 6, and from Columns (2)–(3) of Table 7. We rescale fuzzy RD estimates by 10 (for hours of commercial power) and 3 (for nighttime brightness), and convert all four estimates to annual expenditures per capita in 2010 rupees. Then, we make 10,000 draws from each rescaled sampling distribution, and calculate the 20-year sum of expenditure increases for villages of 300, 1000, or 2000 people (with discount rate r, assuming a constant flow of annual benefits). Finally, we subtract fixed costs per village and variable costs per household. Following Banerjee et al. (2014, p. 51), we assume fixed costs of 1.3 million rupees ("low") or 1.8 million rupees ("high"), and variable costs of 2200 rupees, inflating from 2008 to 2010 rupees. We report the share of 10,000 draws with a positive net present value, for each combination of estimate, village size, discount rate, and fixed costs.

similar per-village costs in the Philippines, Lee, Miguel, and Wolfram (2020b) document per-household costs at least an order of magnitude larger in Kenya.[54]

Table 8 summarizes our expenditure-based welfare analysis, where we report the share of draws from the expenditure treatment effect sampling distribution that generate a positive return on investment (ROI) from rural electrification. For a 300-person village using our preferred 15 percent discount rate (following Lee, Miguel, and Wolfram (2020b)), our fuzzy RD results imply that "full electrification" has less than a 25 percent chance of generating expenditure benefits that exceed upfront costs. This holds for both endogenous treatment variables, with slightly more favorable ROIs using nighttime brightness than hours of com-

54. If we replaced RGGVY's cost norms with Lee, Miguel, and Wolfram (2020b)'s average per-household costs of US $398, this would imply negative returns on investment across all scenarios we present below.

mercial power. For a 1,000-person village, our DD-IV results in the Q1 subsample imply less than a 9 percent chance of a positive ROI. By contrast, our DD-IV results in the Q25 subsample imply a 90 percent probability of positive returns from "fully electrifying" a 2,000-person village. This difference is driven both by greater positive support in the Q25 sampling distribution of per-capita treatment effects, and by lower average costs per household.

Table 9 reports the analogous ROI calculations using consumer surplus from electricity consumption. The implied ROI from "fully electrifying" a 300-person village is unambiguously negative.[55] For a 1,000-person village, the implied ROI remains negative under "high" fixed costs, using our preferred 15 percent discount rate. Electrification appears (barely) benefit-cost positive under "low" fixed costs, and the ROI increases to 1.2 if we also lower the discount rate to 5 percent. For a 2,000-person village, our calculations imply that "full electrification" would likely be a good investment, with ROIs ranging from 1.4 to 5.0.[56] If we restrict our sample to districts with at least 10 hours per day of rural power supply, we find nearly identical ROIs for 300- and 1000-person villages, but much higher consumer surplus for 2000-person villages (3,474 rupees per household-year) and ROIs ranging from 3.5 to 10.2 (see Appendix Table A21).[57]

55. Since our RD strategy cannot provide direct estimates of household electricity consumption, we use our first-stage estimates for the Q1 subsample to derive consumer surplus in a 300-person village.

56. Multilateral development banks often use an internal rate of return (IRR) threshold of 10–12% to benchmark cost-effectiveness (Asian Development Bank (2013); Bonzanigo and Kalra (2014)), though their projects are often projected to have much higher IRRs (The World Bank (2010)). Table 9 implies IRRs of less than 0.5 percent for 300-person villages; 12 (20) percent with high (low) fixed costs for 1,000-person villages; and greater than 50 percent for 2,000-person villages.

57. One might worry that low benefits from electrification are inevitable given intermittent power supply in India's rural villages. Appendix A.3 reports heterogeneous effects for districts with at least 10 hours per day of rural power supply in 2011. For these high-power-quality districts, we find larger first-stage impacts on the extensive and intensive margins of household consumption (see Appendix Table A19). Our DD-IV estimates remain largely unchanged for the Q1 sample. However, they increase in magnitude and gain weak statistical significance for the Q25 subsample (see Appendix Table A20), suggesting that improved reliability is beneficial in larger villages.

Table 9: Return on investment from electrification, using consumer surplus

	Village population		
	300	1000	2000
Monthly kWh per newly electrified HH	53.9	53.9	73.4
CS per newly electrifed HH (Rs/year)	1,377	1,377	1,875
CS created across village (Rs/year)	82,622	275,407	750,121
20-year ROI, low fixed cost			
$r = 0.05$	−0.336	1.214	5.029
$r = 0.15$	−0.635	0.218	2.317
20-year ROI, high fixed cost			
$r = 0.05$	−0.519	0.603	3.365
$r = 0.15$	−0.736	−0.118	1.401
NSS estimation sample	Quintile 1	Quintile 1	Quintiles 2–5

Note. — Using Appendix Tables A8–A9 which estimate first-stage regressions by NSS weight quintile, we divide Column (4) by Column (2) to scale up the intensive margin effect by the (inverse) extensive margin effect of RGGVY. The top row reports the implied increase in kWh per household per month from a new electricity connection; this is close to the 53 kWh/month average for electrified households in the 2010 NSS. Using these consumption levels, we apply the methodology of Lee, Miguel, and Wolfram (2020b) to calculate consumer surplus per household. We assume linear demand, a retail electricity price of 2.64 rupees/kWh (the median price in the 2010 NSS), and a rural electricity demand elasticity of 0.62 (Burgess et al. (2020a); Mahadevan (2020) estimates a similar rural residential elasticity of 0.56). The second row reports annual consumer surplus per newly electrified household. The third row aggregates consumer surplus assuming all village households gain electricity access, using the 2010 NSS median household size of 5 people. The bottom four rows report return on investment, taking the 20-year discounted sum of consumer surplus in the village, and using the same cost assumptions as Table 8. See notes under Table 8 for further details.

Both approaches imply that net welfare gains are unlikely in the small-to-medium villages typically targeted for last-mile electrification. Absent meaningful increases in expenditure or consumer surplus, even RGGVY's modest cost norms appear too high to justify investment, given the opportunity cost of funding for development initiatives. For larger villages, however, we see evidence that electrification can improve welfare, by both (i) generating positive per-capita benefits and (ii) spreading fixed costs of infrastructure over a sizable population.

Our divergent results for small vs. large villages may also help to reconcile conflicting estimates in the existing literature. Lee, Miguel, and Wolfram (2020b) find that electrification is benefit-cost negative in Kenyan villages with an average size of 535 people. In contrast, prior research showing positive impacts of electrification has tended to focus on larger treated populations: approximately 1,200-person villages in the Philippines (Chakravorty, Emerick, and Ravago (2016)); similarly-sized communities in South Africa (Dinkelman (2011)); entire Brazilian counties (Lipscomb, Mobarak, and Barham (2013)); and entire Indian states (Rud (2012)).[58] Our findings suggest that while small villages may lack the necessary conditions to translate electrification into economic development, larger communities may be able to use electricity in more economic productive ways, taking advantage of economies of scale.

7 Conclusion

What are the economic effects of continuing to expand electricity access? This paper evaluates the medium-run welfare impacts of electrification in the context of RGGVY, India's flagship national rural electrification program. RGGVY brought electricity access to the world's largest unelectrified population, affecting over 400,000 villages across rural India. The lessons from RGGVY are highly relevant to ongoing electrification efforts, since RGGVY took place while India was at a similar level of economic development as the majority of today's unelectrified populations. Importantly, RGGVY provided connections to the power grid, which likely represents the future of rural electrification despite the emergence of minigrids (Burgess et al. (2020a)).

58. Summary statistics from Chakravorty, Emerick, and Ravago (2016) suggest an average of 240 households per village, with a mean household size of 5.25. Dinkelman (2011) reports averages of 630 and 765 adults per community, ages 15–59. Assuming two unobserved children per household (consistent with a 5-person mean household size), this implies populations of 1,050 and 1,275 for the average community.

Against this backdrop, we demonstrate that RGGVY significantly shrank—though did not eliminate—India's electricity access gap. Despite increases in electricity access and consumption, the program generated limited economic impacts in the medium term. We scale these program effects using instrumental variables, and can reject meaningful economic benefits from electrifying small villages. Any benefits that may accrue to small villages do not outweigh the costs of electrifying low-population areas. However, we find suggestive evidence that in larger villages, electrification can provide sizable per-capita benefits at lower average costs. These results help to reconcile estimates from the existing rural electrification literature, which has recently found small economic impacts in villages (e.g. Lee, Miguel, and Wolfram (2020b)) and previously found large effects in broader populations (e.g. Rud (2012); Lipscomb, Mobarak, and Barham (2013)).

Our results imply that targeting can help to improve the economic efficiency of last-mile electrification. They also signal the potential importance of targeted complementary investments: we find that reliable power supply increases the benefits from electrification in large villages, but not in small-to-medium villages. Our results also speak to other investments besides electricity. While "first-mile" infrastructure projects have been shown to improve welfare in low-income settings (e.g., transportation networks in Donaldson (2018) and Banerjee, Duflo, and Qian (2020)), last-mile investments may be less likely to generate meaningful changes in well-being (e.g., rural roads in Asher and Novosad (2020)). Policymakers wishing to provide public goods to rural, low-income communities face two challenges: not only are there high costs of providing infrastructure to remote, sparsely populated areas, but these communities may not be in a position to translate improved infrastructure into meaningful economic gains.

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