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Oliver Vanden Eynde and Liam Wren-Lewis

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Centre for Economic Policy Research
33 Great Sutton Street, London EC1V 0DX, UK
Tel: +44 (0)20 7183 8801
www.cepr.org

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JEL Classification: O13, O18, Q15

Keywords: infrastructure, complementarities, Agriculture

Oliver Vanden Eynde - olivervandeneynde@gmail.com
Paris School of Economics, CNRS and CEPR

Liam Wren-Lewis - liamwrenlewis@gmail.com
Paris School of Economics, INRAE

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Complementarities in Infrastructure: Evidence from Rural India*

Oliver Vanden Eynde† Liam Wren-Lewis‡

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Abstract

Complementarities between infrastructure projects have been understudied. Our paper examines interactions in the impacts of large-scale road construction, electrification, and mobile phone coverage programs in rural India. We find strong evidence of complementary impacts between roads and electricity on agricultural production: dry season cropping increases significantly when villages receive both, but not when they receive one without the other. These complementarities are associated with a shift of cropping patterns towards market crops and with improved economic conditions. In contrast, we find no consistent evidence of complementarities for the mobile coverage program.

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†Paris School of Economics, CNRS. Email: `o.vandeneynde@psemail.eu`.

‡Paris School of Economics, INRAE. Email: `liamwrenlewis@gmail.com`.

Introduction

Rural infrastructure is central to the development agenda of most low and middle-income countries. Governments often bundle large-scale infrastructure programs in the belief that complementarities between these interventions are important (Sanchez et al., 2007; Moneke, 2020). However, concentrating infrastructure investments geographically worsens spatial inequalities (Kanbur and Venables, 2005). Evaluating the size of any complementarities is therefore important, but evidence is sparse because infrastructures' non-random placement makes it difficult to credibly identify interactions. In particular, in many contexts the size of the overlap between infrastructure programs makes it difficult to consistently identify both the individual and combined impacts of different programs.

This paper evaluates complementarities in infrastructure programs in rural India. We exploit the independent roll-out of three major programs across the country between 2005 and 2014: PMGSY (roads), RGGVY (electrification), and USOF (mobile telecommunications). These programs were part of India's "Bharat Nirman" campaign of rural infrastructure development. Their management and deployment were independent, however, allowing us to analyze potential complementarities. Because of the almost unprecedented scale of these programs, which covered a substantial share of India's more than 600,000 villages, we are able to provide plausibly causal estimates of the separate and joint provision of different infrastructure types.

Our main outcome of interest is dry season agricultural production. While Indian agricultural productivity rose sharply during the Green Revolution, it has stalled in recent years. Improved dry season cropping is an important potential source of agricultural productivity growth (Saksena et al., 2020; Jain et al., 2021). To the extent that it goes hand in hand with crop diversification, dry season production could also help mitigate climate change impacts (Lal et al., 2017; Sambasivam et al., 2020). Two additional features make dry season cropping particularly well-suited to serve as the main outcome of our current study. First, it is measured at high frequency, so we can use plausible exogeneity in the timing of infrastructure projects to estimate their effect. Second, it benefits from a combination of inputs and market integration, meaning complementarities could be important. Dry season cropping requires water-management which is facilitated by the use of electric pumps. At the same time, access to input markets (in particular, high quality seeds of varieties well suited to the dry season) and output markets boost the returns to irrigation investments (Saksena et al., 2020). Both road access and mobile phone coverage could strengthen market integration and hence could spur dry season cropping through this channel.

We find that the joint provision of an electricity and road connection boosts dry season cropping beyond the effect of each type of infrastructure on its own. These two types of infrastructure are hence complementary in driving rural development. We also present suggestive evidence that the combined provision of electricity and road access increases market crop production and raises asset ownership. These results are consistent with the hypothesis that the joint provision of electricity and road access is necessary to make dry season cropping profitable. Electrification in isolation may not provide the market access needed to grow market crops, while road connectivity on its own may not allow for improved water management. For mobile phone coverage, we do not find consistent evidence of complementarities with either electrification or road access.

Our paper contributes to a recent literature that has studied these or similar infrastructure programs in isolation. On roads, Asher and Novosad (2020) find that PMGSY reduced the agricultural employment share, but had no impacts on agricultural investment or output.[1] On electricity, Burlig and Preonas (2016) do not find significant impacts of RGGVY on measures of economic development other than electricity use.[2] On mobile coverage, Gupta et al. (2020) find (using district-level data) that the USOF program increased farmers use of high yielding seeds and complementary inputs in-

[1]Adukia et al. (2020) show that PMGSY also boosted educational investment, while Asher et al. (2020) find no effect on deforestation. Aggarwal (2018) finds benefits in terms of lower agricultural output prices and increased technology adoption. While seemingly at odds with Asher and Novosad (2020), the study analyzes the impacts at a more aggregate level. One possibility is that different combinations of infrastructure (and their complementarities) drive the district-level results in Aggarwal (2018). Outside of India, Brooks and Donovan (2020) show that the construction of bridges boosted agricultural investment in Nicaragua by protecting farmers' from losing road access in floods.

[2]Rud (2012) finds that electrification in India increased raised industrial output between the 1960s and 1980s. In the 1980s and 1990s, Van De Walle et al. (2017) report significant consumption gains for households that benefited from village electrification. Outside of India, Lipscomb et al. (2013) show that electrification boosted labour productivity in Brazil, but recent experimental evidence from Keyna finds limited economic impacts of rural electrification (Lee et al., 2020).

cluding irrigation.[3] None of these recent papers study complementarities explicitly.[4] Interestingly, in our cross-sectional analysis, the separate effects of these three infrastructure types are in line with the existing literature, but only the joint provision of electrification and road access is associated with increased asset ownership.

Moneke (2020) is to our knowledge the one existing study that causally estimates interactions between electrification and road construction.[5] He shows that, in Ethiopia, the combined provision of electricity and road connections boosted industrialisation. This impact of joint provision is substantially different from the one of road connections on their own. One limitation of this study, however, is that it cannot estimate the impact of electrification on its own. Another difference from our paper is that Moneke (2020) focuses on industrialisation, whereas we focus on rural development. Improving agricultural productivity is a key part of structural change. Our results suggest that the simultaneous provision of electricity and roads is necessary to maximize their economic impacts.

1 Background and data

This paper focuses on the period 2005-2014, when India invested heavily in rural infrastructure provision. Our paper studies three flagship schemes on which we collected detailed implementation data, and for which complementarities could be important: electrification, road construction, and mobile phone coverage. In this section, we provide background details on each of these programs, we introduce our measures of dry season cropping, and we outline the other data sources used in our

[3] Asad (2016) finds positive effects of mobile phone coverage on the adoption of perishable crops in Pakistan. Jensen (2007) and Jensen and Miller (2018) find mobile tower construction in Kerala reduced price dispersion across fish markets and increased competition between boat builders.

[4] Asher and Novosad (2020) and Burlig and Preonas (2016) rely on regression discontinuity approaches for identification. However, the overlap between the RD samples in PMGSY and RGGVY is too small to identify complementarities between these programs. Instead, we will rely on a event study design and use high-frequency outcomes.

[5] Chaurey and Le (2021) implicitly point at complementarities in rural infrastructure development in India. They find that a small-scale infrastructure program (aimed at so-called 'backward' villages) reduced agricultural employment, and more so in villages with prior paved roads and electricity connections. However, the approach of their study does not allow for the causal identification of complementarities.

paper. Details of the timing of the infrastructure programs are given in Figure 1.

1.1 Rural Electrification: Rajiv Gandhi Grameen Vidyutikaran Yojna (RGGVY)

Since independence, the Indian Government has launched multiple village electrification schemes, but full electrification was still a far-off goal in the early 2000s. To accelerate the pace of electrification, the government launched an ambitious flagship scheme – RGGVY – in 2005. The aim of the program was to secure an electricity supply to all un-electrified villages as well as to provide free electricity connections to all below poverty line (BPL) households. All existing rural electrification programs were merged into RGGVY in 2005. The program targeted more than 360,000 villages.

The office overseeing the program was the Rural Electrification Corporation (REC), while the implementation was delegated to the State Utility Providers. We obtained administrative data on the implementation of the program directly from the REC. This information is matched to 2001 census villages. As our main measure of electrification, we use the completion date at the village level. Villages for which we do not have a completion date are not included in the sample.

1.2 Rural Road Construction: Pradhan Mantri Gram Sadak Yojana (PMGSY)

The government launched PMGSY in 2000 with the goal of connecting unconnected villages to the road network through an all-weather road. The proposed network of roads was determined in 2001, and the subsequent implementation of PMGSY has consisted of the gradual realisation of this "Core Network". Habitations with larger populations were prioritised. The program has been described as "unprecedented in its scale and scope" (Aggarwal, 2018), with roadwork for over 125,000 habitations completed by 2016. The program was coordinated by the National Rural Roads Development Agency (NRRDA), even if most implementation decisions were taken at the state level. In contrast to RGGVY, which relied entirely on public sector providers, the road construction under PMGSY was carried out by private contractors. Our information on PMGSY implementation comes from the SHRUG dataset, which provides data at the level of 2001 Census villages.[6] As our main measure of road connectivity, we use the road completion date at the village level.

1.3 Mobile Phone Coverage: Universal Service Obligation Fund (USOF)

The National Telecom Policy (1999) introduced USOF as a national program to increase telecom services in rural India (Noll and Wallsten, 2006). USOF consists of different programs, but we focus on Phase I of the Shared Mobile Infrastructure Scheme, which encouraged private mobile infrastructure development and infrastructure sharing in rural areas. Under this scheme, infrastructure providers were subsidized to construct telecom towers in specified multi-village clusters. The infrastructure could be shared among providers to install their own equipment to provide mobile service to the cluster. The Department of Telecommunications proposed tower locations according to population eligibility criteria. Towers were approximately grouped by district, and for each group, providers could bid for annual subsidies. Once awarded the subsidy for a group, providers were allowed to shift the location and number of the towers. Shifts in location were considered justified if coverage was already available at the selected location, construction was too difficult given the soil or geography, or coverage could be improved at a nearby location. Providers were required to ensure towers were not built within three kilometers of any existing cellular tower. Overall, 7,874 towers were proposed, and 7,353 were actually built under phase one, with the median proposed tower site finally having a tower built 3km away. We collected administrative data on the implementation of the program directly from the Department of Telecommunication. As the implementation date, we use the date at which actual towers were commissioned.[7] We then calculate the distance of villages to proposed and constructed towers to villages based on their coordinates.

[6] The Socioeconomic High-resolution Rural-Urban Geographic Platform (SHRUG) was constructed by Asher et al. (2021). Although we collected data from administrative sources on PMGSY independently, we use the SHRUG data for consistency with the recent literature.

[7] When the commissioning date is missing, we use the earliest date a tower was commissioned in the sub-district if available, and otherwise we use the earliest date in the district or state.

1.4 Dry season cropping in India

India has two main cropping seasons: (i) Kharif, the monsoon season from July to October; and (ii) Rabi, the dry season from October to March.[8] Only about 35% of total agricultural land in India is irrigated and two thirds of cultivated land is entirely dependent on rainfall.

Dry season agricultural production can be measured from satellite imagery based on the observed vegetation ("greenness"). We use such measures from three sources. The first is the annual Land Use Land Cover maps produced by the Indian Space Research Organisation (ISRO). This uses ISRO's Resourcesat satellites and gives an estimation of land use at a 56m resolution.[9] For this and other gridded data, we calculate measures at the village level by overlapping with Thiessen polygons constructed using the 2001 Census village coordinates. The second source is a measure of winter cropping produced by Jain et al. (2017). This uses the Enhanced Vegetation Index from the MODIS satellites to estimate the share of each 1km pixel that is cropped during the winter. The third source is from Asher and Novosad (2020), who compute the change in the Normalized Difference Vegetation Index in the Rabi season, also from MODIS. Since each of these sources is attempting to measure the same latent variable - i.e. dry season cropping - our preferred outcome measure is an inverse covariance weighted index which combines the information from each source (Anderson, 2008). In the online appendix, we validate this approach — Table A4 shows that each measure is partially correlated with the extent of dry season cropping in the Indian Health and Development Survey (IHDS) and, moreover, the index has a higher R^2 than each one of the individual measures.

1.5 Additional data sources

We use information from the 2001 Census as control variables in robustness checks. Information from the 2011 population census and the 2011 socio-economic and caste

[8] The Zaid agricultural season runs from April to June, but is characterised by much lower cultivation due to its hot and dry conditions. A brief introduction to Indian agricultural and its sensitivity to climate change can be found here: https://ccafs.cgiar.org/regions/south-asia/india.

[9] The LULC 1:250K data from Bhuvan is described by the Indian Space Research Organisation (2010) and here: https://bhuvan.nrsc.gov.in/bhuvan_links.php

census (SECC) is used for additional validation and mechanism results. We also use measures of nightlight emissions at the village level to validate our electrification measures. All of these variables are taken from the SHRUG database or, if not available in SHRUG, from the replication data of Asher and Novosad (2020) or from our own copy of the 2001 Census. To validate our dependent variables and get information on crop sales we use the IHDS data. To validate our mobile program implimentation we use data from the Mobile Coverage Explorer.[10] Finally, we conduct additional analysis at the sub-district level using scraped data from the Agriculture Census (see Section A of the Online Appendix).

2 Empirical strategy and results

2.1 Empirical strategy

To estimate complementarities in the programs, we rely on the plausible exogeneity of their timing.[11] While each of the programs we study had rules that linked eligibility to population thresholds, exploiting these thresholds would not be well suited to our analysis since adherence to the rules was low and the overlap between the windows around eligibility cut-offs was small. By using variation in implementation rather than eligibility, we have enough statistical power to estimate complementarities between each pair of infrastructure programs. The overlap between all three programs is, however, still too small for meaningful inference about potential three-way interactions.

Our general estimating equation for identifying the impacts of two infrastructure types P and Q is:

[10]This data source is described here: https://www.collinsbartholomew.com/mobile-coverage-maps/mobile-coverage-explorer/

[11]A similar approach is used to estimate the impacts of the PMGSY program on education and deforestation in Adukia et al. (2020) and Asher et al. (2020).

$$
\begin{aligned}
y_{i,s,t} = & \sum_{\tau \geq -5, \tau \neq -1} \zeta_\tau (\mathbb{1}(t = t^P_{i,s} + \tau)) + \\
& \sum_{\kappa \geq -5, \kappa \neq -1} \zeta_\kappa (\mathbb{1}(t = t^Q_{i,s} + \kappa)) + \\
& \sum_{\delta \geq -5, \delta \neq -1} \zeta_\delta (\mathbb{1}(t = t^{P \times Q}_{i,s} + \delta)) + \gamma_{s,t} + \eta_i + \epsilon_{i,s,t}
\end{aligned} \tag{1}
$$

In this event study style equation, the term $t^P_{i,s}$ is the time at which village i in state s first got access to infrastructure P. The outcome $y_{i,s,t}$ is our measure of dry season cropping at time t. In addition to village fixed effects, our main specification includes state-specific year fixed effects, to control for the large differences in agricultural practices between Indian states. We cluster standard errors at the sub-district level to account for spatial correlation between locations in the same administrative unit.

As highlighted by Borusyak and Jaravel (2018), we cannot identify linear pretrends in this event study design because of the staggered nature of our treatment, but we can identify a trend break at the time of treatment. We estimate dynamic effects and plot these in the figures. We also calculate the corresponding average treatment effect by taking a weighed average of the post-treatment coefficients, with weights given by the share of observations in a given time-to-treatment group. Our reference points are the year just prior to treatment and all periods more than five years before treatment.[12]

The identification assumption in this design is that the villages in each treatment group would have followed parallel trends in the absence of treatment. We think the context supports this assumption: village-level implementation was the result of a multitude of local conditions and higher-level administrative factors which are unlikely to have been correlated with sudden (future) improvements in agricultural

[12]For consistency, we use equation 1 as our main specification for all event study graphs and tables. In Figure A5 and Table A10, we show the main results for a semi-dynamic model, which is more efficient in the absence of pre-trends (Borusyak and Jaravel, 2018). In Figure A6, we adapt the method and program proposed by de Chaisemartin and D'Haultfœuille (2020). Their method accounts for heterogenous treatment effects, but it does not allow for the estimation of interacted treatments. These alternative estimation strategies yield qualitatively similar results.

productivity.[13] Our strongest evidence in support of this assumption will come from the absence of pre-trends and the immediate impact of the programmes.

When considering mobile tower construction, an important limitation of the event study design is that most towers were built around the same time (in 2008 or 2009). We therefore use an additional source of identifying variation, following Gupta et al. (2020), by including villages that were supposed to be covered by a tower (i.e., within 5 kilometers of the proposed tower location) but in the end were not. In other words, within the population of villages which were proposed to be covered by the program, we compare villages that were actually covered to those that were not. We then use the construction date of the actual tower to estimate time-to-treatment dummies.[14] We include provider-by-year fixed effects to account flexibly for time patterns that are specific to the broader areas that are covered by a particular provider.[15] To estimate complementarities of mobile connectivity with (i) PMGSY and (ii) RGGVY, the sample is restricted respectively to: (i) villages covered by proposed towers and by PMGSY, and (ii) villages covered by proposed towers and by RGGVY. As before, our identification strategy relies on a common trend assumption. It is unlikely that tower locations were changed in a way that correlates to the agricultural potential of villages (Gupta et al., 2020), and our event-studies will offer support for this assumption.

[13]For PMGSY alone, our design is identical to the one in Adukia et al. (2020). These authors find that, apart from population size, village characteristics had low explanatory power for the implementation time. In Table A8, we present robustness checks in which we control explicitly for time-varying effects of village-level characteristics.

[14]For the mobile phone coverage results, we cluster standard errors two-way at the sub-district and proposed tower level, to account for correlation between locations covered by the same tower. In terms of the estimating equation, villages that are covered by a proposed tower but not by an actual tower, are treated as having a time to treatment more negative than -5, so that they enter the control group. A very small number of towers were completed in 2011 and 2012. Villages near these towers are not included in our sample since we are unsure what caused these abnormal delays in completion.

[15]As described in section 1.3, providers bid for larger areas and have influence over the final tower locations. The set of providers includes both private firms and India's public telecoms provider, and the bidding and location strategies could differ by provider. As program implementation and the characteristics of coverage areas are likely to differ by provider, the fixed effects ensure that such differences are not driving our findings.

2.2 Results

Figure 2 shows event study results for our main outcome of interest, the index of dry season cropping. Panel A shows strong evidence of complementarities between road connectivity and electrification. The share of land under dry season cropping jumps by up to 0.1 standard deviations in the five years following the joint provision of road access and electricity supply. Panels B and C suggest that the separate or joint provision of mobile phone connectivity did not spur dry season cropping. The event studies also lend support to our main identification assumption: the pre-trends for joint or separate provision are flat for all types of infrastructure in all samples.[16]

Table 1 shows the average impact of the separate and joint provision of the infrastructure programs, which is calculated as the sum of the event study coefficients weighted by the size of each post-treatment group. For our preferred outcome of dry season cropping, the inverse covariance weighted index, the average effect of joint provision (in addition to the effects of separate provision) is 0.07 standard deviations. For the constituent measures, which are in log points, coefficients correspond to an increase in dry season cropping of between 6% and 12%. There is no consistent evidence of a similar impact on dry season cropping from the separate provision of infrastructure, or the joint provision of mobile phone coverage with either road access or electricity supply.[17] We also do not find a significant interactions with mobile phone coverage when we use a sample that is similar to the one of Panel A.[18]

[16]For each of the infrastructures and their interactions in each of the samples, we undertake an F-test that the pre-construction coefficients are all equal to zero. From these tests, we obtain a p-value greater than 0.1 for each set of terms except those on "electricity and mobile" in Panel B. Indeed, examining this panel we can see that both the -2 and -3 terms are significantly different from zero. Since the signs are different, however, we believe these are more likely to be the result of chance than evidence of a threat to our identification of the post-treatment impacts.

[17]There is a weakly negative effect of the interaction of road and mobile phone coverage (column 4 of Table 1, Panel C). This effect should not be over-interpreted, because (i) it is not consistent across dry season cropping measures and only marginally significant for our preferred outcome in column (4); (ii) it is driven by time periods that are far away from the treatment (see Panel C of Figure 1); and (iii) it does not survive robustness checks (e.g. Table A6 or Table A10).

[18]In Table A6, we estimate the impacts of the mobile phone program and its interactions with the other two programs while we weight observations by the number of villages in the district in the roads plus electricity sample. The positive and significant effects of the separate provision of electricity (column 1) and roads (column 2) are consistent with these effects including any complementarities between the programs and existing roads/electricity.

2.3 Robustness

In the online appendix we present results from several important robustness checks. One potential concern is that the estimated complementarity between roads and electricity is picking up heterogeneous effects of one of the programs along a dimension that is correlated with the other program's provision. An obvious such dimension is time, since villages are more likely to have receive both programs in later years. To control for this, an extended version of our model allows for the effects of both programs to have variable effects depending on the year (in addition to how long the village has had the program). Results are reported in column (1) of Table A8 - coefficients are similar to the baseline specification. This is also the case when we allow for heterogeneous effects by state - column (2) of Table A8 - or a range of variables from the 2001 Census (Figure A4).

A similar concern is that our results may be biased if program timing is correlated with an omitted variable that causes differential trends in dry season cropping. To mitigate this concern, in column (4) of Table A8 we include year-effects of a large set of control variables. Coefficients remain stable. One particular omitted variable we may worry about is conflict, since both project completion and dry season cropping may be boosted by reductions in violent conflict. However, the results are similar if we restrict to a subsample of districts that are unaffected by conflict (column 6 of Table A8).[19] One could similarly be concerned that actual project completion responds to unexpected shocks that also drive sudden shifts in dry-season cropping. In column (5) we therefore use the planned completion date of the road (as agreed in the construction contract) instead of the actual completion date. The complementarity result goes through in this specification, suggesting that deviations from the planned schedule are not driving our effect. Finally, to remove the possibility of any district-level omitted variables, in column (7) we include district-by-year fixed effects (instead of state-by-year fixed effects). This is a demanding specification given that a large amount of variation in program timing is at the district level, but we still find a significant positive coefficient on the complementarity result.

[19] For this check, we exclude Jammu and Kashmir, all North-Eastern states, and all Maoist-affected districts (from Ghatak and Vanden Eynde, 2017).

A range of further tests show that the complementarity result is robust to other changes in specification. We already allow for some spatial correlation by clustering our standard-errors at the sub-district level, but in columns (7) and (8) we show that standard errors are similar when clustering at the district level, or with Conley standard errors (Conley, 1999). Table A7 then investigates the sensitivity of the complementarity between roads and electricity access to the logarithmic transformation we use for our dry season cropping measures. While the coefficients are estimated imprecisely for certain outcomes, the coefficient remains large and significant for an index of the level measures (column 4). Finally, we investigate whether the result is driven by a particular ordering of the two programs by splitting our sample in three according to program ordering. Table A9 then shows that complementarities between road access and electrification exist in all three of these subsamples.

One potential issue in interpreting the results is that differences in program impacts may stem from differences in how effective the programs were in providing infrastructure rather than differential impacts of infrastructure provision.[20] In the online appendix we therefore validate that the infrastructure programs did consistently lead to substantive increases in infrastructure provision. We validate the electrification treatment in Figure A3 using the event study strategy on night light data. In both samples, electrification is followed by a sharp increase in night light emissions. Importantly, however, there is no evidence of interaction effects - i.e. the combined programs do not increase nightlights beyond the sum of the two direct impacts. In columns (1) and (2) of Table A11 we show that villages that received roads before 2011 according to the administrative completion measure are more likely to report a road connection in the 2011 census. The probability of having a road in the census is not higher for villages that are both electrified and covered by the PMGSY scheme.[21] In

[20] On roads, for instance, Lehne et al. (2018) show that PMGSY implementation suffered from political corruption affecting program completion. On electricity, the supply of electricity to rural communities is frequently rationed, even when there is a physical network (Burgess et al., 2020; Ryan and Sudarshan, 2020).

[21] There is a positive relationship between electrification and road connectivity as measured by the census, and a negative relationship with the interaction of electricity and roads. These coefficients are significant but small in size compared to the coefficient on road access. Further investigation reveals that the effects of electrification disappears when we allow for the roads program to have heterogeneous effects by state (results not reported), which our main results are robust to (column 1 of Table A8). Moreover, the negative effect on the interaction term would work against the positive effects

columns (3) and (4) we show that villages coded as having received mobile coverage from the USOF program by 2011 are more likely to report coverage in 2012 based on the Mobile Coverage Explorer data.

2.4 Discussion

The combination of the electricity and roads programs has a large impact on dry season cropping. In this subsection, we analyze whether there are impacts of this combination of programs on other related outcomes. Unfortunately, few outcomes are measured annually at the village-level, which prevents us from using a pure event study approach. We can take advantage, however, of two censuses - the 2011 Census of India and the Socio-Economic Caste Census. Since these censuses were undertaken in 2011/2012, when some (but not all) of our villages had received the infrastructure programs, we can compare outcomes in villages that happen to have received the program before 2011 to those that received them afterwards.

In Table 2 we look at a range of outcomes using this cross-sectional approach. To be consistent with the event study, we keep the same sample of villages and use only state fixed effects as controls. Column (1) confirms this approach give similar results to our event study — the joint provision of electricity and road access is associated with an increase in our dry season cropping index (relative to their isolated provision).

Roads could encourage dry season cropping because lower transport costs increase the profitability of selling crops at market. We lack data on the proportion of a village's agricultural output that is sold on the market, but we can use the IHDS to identify categories of crops which are both grown in the dry season and predominantly sold rather than directly consumed. Three types of crops fit these criteria - fruits, vegetables, and spices (see Table A12 for the share of each crop category produced and sold in the rabi season). In column (2) of Table 2 we then indeed find a significant positive correlation between the interaction of electricity and roads and the reporting of growing crops in these categories.

In columns (3)-(5), we focus on important outcomes studied by Asher and Novosad (2020), who analyze the impact of road access through PMGSY. Our results for PMGSY

we find on dry season cropping.

on its own are consistent with theirs - in column (3) road access is associated with a reduction in the share of households whose main source of income is cultivation, but there is no significant association of road access on its own with asset holdings or the poverty rate (columns 4 and 5).[22] Similarly, there is no significant association between assets and electrification on its own, which is consistent with Burlig and Preonas (2016). We do, however, find that the *joint* provision of electrification and road access is associated with higher asset levels and lower poverty. Combined with our earlier findings, the positive coefficient on the interaction term suggests that complementarities boosted incomes through higher agricultural productivity.

In the appendix, we show the equivalent tables for the provision of mobile coverage and electrification (Table A14), and the provision of mobile coverage and road access (Table A15). Again, there is no evidence of complementarities with mobile phone coverage.

Our results for Census outcomes should be interpreted with some caution as they are based on a cross-sectional comparison of villages. In Table A13 of the online appendix we show that the results of Table 2 are generally robust to controlling for a long list of village-level characteristics.[23] We also use four waves of the agricultural census to analyze impacts of the electrification program at the sub-district level. The results support our cross-sectional analysis by suggesting that electrification increased the share of land dedicated to fruits, vegetables, and spices (see section A of the online appendix and, in particular, Table A1).

The previous paragraphs rationalise the complementarities for roads and electrification, but ex ante we could have expected similar impacts from the combined provision of mobile phone coverage and one of the other programs. The null results for mobile phone coverage may be in part because the impact of the public program we study was short-lived, as private cell phone networks developed quickly during our period of study.[24] Alternatively, mobile coverage may not be as important in the

[22] The asset index comes from Asher and Novosad (2020). The poverty rate comes from SHRUG and is estimated using the same asset base in the SECC.

[23] We use post-double selection lasso to determine which control variables enter the model.

[24] Table A11 supports this interpretation: the estimated effect of USOF coverage on measured mobile phone coverage in 2012 is less than a third of the effect of PMGSY coverage on road access in the 2011 census. However, the event studies of Figure 2 show no impact even in the immediate aftermath of

decision on whether to crop during the dry season, even if it may encourage other agricultural investments.

Our results suggest that the joint provision of roads and electricity provides real benefits for farmers in terms of their agricultural productivity. Our paper does not, however, provide a full welfare analysis. A key concern in our context is that dry season cropping could go hand-in-hand with fires or ground-water depletion.[25] In the online appendix Table A17, we show, however, that the joint provision of electricity and roads does not appear to significantly increase fires. Our result on complementarities also holds for villages that are characterised by low ground water depletion, and for villages that have access to non-well irrigation. These results suggest that the observed increase in dry season cropping does not necessarily come at the expense of sustainability, even if a full analysis of the welfare impacts is beyond the scope of this paper.

3 Conclusion

We find strong evidence of complementarities between electricity provision and road access: dry season cropping increases when villages receive both a new road and electrification. It does not increase when villages receive either piece of infrastructure alone. This complementary effect is consistent with the hypothesis that dry season agricultural production only becomes profitable when improved water management is combined with better market access. In line with this idea, joint provision of electricity and roads is associated with a shift of cropping patterns towards market crops and an improvement in living conditions.

Of course, the impacts we document and their underlying mechanisms could be specific to Indian agriculture as well as the types of infrastructure we focus on. In this sense, there is ample scope for future research to examine complementarities in alternative settings. An important challenge of such work will be to establish plausibly causal identification of complementarities. Our paper leverages the unparalleled

tower construction.

[25] Blakeslee et al. (2020) document the growing water scarcity in India and the adaptation strategies of farmers.

scale of India's rural development efforts. Credible identification of complementarities in other settings may require strictly implemented eligibility rules or deliberate experimenting by policy-makers. In spite of these challenges, the careful study of complementarities could have large returns. Infrastructure development is central to the development agenda of any low and middle-income country. Our results suggest that, when governments plan the implementation of major infrastructure projects, the simultaneous provision of different types of infrastructure could be essential to generate tangible economic benefits.

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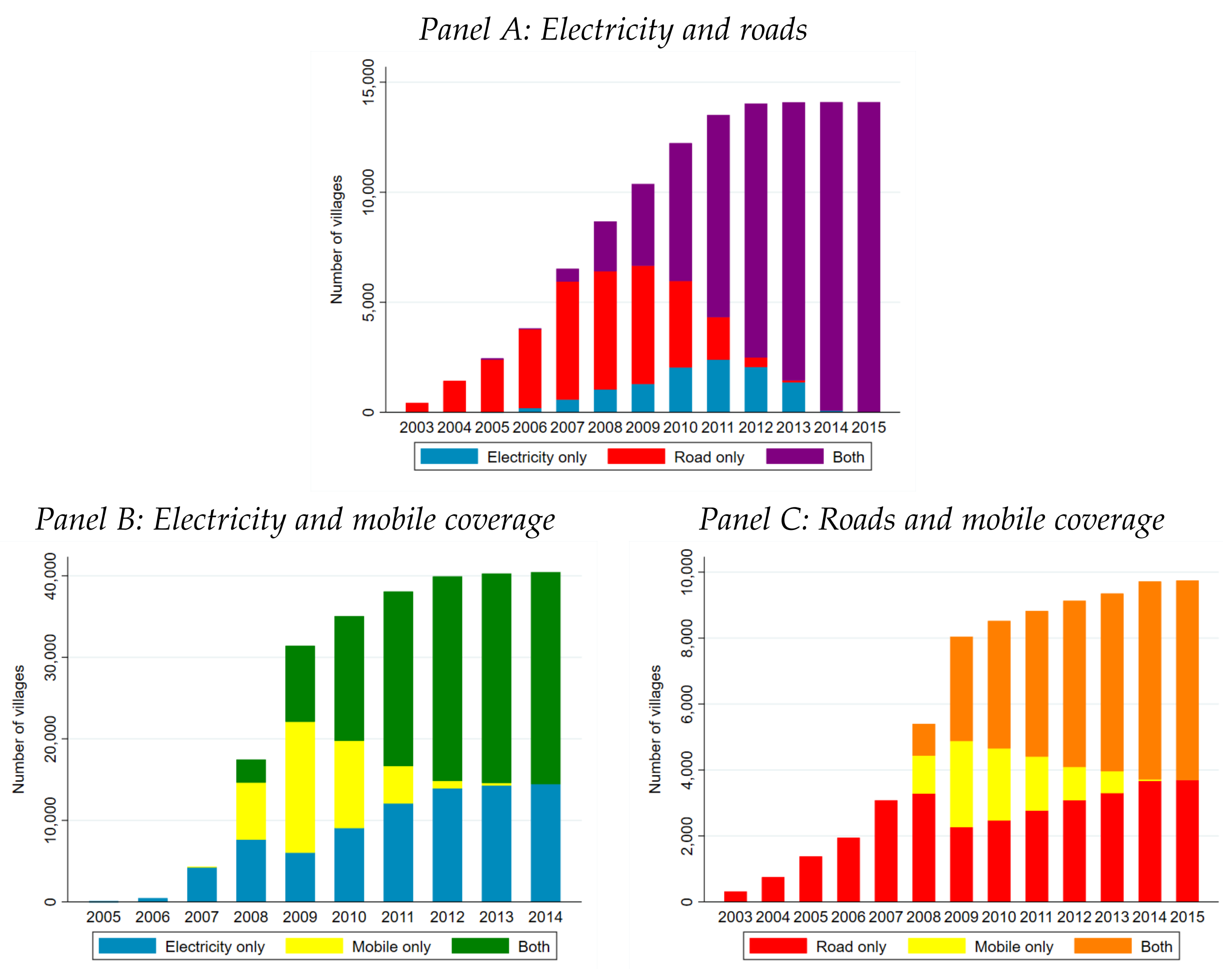
Tables and Figures for Main Text

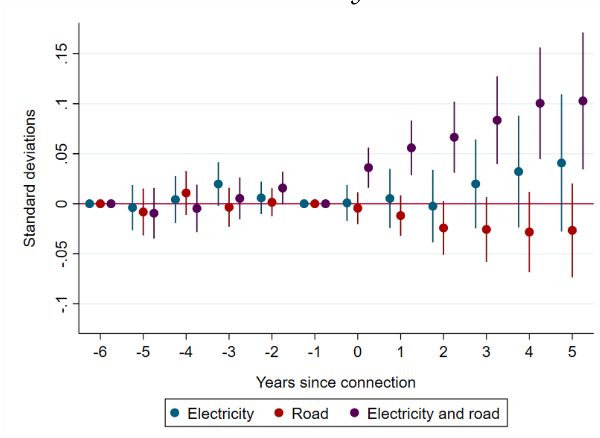
Figure 1: Timing of programs in each sample



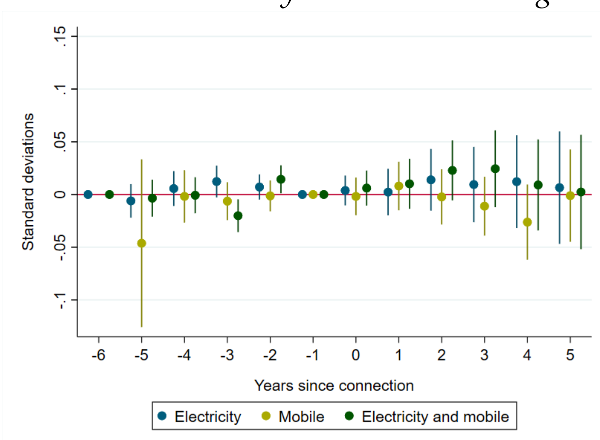
Notes: The figures show, for each year, the number of villages which have received one or both of the two relevant programs in each of the three samples we use in the analysis. For electricity and roads, we restrict to villages that receive the program by 2015; for mobile phone coverage, we restrict to villages that were proposed to be covered by the program.

Figure 2: Impact of infrastructure programs on dry season cropping

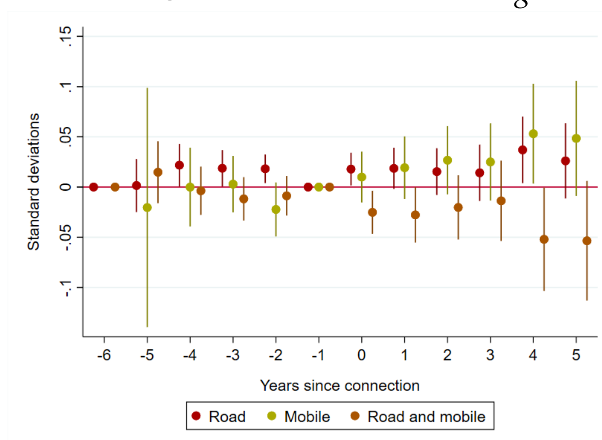
Panel A: Electricity and roads



Panel B: Electricity and mobile coverage



Panel C: Roads and mobile coverage



Notes: This figure displays event study coefficients resulting from estimating equation 1. The joint coefficients (e.g. on 'Electricity and road') should be interpreted as the *additional* impact of having both programs above the estimated individual impacts of each program. The outcome is the (standardized) inverse-covariance weighted index of dry-season cropping measures, which is constructed as described in the text. Details of the infrastructure programs are provided in the main text. The sample in Panel A includes villages that were covered by the road and electrification programs; in Panel B by a proposed tower and the electrification program; and in Panel C by a proposed tower and the roads program. The treatment time is the completion date of a specific infrastructure program. The graphs plot time-to-treatment coefficients and their 95% confidence intervals. The model includes village fixed effects, state-by-year fixed effects, and (in panels B and C only) provider-by-year fixed effects. Standard errors are clustered at the sub-district level in panel A and (two-way) at the sub-district and proposed tower level in panels B and C.

Table 1: Impacts of infrastructure programs on dry season cropping

	Log of 1 + percentage land cropped during dry season (ISRO) (1)	(Jain et al.) (2)	Log of Δ NDVI in dry season (Asher & Novosad) (3)	Dry season cropping index (4)
Panel A: Electricity and roads				
Electricity	-0.026	0.022	0.11*	0.015
	(0.038)	(0.033)	(0.066)	(0.019)
Road	-0.036	-0.001	-0.082	-0.025
	(0.039)	(0.029)	(0.075)	(0.018)
Elec and road	0.1***	0.058**	0.12*	0.067***
	(0.036)	(0.029)	(0.068)	(0.017)
Observations	140590	111530	137690	140910
Sub-districts	1364	1236	1339	1365
Mean of dep. var.	2.49	1.01	2.58	0
Panel B: Electricity and mobile phones				
Electricity	-0.009	0.05**	-0.016	0.009
	(0.037)	(0.024)	(0.053)	(0.015)
Mobile	0.016	-0.009	0.014	-0.004
	(0.031)	(0.019)	(0.052)	(0.013)
Elec and mobile	0.002	0.030	0.004	0.014
	(0.031)	(0.021)	(0.053)	(0.014)
Observations	403540	320990	398950	404460
Sub-districts	6033	5494	5949	6037
Mean of dep. var.	2.62	1.18	2.68	0
Panel C: Roads and mobile phones				
Road	0.032	0.053*	0.066	0.023
	(0.025)	(0.028)	(0.061)	(0.014)
Mobile	0.021	0.030	0.16**	0.028
	(0.032)	(0.035)	(0.075)	(0.018)
Road and mobile	-0.004	-0.044	-0.18**	-0.03*
	(0.029)	(0.033)	(0.072)	(0.016)
Observations	97220	72570	95900	97460
Sub-districts	3855	3083	3786	3857
Mean of dep. var.	2.81	1.62	3.49	0

Notes: Coefficients are an average of post-treatment coefficients in equation 1, weighted by the number of observations in each treatment bin. The joint coefficients (e.g. 'Elec and road') should be interpreted as the *additional* impact of having both programs above the estimated individual impacts of each program. *Combined* effects of the three programs are given in Table A5. The sample in Panel A includes villages that were covered by both electricity and road programs; in Panel B by a proposed tower and the electrification program; and in Panel C by a proposed tower and the roads program. The dry season cropping index is the (standardized) inverse correlation weighted index of the measures of dry season cropping presented in the first three columns. The model includes village fixed effects, state-by-year fixed effects, and (in panels B and C only) provider-by-year fixed effects. Standard errors are clustered at the sub-district level in panel A and (two-way) at the sub-district and proposed tower level in panels B and C. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2: Census outcomes - Electricity and roads

	Δ Dry season cropping index (1)	Reports growing fruit, vegetable, or spice (Census) (2)	Share of households main inc. source is cultivation (SECC) (3)	Asset Index (SECC) (4)	Poverty rate (SECC) (5)
Electricity	-.0051	-.0018	-.016	.022	-.01
	(.022)	(.014)	(.014)	(.029)	(.0099)
Road	-.018	-.017	-.022*	.017	-.012
	(.019)	(.012)	(.013)	(.027)	(.0075)
Elec and road	.047*	.027*	-.021	.11**	-.02*
	(.027)	(.014)	(.017)	(.044)	(.011)
R^2	.1	.097	.13	.11	.094
Observations	13972	8353	13893	10585	13956
Sub-districts	1359	1073	1334	1189	1355
Dep. var. mean	.21	.083	.4	-.22	.45

Notes: Regressions at the village level. Results in the 'Electricity' and 'Road' rows are the coefficients on dummy variables indicating whether the village has received the program by the beginning of 2011 - the 'Elec and road' row reports the coefficient on the interaction of these two terms. The sums of these three sets of coefficients can be found in Table A16. The outcome in column (1) is the change between 2005 and the average between 2010 and 2012 of the standardized inverse covariance weighted index of dry-season cropping. The outcomes in columns (2)-(5) are based on the 2011 Indian Population Census or the 2011 Socio-economic caste census (SECC). The sample only includes villages that were covered by both programs. The model includes state fixed effects. Standard errors are clustered at the sub-district level; they are bootstrapped in column (5) to account for the construction of the poverty measure; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix to “Complementarities in Infrastructure: Evidence from Rural India”

For Online Publication

May 12, 2021

A Results based on the Agricultural Census

We complement our analysis by using the Indian Agricultural Census at the sub-district level.[1] We combine data from 4 waves (1995-96, 2000-01, 2005-06, and 2010-11) to construct a panel of agricultural outcomes. The agricultural census provides more precise information on cropping patterns than the population census: the former measures the areas cropped, whereas the latter just lists the three main crops. Moreover, the agricultural census aims to provide consistent data over time going back to 1995, whereas the population census only provides data on crops grown in 2011.

One important limitation of the agricultural census, however, is that it does not provide information at the village level; the lowest administrative unit available is the sub-district. This means we adapt our identification strategy in the following way. To estimate the impact of the electrification program, we continue to exploit variation in the timing at which villages were electrified by regressing outcomes on the share of villages in the sub-district which were electrified between 2006 and 2010 while controlling for the share of villages that were electrified at any point in time. In this way, we are again only using variation in the timing of the program's roll-out within a sub-district, and not the ultimate share of the sub-district which was covered by the program. In other words, we estimate the following equation:

[1] These data were downloaded from http://agcensus.nic.in/ between January 2019 and December 2020.

$$y_{i,s,t} = \sum_{\substack{\tau \in \\ 1995, \\ 2000, \\ 2010}} \mathbb{1}(t = \tau) \left(\beta_\tau s_{i,s}^{2006-2010} + \lambda_\tau s_{i,s}^{\text{ALL}} \right) + \gamma_{s,t} + \eta_i + \epsilon_{i,s,t} \tag{2}$$

Here $y_{i,s,t}$ is the outcome variable in subdistrict i of state s in year t, $s_{i,s}^{\text{ALL}}$ is the share of villages in the subdistrict covered by the program, and $s_{i,s}^{2006-2010}$ is the share of villages in the subdistrict which were covered by the program between the beginning of 2006 and the end of 2010. Since we are including subdistrict fixed effects, we are essentially doing a diff-in-diff, looking at the change of the outcome between 2005 and the reference year. In this sense, we should interpret the coefficient β_{2010} as the impact of village electrification, and the coefficients β_{1995} and β_{2000} as placebos which test for pre-trends.

While we therefore preserve the essence of the identification strategy for the impact of electrification, we cannot use the same strategy to identify the impact of the roads program at the subdistrict level. This is because in the large majority of subdistricts, only a small share of villages were included in the roads program, meaning that we would lack the power to detect relevant impacts. To assess evidence of complementarities between electricity and roads, we therefore split the sample between sub-districts that have a share of road connections above or below the median in 2005, based on the PMGSY program and roads reported in the 2001 census.

The results of this exercise are reported in Table A1. In spite of the different level of analysis and empirical approach, it confirms our main findings. First, to test for the consistency of this empirical strategy, in columns (1) and (2) we show the effect of electrification on our main index of dry-season cropping at the sub-district level.[2] Electrification boosts dry-season cropping, but only if the share of villages with road access is sufficiently high. In columns (3) and (4), we show that electrification boosts irrigation (as measured in the agricultural census). Finally, in columns (5) and (6) we also see an increase in the share of land used for fruits, vegetables, and spices. As

[2]Our remote sensing measures are not available for the two earliest agricultural census years (1995-96 and 2000-01), and therefore for this outcome we cannot estimate the placebo coefficients β_{1995} and β_{2000}.

shown in Table A12, these are the crop categories for which most of the production takes place in the rabi season and is sold rather than directly consumed.

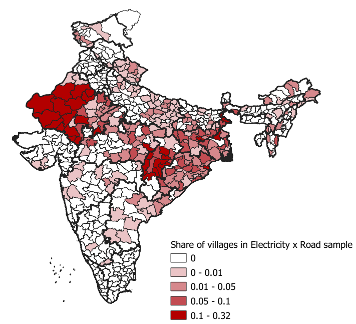
Table A1: Agricultural census outcomes

	Dry season cropping index		Log of 1+ percent of land irrigated		Log of 1+ percent of land fruits, spices, or vegetables	
	Below median roads	Above median roads	Below median roads	Above median roads	Below median roads	Above median roads
	(1)	(2)	(3)	(4)	(5)	(6)
Share electrified $\times$ Year = 2010	-.053	.17***	.1	.086*	.088	.22***
	(.047)	(.032)	(.091)	(.045)	(.093)	(.064)
Share electrified $\times$ Year = 2005	.	.	.	.	.	.
Share electrified $\times$ Year = 2000			-.12	.042	-.027	.074
			(.078)	(.053)	(.098)	(.066)
Share electrified $\times$ Year = 1995			-.047	-.13	-.044	.066
			(.14)	(.1)	(.14)	(.13)
P-val, 2010 coefs equal	.000091		.89		.25	
Observations	3338	3474	5450	5419	5450	5419
Sub-districts	1669	1737	1669	1737	1669	1737
Dep. var. mean	-.12	.11	2.9	3.5	1.6	1.7

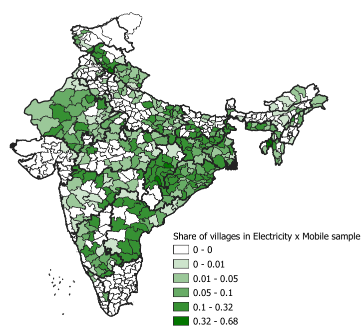
Notes: Regressions at the sub-district level. The outcome in columns (1) and (2) is the (standardized) inverse correlation weighted dry-season cropping index based on remote sensing sources. The outcomes in columns (3)-(6) are based on the agricultural census. Coefficients for the 2005 year are omitted since we include sub-district fixed effects. The first row after the coefficients reports the p-value of the test that the relevant coefficients in the first row are equal - i.e. whether there is a significant difference in these results between the below-median roads sample and the above-median roads sample. State-year fixed effects are also included in all columns. Standard errors are clustered at the sub-district level; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B Additional Figures

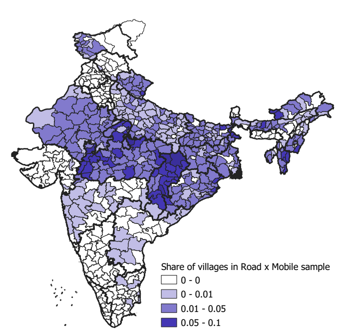
Figure A1: Sample - share of villages per district



Panel A: Electricity and Roads



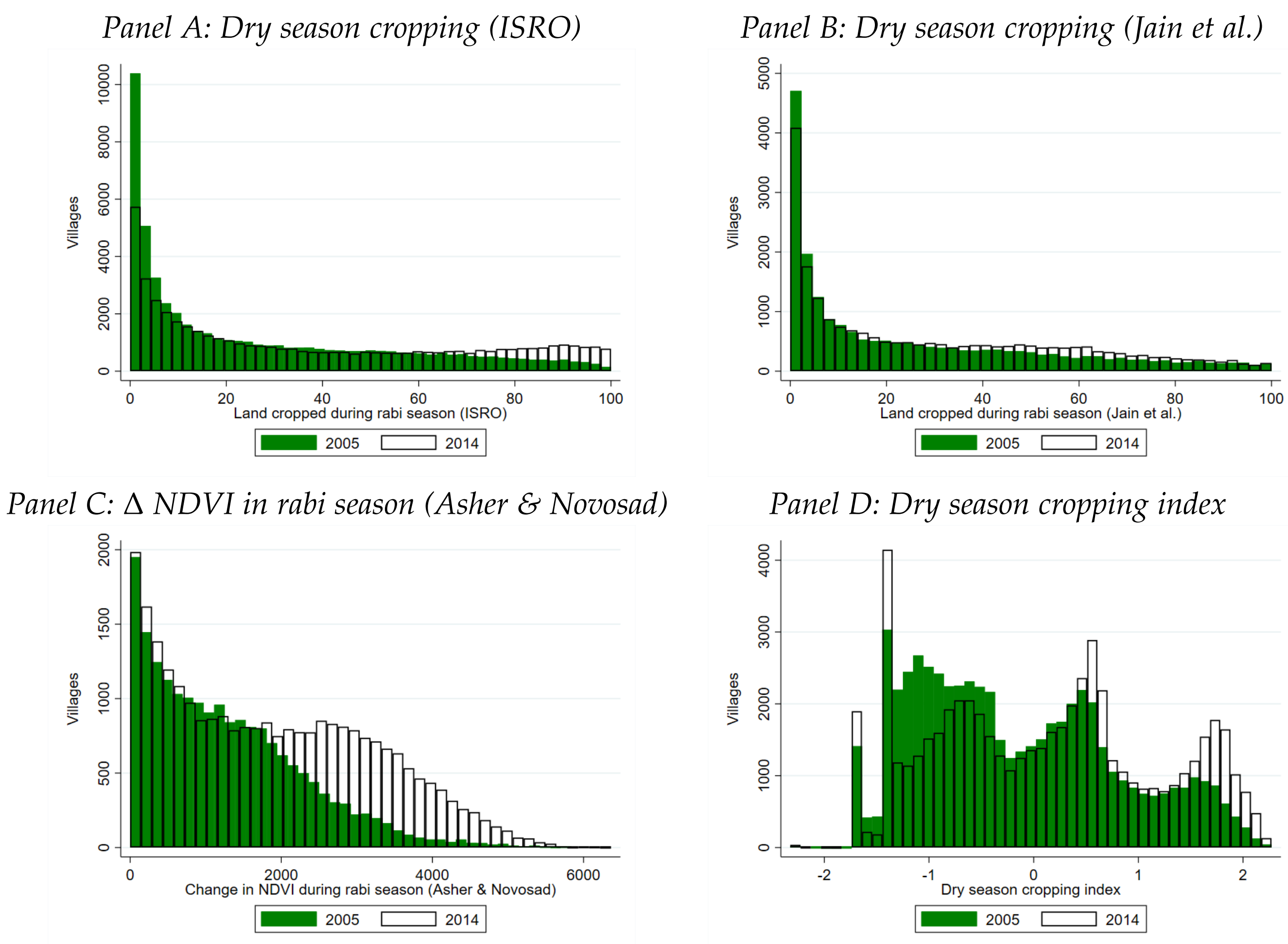
Panel B: Electricity and Mobile



Panel C: Roads and Mobile

Notes: The map shows the share of villages, out of the total number of villages in a district, that are included in each of our three event study samples.

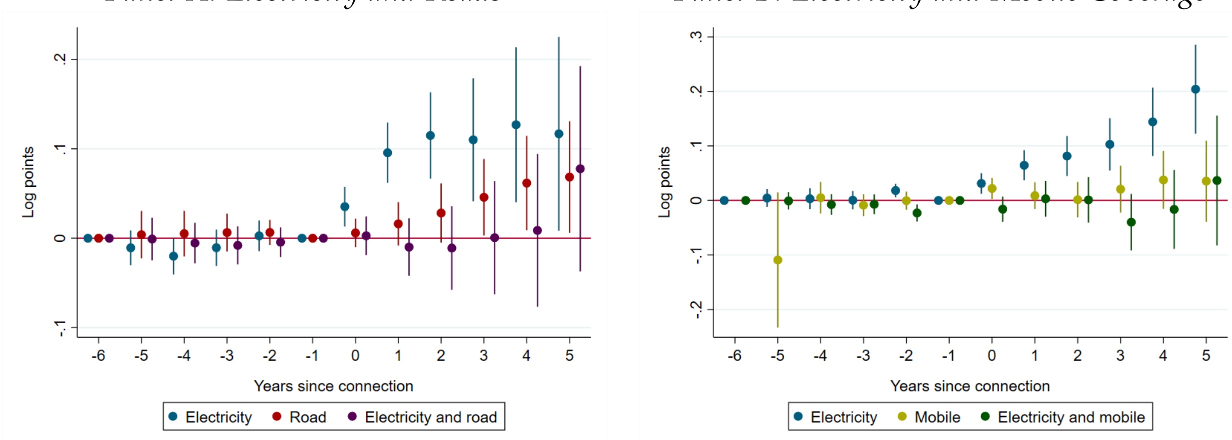
Figure A2: Distribution of raw dependent variables and index of logs



Notes: Panels A, B, and C show the distribution of the positive values of each of the three dry season cropping measures. Zeros are omitted to make the graphs easier to read (the number of villages with zero or negative values is reported in Table A3). The sample for these graphs is the union of the 3 samples for the pair-wise combinations of the programs. Panel D shows the distribution of the inverse covariance weighted index calculated on this union.

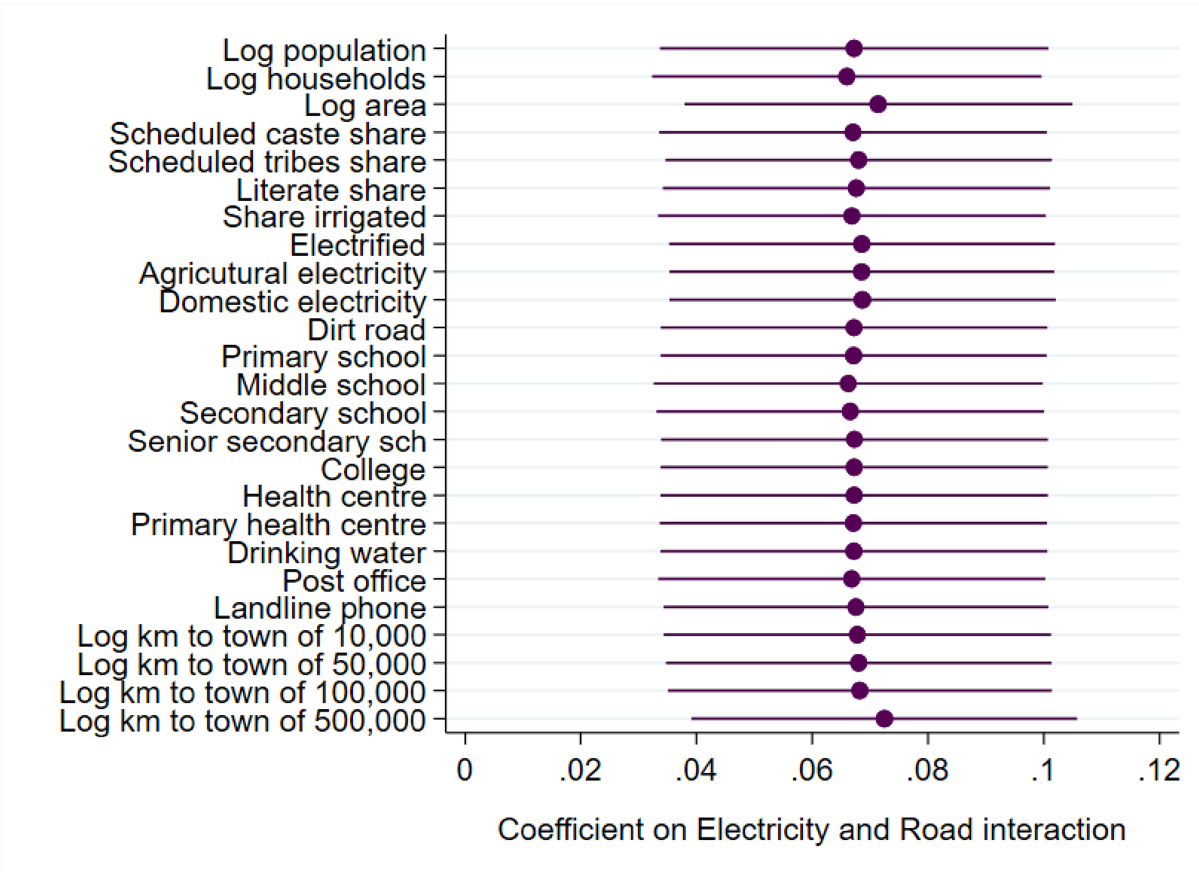
Figure A3: Impact of programs on night lights

Panel A: Electricity and Roads *Panel B: Electricity and Mobile Coverage*



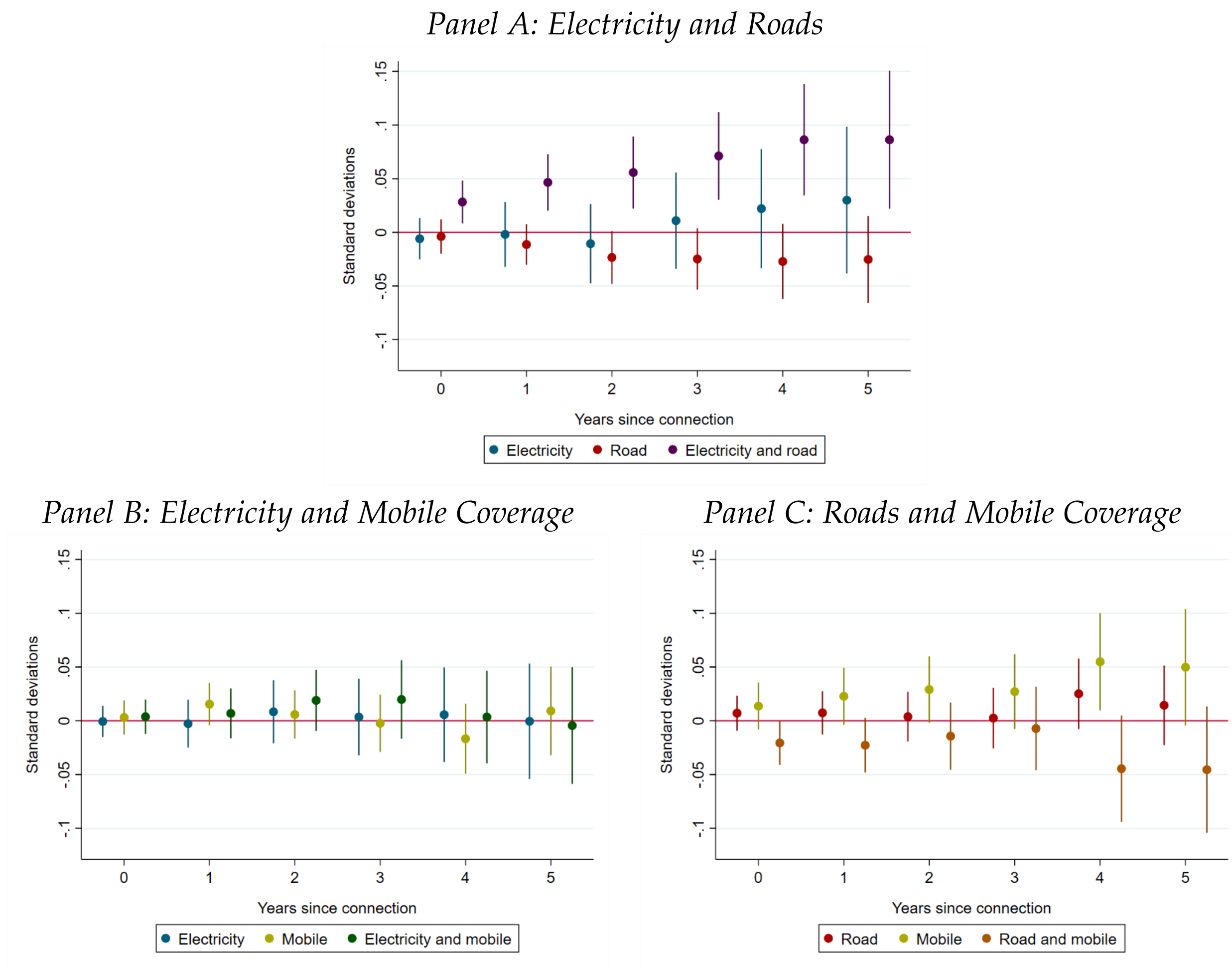
Notes: Village-year level panel data between 2005 and 2014. The outcome is the logarithm of night-light emissions. The sample includes villages that were covered by: both electricity and road programs in panel A; and by a proposed tower and the electrification program in panel B. The event study is estimated according to equation 1. The treatment time is the completion date of a specific infrastructure program at the village level. The graphs plot time-to-treatment coefficients and their 95% confidence intervals. The model includes village and state-by-year fixed effects in both panels, as well as provider-by-year fixed effects in Panel B. Standard errors are clustered at the sub-district level in Panel A and (two-way) at the sub-district and proposed tower level in Panel B.

Figure A4: Robustness to heterogeneity of infrastructure impacts by baseline characteristics



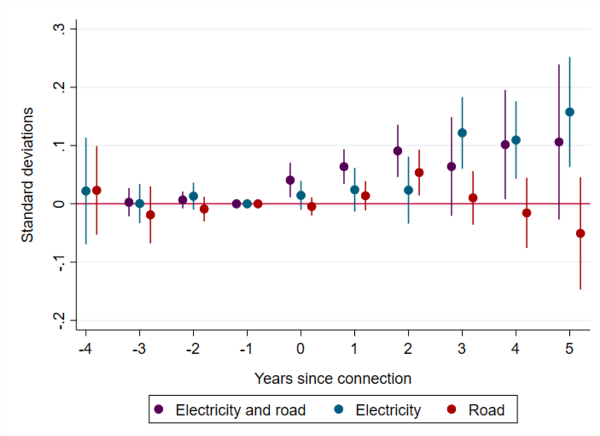
Notes: Each point in this figure displays the average of post-treatment coefficients in equation 1 weighted by the number of observations in each treatment bin when the dependent variable is the (standardized) inverse-correlation weighted index of dry-season cropping, as in column (4) of Panel A of Table 1. Each point comes from a separate regression, which adds to the baseline regression two interaction terms involving the relevant control variable: one interacted with a dummy for whether the village has received the electricity program, and one for whether it has received the roads program. In this way, we allow for heterogeneous impacts of the program according to these baseline characteristics. 95% confidence intervals are included.

Figure A5: Impact of infrastructure programs on dry season cropping - semi-dynamic model



Notes: The event studies displayed here are identical to those in Figure 2, except that pre-treatment coefficients are not estimated. See notes to Figure 2 for more details.

Figure A6: Estimates using Chaisemartin and d'Hautefeuille estimation approach



Notes: This figure presents results using the Stata command provided by Chaisemartin and d'Hautefeuille (2020). Since their estimation strategy is not designed to estimate the interaction of multiple treatments, the electricity coefficients are estimated amongst the set of observations where villages do not have roads. Similarly, the road coefficients are estimated on the observations where villages do not yet have electricity. We then estimate the interacted effect on the full sample while controlling for dynamic impacts of electricity and roads. We allow for state-specific year effects and cluster standard errors at the sub-district level.

C Additional Tables

Table A2: Summary statistics

	Electricity & Roads N = 14091		Electricity & Mobile N = 40450		Roads & Mobile N = 9746	
	Mean (1)	S.d. (2)	Mean (3)	S.d. (4)	Mean (5)	S.d. (6)
Electrified	.49	.5	.61	.49	.54	.5
Dirt road	.92	.28	.82	.38	.94	.24
Tar road	0	0	.43	.49	0	0
Population	896	813	1015	4058	1030	929
Area (hectares)	448	627	378	656	393	529
Scheduled caste (share)	.16	.2	.19	.21	.15	.18
Literate (share)	.43	.16	.46	.17	.43	.16
Primary school	.86	.35	.74	.44	.87	.34
Received other program	.23	.42	.08	.27	.33	.47

Notes: Village level observations in the samples for each pair-wise combination of infrastructure programs. For the 'Electricity & Mobile' sample, four villages are not contributing to the estimation since all the villages in one sub-district have no variation in dry-season cropping; they are dropped from the reported regression output in Table 1, but are part of our sample.

Table A3: Observations of dependent variables

	Number of villages with observations	Number of villages with zero or negative values	
		in 2005	in 2014
	(1)	(2)	(3)
Land cropped during rabi season (ISRO)	57697	5210	8741
Land cropped during rabi season (Jain et al.)	45447	24763	23039
Change in NDVI during rabi season (Asher & Novosad)	56870	37715	30377

Notes: Column (1) reports the number of villages with observations for each of the three measures of dry season cropping. The sample consists of the union of the 3 samples for the pair-wise combinations of the programs.

Table A4: Validation of dependent variables using IHDS

	(1)	(2)	(3)	(4)	(5)
Log of land cropped during rabi season (ISRO)	.12***			.066***	
	(.02)			(.022)	
Log of land cropped during rabi season (Jain et al.)		.044***		.035***	
		(.014)		(.013)	
Log of Δ NDVI during rabi season (Asher & Novosad)			.051***	.036***	
			(.0066)	(.007)	
Dry season cropping index					.18***
					(.023)
Observations	617	465	595	618	618
Adjusted R^2	0.10	0.02	0.12	0.15	0.13

Notes: Observations are at the village level among villages in IHDS I which we are able to match to our remote sensing data. The dependent variable is the log of 1 + the percentage of land cultivated during the rabi season from IHDS. Observations are weighted by the number of villages in at least one of our samples in the state. In column (4), we replace missing values of the remote sensing measures with zeros and include dummy variables indicating whether each measure is missing. The dry season cropping index used in column 5 is the standardized inverse covariance weighted index calculated on this sample of villages.

Table A5: Combined effects of programs

	Log of 1 + percentage land cropped during dry season (ISRO)	(Jain et al.)	Log of Δ NDVI in dry season (Asher & Novosad)	Dry season cropping index
	(1)	(2)	(3)	(4)
Panel A: Electricity and roads				
Combined total effect	0.039	0.076*	0.15*	0.055**
	(.052)	(.04)	(.09)	(.024)
Observations	140590	111530	137690	140910
Sub-districts	1364	1236	1339	1365
Mean of dep. var.	2.49	1.01	2.58	0
Panel B: Electricity and mobile phones				
Combined total effect	0.009	0.066**	0.018	0.018
	(.043)	(.027)	(.063)	(.017)
Observations	403540	320990	398950	404460
Sub-districts	6033	5494	5949	6037
Mean of dep. var.	2.62	1.18	2.68	0
Panel C: Roads and mobile phones				
Combined total effect	0.050	0.041	0.064	0.023
	(.034)	(.037)	(.078)	(.019)
Observations	97220	72570	95900	97460
Sub-districts	3855	3083	3786	3857
Mean of dep. var.	2.81	1.62	3.49	0

Notes: This table reports results from the same regressions as those summarized in Table 1 - please see notes to that table for more details. In this table, we combine the underlying coefficients with appropriate weights to produce an estimated overall impact of receiving both programs in each of our samples. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A6: Mobile interactions in similar sample to electricity and road sample

	Electricity and mobile	Roads and mobile
Electricity	0.049**	
	(0.025)	
Road		0.044**
		(0.022)
Mobile	0.023	0.017
	(0.024)	(0.029)
Elec and mobile	-0.004	
	(0.024)	
Road and mobile		0.011
		(0.028)
Observations	327230	69880
Sub-districts	1649	1109
Mean of dep. var.	-.251	-.395

Notes: The dependent variable is the (standardized) inverse-correlation weighted index of dry season cropping. This table reports results similar to column (4) of Panels B and C of Table 1 (i.e. the model given in equation 1) only with observations weighted by the number of villages in the district in the roads and electricity sample to make the results more comparable to Panel A of Table 1. The sample in column (1) includes villages that were covered by a proposed mobile phone tower and the electrification program; and in column (2) by a proposed mobile phone tower and the roads program. The model includes village fixed effects, state-by-year fixed effects, and tower provider-by-year fixed effects. Standard errors are (two-way) clustered at the sub-district and proposed tower level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A7: Event study results with linear outcomes

	Percentage land cropped during dry season (ISRO)	Percentage land cropped during dry season (Jain et al.)	Δ NDVI in dry season (Asher & Novosad)	Dry season cropping index
	(1)	(2)	(3)	(4)
Electricity	0.820	0.270	66**	0.036*
	(0.6)	(0.52)	(30)	(0.019)
Road	-0.620	0.093	-17.000	-0.013
	(0.52)	(0.44)	(29)	(0.016)
Elec and road	1.7***	0.440	30.000	0.046***
	(0.55)	(0.45)	(30)	(0.018)
Observations	140590	111530	137690	140910
Sub-districts	1364	1236	1339	1365
Mean of dep. var.	26.9	10	96.2	0

Notes: Regressions at the village-year level (2005-2014). Details of the dry-season cropping measures and infrastructure programs are provided in the main text; this table uses these measures in levels instead of the log transformation. The dry season cropping index is the (standardized) inverse correlation weighted index of the linear measures of dry-season cropping presented in the first three columns. The sample includes villages that were covered by both programs. Coefficients are estimated as the sum of post-treatment coefficients in equation 1, weighted by the share of observations in each treatment bin. The model includes village fixed effects and state-by-year fixed effects. Standard errors are clustered at the sub-district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A8: Robustness

	R + E het by year (1)	R + E het by state (2)	Controls by year FEs (3)	Only districts without conflict (4)	Using planned road date (5)	District by year FEs (6)	District clustered standard errors (7)	Spatially clustered standard errors (8)
Electricity	0.032	0.025	0.012	0.013	0.030	-0.010	0.015	0.015
	(0.021)	(0.021)	(0.017)	(0.037)	(0.021)	(0.015)	(0.028)	(0.02)
Road	-0.017	-0.018	-0.027	-0.035	-0.001	-0.001	-0.025	-0.025*
	(0.011)	(0.026)	(0.017)	(0.03)	(0.02)	(0.014)	(0.02)	(0.015)
Elec and road	0.068***	0.068***	0.067***	0.06**	0.045**	0.027**	0.067***	0.067***
	(0.021)	(0.017)	(0.016)	(0.028)	(0.019)	(0.013)	(0.019)	(0.016)
Observations	140910	140910	140910	81470	133960	140720	140910	140910
Sub-districts	1365	1365	1365	642	1304	1346	242	N/A

Notes: This table provides a number of alternative specifications to the model for which results are presented in column (4) of Table 1, Panel A - model specification is as described in the notes to that table unless stated otherwise. The dependent variable is the (standardized) inverse correlation weighted index of dry season cropping. Column (1) includes the terms relating to electricity and road program timing interacted with year fixed effects. The reported coefficients for these terms are then weighted according to the number of villages within each state that contribute to the estimation of each term. Column (2) uses the same method as column (1) only replaces year fixed effects with state fixed effects. The control set interacted with year fixed effects in column (3) is from the 2001 census and includes: the log of the village population, the log of the number of households in the village, the log of the village area, the scheduled caste share, the scheduled tribe share, the literate share, indicators of power supply (general, agricultural, and domestic), the presence of a dirt road, the presence of a primary/secondary/senior secondary school or college, the presence of a health center or primary health centre, a drinking water indicator, the presence of a post office, the availability of a landline phone connection, and the log of the distance to settlements with at least 10k, 50k, 100k or 500k people. In column (4), we drop districts that are affected by India's Maoist conflict. In column (5), the treatment date for road access is based on the planned completion date rather than the actual completion date. Column (6) is as the baseline regression but with district-year fixed effects instead of state-year fixed effects. In columns (1) to (6), standard errors are clustered at the sub-district level; in column (7) standard errors are clustered per district; in column (8) we allow for auto-correlation of up to 10 years and spatial clustering amongst villages up to 100km away using a uniform spatial weighting kernel. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A9: Results by order of programs

	Received programs in same year (1)	Received electricity first (2)	Received road first (3)
Electricity		0.007	
		(0.023)	
Road			-0.067**
			(0.029)
Elec and road	0.075*	0.032**	0.087***
	(0.041)	(0.014)	(0.021)
Observations	13730	39070	88090
Sub-districts	537	871	1053
Mean of dep. var.	-.146	-.072	.055

Notes: The dependent variable is the (standardized) inverse correlation weighted index of dry-season cropping and the model is the baseline as in Table 1. The sample in column (1) is villages that received the electricity and roads programs in the same year; in column (2) villages that first received the electricity program and then received the roads program in a later year; and in column (3) villages that received the roads program and then received the electricity program in a later year. Coefficients are estimated as the sum of post-treatment coefficients in equation 1, weighted by the number of observations in each treatment bin. The model includes village fixed effects and state-by-year fixed effects. Standard errors are clustered at the sub-district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A10: Impacts of infrastructure programs on dry season cropping - semi-dynamic estimation

	Log of 1 + percentage land cropped during dry season (ISRO) (1)	Log of 1 + percentage land cropped during dry season (Jain et al.) (2)	Log of Δ NDVI in dry season (Asher & Novosad) (3)	Dry season cropping index (4)
Panel A: Electricity and roads				
Electricity	-0.063	0.032	0.100	0.006
	(0.04)	(0.032)	(0.073)	(0.019)
Road	-0.062*	0.013	-0.055	-0.024
	(0.033)	(0.028)	(0.072)	(0.016)
Elec and road	0.1***	0.045*	0.070	0.056***
	(0.035)	(0.026)	(0.068)	(0.016)
Observations	140590	111530	137690	140910
Sub-districts	1364	1236	1339	1365
Mean of dep. var.	2.49	1.01	2.58	0
Panel B: Electricity and mobile phones				
Electricity	-0.022	0.058**	-0.040	0.003
	(0.037)	(0.024)	(0.052)	(0.015)
Mobile	0.023	-0.004	0.040	0.005
	(0.027)	(0.016)	(0.047)	(0.011)
Elec and mobile	0.001	0.023	-0.016	0.010
	(0.03)	(0.02)	(0.053)	(0.014)
Observations	403540	320990	398950	404460
Sub-districts	2147	1989	2110	2148
Mean of dep. var.	2.62	1.18	2.68	0
Panel C: Roads and mobile phones				
Road	0.011	0.037	0.048	0.011
	(0.025)	(0.028)	(0.058)	(0.014)
Mobile	0.029	0.030	0.14**	0.03*
	(0.028)	(0.029)	(0.065)	(0.016)
Road and mobile	-0.004	-0.037	-0.12*	-0.024
	(0.029)	(0.031)	(0.066)	(0.016)
Observations	97220	72570	95900	97460
Sub-districts	1602	1269	1556	1604
Mean of dep. var.	2.81	1.62	3.49	0

Notes: The regressions are identical to the ones in Table 1, except that pre-treatment coefficients are not estimated here. See notes to Table 1 for details.

Table A11: Validation of road and mobile programs

	Reports tar road in 2011 census		Share of village with mobile coverage in 2012	
	(1)	(2)	(3)	(4)
Road	.3***	.3***		.011
	(.017)	(.021)		(.018)
Mobile		.0087	.067***	.093***
		(.018)	(.015)	(.017)
Electricity	.05***		.025	
	(.019)		(.017)	
Elec & Road	-.06***			
	(.021)			
Elec & Mobile			-.0036	
			(.016)	
Road & Mobile		-.00085		-.0023
		(.023)		(.02)
Observations	14070	9737	40357	9722
Sub-districts	1361	1603	2147	1602
Dep. var. mean	.47	.57	.83	.81

Notes: Regressions at the village level; the specification is analogous to Table 2. The outcomes in columns (1) and (2) are based on the village amenities in the 2011 Indian Population Census. The outcome in columns (3) and (4) are from the Mobile Coverage Explorer data. Details of infrastructure programs are provided in the main text. The sample includes: villages that were covered by both the electricity and roads programs in column (1); those covered by a proposed telecommunications tower and the roads program (columns 2 and 4); and those covered by a proposed tower and the electrification program (column 3). Results in the individual infrastructure program rows are the coefficients on dummy variables indicating whether the village has received the program by the beginning of 2011 - the following rows reports the coefficients on the interaction of the relevant terms. The model includes state fixed effects and, in columns (2) to (4), mobile provider fixed effects. Standard errors are clustered at the sub-district level in column 1 and (two-way) at the sub-district and proposed tower level in columns (2) to (4). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A12: Estimated share of crop produced and sold in rabi season

	Share of total production grown in rabi season	Share of rabi production sold	Share of total production produced in rabi season and sold
Fruits	0.71	0.98	0.69
Spices	0.83	0.73	0.60
Vegetables	0.64	0.94	0.60
Wheat	1.00	0.39	0.39
Pulses	0.68	0.56	0.38
Oilseeds	0.48	0.75	0.36
Sugarcane	0.35	0.89	0.31
Maize	0.04	0.90	0.03
Rice	0.05	0.54	0.03
Other Cereals	0.04	0.37	0.01
Plantation	0.01	0.94	0.01
Fibres	0.00	0.40	0.00

Notes: Statistics are calculated from IHDS, with crop categories defined to be consistent with the Agricultural Census. Crop production is aggregated by season across all farmers in the survey, weighted by the number of villages in the district in our main sample of analysis (i.e. villages covered by both the roads program and the electricity program).

Table A13: Census outcomes - Electricity and roads, with post double selection controls

	Δ Dry season cropping index	Reports growing fruit, vegetable, or spice (Census)	Share of households main inc. source is cultivation (SECC)	Asset Index (SECC)	Poverty rate (SECC)
	(1)	(2)	(3)	(4)	(5)
Electricity	-.0018	-.0023	-.019	-.00045	.00024
	(.021)	(.013)	(.014)	(.023)	(.0099)
Road	-.016	-.018	-.022**	-.02	.0064
	(.018)	(.012)	(.011)	(.02)	(.0075)
Elec and road	.057**	.028**	-.0073	.057*	-.012
	(.026)	(.014)	(.015)	(.033)	(.011)
R^2	.12	.1	.17	.34	.32
Observations	13972	8353	13893	10585	13956
Sub-districts	1359	1073	1334	1189	1355
Dep. var. mean	.21	.083	.4	-.22	.45

Notes: This table presents results of the same model as displayed in Table 2 only with additional control variables included - please see notes to that table for details. The control variables included differ across columns and are selected by post-double selection lasso from the following set of variables from the 2001 Census: the log of the village population, the log of the number of households in the village, the log of the village area, the scheduled caste share, the scheduled tribe share, the literate share, indicators of power supply (general, agricultural, and domestic), the presence of a dirt road, the presence of a primary/secondary/senior secondary school or college, the presence of a health center or primary health centre, a drinking water indicator, the presence of a post office, the availability of a landline phone connection, and the log of the distance to settlements with at least 10k, 50k, 100k or 500k people. Results in the 'Electricity' and 'Road' rows are the coefficients on dummy variables indicating whether the village has received the program by the beginning of 2011 - the 'Elec and road' row reports the coefficient on the interaction of these two terms. The sums of these three sets of coefficients can be found in Table A16. The model includes state fixed effects. Standard errors are clustered at the sub-district level in columns (1)-(4); they are bootstrapped in column (5) to account for the construction of the poverty measure. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A14: Census outcomes - Electricity and mobile

	Δ Dry season cropping index	Reports growing fruit, vegetable, or spice (Census)	Share of households main inc. source is cultivation (SECC)	Asset Index (SECC)	Poverty rate (SECC)
	(1)	(2)	(3)	(4)	(5)
Panel A: No controls					
Electricity	.034**	.0074	-.014	.003	-.01
	(.017)	(.013)	(.011)	(.027)	(.0076)
Mobile	.012	-.016	.01	.024	-.011*
	(.012)	(.013)	(.011)	(.022)	(.0058)
Elec and mobile	-.0022	.013	.0016	-.027	.0044
	(.016)	(.014)	(.012)	(.027)	(.0066)
R^2	.21	.15	.11	.13	.39
Panel B: Post-double selection controls					
Electricity	.041**	.0039	-.015	-.029	.0013
	(.017)	(.012)	(.011)	(.023)	(.0076)
Mobile	.016	-.016	.011	-.00056	-.0049
	(.012)	(.013)	(.0099)	(.018)	(.0058)
Elec and mobile	-.0052	.012	.0013	-.0051	.0031
	(.016)	(.014)	(.011)	(.023)	(.0066)
R^2	.21	.15	.14	.24	.5
Observations	38507	30147	37745	22566	38462
Sub-districts	2142	1816	2038	1853	2142
Dep. var. mean	.2	.092	.39	-.19	.37

Notes: This table presents results of the same models as displayed in Tables 2 and A13 , only in this case looking at the interaction of electricity and mobile towers rather than the interaction of electricity and roads. The sample includes villages that were covered by the electricity program and were proposed to be covered by the mobile program. Results in the 'Electricity' and 'Mobile' rows are the coefficients on dummy variables indicating whether the village has received the program by the beginning of 2011 - the following row reports the coefficient on the interaction of these two terms. The sum of these three sets of coefficients can be found in Table A16. The model includes state fixed effects and mobile provider fixed effects . Standard errors are (two-way) clustered at the sub-district and proposed-tower levels in columns (1)-(4); they are bootstrapped at the sub-district level in column (5) to account for the construction of the poverty measure; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A15: Census Outcomes - Roads and Mobile

	Δ Dry season cropping index (1)	Reports growing fruit, vegetable, or spice (Census) (2)	Share of households main inc. source is cultivation (SECC) (3)	Asset Index (SECC) (4)	Poverty rate (SECC) (5)
Panel A: No controls					
Road	-.0051	.0097	-.023*	.026	-.012
	(.023)	(.012)	(.014)	(.028)	(.0095)
Mobile	.00064	.0051	.0044	.03	-.014*
	(.024)	(.01)	(.013)	(.025)	(.008)
Road and mobile	.0046	-.025*	.0066	-.029	8.6e-06
	(.027)	(.014)	(.016)	(.031)	(.0098)
R^2	.16	.15	.12	.14	.13
Panel B: Post-double selection controls					
Road	-.0067	.0097	-.011	-.0063	-.0041
	(.023)	(.012)	(.013)	(.023)	(.0095)
Mobile	.0021	.0045	.015	.012	-.004
	(.024)	(.01)	(.012)	(.023)	(.008)
Road and mobile	.0041	-.026*	-.00023	-.017	-.0031
	(.027)	(.014)	(.015)	(.026)	(.0098)
R^2	.17	.15	.17	.33	.3
Observations	9675	6697	9350	6957	9673
Sub-districts	1598	1304	1505	1376	1598
Dep. var. mean	.29	.052	.4	-.19	.45

Notes: This table presents results of the same models as displayed in tables 2 and A13 for electricity and roads, only in this case looking at the interaction of roads and mobile towers. The sample includes villages that were covered by the roads program and were proposed to be covered by the mobile program. Results in the 'Roads' and 'Mobile' rows are the coefficients on dummy variables indicating whether the village has received the program by the beginning of 2011 - the following rows reports the coefficient on the interaction of these two terms. The sum of these three coefficients can be found in Table A16. The model includes state fixed effects and mobile provider fixed effects. Standard errors are (two-way) clustered at the sub-district and proposed-tower levels in columns (1)-(4); they are bootstrapped at the sub-district level in column (5) to account for the construction of the poverty measure; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A16: Census Outcomes - Combined effects of programs

	Δ Index of dry season cropping	Reports growing fruit, vegetable, or spice (Census)	Share of households main inc. source is cultivation (SECC)	Asset Index (SECC)	Poverty rate (SECC)
Panel A(i): Electricity and roads - no controls					
Combined coefficient	0.025	0.008	-0.06***	0.14***	-0.043***
	(0.024)	(0.015)	(0.015)	(0.033)	(0.01)
Panel A(ii): Electricity and roads - post-double selected controls					
Combined coefficient	0.04*	0.008	-0.048***	0.036	-0.005
	(0.023)	(0.014)	(0.014)	(0.023)	(0.013)
Observations	13972	8353	13893	10585	13956
Sub-districts	1359	1073	1334	1189	1355
Mean of dep. var.	.213	.083	.4	-.216	.445
Panel B(i): Electricity and mobile - no controls					
Combined coefficient	0.044***	0.004	-0.002	0.000	-0.017**
	(0.017)	(0.013)	(0.011)	(0.027)	(0.0076)
Panel B(ii): Electricity and mobile - post-double selected controls					
Combined coefficient	0.052***	-0.000	-0.002	-0.035	-0.000
	(0.017)	(0.013)	(0.01)	(0.023)	(0.0096)
Observations	38507	30147	37745	22566	38462
Sub-districts	2142	1816	2038	1853	2142
Mean of dep. var.	.197	.092	.392	-.195	.375
Panel C(i): Roads and mobile- no controls					
Combined coefficient	0.000	-0.011	-0.012	0.027	-0.026***
	(0.022)	(0.011)	(0.012)	(0.026)	(0.0089)
Panel C(ii): Roads and mobile - post-double selected controls					
Combined coefficient	-0.000	-0.012	0.004	-0.011	-0.011
	(0.023)	(0.011)	(0.012)	(0.023)	(0.012)
Observations	9675	6697	9350	6957	9673
Sub-districts	1598	1304	1505	1376	1598
Mean of dep. var.	.29	.052	.403	-.188	.446

Notes: This table presents the combined coefficients (i.e. the sums) of the three relevant terms in Tables 2, A13, A14 and A15. Please see notes to these tables for further details. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A17: Results related to negative externalities

	Fires in winter months (1)	Log of burnt area in winter months (2)	Dry season cropping index in villages with non-well irrigation (3)	Dry season cropping index in districts with low water depletion (4)
Electricity	0.022	-0.015	0.002	-0.014
	(0.027)	(0.015)	(0.032)	(0.019)
Road	0.054	-0.020	0.001	-0.009
	(0.048)	(0.016)	(0.037)	(0.019)
Elec and road	-0.060	0.015	0.057*	0.046***
	(0.05)	(0.013)	(0.032)	(0.017)
Observations	140590	140590	26500	79330
Sub-districts	1364	1364	713	897
Mean of dep. var.	.13	.047	.171	-.186

Notes: Fire measures are based on the Fire Information for Resource Management System (FIRMS) dataset and the burnt area is from MODIS Burned Area Pixel product, version 5.1. Villages included in column (3) are those where most of the irrigated area is recorded as coming from canals, tanks, lakes, or rivers in the 2001 census. Villages included in column (4) are those with a base water depletion score of ”low” or ”low-medium” according to the WRI Aqueduct 3.0. Details of the infrastructure programs are provided in the main text. Regressions at the village-year level (2005-2014). Coefficients are estimated as the sum of post-treatment coefficients in equation 1, weighted by the number of observations in each treatment bin. The model includes village fixed effects and state-by-year fixed effects. Standard errors are clustered at the sub-district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.