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Argawal, S, Mukherjee, A and [Naaraayanan, L](#)

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Roads and Loans

Sumit Agarwal

National University of Singapore, Singapore

Abhiroop Mukherjee

Hong Kong University of Science and Technology, Hong Kong

S. Lakshmi Naaraayanan

London Business School, UK

Does financing respond to changes in productive opportunities, even for the world's poor? We answer this question by examining the response of private bank financing to an infrastructure program that brought road access to unconnected Indian villages. This program prioritized roads for villages above specific population thresholds, allowing us to exploit the resultant discontinuities for identification. Using detailed data from a large bank, we find that 75% more villagers get loans, and the average amount lent to them is 30%–35% higher, in villages just above these thresholds. District-level analyses further suggest that roads and loans are complements in the growth process. (*JEL* G21, O16)

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One of the first principles of finance is that capital should flow to its most productive uses. So when productive opportunities improve, financing should flow to those who see these gains, allowing them to fully realize potential benefits. Naturally, then, a very large literature is devoted to understanding the

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response of financing to productivity changes (starting with [Schumpeter 1911](#); more recently [King and Levine \(1993a, 1993b\)](#)); and this literature has indeed been helpful in influencing many important economic policies of our time. But does private, profit-motivated financing truly respond to productivity changes, not just for large corporations or rich households in countries with developed markets but also in the day-to-day lives of the world's poor?

This is a very important issue because about half of world population – nearly 3.5 billion people – still live in rural areas, often characterized by poverty. As [Levine \(2008\)](#) points out,

“... the operation of the formal financial system is profoundly important for the poor. It influences how many people are hungry, homeless and in pain. It shapes the gap between the rich and the poor. It arbitrates who can start a business and who cannot, who can pay for education and who cannot, who can attempt to realize one's dreams and who cannot.”

But whether banks will respond to and facilitate productivity improvements for the rural poor is far from obvious. Private, profit-motivated banks have typically only begun lending in rural areas recently and face various social, political, and economic impediments in this new setting, leading to much skepticism about their efficacy (e.g., [Basu \(2006\)](#)).

We shed light on this issue by examining a shock to productive opportunities arising from a large rural road-building initiative in India. Program rules allow us to exploit population-based discontinuities in road construction to identify lending effects using a novel, proprietary loan-level data set. We find that our bank lends to 75% more villagers, and the average amount lent to them is about 30%–35% higher for villages right above the thresholds used for road construction, compared to those just below.

The road-building program we study is among many such infrastructure projects being undertaken in various parts of the world, projects that are thought to be key to unlocking productivity increases among growing populations of surplus rural labor. Hundreds of thousands of miles of such roads have been built throughout Asia, Africa, South America, and Eastern Europe in the past two decades. India alone built 1.96 million kilometers of rural roads between 2000 and 2016. But the type of productive opportunities policy makers often talk about as examples of trickle-down benefits, for example, opening or expanding village grocery shops or changing crop patterns from subsistence cereal farming to more profitable market-based crops, very often require the availability of financing (e.g., [Aghion and Bolton 1997](#); [King and Levine 1993b](#); [Levine 1997](#)). Much of the policy discourse typically assumes that such financing to households will automatically follow once roads are built. But this assumption sits in stark contrast to a substantial literature pointing out inefficiencies in rural financial markets (see, e.g., [Conning and Udry \[2005\]](#) or, more recently, [Agarwal et al. \[2017\]](#), for further references).

This literature points out, for example, that state-subsidized financial institutions—the main lending sources in rural areas, when and where present—have had significant difficulties in terms of both outreach and profitability. Some of these problems are endemic, for example, a lack of political will to allow the independent operation of rural financial institutions, leading many to question their capacity to effectively meet rural credit demand (e.g., Coffey 1998; Satish 2004).¹ Moreover, governments financing some of these infrastructure projects face tight budget constraints and are often under heavy debt; this is especially true in poorer countries. Given the typical loss-making nature of state lending in rural areas, these governments often cannot afford to simultaneously finance infrastructure, as well as provide loan financing through state-owned banks to realize its productivity benefits.

But is there a way for profit-motivated private banks to lend a helping hand? Private banks have been increasingly interested in rural banking across the developing world as of late.² Rural banking divisions of private banks are, however, typically small, and mostly these banks are just starting to operate in these greenfield markets. This motivates our main question: could these private sector financiers respond to changing productive opportunities in rural areas in the way policy makers expect them to?

Moreover, even if financing does follow infrastructure improvements, does it disproportionately benefit the relatively rich villagers who had assets prior to the infrastructure being built, and were therefore in a better position to exploit the resultant opportunities? Or does it benefit the poorer parts of society more, namely, people who were excluded from formal finance before, but can now find a way in (Beck et al. 2007)?

One reason these questions have not already been answered is the difficulty researchers face in accurately identifying the causal impact of new infrastructure. The difficulty arises in identifying an appropriate counterfactual or comparison group. Although one can observe what happens before and after new infrastructure is constructed in “treated” areas, it is difficult to attribute the change exclusively to the project and not to any other environmental or policy factors that may also have been changing at the same time. If infrastructure were located randomly, a natural comparison group would be locations that did not (randomly) receive infrastructure, allowing us to assess program impact. Infrastructure, of course, is not placed randomly in practice, making comparisons with untreated areas problematic.

We find a way to progress by exploiting a policy directive surrounding a major public road construction program in India. The objective of

¹ As an example of the acuteness of problems, state-owned banks in many countries that have had rural operations for decades are known to have hired management consulting firms to advise them on how to better approach rural banking, as recently as 2010 (see, e.g., Shankar 2010).

² See, for example, Rebello (2013) for India, Coopers 2010, p. 5) for China, and Citizen (2018) for Tanzania, among many others.

this nationwide program—called the *Pradhan Mantri Gram Sadak Yojna*, (henceforth PMGSY)—was to provide all-weather road connectivity to hitherto unconnected villages. The roads program we study created a nearly random comparison group for policy evaluation, by explicitly focusing on building new roads to connect all villages above explicit population thresholds. By doing so, program rules allowed for discontinuities in the probability of treatment at these village population thresholds, which we exploit to identify our effects. For example, villages with populations just above a round figure, say 500, were to be prioritized under the program. Under the assumption that villages with populations just below the threshold are very similar to those above, especially if they are located in close geographic proximity, the resultant variation in roads is quasi-random. Asher and Novosad (2020) show that these thresholds indeed predict actual road construction using data from six Indian states; we verify that this is also true in our sample, which comes from the states of *Odisha* in the east and *Uttarakhand* in the north of the country. Above-threshold villages are about 60% more likely to have received a road in our sample, relative to those below.

Our empirical analysis is made possible by our access to a unique, proprietary loan-level data set from one of India’s largest private lenders. We begin our analysis by examining the effect of population thresholds on the external margin of lending. Our evidence shows that our bank lends to 75% more villagers in villages with population above the cutoff, relative to those below. Net loan disbursement as a proportion of income also shows a significant jump of 30%–35% at the cutoff, even after controlling for the entire set of borrower characteristics that the bank cares about and collects information on. Other loan characteristics, however, do not vary at the cutoff: loans in connected villages are similar to those in unconnected ones in terms of default probability, maturity, and interest rates. These results are robust; they also do not show up in a placebo test looking at villages around the same cutoffs, but those that were all connected more than a decade ago under a different program that did not use population-based cutoffs.

While our setting buys us an important advantage in terms of identification, we face two difficulties. First, to keep our treatment (above-cutoff) and control (below-cutoff) sample comparable, we need to restrict our bank lending data to villages with populations close to the cutoffs. Once we do this, we are left with 48 (58) villages with a cutoff of 200 (250) in the bank lending sample, which does not allow us to use a full-fledged village-level regression discontinuity design involving higher-order polynomials, etc. Instead, it is best to think of our research design in terms of a treatment-control setup, wherein the “new road” treatment is administered to a few villages at random – chosen depending on which side of the cutoff they were at – and the rest are controls. We do, however, have detailed within-village data, which we exploit by performing our detailed analysis at the individual borrower-level, similar to papers examining differences in individual firm-level

outcomes across two groups that are exogenously subjected to different policies or regulations.

Second, the reduced-form nature of our analysis makes it difficult for us to quantify the magnitudes of demand shifts (roads increase marginal productivity of capital in newly connected villages, so villagers demand more loans) versus supply-side shifts (the bank finds it easier to screen or monitor borrowers in connected villages). While we are open to both explanations, our data allow us to study the underlying drivers of our main results through a few further tests. For example, we uncover evidence that almost all our results come from *productive* loans (loans taken out for crops, micro enterprises, etc.), consistent with lending responding to changes in productive opportunities. On the other hand, loan amounts granted for consumption uses are actually *lower* in villages with populations above thresholds, ruling out wealth effects driving our results. Also, we present a test based on the variation in loan contract terms (similar to [Fisman et al. 2017](#)) to examine whether the higher lending in connected villages is due to better soft information and/or screening ability (a supply side explanation). The logic of the test is as follows: if the bank really had more soft information on borrowers in connected villages, then it should be able to better differentiate between—and therefore offer different loan contracts to—borrowers who look identical to an outsider based on hard information but who the bank knows are different based on its soft information. Loan contracts offered to similar-looking borrowers, then, should show more variation in connected villages relative to unconnected ones. However, we do not uncover any such evidence. Of course, such demand and supply effects are not mutually exclusive; both could be at play here. Ultimately, whether or not *equilibrium* financing responds to new rural roads is important for many lives and livelihoods, and hence, for policy, even if we cannot conclusively pin down the exact sizes of shifts in demand and supply curves.

Next, we examine the distributional consequences of connectivity from our lending sample. Our data allows us to focus on individual-level differences. This is a critical step in understanding the trickle-down effects of development, as well as for the financial inclusion and inequality literature (e.g., [Aghion and Bolton 1997](#); [Beck 2012](#); [Beck et al. 2008](#), [Demirguc-Kunt and Levine 2009](#)). We find that villagers with less assets benefit more. This is consistent with the view that productivity shocks relax collateral constraints, and improve financial inclusion for those lacking traditional collateralizable assets ([Agarwal et al. 2017](#)).

In the last section of the paper, we address the macro implications of our findings using data from beyond our bank loan sample. Unfortunately, we lack detailed village-level lending data in this broader sample, so we cannot use population threshold-based cutoffs here. Instead, we use Reserve Bank of India (RBI) data on overall private lending activity by sector (e.g., rural, urban) aggregated at the district level for 19 Indian states. Our evidence suggests that higher lending and deposits follow rural road-building well beyond our baseline

bank-loan sample. These findings are robust to controlling for many political and economic variables that might simultaneously affect financial development and economic growth, fixed effects at the district level, as well as state-year fixed effects. While increases in rural lending follow rural road-building in a district, there is little impact on urban lending within the same district, as one might expect.

Finally, we find a significant association between rural road-building and output growth, but only in regions with better rural credit markets. In these regions, rural roads are followed by higher district-level gross domestic product (GDP) growth rates, particularly in the agricultural sector. Growth effects are statistically indistinguishable from zero in areas with less developed rural financial markets. Roads and loans, therefore, seem to be complements in the growth process.

Our paper contributes to the growing literature on the role of financing in economic development and poverty alleviation (Allen et al. 2021; Beck 2012; Beck et al. 2007, 2014; Black and Strahan 2002; Brown et al. 2019; Burgess and Pande 2004; Demircuc-Kunt and Levine 2008a, 2008b; Demircuc-Kunt 2013; Ji et al. 2021; King and Levine 1993a, Levine 2005; Vig 2013; Visaria 2009, among others.) A major part of our contribution comes from our ability to provide elusive causal evidence on how private financing responds to productivity changes. The other part comes from our evidence on the importance of such financing in reaping the benefits of infrastructure development: our macro evidence shows that roads lead to growth only in regions that have better access to finance. More specifically, in the context of financing in a rural setting in India, our paper is related to Burgess and Pande 2004 and Agarwal et al. (2017), who both examine the effect of government-led expansion of credit and savings facilities. Unlike these papers, we look at whether private-sector financing *responds* to infrastructure projects, thereby enabling economies to reap more benefits from them. Our study is related to Das et al. (2019) and Naaraayanan and Wolfenzon (2022), who focus on lending around the construction of India's Golden Quadrangle highway network. Different from their focus on industries and corporations, we study the impact on households, on whom we provide causal evidence using a discontinuity design-based identification strategy. Moreover, our granular data allow us to show that even the rural poor, who are traditionally excluded from bank lending, can also gain access to financing when infrastructure development alters productive opportunities. This latter result suggests the potential for higher welfare multipliers from infrastructure projects, compared to a world in which their benefits are more concentrated.

The PMGSY program also has been used by Asher and Novosad (2017, 2020), who show that new roads led to a reallocation of village labor from agriculture to wage labor, by Mukherjee (2011) and Adukia et al. (2020), who examine schooling decisions, by Agarwal et al. (2021) to examine stock market participation, and by Agarwal et al. (2022), who study changes

in female entrepreneurship. [Shamdasani \(2021\)](#) and [Aggarwal \(2018\)](#) have also examined the effects of this program on rural households, and find evidence of improvements in productivity for affected villages. Our paper makes three clear contributions relative to these studies. First, while [Asher and Novosad \(2020\)](#) find that in an average rural village in India, which typically lacks access to credit in rural India, roads have little impact on economic outcomes, we show that it is possible to see some benefits of connectivity in regions with better access to finance. This is a key difference: such complementarity between physical and financial infrastructures highlights that policy makers might need to address multiple hindrances simultaneously to spur growth. It also suggests that accounting for the possibility of heterogeneous impact is important for policy evaluation. Second, our outcome of interest—financing responses to productivity shocks—is very different from any of these papers, yet such allocative efficiency issues are first-order in finance. Third, our unique individual-loan-level data set allows us to study who benefits from such financing; this is an independently important question from the point of view of the distributional effects of infrastructure and inequality.

Our evidence also adds to extant literature estimating the effects of public infrastructure in low- and middle-income countries. This literature generally finds economically meaningful effects of such projects on a wide range of outcomes. Specifically, transportation infrastructure has been shown to raise the value of agricultural land ([Donaldson and Hornbeck 2016](#)), increase agricultural trade and income ([Donaldson 2018](#)), reduce the risk of famine ([Burgess and Donaldson 2012](#)), increase migration ([Morten and Oliveira 2014](#)), and accelerate urban decentralization ([Baum-Snow et al. 2017](#)). In addition, the evidence is mixed about whether transportation costs can increase ([Ghani et al. 2016, 2017](#); [Storeygard 2016](#)), decrease ([Faber 2014](#)) or leave unchanged ([Banerjee et al. 2020](#)) growth rates in local economic activity. Relative to these papers, our bank data set allows us to present novel evidence on detailed rural financing outcomes, and our district-level analysis allows us to document the complementarity of physical infrastructure and financial development.

Finally, the empirical literature has often found mixed evidence on the effects of infrastructure on inequality. In a recent survey, [Calderon and Serven \(2004\)](#) note that cross-country empirical studies often find weak and suggestive evidence that infrastructure reduces inequality. Within-country studies, however, offer mixed evidence. For example, [Artadi and Sala-I-Martin \(2004\)](#) find that infrastructure spending may have contributed to income inequality in Africa, whereas [Khandker et al. \(2009\)](#) find that the poorest households in Bangladesh benefitted the most from road improvement projects. Given these mixed results, a clear need for more work on identifying the impact of road construction on local inequality emerges. Section 4 in our paper will take a modest step toward this goal.

1. Data

Our main data source is a proprietary, rural, and bank-account-level data set that we obtained from one of India's largest private banks. One of the main obstacles limiting research questions like ours is the lack of availability of granular private financing data at the individual level, particularly in the case of small villages. Our data comes from the coastal district of *Ganjam* in the eastern state of *Odisha*, and from the mountainous districts of *Tehri Garhwal*, *Uttarkashi*, *Chamoli*, and *Garhwal* in the northern state of *Uttarakhand*. Note that while most of our villages within our bandwidth are in *Uttarakhand* (45 of 58), the density of our bank's presence (in terms of total amount lent or number of borrowers) is substantially greater in *Odisha*. For example, of 1,084 villagers with whom the bank has ever had a lending relationship, 792 are from *Odisha*.³ Our data set contains information on individual accounts and transactions in loans over the period 2009–2014. The data also contain relatively detailed demographic information, such as the borrower's sex, education, assets, and income, as provided at the time of the bank account opening. The bank further provides asset values and the breakdown of assets on the number of dwellings owned, the type of dwelling (brick or mud), number of livestock, etc. However, the values of these subcategories are not available.

We obtain data on road construction in India from the website of *Pradhan Mantri Gram Sadak Yojna* (PMGSY), the road-building program we study. The data, which we scrape, include detailed information on road sanction and completion dates. The PMGSY data are structured to consist of information both at the habitation level and at the road level. We conduct our analysis at the village level and perform a time-intensive manual match to villages in our bank data.

To do so, we first perform a hand-match for each village from our bank lending sample to the habitations and villages receiving rural roads under the PMGSY program, and finally to the 2001 population census. Our match finds that the smallest unit of analysis happens to be a village in 54 of 58 cases, making village- and habitation-level analyses identical for most of our sample. However, we find that four of the villages (all in the state of *Uttarakhand*) have habitations associated with them. For these four villages, we consider a village to be treated under PMGSY if at least one habitation in the village that was previously unconnected to the paved “all-weather” road network received a (completed) road during our sample period. As a robustness test, in panel A of [Internet Appendix Table 8](#), we drop these four villages with habitations and find very similar results.

We successfully match over 85% of habitations listed on the PMGSY website to their corresponding census villages. Further, the hand-match of the administrative road data to our proprietary data set at the village-level yields a

³ Resultant power considerations limit our ability to perform tests contrasting the two states.

match of 270 villages spread over 15 blocks and two states across data sets. We also use data on demographics and village-level amenities (such as electricity, the distance to the nearest town, schools) from the 2001 population census and the previously listed PMGSY webpage. Finally, we look at all unconnected villages in year 2009 (which is the year our bank started lending in this area), which ensures that we are indeed capturing the effect of *newly constructed* rural roads.

We supplement this data set with district-level GDP data from Indicus Analytics, aggregate district-level lending data from the Reserve Bank of India (RBI), and various district-level time-varying economic and political variables from the *International Crops Research Institute for the Semi-Arid Tropics* (ICRISAT) and the Election Commission of India (ECI).

2. PMGSY and Empirical Strategy

2.1 The PMGSY program

The main challenge in identifying the impact of infrastructure investments on financing—even if one had the data required to measure outcomes—is the endogenous placement of such infrastructure. Political favoritism or local economic conditions, among other factors, could be directly correlated with both road placement and the outcomes of interest, which can render OLS estimates biased (Beck 2008). In this section, we will describe the empirical strategy we use to make progress on identification.

Our identification strategy is based on guidelines set forth by a national road building program, called *Pradhan Mantri Gram Sadak Yojana* (PMGSY). The central government launched this program in December of 2000 to provide access to “all-weather” roads to the 74% of India’s population that lives in villages. PMGSY proved to be one the largest rural road programs the world has ever seen, with 480,000 kilometers of rural roads built under it by 2016, doubling the size of India’s rural road network.

The program mainly focused on hitherto unconnected villages, defined as those without any preexisting all-weather road within 500 meters of its boundaries, and its aim was to construct roads to connect these villages to the closest town or market center. Program guidelines prioritized villages to receive new roads based on population. At the time most of these roads were constructed, the last nationwide official population record was from the 2001 census. The instructions required state officials to target villages in the following order: (1) villages with population greater than 1,000; (2) villages with populations greater than 500; and (3) villages with populations greater than 250.

Our identifying assumption is therefore that even if selection into road connectivity could be determined by many factors in general, these factors are not likely to change discontinuously at these population thresholds. Hence, if these rules were followed by the officials in charge, which we can test, we

can estimate the effect of road connectivity on financing outcomes using a discontinuity design. Note that throughout the paper, we will use thresholds of 500 and 1,000, but not 250, because there are no villages below the 250 population threshold where our bank lends.

Papers before us, for example, [Asher and Novosad \(2020\)](#) have used PMGSY-based discontinuity before, and have shown its validity/strength as an instrument. However, their results were for six states of India, not just for *Odisha* or *Uttarakhand*. We show that instrument strength/validity extends to *Odisha* and *Uttarakhand*, as well as to our bank-lending sample in the next sections.

2.2 Empirical strategy

Our bank data comes from villages in *Odisha* and *Uttarakhand*. We first test for threshold manipulation under the PMGSY program. This is important to understand whether, for example, a powerful politician was getting local officials to systematically classify some villages with populations below the threshold as being above it, so that these villages get roads. This can be problematic for identification, since then we will not know whether any lending effect we identify in these villages that get roads is indeed attributable to the road connectivity, or to the same politician's simultaneous influence on bank lending. To make sure that our estimates are not confounded by such issues, we use population figures from the 2001 census, – which was conducted before the finalization of the PMGSY policy cutoffs. While this may produce noise in the estimates if the road-building authorities used more updated figures, it ensures validity.⁴ Still, we check for any indication of manipulation using tests for discontinuities in the density of our running variable, population ([McCrary 2008](#)).

In [Figure 1](#), where we plot the histogram of villages in *Odisha* by population, we can see that there are no discrete jumps in population around the PMGSY thresholds of 500 and 1,000, indicating no manipulation for these thresholds. In [Figure 2](#), we provide a formal test of discontinuity following [McCrary \(2008\)](#). To provide one summary test, we first combine our thresholds of 500 and 1,000 into one above-cutoff variable. We do so through a normalized measure of village population, which we create by subtracting the closest threshold from each village's population; our above-cutoff variable takes the value of one for villages with normalized populations just above zero.⁵ The point estimate for the discontinuity at the cutoff is 0.08, with a standard error of 0.058; so we fail to reject the null hypothesis of no discontinuity in the running variable.

⁴ As we show in [Section 3](#), our estimates still retain enough power to ensure that our cutoffs are not weak instruments.

⁵ As we explain below, we only have limited observations for the bank lending sample. As a result, we do not have enough power to examine the two cutoffs separately in most of our following tests. Instead, we make use of this combined above-cutoff variable. Hence, to be consistent, we present results with normalized population here as well.

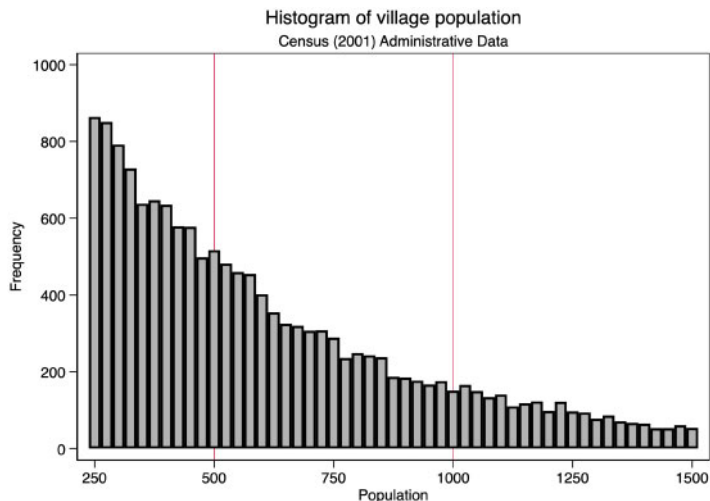


Figure 1
Distribution of village populations around different population thresholds as outlined under the PMGSY guidelines

This figure is a histogram of village populations as recorded during the 2001 population census. The vertical lines represent the program eligibility cutoffs as defined in PMGSY at 500 and 1,000. The sample consists of villages in Odisha and Uttarakhand that did not have paved roads at the start of our sample.

Second, in Figure 3, we examine the geographic clustering of above- versus below-cutoff villages in our two states, and find that the above- and below-cutoff villages come from geographically proximate areas within each state.

Further, since our identifying assumption is that crossing the population threshold discontinuously affects the probability of receiving a road under PMGSY, but not other things at the village level, there should be no jumps in other village characteristics (baseline covariates) at the population thresholds (Imbens and Lemieux 2008). In Figure 4, we examine a scatter plot of means of various village characteristics by different population bins (each of size 25) around the threshold, to check for discontinuities of baseline covariates, and find no such evidence. The characteristics we examine include the presence of schools, health centers, electricity, presence of a telegraph office, the distance from the nearest town, the percentage share of scheduled castes or tribes in population, and land irrigated. Panel A of Internet Appendix Table 1 shows that none of these characteristics is statistically different across cutoffs, even in a regression setting.

In Figure 5, however, when we examine a scatter plot of the proportion of villagers in each population bin with access to a road, we find clear indication of a significant jump in the probability of receiving a road just above the population cutoff relative to just below. To check for the statistical stability of this jump, we estimate the following regression specification as a first step, where we examine the effect of population cutoffs on actual PMGSY road

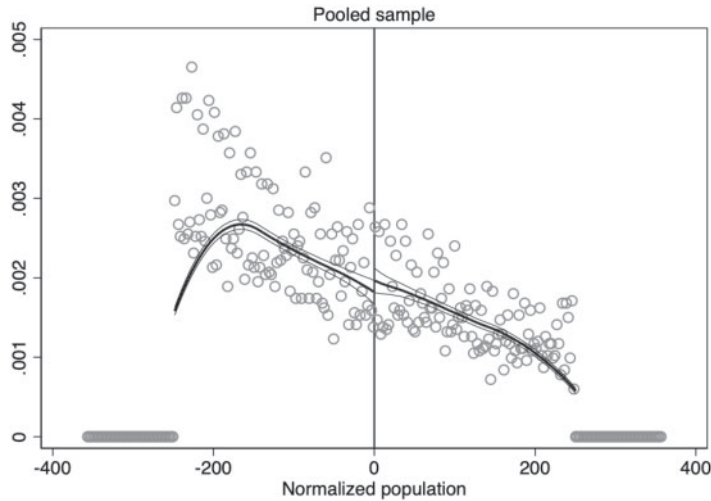


Figure 2
McCrary test for discontinuity in the running variable

This figure plots nonparametric regressions of the distribution following McCrary (2008), testing for a discontinuity at zero. The village population is normalized by subtracting the population threshold by either 500 or 1,000. The sample consists of villages in Odisha and Uttarakhand that did not have paved roads at the start of our sample. The point estimate for the discontinuity is 0.080, with a standard error of 0.058.

construction:

$$\begin{aligned}
 Road_{s,v} = & \beta_0 + \beta_1 1[Population_above_threshold] \\
 & + \beta_2 1[500 - h \leq pop_{s,v} < 500 + h] * (pop_{s,v} - 500) \\
 & + \beta_3 1[1,000 - h \leq pop_{s,v} < 1,000 + h] + \\
 & \beta_4 1[1,000 - h \leq pop_{s,v} < 1,000 + h] * (pop_{s,v} - 1,000) + \sigma_s + \epsilon_{s,v},
 \end{aligned}
 \tag{1}$$

where $Road_{s,v}$ measures whether the village v in state s —unconnected as of 2009—received a PMGSY road by the end of 2014, and $pop_{s,v}$ is the baseline village population as recorded in 2001 census, and σ_s are state fixed effects. We restrict our sample to villages with population within a certain bandwidth around the threshold, such that $pop_{s,v} \in [c-h, c+h]$, where h is the population bandwidth around a threshold of c .

Our main specifications allow for piecewise linearity, that is, we allow outcome variables to be related to population differently in villages with populations around 500 and 1,000, with different slopes and different intercepts. In [Internet Appendix Table 8](#), we show that our results are robust to alternative functional forms, such as restricting slopes and intercepts to be the same.

Note that our identification comes from threshold effects based on population. As a matter of deliberate choice, we *do not exploit the differences*

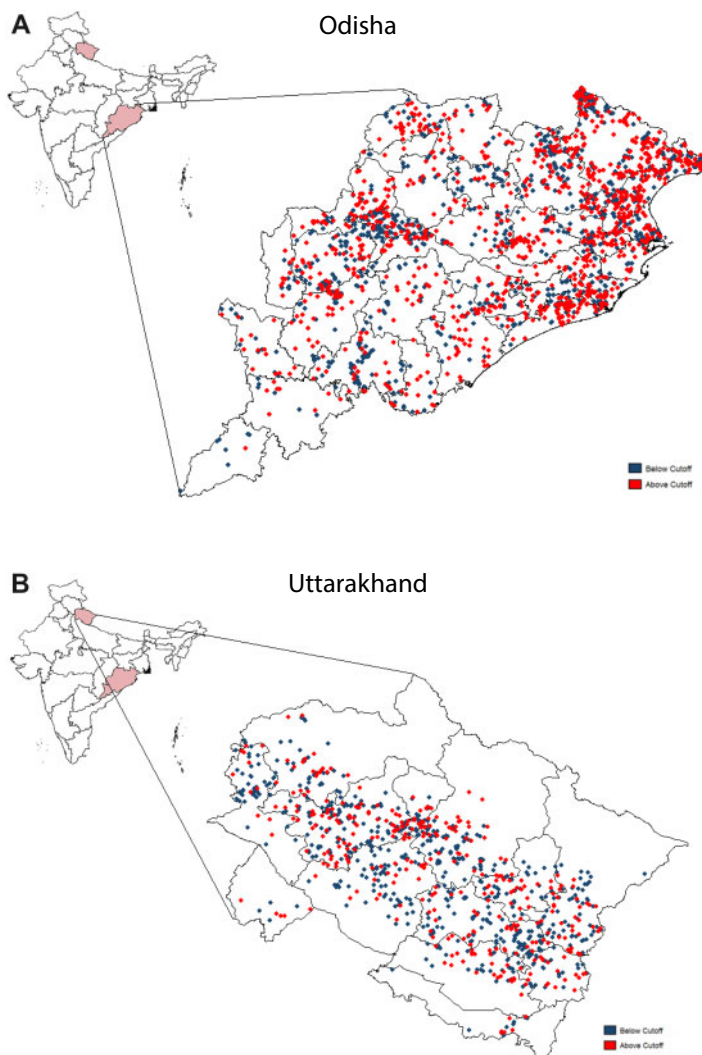


Figure 3
Distribution of unconnected villages by population cutoff

This figure illustrates the distribution of unconnected villages in all districts of Odisha (panel A) and Uttarakhand (panel B). The sample consists of villages that did not have paved roads at the start of our sample as recorded in the 2001 population census. Blue-shaded regions represent villages right below the population cutoff, and the red-shaded regions represent villages right above the population cutoff.

in timing of road construction within these threshold groups. There is indeed variation in *when* individual villages receive roads, but this time variation is largely *endogenous*. While the government rules specify that villages with population 1,080, say, should get roads before a village with population 920, it does not specify whether a village with populations 1,080 should get a

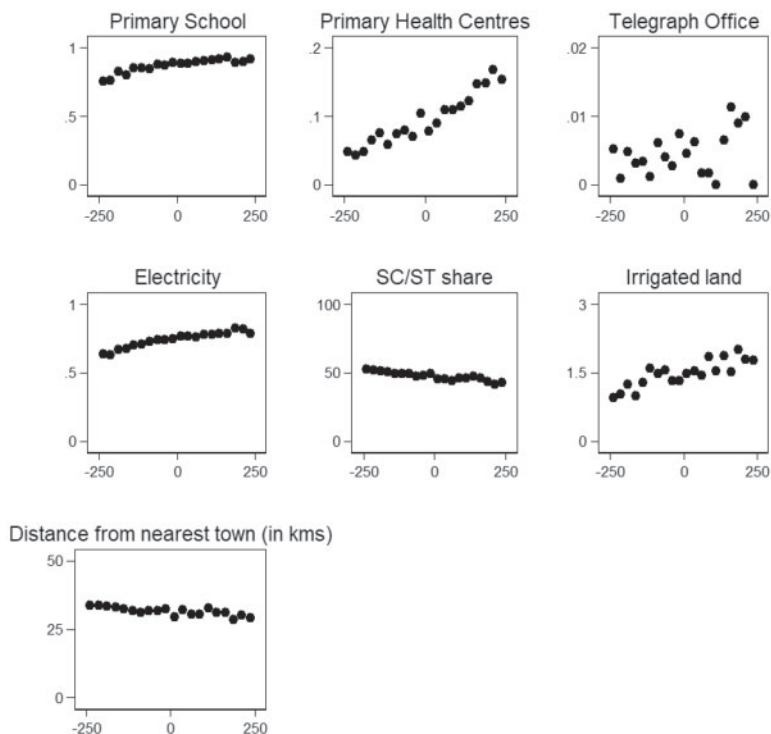


Figure 4
Balance of baseline village characteristics

This figure plots means of baseline village characteristics over normalized population. Points to the right of zero are above treatment thresholds, and points to the left of zero are below treatment thresholds and the bin width is 25 on either side of the threshold. The sample consists of villages in Odisha and Uttarakhnad that did not have paved roads at the start of our sample, as recorded in the 2001 population census.

road before or after one with population 1,170. Therefore, we do not use any time-series information in our formal tests; instead, we take a snapshot of our cross-sectional data at the last available year-end, and exploit the discontinuity based on where our borrowers live.⁶

Next, we turn to our bank lending sample to test our key hypotheses. Since our bank loan data contain few small villages to start with (not surprisingly, our bank, like other private banks, finds it more fruitful to lend to larger habitations with existing roads), we choose 200 and 250 as our bandwidths for estimation purposes. Unfortunately, the number of villages falls rapidly if we restrict bandwidth further, and the resultant decline in statistical power makes our estimates lose significance.

⁶ We take the last available year in our data set as it allows us to measure financing responses which might take time to show up in our data, that is, to ensure that we can measure effects even if financing takes time to respond to road connectivity.

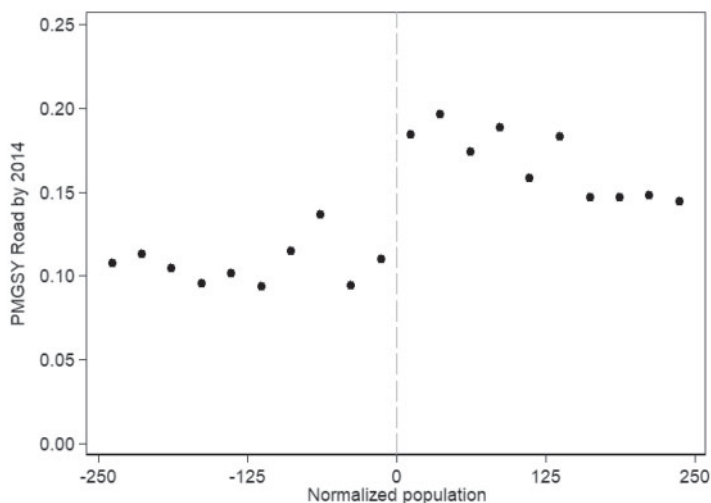


Figure 5

First stage: Effect of road prioritization on the probability of a road by 2014

The figure plots the probability of a village receiving road access under PMGSY by 2014 by village population as recorded in the 2001 population census. The village population is normalized by subtracting the population threshold by either 500 or 1,000. The bin width is 25 on either side of the threshold. The sample consists of villages in Odisha and Uttarakhand that did not have paved roads at the start of our sample, as recorded in the 2001 population census.

Even so, we are only left with a total of 48 villages in our bank loan sample within a bandwidth of 200, and 58 within 250; so, again, to retain enough statistical power, we cannot estimate the thresholds of 500 and 1,000 separately. In addition, given such a limited sample, we cannot employ a full-fledged regression discontinuity design with higher-order polynomials, etc. (e.g., we do not have enough villages to construct something similar to Figure 5 within the bank-lending sample). It might therefore be better to think of our test design in the bank loan sample as a treatment-control setup, where some villages are given the treatment (new roads) in a quasi-random way depending on 2001 population.

We carefully check to ensure the validity of this identification assumption—that the assignment is indeed likely to have been random—by showing that above- and below-cutoff villages are very similar. They are similar in terms of size (by design, since our sample is restricted to a bandwidth of 200/250 from the cutoffs), access-to-banking (our bank was the only lender operating in this precise area at that time in both above- and below-cutoff villages). Panel B of [Internet Appendix Table 1](#) shows that above- and below-cutoff villages—even within our bank lending sample—are also very similar in terms of village-level characteristics like electricity connections, irrigated land, percentage of minority subgroups (scheduled caste, SC; schedule tribe, ST), primary health care centers, primary schools, and the distance from the nearest town. Further,

we also ensure that there was no other government program at the time with a population-based cutoff rule (that could have differentially affected our treatment and control sample).

In our bank-lending sample, we estimate the effect of roads on our financing variables by running a reduced-form specification, using population-based cutoffs, as below.

$$\begin{aligned}
 Y_{s,v} = & \gamma_0 + \gamma_1 1[\text{Population_above_threshold}] \\
 & + \gamma_2 1[500 - h \leq \text{pop}_{s,v} < 500 + h] * (\text{pop}_{s,v} - 500) \\
 & + \gamma_3 1[1,000 - h \leq \text{pop}_{s,v} < 1,000 + h] \\
 & + \gamma_4 1[1,000 - h \leq \text{pop}_{s,v} < 1,000 + h] * (\text{pop}_{s,v} - 1,000) + \sigma_s + \epsilon_{s,v}
 \end{aligned}
 \tag{2}$$

where $Y_{s,v}$ are the borrower-level outcomes of interest. All other variables are as described above. Standard errors are bootstrapped throughout (the [Internet Appendix](#) shows that the results with robust standard errors or stratified bootstrapping, which accounts for correlation within villages are very similar, if not stronger).

3. Results

3.1 Do population cutoffs predict road construction?

Table 1 formalizes the visual evidence in Figure 5 by presenting first-stage estimates from Equation (1) using the census sample of all unconnected villages in the states of *Odisha* and *Uttarakhand*.⁷ Here, our unit of observation is a village. The estimates imply a 6.6- to 6.8-percentage-point increase in the probability of treatment around the cutoff. The unconditional probability of getting a road is about 11%–12%, this is about a 57%–60% jump. This jump is highly statistically significant, with F-statistics of 39.2 and 44.9 for our two bandwidths, implying that we are not subject to a weak instrument problem. Note that although we present results for the bandwidths of 200 and 250 here to be consistent with the rest of the paper, these results are robust to other bandwidths. We show evidence for bandwidths of 100 and 150 in [Internet Appendix Table 2](#).

Next, we concentrate on our bank loan sample. In that sample, when we examine a bandwidth of 200 (250), we have 19 (21) villages above the cutoff and 29 (37) below the cutoff. Of these 19 above-cutoff villages, 11 were connected to the road network by the end of 2014 (58%), whereas 11 of 29 (37%) of the below-cutoff ones received roads at the same time. With a bandwidth of

⁷ This is the set of villages without paved roads, according to the 2001 census. We leave out those that received a PMGSY road between 2001 and 2009, the year our bank started lending.

Table 1
First-stage effect of road priority on PMGSY road treatment, 2009–2014

Bandwidth	±200 (1)	±250 (2)
Above cutoff	0.068*** (0.011)	0.066*** (0.010)
Control group mean	0.12	0.11
F-statistic	39.22	44.94
State fixed effects	Yes	Yes
Threshold fixed effects	Yes	Yes
Observations	11,136	14,205

The table presents first-stage estimates from Equation (1) of the effect of being above the population threshold on a village's probability of receiving a road under PMGSY by 2014. The dependent variable is an indicator variable that takes the value one if a village received a PMGSY road before 2014. Column 1 presents results for villages within 200 of the population threshold (300–700 for the 500 threshold and 800–1,200 for the 1,000 threshold), and column 2 expands the sample to include villages within 250 of the population threshold. The regression specification includes state and threshold fixed effects. The sample consists of all the villages in Odisha and Uttarakhand that did not have paved roads at the start of our sample as recorded in the 2001 population census. We report bootstrapped standard errors below the point estimates. Standard errors are in parentheses. * $p < .1$; ** $p < .05$; *** $p < .01$.

250, the corresponding numbers are 13 of 21 above, and 12 of 37 below. So the jump at cutoffs is also present in our bank-lending sample. Note that the base rate for a village getting a road in this sample is much higher than that for the entire sample. This is because our bank villages (both above- and below-cutoff) are less remote compared to the average unconnected village. Our bank was experimenting with rural banking in villages not too far (about 30 km, on average) from district towns, that is, in villages mainly around *Behrampur* in *Odisha* and *New Tehri* in *Uttarakhand*.

Overall, our results confirm a significant increase in the probability of treatment, that is, the probability of receiving a new rural road, around the population threshold. Thus, the [Asher and Novosad \(2017, 2020\)](#) results on the validity of PMGSY also hold if we focus on *Odisha* and *Uttarakhand* and are consistent with patterns even within our bank-lending sample.

3.2 Summary statistics for our bank lending data

First, we outline the geography of our sample within *Odisha* and *Uttarakhand* in Figure 6. In this figure, we plot the locations of our 58 sample villages within bandwidth. As already mentioned, above- and below-cutoff villages are geographically very close to each other within each state. In other words, as a group above-cutoff villages are likely to be very similar to those below, for example, in terms of topography or climate.

Table 2 shows the summary statistics for our bank data set. The bank data we use contain cash flow information on each loan granted, and the sample consists of all individuals who had some kind of record with our bank by the end of the calendar year 2014. We present means and standard deviations for our main variables of interest.

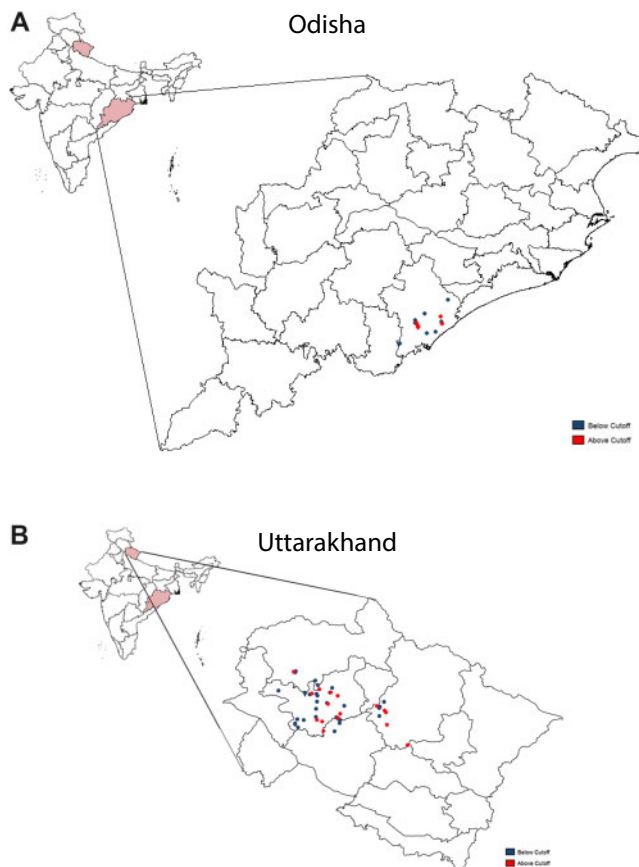


Figure 6
Geographic dispersion in the bank lending sample

The figure displays the geographic dispersion of the villages based on the cutoff in our bank lending sample. The sample consists of villages in the Ganjam district of Odisha (panel A) and the districts of Chamoli, Garhwal, Tehri Garhwal, and Uttarkashi of Uttarakhand (panel B).

Panel A presents main loan characteristics observed in our data set. Columns 1 and 2 (3 and 4) present means for borrowers residing in villages with populations within 200 (250) of the population thresholds (e.g., 300–700 and 800–1200 for 200). *ln (Net disbursement)* is the logarithm of the net loan outstanding per villager (the natural logarithm of the (net) loan amount disbursed, calculated as the loan amount outstanding for each borrower at the end of the calendar year 2014 net of all repayments on that loan). *Loan maturity* is the average loan maturity for each borrower, and *Interest rate (%)* is the average interest rate across loans for each borrower. Interest rate information is not directly reported in our data, but we are able to back it out using information on loan amounts, type, installment payments, and maturity.

Table 2
Summary statistics: Bank loan sample

Bandwidth	±200		±250	
	(1) Mean	(2) SD	(3) Mean	(4) SD
<i>A. Loan characteristics</i>				
Net disbursement (Rs.)	28,091	19,106	28,064	19,589
Loan maturity (years)	3.06	0.15	3.06	0.15
Interest rate (%)	14.7	3.7	14.7	3.8
Overdue amount (%)	0.12	2.25	0.12	2.19
<i>B. Borrower characteristics</i>				
Age (years)	37	9	37	9
Female (%)	25	43	25	43
Schooling (%)	87	33	87	34
Annual income (Rs.)	131,892	88,718	132,286	89,368

The table presents means and standard deviations for our primary variables of interest. Our sample is a proprietary rural bank-account-level data set from one of India's largest publicly traded banks. Panel A presents main loan characteristics observed in our data set, and panel B presents borrower characteristics for our main sample. Columns 1 and 2 present means and standard deviations for borrowers residing in villages with populations within 200 of the population threshold (300–700 for the 500 threshold and 800–1,200 for the 1,000 threshold), and columns 3 and 4 present means and standard deviations expanding the sample to include villages within 250 of the population thresholds. *Net disbursement* is the net loan amount disbursed. For each borrower, we compute the net loan amount disbursed as loan amount disbursed minus any repayment made by the end of the calendar year 2014. *Loan maturity* is the maturity in years for each borrower while *Interest rate* is the average interest rate across loans for each borrower. *Overdue amount* captures the fraction of loan amount disbursed that was overdue by the end of 2014. *Age* is in years and *Female* is an indicator variable equal to one if the borrower is a female. We create an indicator measure, *Schooling*, which takes the value of one if the borrower has attended a school, and zero otherwise. *Annual income* is the individual income of the borrower at the time of opening an account with the bank. The bank loan sample consists of individuals from 58 villages in Odisha and Uttarakhand in which the bank lent and who had a loan with the bank by the end of the calendar year 2014.

To measure loan performance, we create a variable *Overdue amount (%)* that captures the fraction of loan amount disbursed that was overdue at our time of measurement.

Panel B presents borrower characteristics for our main sample. *Age* is in years and *Female* is an indicator variable equal to one if the borrower is a female. To measure the borrower's education level, we create an indicator, *School education*, that takes the value of one if the borrower has ever attended any school class, and zero otherwise. We also use information on borrower incomes. All these are reported to the bank at the time of opening the account.

We find that average net loan amount disbursed in our overall sample is around Rs. 28,000 (about US\$480, at US\$1 = Rs. 58, the exchange rate at that time). Loan maturity is about 3 years on average, and the average interest rate is 14.7%. Defaults are very rare in our sample, with only 0.12% of loans granted being overdue at an average point in time. Bank officials indicate to us that these low defaults are a feature of borrowers being desperate to maintain a good record with the bank for future borrowing possibilities, as their only other source of credit in these villages are the local moneylenders (who charge usurious interest rates).

The average monthly income for individual borrowers is about Rs. 11,000 (about US\$200), translating to a little over Rs. 131,000 per year.⁸ Defaults, maturity, and interest rates are very similar across these states. The average borrower in our sample is 37 years old. Men account for about 75% of all loans and over 87% of those who receive a loan have attended some school class at some point in their lives. This is a higher education level than in the underlying population, which had an average literacy rate of 63% in the 2001 population census.

3.3 The extensive margin: Village-level results

In this section, we examine whether the bank is more likely to have lent to villagers in above-cutoff villages. We do so in two steps: first, we examine extensive margin at the village level; that is, whether the bank is more likely to have given credit in an above-cutoff village, relative to a village below-cutoff. Then we examine whether the bank lent to a higher number of villagers in above-cutoff villages relative to those below.

To examine this evidence, we first augment our sample villages in which our bank made loans with an equal number of villages with the highest propensity scores for bank lending. Here, the objective is to develop a list of very similar villages that the bank could have potentially lent in, so that we can create measures of the likelihood of our bank lending in above- and below-cutoff villages. These propensity score-matched villages come from the same state, district, and block as our sample villages, and are further matched to their nearest neighbors on village population, primary school presence, the balance of lower castes (an indicator for both the level of development and political balance in rural India), and distance to the nearest town. These variables are taken from the 2001 population census.⁹ [Internet Appendix Table 3](#) presents covariate balance for our matching variables, and shows that our propensity-matched villages are very similar to those that the bank actually lent in.

[Table 3](#) presents our population cutoff-based discontinuity estimates of the impact of new roads on the extensive margin for our bank loan sample. The dependent variable in panel A, *Bank entry*, is an indicator variable that takes on the value one for a particular village if at least one individual from that village received a loan from our bank (zero otherwise), so we run a logistic regression specification here. Column 1 presents discontinuity estimates for villages with populations within 200 of the population threshold (300–700 for the 500 threshold and 800–1,200 for the 1,000 threshold), and column 2 uses a threshold of 250. We find that the odds of our bank lending to someone from

⁸ We account for potential state-level differences in lending parameters by using state fixed effects in all our specifications and state-year fixed effects in our panel regressions in [Tables 11](#) and [12](#).

⁹ We measure the proportion of scheduled caste (SC) and scheduled tribe (ST) villagers in each village to obtain the balance of lower castes.

Table 3
Impact of new roads on lending: Extensive margin

Bandwidth	± 200 (1)	± 250 (2)
<i>A. Bank entry</i>		
Above cutoff	2.003* (1.059)	1.733** (0.858)
Control group mean	0.439	0.440
State fixed effects	Yes	Yes
Threshold fixed effects	Yes	Yes
Observations	93	116
<i>B. Number of customers</i>		
Above cutoff	0.938** (0.409)	0.652* (0.374)
Control group mean	1.012	0.867
State fixed effects	Yes	Yes
Threshold fixed effects	Yes	Yes
Observations	93	116

The table presents reduced-form estimates of the effect of new rural roads on the propensity of the bank to enter a village in our sample. Column 1 presents reduced-form estimates for villages within 200 of the population threshold (300–700 for the 500 threshold and 800–1,200 for the 1,000 threshold), and column 2 presents reduced-form estimates expanding the sample to include villages within 250 of the population threshold. The dependent variable in panel A, *ExtMargin*, is an indicator variable that takes on the value one if an individual in the village received a loan from the bank, while the dependent variable in panel B, *ln(Customers)*, is the natural logarithm of one plus the number of customers served by the bank in each village. We construct the control group villages using propensity score matching. Specifically, we require the control group villages to be in the same block and match them on the following village-level covariates as recorded in the 2001 population census: fraction of SC/ST population, village population, presence of primary school, and distance from the nearest town. [Internet Appendix 3](#) presents the covariate balance. All specifications include state and threshold fixed effects. Panel A reports the odds ratio, which is estimated using a logit specification, and the coefficients in panel B are estimated using an ordinary least squares (OLS) specification. For each regression, the outcome mean for the control group (villages with a population below the threshold) is also reported. We report bootstrapped standard errors below the point estimates.

Standard errors are in parentheses. * $p < .1$; ** $p < .05$; *** $p < .01$.

a given village is twice as high for villages above-cutoff as compared to those below.

The dependent variable in panel B, *Number of customers*, is the logarithm of (one plus) the number of villagers from each village that received a loan from our bank. Columns 1 and 2 are analogous to panel A in terms of bandwidths. Here, again, we find that a new road is associated with a significant increase in number of villagers who receive financing. The lower of our two estimates (with bandwidth 250) shows that relative to the mean of our dependent variable in below-cutoff villages, the bank lends to 75% more borrowers in an above-cutoff village.

Note that villagers from surrounding villages come to take loans at the bank's branches, which are typically located in much larger villages or subdivisional towns. Bank officials indicated to us that there is no official policy of actively going out to different villages to seek out customers. In this setting, the jump in the number of customers we see around the cutoff is consistent with a demand-side story, where villagers who lacked profitable investment opportunities before but recently gained access to the road network seek out these loans.

Table 4
Impact of new roads on the lending quantities

Bandwidth	±200		±250	
	(1)	(2)	(3)	(4)
Above cutoff	0.025** (0.012)	0.025** (0.012)	0.025** (0.011)	0.030*** (0.011)
Age (years)		-0.001** (0.000)		-0.000* (0.000)
Land		0.006 (0.007)		0.003 (0.007)
log(1+assets)		0.002* (0.001)		0.002** (0.001)
School education		0.009 (0.007)		0.009 (0.006)
Female		-0.051*** (0.006)		-0.049*** (0.006)
Control group mean	0.083	0.083	0.085	0.085
State fixed effects	Yes	Yes	Yes	Yes
Threshold fixed effects	Yes	Yes	Yes	Yes
Observations	1,032	1,032	1,084	1,084

The table presents reduced-form estimates from Equation (2) of the effect of new rural roads on lending activity within the villages. Columns 1 and 2 present reduced-form estimates for villages within 200 of the population threshold (300–700 for the 500 threshold and 800–1,200 for the 1,000 threshold), and columns 3 and 4 present reduced-form estimates expanding the sample to include villages within 250 of the population threshold. The dependent variable, *NetDisburse/Inc*, is the net loan amount disbursed divided by household income of each borrower. For each borrower, we compute the net loan amount disbursed as loan amount disbursed minus any repayment made by the end of the calendar year 2014. Our bank loan sample consists of individuals who had a loan with the bank by the end of the calendar year 2014. We include villages in Odisha and Uttarakhand that did not have paved roads at the start of our sample as recorded in the 2001 population census. The specification in columns 2 and 4 include baseline borrower-level controls for age, land ownership, household assets, education, sex, and household income. All specifications include state and threshold fixed effects. For each regression, the outcome mean for the control group (villages with a population below the threshold) is also reported. We report bootstrapped standard errors below the point estimates.

Standard errors are in parentheses. * $p < .1$; ** $p < .05$; *** $p < .01$.

However, we cannot rule completely out the supply side story here that bank employees find it easier to provide information on the bank's loan products to connected villagers, and hence these villagers are more likely to be served. We return to this issue in Section 3.9.

3.4 Loan quantities

In Table 4, we focus on the loan amounts granted at the intensive margin, that is, within the sample of borrowers registered with the bank.

Our main dependent variable is $\frac{\text{Net Disbursement}}{\text{Annual Income}}$ (panel A).¹⁰ The coefficient of interest in these regressions give us a sense of how outstanding loans to the average villager on the bank's balance sheet differs in above- versus below-cutoff villages.

Columns 1 and 2 present coefficient estimates for villages with populations within 200 of the population threshold, and columns 3 and 4 present estimates expanding the sample to include villages within 250. Columns 1 and 3 present

¹⁰ We scale by annual household income, instead of annual individual income, since the bank looks at the former variable in its decisions.

results with state and threshold fixed effects but no other control variables. One advantage of getting these data from the bank itself is that we have access to—and can therefore control for—borrower-level characteristics that the bank looks at in its lending decisions. So, in columns 2 and 4 we control for age, value of assets, education, sex, and a dummy variable indicating land ownership.

Our coefficients for the *Above cutoff* variable are similar across specifications and suggest that the expansion of lending activity extends to the intensive margin. Not only do more villagers living in villages above cutoffs get loans, they also get significantly *larger* loans. In terms of economic magnitudes, the net amount lent to an average villager above the population cutoff is 30–35% higher than that to an average villager below the cutoff.

Looking at the coefficient estimates on other borrower characteristics, we find that villagers with more collateralizable assets are likely to get higher loan amounts from our bank. Younger and more educated villagers also seem to get higher loan amounts, although the latter results are not statistically significant. Women get lower loan amounts than men. One possible explanation for this latter result could be that these agrarian societies are sex- and gender-biased, and the bias shows up even in bank lending decisions; another explanation could be that the bias is in the demand side. When a family decides to take a loan, they apply for the loan under the male member's name. Further, in [Internet Appendix Table 4](#), we show that our results are not driven by the denominator, that is, income; lending is higher in above-cutoff villages even for our unscaled measure (the logarithm of net disbursement).

Overall, our evidence suggests that the lack of productive opportunities and infrastructure may be one reason behind lower banking penetration levels in developing economies (Agarwal et al. 2017), both on the extensive and on the intensive margins.

3.5 Loan maturities and performance

In this section, we examine the maturity structure of loans granted, and their performance. If the flow of increased financing to areas with recently improved infrastructure indeed reflects improvements in productive lending opportunities, we expect loan performance, that is, default behavior, not to be worse than in unconnected villages. Performance could either remain unchanged or improve. Given that we should measure maturity and performance only on similar loans, we add loan-purpose fixed effects in our regressions. Table 5 presents coefficient estimates from Equation (2) on the effect of the population threshold-based discontinuity on the maturity and quality of loans disbursed. Columns 1–3 present coefficient estimates for villages with populations within 200 of the population threshold, and columns 4–6 present estimates expanding the sample to include villages within 250. When we examine loan structure, we generally find that loans made out to villagers in above- and below-cutoff villages are of very similar maturity. This is

Table 5
Impact of new roads on loan maturity and quality

Bandwidth	±200			±250		
	log maturity (1)	overdue amount (2)	% overdue amount (3)	log maturity (4)	overdue amount (5)	% overdue amount (6)
Above cutoff	-0.009 (0.020)	-164.872 (211.679)	0.057 (0.355)	-0.028 (0.019)	-184.306 (190.637)	-0.066 (0.361)
Control group mean	1.11	104.7	0.12	1.11	100.6	0.12
Loan-purpose fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Threshold fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	630	630	630	665	665	665

The table presents reduced-form estimates from Equation (2) of the effect of new rural roads on quality of loan disbursed. Columns 1 through 3 present reduced-form estimates for villages within 200 of the population threshold (300–700 for the 500 threshold and 800–1,200 for the 1,000 threshold), and columns 4 through 6 present reduced-form estimates expanding the sample to include villages within 250 of the population threshold. The dependent variable in columns 1 and 4 is natural logarithm of loan maturity. To measure loan performance, we create two measures: (1) *% overdue amount*, captures the fraction of loan amount disbursed that was overdue, and (2) *overdue amount*, is the total loan amount that was overdue. The dependent variable in columns 2 and 5 is *Total overdue amount*, while in columns 3 and 6 it is *% overdue amount*. Our sample consists of individuals who had a loan with the bank by the end of the calendar year 2014. We include villages in Odisha and Uttarakhand that did not have paved roads at the start of our sample as recorded in the 2001 population census. All specifications include loan purpose, state, and threshold fixed effects and baseline borrower-level controls for age, land ownership, household assets, education, and sex. For each regression, the outcome mean for the control group (villages with a population below the threshold) is also reported. We report bootstrapped standard errors below the point estimates.

Standard errors are in parentheses. * $p < .1$; ** $p < .05$; *** $p < .01$.

particularly evident when the economic magnitude of the coefficients for maturity are put into perspective by benchmarking against the control group mean, that is, the average (log) maturity in below-threshold villages, which is 1.11 (corresponds to an average maturity of 3 years, as in Table 2).

To measure loan performance, we create two measures: (1) the total loan amount that was overdue (*Overdue amount*), and (2) the overdue amount as a percentage (*% Overdue*), which captures overdue amount as a fraction of total loan disbursed. The evidence from the table generally suggests that individuals in villages above the threshold had slightly better repayment behavior than those in villages below, although our estimates are not precise. Note that one reason behind the lack of significance is that the control group means themselves indicate a very low level of default; for example, the overdue amount is on average 0.12% even in below-cutoff villages. Default is very rare in our entire sample, as mentioned in Section 3.2.

Overall, both maturity and performance seem largely similar for loans made to villagers more likely to have received a new road.

3.6 Loan interest rates

In this section we present discontinuity estimates for interest rates (Table 6) on loans. The dependent variable is *Interest rate*, the average interest rate across loans for each borrower (most borrowers have only one loan, a few have two loans, typically of the same type, for example, both crop loans). Again, to ensure

Table 6
Impact of new roads on interest rates

Bandwidth	± 200 (1)	± 250 (2)
Above cutoff	-0.002 (0.006)	-0.005 (0.005)
Control group mean	0.15	0.15
Loan-purpose fixed effects	Yes	Yes
State fixed effects	Yes	Yes
Threshold fixed effects	Yes	Yes
Observations	630	665

The table presents the effect of new rural roads on interest rates on loan disbursed in sample villages. Column 1 presents reduced-form estimates for villages with populations within 200 of the population threshold (300–700 for the 500 threshold and 800–1,200 for the 1,000 threshold), and column 2 presents reduced-form estimates expanding the sample to include villages within 250 of the population thresholds. The dependent variable is the average interest rate across loans for each borrower. Our sample consists of individuals who had a loan with the bank by the end of the calendar year 2014. We include villages in Odisha and Uttarakhand that did not have paved roads at the start of our sample as recorded in the 2001 population census. All specifications include loan purpose, state, and threshold fixed effects and baseline borrower-level controls for age, land ownership, household assets, education, and sex. For each regression, the outcome mean for the control group (villages with a population below the threshold) is also reported. We report bootstrapped standard errors below the point estimates. Standard errors are in parentheses. * $p < .1$; ** $p < .05$; *** $p < .01$.

that we compare interest rates only on similar loans, we add loan-purpose fixed effects in this table.

We find that interest rates on loans made out to villagers living in villages above the threshold were very similar to those on loans in below-threshold villages. These differences are not only statistically insignificant but also small in terms of economic magnitude. Relative to an average interest rate of 15%, average interest rates in above-cutoff villages are between 14.5% and 14.8%.

So overall, controlling for loan type, the loans made to newly connected villagers looked very similar to the ones made out to those likely to lack connectivity: the connected villagers were just getting more of these loans, and were paying them back at similar (if not slightly higher) rates.

Note that in our setting, the bank's aggregate lending to the rural sector was tiny relative to the size of its overall balance sheet. So supplying more capital to more profitable rural sector projects was not subject to any binding balance-sheet constraint. The situation with rural banking was probably not far from a highly elastic supply curve. Hence, using these interest rate (loan price) results to tease out relative shifts of demand versus supply is difficult to do.

3.7 Robustness

In the [Internet Appendix](#), we present various tests to assess the robustness of our main results. First, we examine robustness with respect to various alternative standard error structures. In [Internet Appendix Tables 5 and 6](#), we show results for heteroscedasticity and autocorrelation-robust standard errors (results become statistically stronger, if anything). Then, in [Internet Appendix](#)

Table 7, we present standard errors from a stratified (at the village level) bootstrap procedure. This can account for any potential correlation across different observations coming from the same village. Again, if anything, our results get stronger statistically.

Next, in Internet Appendix Table 8, we present further robustness results for our baseline intensive margin specification on lending amounts. We find that our results are robust to dropping the four villages with habitations in Uttarakhand (panel A), various different specifications where we explore alternative types of piecewise linearity (same slopes around the two cutoffs, same slope and intercepts, panels B and C) and the choice of data winsorization (panel D).

Finally, we examine the issue of “evergreening.” “Evergreening” refers to banks’ unwillingness to recognize bad loans on their books by giving back-to-back follow-up loans to be used by the borrower just to pay off the previous bad loan. In the last panel of Internet Appendix Table 8 we rule out our effects being driven by “evergreening” in above-threshold villages, by showing that our results remain similar even if we only look at first-time borrowers, or borrowers for whom the loans granted are not back-to-back; that is, the current loan issue date is at least 1 year after the last installment pay date of his or her previous loan.

One caveat, however, is that we do not have data to distinguish between whether the increase in formal financing that we document was a replacement for informal financing (e.g., village moneylenders). Note that if there were a general tendency to replace such informal borrowing with formal finance, it would affect both below- and above-cutoff villages; so our caveat here is relevant if newly connected villagers somehow had a greater tendency to replace informal with formal finance. While this is possible, even if this were the case, any replacement of informal with formal finance might still be considered a positive development given the prevalence of usurious interest rates and brutal enforcement associated with village moneylenders.¹¹

3.8 A falsification test

In this subsection we conduct a placebo test to explore the possibility that some factor other than the road treatment associated with population-cutoffs may be spuriously driving our results.

In our placebo exercise, we run our baseline specification for the set of villages that had populations similar to those in our earlier tables, but were *already connected* to the road network in 2001. Importantly, all of these villages were connected to the road network under a *different program that had nothing to do with population based cutoffs*. For this sample, therefore, there is

¹¹ For one of many unfortunate horror stories involving village moneylenders and their ways in India, see Guha Ray (2018). Also, village moneylenders typically lend money for short periods (months, or even days, and the average maturity for our bank loans is about 3 years. Such longer-term loans at lower rates might allow villagers to invest in productive activities with longer durations, such as replacing subsistence crops with cash crops.

Table 7
Falsification test: Already connected villages

Bandwidth	±200 (1)	±250 (2)
Above cutoff	0.004 (0.009)	-0.005 (0.008)
Control group mean	0.064	0.072
State fixed effects	Yes	Yes
Threshold fixed effects	Yes	Yes
Observations	2,256	2,675

The table presents reduced-form estimates of the effect of population thresholds on loan disbursed on a placebo sample of villages, that were already connected, within Odisha and Uttarakhand. Specifically, we include villages that were already connected at baseline and hence the PMGSY thresholds were not applicable to them. Further, we restrict the villages to be within the same block and having similar amenities as recorded in the 2001 population census. Column 1 presents estimates for villages within 200 of the population threshold (300–700 for the 500 threshold and 800–1,200 for the 1,000 threshold), and column 2 presents estimates expanding the sample to include villages within 250 of the population threshold. The dependent variable, *NetDisburse/Inc.*, is the net loan amount disbursed divided by household income of each borrower. For each borrower, we compute the net loan amount disbursed as loan amount disbursed minus any repayment made by the end of the calendar year 2014. All specifications include state and threshold fixed effects and baseline borrower-level controls for age, land ownership, household assets, education, and sex. For each regression, the outcome mean for the control group (villages with a population below the threshold) is also reported. We report bootstrapped standard errors below the point estimates.

Standard errors are in parentheses. * $p < .1$; ** $p < .05$; *** $p < .01$.

no discontinuous increase in probability of road treatment at the population threshold, although our estimation methodology remains identical. Table 7 reports the estimates from this exercise.

We find no evidence of any effect on loan outcomes for the placebo sample, both in terms of economic magnitude and statistical significance, indicating that our results are unlikely to be driven by other discontinuous differences in villages around the cutoffs whose effect we spuriously attribute to new roads.

3.9 Mechanisms

We have found that lending activity responds to new road connectivity in the previous sections. But can we say something more about the underlying mechanism?

One demand-side explanation here is that greater productive opportunities result in a higher demand for loans, for example, by farmers who need funding to move from subsistence cereal cultivation to cash crops. On the other hand, supply-side mechanisms also could be at play: for example, the bank might find it easier to sell loan products and reach villagers in connected villages, or bank employees might find it easier to screen or monitor borrowers in connected villages.

Note that while we have found that the quantity of loans responds, and the price (interest rate) typically does not, this by itself cannot be taken as evidence of a similar shift in both curves. This is because bank officials told us that loan supply at the level of these small villages is such a tiny part of the bank's overall balance sheet that the supply curve is highly elastic at this level. We are

Table 8
Impact of new roads on loan types

Bandwidth	Productive loans		Nonproductive loans	
	±200 (1)	±250 (2)	±200 (3)	±250 (4)
Above cutoff	0.043*** (0.011)	0.045*** (0.010)	-0.044*** (0.011)	-0.038*** (0.009)
Control group mean	0.047	0.047	0.066	0.067
State fixed effects	Yes	Yes	Yes	Yes
Threshold fixed effects	Yes	Yes	Yes	Yes
Observations	1,032	1,084	1,032	1,084

This table presents reduced-form estimates quantifying the effect of new rural roads on lending activity based on the type of loan disbursed. Columns 1 and 3 present reduced-form estimates for villages within 200 of the population threshold (300–700 for the 500 threshold and 800–1,200 for the 1,000 threshold), and columns 2 and 4 present reduced-form estimates expanding the sample to include villages within 250 of the population threshold. Columns 1 and 2 present results for *Productive loans*, and columns 3 and 4 present results for *Nonproductive loans*. We partition the loan sample based on whether the financing is provided for productive uses (*Productive loans*), such as business expansion, asset acquisition, and working capital needs, while financing provided for other purposes, such as consumption needs, marriage and festival expenses, are classified as (*Nonproductive loans*). The dependent variable, $\ln(\text{NetDisburse}/\text{inc})$, is the natural logarithm of one plus total net productive (nonproductive) loan amount disbursed divided by household income of each borrower. For each borrower, we compute net productive (nonproductive) loan amount disbursed as the total productive (nonproductive) loan amount disbursed minus any repayment made by the end of the calendar year 2014. We include villages in Odisha and Uttarakhand that did not have paved roads at the start of our sample as recorded in the 2001 population census. All specifications include state and threshold fixed effects and baseline borrower-level controls for age, land ownership, household assets, education, and sex. For each regression, the outcome mean for the control group (villages with a population below the threshold) is also reported. We report bootstrapped standard errors below the point estimates.

Standard errors are in parentheses. * $p < .1$; ** $p < .05$; *** $p < .01$.

therefore open to both demand- and supply-based drivers of our findings, and focus on what we can learn from our data about these drivers in this section.

3.9.1 Evidence from loan uses. First, we examine what uses the increase in financing in connected villages was being put to. For this, we partition the loan sample based on whether the financing was provided for productive uses versus other nonproductive uses.

Table 8 presents our estimates of the impact of new roads on loan use. We partition the loan sample based on whether the financing was provided for productive uses (*Productive loans*), such as crop and micro-enterprise loans, and loans for business expansion, asset acquisition, and working capital needs or other uses, such as consumption needs, marriage, and festival expenses (*Nonproductive loans*). Columns 1 and 2 present results for productive loans, and columns 3 and 4 present results for nonproductive loans.

We find that the higher financing was mostly for productive purposes within above-cutoff villages. This is consistent with the bank lending more to newly connected villages who see higher productive opportunities, as documented by Shamdāsani (2021) and Aggarwal (2018) (who show higher use of yield-improving fertilizers and hybrid seeds on farms, as well as transitions from subsistence to market-oriented farming, in connected villages). Interestingly,

we find lower financing for consumption in these same villages. The bank provided less nonproductive loans in above-cutoff villages. These findings suggest that our main results in Table 4 are not purely driven by wealth effects. Had it been so, we should also have seen increases in consumption loans being made out to the newly connected villages.

Finally, note that some of the loans reported by borrowers—and classified by the bank—as productive could possibly have been used for consumption purposes. In addition, it may be hard to distinguish between these two loan purposes as there may not really be two different set of accounts in many household enterprises. These would make our classification noisy. We do not, however, have reason to believe that such misclassification would have shown a discontinuous jump at our population cutoffs. Similarly, Indian government directives urge banks to lend to certain sectors on a priority basis, and crop loans and micro enterprise loans fall under this purview. But priority lending policy again is not discontinuous at population threshold-based cutoffs. Therefore, such policy directives are unlikely to be driving our results.

3.9.2 Variability in contract terms. Another possibility is that it is the ease of collecting soft information/monitoring, conditional on having reached the borrower, that leads banks to be more willing to lend to connected villagers. Specifically, banks might have more/better soft information on borrowers in villages with roads (where information is easier to collect), so they are willing to supply more loans at the margin. Note that our results on default rates being similar across the cutoff does not provide support for this hypothesis: if the bank did have better information on connected villagers, default rates should have been lower for them. However, default rates are so low in our sample in both below- and above-cutoff villages that this explanation perhaps warrants further attention.

Information differences are difficult to observe directly, especially if the bank's advantage in connected villages is on soft information (recall that hard information that the bank collects on borrowers is already controlled for in our tests). However, an information-based theory that can potentially explain our results here would make one other testable prediction: if the bank really had more information on borrowers in connected villages, then it should be able to better screen borrowers with the same observables. That is, loan contracts will look different for two borrowers who may look identical to an outsider based on hard information, but on whom the bank has soft information to differentiate. For example, [Cornell and Welch \(1996\)](#) suggest that proximity may reduce information asymmetry in a lending transaction by improving the precision of the signal that the officer obtains about a borrower. Their model predicts that proximity should increase the variance of loan sizes, as the officer's distribution of prior beliefs of borrower quality widens with the more precise signal.

This yields an empirically testable hypothesis: if our results are driven by soft information or better screening, loan contract terms will be more variable

Table 9
Testing for the variability of loan contract terms

	Below threshold (1)	Above threshold (2)	Difference of means (1) - (2)	<i>p</i> -value on difference
<i>A. Bandwidth, ± 200</i>				
Loan amount	0.619	0.635	-0.016	.894
Interest rate	0.273	0.217	0.056	.704
Loan maturity	0.103	0.095	0.008	.869
<i>B. Bandwidth, ± 250</i>				
Loan amount	0.605	0.682	-0.078	.512
Interest rate	0.249	0.249	0.001	.997
Loan maturity	0.092	0.098	-0.006	.895

The table presents results that test the variability of loan contract terms between similar groups of borrowers in above- versus below-cutoff villages. We divide our sample into groups of borrowers within above- and below-cutoff villages. For each state in our sample, we divide groups based on observable characteristics, such as sex, whether the borrower is educated, household asset size, and age. We generate four groups based on sex and school education, and within each of these groups, we further create three groups based on household asset size and borrower age. Within each of these groups, we compute the coefficient of variation of loan contract terms: loan amounts, interest rates, and maturity. We then test for the difference in the coefficient of variation of loan contract terms between above and below-cutoff villages. Columns 1 and 2 report the coefficient of variation within below and above-cutoff villages, respectively. The next two columns report difference in means and *p*-value on tests for equality of means respectively. Panel A reports values for villages within 200 of the population threshold (300–700 for the 500 threshold and 800–1,200 for the 1,000 threshold), and panel B expands the sample to include villages within 250 of the population threshold.

* $p < .1$; ** $p < .05$; *** $p < .01$.

within groups of borrowers—who are similar on observables—in connected villages relative to unconnected ones. We test this prediction here. Note that our test here is similar in spirit to that conducted by [Fisman et al. \(2017\)](#).¹²

To test this hypothesis, we divide borrowers into groups based on observable characteristics, such as sex, education, household assets, and age. We generate two groups each based on sex and school education, and within each of these groups, we create three further groups based on household assets and borrower age. Within each of these groups—now, we are looking at observationally similar borrowers—we compute the coefficient of variation of loan contract terms: loan amounts, interest rates, maturity. We then test whether the *coefficient of variation* of loan contract terms are different in above-cutoff villages relative to below-cutoff villages. The prediction from the soft information story will be that the coefficient of variation will be higher for above-cutoff villages.

We find in Table 9 that the variability of loan contract terms in above-versus below-cutoff villages is very similar; their differences are small, and statistically indistinguishable from zero. In sum, we do not find support for a soft information-based supply-side channel.

¹² In their paper, they show that loans issued by officers from the same religion and/or caste group in India have a substantially larger size dispersion, relative to those made by outgroup officers. They suggest that this evidence is consistent with information advantages in within-group transactions, indicative of “soft” information.

Finally, conversations with bank officials suggests that bank operates on a nodal-branch network: bank branches are set up where villagers from surrounding villages come to take loans. Given that the bank interest rates are far lower than the local moneylenders', and that these are severely underbanked areas with no competition from alternative formal lenders (e.g., our bank is the only formal or institutional lender in all of our sample villages); the demand for the bank's services are ample enough that the bank does not need to actively go out to these villages and market itself.

Overall, then, our evidence seems to favor the demand-based rather than the supply-based drivers being dominant in our context, although we cannot definitively rule out that both demand and supply curves might have shifted outward in response to connectivity.

4. Distributional consequences of connectivity: Evidence from loan financing

We have thus far established the causal impact of rural roads on lending flows. In this section, we examine the heterogeneity of the treatment effects based on baseline borrower characteristics. Under the assumption (e.g., our aggregate evidence) that financing indeed flows to those who see largest productivity changes, these estimates also can be interpreted as being useful to understand who benefits more from transportation infrastructure. Also, in our discussion of these results below, we will focus on loan amounts, as there is no meaningful difference in default behavior or interest rates of note across different types of borrowers (although these numbers are also reported in the table).

Here, we use individual-level data to examine the distribution of treatment effects across subgroups with different household assets and income. We exploit the data on demographic information, such as the sex of the borrower, and importantly, information on the borrower's assets, income, and education at the time of the bank account opening. We present these results in Table 10. Columns 1–3 (4–6) present discontinuity estimates for villages with populations within 200 (250) of the population thresholds.

Results from Table 10 suggest that new roads seem to alleviate collateral constraints among borrowers. Low asset households seem to benefit disproportionately more from connectivity. The coefficient for *Low assets*, a dummy variable that takes a value of one if the borrower's asset value is below the sample median, suggests that loan sizes for this group is higher by about 25% in above-cutoff villages. This is interesting because this evidence rules out connectivity driving our effects through increases in land value. Had the higher loan amounts to the newly connected villagers reflected roads increasing the value of their collateral, for example, existing land, and therefore borrowing capacity, our effects would have been stronger among those with higher assets. This is important, because giving access to financial markets to landless peasants – some of the poorest sections of village society in India – has long been a focus of policy for governments. Further, to ensure that

Table 10
Heterogeneous effects of new rural roads, by borrower characteristics

Bandwidth	± 200			± 250		
	Loan amount (1)	% Overdue amount (2)	Interest rate (3)	Loan amount (4)	% Overdue amount (5)	Interest rate (6)
Above cutoff	-0.020 (0.026)	0.664 (1.344)	0.012 (0.016)	-0.013 (0.025)	0.555 (1.240)	0.007 (0.016)
Age (years)	-0.001** (0.000)	-0.000 (0.004)	-0.000 (0.000)	-0.000 (0.000)	0.002 (0.004)	-0.000 (0.000)
Low assets	0.004 (0.007)	0.244 (0.243)	-0.000 (0.003)	0.004 (0.007)	0.228 (0.237)	-0.000 (0.003)
School education	-0.006 (0.007)	-0.921 (0.798)	0.000 (0.004)	-0.004 (0.006)	-0.811 (0.726)	-0.001 (0.004)
SC/ST/OBC	-0.002 (0.008)	0.166 (0.186)	0.000 (0.003)	-0.002 (0.008)	0.146 (0.170)	0.000 (0.003)
Female	-0.015*** (0.005)	0.282 (0.280)	0.006* (0.003)	-0.016*** (0.005)	0.248 (0.262)	0.005* (0.003)
Above cutoff x Age (years)	0.001 (0.000)	-0.019 (0.021)	-0.000 (0.000)	0.000 (0.000)	-0.020 (0.020)	-0.000 (0.000)
Above cutoff x Low assets	0.021** (0.010)	-0.206 (0.233)	-0.005 (0.004)	0.020** (0.010)	-0.209 (0.213)	-0.004 (0.004)
Above cutoff x School education	0.015 (0.011)	0.902 (0.788)	0.007 (0.009)	0.012 (0.010)	0.750 (0.697)	0.008 (0.008)
Above cutoff x SC/ST/OBC	-0.011 (0.011)	-0.563 (0.393)	-0.006 (0.005)	-0.008 (0.010)	-0.508 (0.362)	-0.007 (0.005)
Above cutoff x Female	0.001 (0.010)	-0.464 (0.345)	-0.009* (0.005)	0.002 (0.010)	-0.439 (0.317)	-0.008 (0.005)
Control group mean	0.083	0.12	0.15	0.085	0.12	0.15
Loan-purpose fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Threshold fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,032	630	630	1,084	665	665

The table presents reduced-form estimates of the heterogeneous effects of new rural roads by borrower characteristics for the sample of villages. Columns 1 through 3 present reduced-form estimates for villages within 200 of the population threshold (300–700 for the 500 threshold and 800–1,200 for the 1,000 threshold), and columns 4 through 6 present reduced-form estimates expanding the sample to include villages within 250 of the population threshold. The dependent variable in columns 1 and 4 is the net loan amount disbursed divided by the household income for each borrower. The dependent variable in columns 2 and 5 is the fraction of the loan amount disbursed that was overdue, while in columns 3 and 6 it is the average interest rate across loans for each borrower. For each borrower, we compute the net loan amount disbursed as the loan amount disbursed minus any repayment made by the end of the calendar year 2014. We interact Above cutoff with the following characteristics: *Age (years)* a continuous variable that captures the age of the borrower in years at the time of opening the bank account, *Low assets* a dummy variable that takes the value of one if the borrower’s household assets at the time of opening the bank account is below the sample median or zero otherwise, *School education* a dummy variable that takes the value of one if the borrower has ever attended any school class at the time of opening a bank account or zero otherwise, *SC/ST/OBC* an indicator for the whether the borrower belongs to any of the minority subgroups (scheduled caste, scheduled tribe, or other backward castes), and *Female* an indicator for whether the sex of the borrower is female. Our sample consists of individuals from the sample of villages in Odisha and Uttarakhand who had a loan with the bank by the end of the calendar year 2014. All specifications include loan purpose, state, and threshold fixed effects. For each regression, the outcome mean for the control group (villages with a population below the threshold) is also reported. We report bootstrapped standard errors below the point estimates.

Standard errors are in parentheses. * $p < .1$; ** $p < .05$; *** $p < .01$.

errors in measuring assets are not driving our results, we perform a robustness test by creating a *Low wealth* dummy. This dummy takes a value of one if a borrower has below-median assets as well as at least one other independent

piece of confirming evidence on their financial status, that is, either below-median landholdings (in acres) or below-median jewelry (in grams).¹³ Results remain virtually identical ([Internet Appendix Table 9](#)).

In terms of other important issues in lending in rural India, we find that on average women get smaller loans, and are charged about half a percent higher interest rates (relative to a sample mean of 15%). While we find that these loan amount differences persist in above-cutoff villages, the interest rate charged to women seem to converge to those for men in these villages. However, the difference in interest rates between above- and below-cutoff villages is not statistically significant in one of our specifications, so this evidence has to be interpreted with caution. We also examine differences between lending to minority subgroups which include individuals classified as scheduled castes, schedule tribes, and other backward castes (who are often poorer and have less opportunities) and others in our data, and find no significant differences. Finally, we observe that loan disbursements are higher in newly connected villages for villagers who have ever attended school. These effects are however not statistically significant. Still, these results on the educated are directionally consistent with [Mukherjee \(2011\)](#) and [Adukia et al. \(2020\)](#), who show that PMGSY increases school enrollment. If villagers saw benefits of the road accrue to the school-educated, this might encourage them to send their children to school. Note that while higher education (high school or college) is typically lower among villagers who lack assets, many of them had basic schooling (87% of all borrowers had basic school education in our sample); our results indicate more loans going to this group of newly connected villagers.

5. External Validity and Macroeconomic Effects

One concern with discontinuity designs like ours is that the identification comes at the cost of external validity of findings. Unfortunately, our proprietary data do not allow us to examine the causal impact of new roads on lending across a more general geography nor do we have any other data at the same level of granularity to allow for a similar analysis. However, the Reserve Bank of India (RBI) does provide macro-level data, aggregated by districts, on overall lending activity by sector (rural, urban, etc.). We use these data to examine the macro associations between roads and lending, and examine whether these effects are qualitatively consistent with our earlier results.

The main cost that we incur to translate things to the macro-level is that we lose tightness of identification. The RBI data are not at the village level, so we cannot identify effects based on the program discontinuity; still, we do our best to account for many time-varying control variables, as well as for state-level macroeconomic trends through the use of high-dimensional

¹³ Note that we do not have rupee values of landholding or jewelry separately for our borrowers.

fixed effects. Our main explanatory variable here is the length of road built under the same PMGSY program in the past 3 years at the district level. This allows our effects to show up even if they take some time to manifest. All our empirical specifications include district fixed effects to control for district-level time-invariant characteristics. We augment this by adding state-by-year fixed effects to remove time-varying local economic confounds (e.g., regional macroeconomic shocks).

We first regress aggregate changes in annual lending, the number of bank branches, and the total deposits across all private banks in each district on the aggregate length of PMGSY road constructed in the past 3 years. Table 11 presents results. Panel A presents results without any control variables, but with district and state-year fixed effects. In columns 1–3, we examine private bank activity in *rural* areas. Here, we find an increase in rural lending and deposits following the construction of new rural roads. The number of bank branches does not seem to respond. The coefficient for credit indicates that every one-standard-deviation increase in the length of new rural roads is followed by about 10.52% increase in rural lending (coefficient of 0.466, multiplied by standard deviation of 2, relative to the mean of the dependent variable being 8.86). Similarly, higher deposits also follow: here, a one-standard-deviation increase in roads is followed by a 17.98% increase (coefficient of 0.705, multiplied by standard deviation of 2, relative to the mean of the dependent variable being 7.84).

Next, in columns 4–6, we examine *urban* lending in the same districts using the same specification. There is no response in urban lending, in terms of both economic magnitudes and statistical significance. This is consistent with our effects being a likely response to *rural* roads – which affects rural areas disproportionately – and not some overall macroeconomic or political change in these districts.

In panel B, our specification accounts for a number of economic and political variables that might simultaneously affect bank activity and road construction. As control variables, we incorporate various economic and political indicators that might simultaneously affect financial development and economic growth. First, we rely on the *Village Dynamics in South Asia* (VDSA) data set, maintained by the *International Crops Research Institute for the Semi-Arid Tropics* (ICRISAT). We use these data to construct district-level time-varying control variables for the total geographical area under land use (reflecting, e.g., better irrigation facilities, which could create higher agricultural growth as well as change lending by reducing the risk of crop failures). We also control for the field wage for males, which is a key indicator of growth frequently used by policy makers in the rural Indian context. Next, we add the fraction of population that is literate; literacy is often thought to be an impediment to both rural growth and to villagers accessing formal finance (which, e.g., requires them to fill out many forms).

Table 11
Macro evidence: New rural roads and bank credit

Dependent variable	$\Delta \log(1+\text{rural}...)$			$\Delta \log(1+\text{urban}...)$		
	Credit (1)	Branches (2)	Deposit (3)	Credit (4)	Branches (5)	Deposit (6)
<i>A. Without controls</i>						
$\log(\text{Sum road}_{t-3,t-1})$	0.466* (0.273)	-0.071 (0.223)	0.705** (0.330)	0.192 (0.561)	-0.058 (0.242)	0.105 (0.459)
Mean of dep. var.	8.86	6.56	7.84	12.7	11.2	11.8
Controls	Yes	Yes	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State \times year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,524	2,524	2,524	2,524	2,524	2,524
<i>B. With controls</i>						
$\log(\text{Sum road}_{t-3,t-1})$	0.465* (0.269)	-0.088 (0.224)	0.684** (0.331)	0.207 (0.570)	-0.059 (0.242)	0.206 (0.465)
Mean of dep. var.	8.86	6.56	7.84	12.7	11.2	11.8
Controls	Yes	Yes	Yes	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State \times year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,524	2,524	2,524	2,524	2,524	2,524

The table examines the relationship between new rural roads on private sector bank credit extended for the period 2004 to 2012. Our sample consists of districts from the 19 states for which we have nonmissing control variables. Panel A presents estimates from a specification that excludes district-level time-varying control variables, and panel B presents estimates from a specification that includes district-level time-varying covariates. Across both panels, the dependent variable in columns 1 and 4, $\Delta \ln(1+\text{credit})$, is the annual difference in the natural logarithm of one plus total rural(urban) bank credit extended by private sector banks within a district over periods $t+1$ and t . In columns 2 and 5, the dependent variable, $\Delta \ln(\text{Branches})$, is the annual difference in the natural logarithm of one plus the total number of rural(urban) private sector bank branches within a district over periods $t+1$ and t . In columns 3 and 6, the dependent variable, $\Delta \ln(\text{Deposits})$, is the annual difference in the natural logarithm of one plus total rural (urban) private bank deposits within a district over periods $t+1$ and t . For each state, we aggregate the total kilometers of road constructed under PMGSY at the district level. $\ln(\text{Sum road}_{t-3,t-1})$, is the natural logarithm of one plus sum of the length of new roads (in kilometers) constructed under PMGSY within a district over periods $t-1$, $t-2$, and $t-3$. The control variables include the total geographical area under land use, field wages for males, the literate population fraction, the vote margins for candidates from the two main political parties (i.e., the Bhartiya Janata Party, BJP; the Congress Party), and the average vote margin difference between the candidates. All specifications include district and state \times year fixed effects. All estimates are multiplied by 100 for ease of interpretation. Standard errors, clustered at the district level, are reported below the point estimates.

Standard errors are in parentheses. * $p < .1$; ** $p < .05$; *** $p < .01$.

Next, we add information on political balance and competition, collected from the *Election Commission of India* (ECI). In particular, we compute the district-level vote margins for the two leading political groups in our states, as well as the difference between the winning and the runner-up candidate in the general elections. The latter variable is a measure of political competition. These political controls account for the possibility that constituencies aligned with the party in power at the state/central level, and/or closely contested areas, could see more resources devoted to them, which might simultaneously affect infrastructure and/or financial development and economic growth. We find very similar results to those in panel A.

In our final test, presented in Table 12, we come back to the motivation we started with. Should we care about the availability of finance in relation to building infrastructure? This issue assumes importance especially in light

Table 12
Macro evidence: New rural roads, bank credit and economic growth

Dependent variable	$\Delta \log(\text{GDP}..)$		
	Overall (1)	Agriculture (2)	Services (3)
<i>A. Without controls</i>			
$\log(\text{Sum road}_{t-3,t-1}) \times \text{High rural credit}$	0.203** (0.086)	0.519** (0.248)	0.023 (0.104)
$\log(\text{Sum road}_{t-3,t-1}) \times \text{Low rural credit}$	-0.052 (0.122)	0.280 (0.327)	-0.072 (0.112)
Mean of dep. var.	7.32	6.25	6.54
Controls	No	No	No
District fixed effects	Yes	Yes	Yes
State x year fixed effects	Yes	Yes	Yes
Observations	2,766	2,766	2,766
<i>B. With controls</i>			
$\log(\text{Sum road}_{t-3,t-1}) \times \text{High rural credit}$	0.189** (0.087)	0.496* (0.253)	0.016 (0.106)
$\log(\text{Sum road}_{t-3,t-1}) \times \text{Low rural credit}$	-0.069 (0.121)	0.237 (0.332)	-0.078 (0.114)
Mean of dep. var.	7.32	6.25	6.54
Controls	Yes	Yes	Yes
District fixed effects	Yes	Yes	Yes
State x year fixed effects	Yes	Yes	Yes
Observations	2,766	2,766	2,766

The table examines the role of depth of credit markets in mediating the relationship between new rural roads and growth of the districts for the period 2004 to 2012. Our sample consists of districts from the 19 states for which we have nonmissing control variables. Panel A presents estimates from a specification that excludes district-level time-varying control variables, and panel B presents estimates from a specification that includes district-level time-varying covariates. Across both panels, the dependent variable in columns 1 through 3, $\Delta \ln(\text{GDP})$, is the annual difference in the natural logarithm of gross domestic product (GDP) for each district over periods $t+1$ and t . We present the estimates for Overall GDP (column 1), GDP for Agriculture (column 2), and GDP for Industry & Services (column 3). For each state, we aggregate the total kilometers of road constructed under PMGSY at the district level. $\ln(\text{Sum road}_{t-3,t-1})$, is the natural logarithm of one plus sum of length of new roads (in km) constructed under PMGSY within a district over periods $t-1$, $t-2$, and $t-3$. *High (low) rural credit* is defined based on whether the rural credit per capita in a given year is above (below) the median rural credit per capita. The control variables include the total geographical area under land use, field wages for males, the literate population fraction, the vote margins for candidates from the two main political parties (i.e., the Bhartiya Janata Party, BJP; the Congress Party), and the average vote margin difference between the candidates. All specifications include district and state \times year fixed effects. All estimates are multiplied by 100 for ease of interpretation. Standard errors, clustered at the district level, are reported below the point estimates. Standard errors are in parentheses. * $p < .1$; ** $p < .05$; *** $p < .01$.

of Asher and Novosad (2020), who find little or no effects of road-building activity on village income or output-related outcomes. Analogous to Table 11, panel A presents results with district and state-year fixed effects, and panel B additionally controls for same district-level time-varying controls described above.

Here, we look at changes in district-level GDP growth rates following the construction of new rural roads during the previous 3 years, depending on the depth of the rural credit market in each district. Our findings suggest that while roads are not always followed by higher output on average (Asher and Novosad

2020), such higher output can follow *when roads are constructed in areas with better credit markets*.¹⁴

A one-standard-deviation increase in rural road length in the previous 3 years is associated with a 40.6 basis points (0.203 times standard deviation 2) higher district GDP growth rate (on a mean GDP growth rate of 7.32% during this period) in districts with an above-average rural credit to GDP ratio. There is no discernable effect of roads on districts with below-average depth of rural credit markets.

In columns 2 and 3, we look at GDP growth—still at the district level—but broken down by sectors. Our effects come only from agriculture, not from industries. A one-standard-deviation increase in new the length of new rural roads is followed by an approximately 1% increase in district-level agrarian GDP growth rate, relative to a base of 6.25%. The analogous number for industrial GDP is statistically, as well as economically, close to zero. This, again, is what one would expect if the effects occur through village roads since these small villages are predominantly agricultural. Just as in Table 11, adding time-varying district-level control variables in panel B does not alter these results meaningfully.

Overall, our macro evidence suggests that financing is important to realize the benefits of connectivity, and that such financing indeed follows rural road-building well beyond our baseline bank-loan sample. Our conclusions, however, need to be tempered by the fact that access to credit in rural areas remains very low in India and in other developing countries, and branch expansion does not seem to respond to roads. Much, therefore, remains to be done.

6. Conclusion

Increasing infrastructure investments are a key component of growth strategy in many countries, and a particular focus of policy now (e.g., China’s massive “Belt and Road Initiative”). Although it is typically assumed that financing to households will follow once roads are built, allowing them to make the best use of new productive opportunities, little is known about whether this really happens, especially in poor countries. Moreover, even if financing does follow infrastructure improvements, does it disproportionately benefit the rich who had assets prior to the infrastructure being built, and were in a better position to exploit the resultant opportunities? Or does it benefit the poor too who were excluded from formal finance before, but can now find a way in?

We use a population-based discontinuity setting around a large rural road construction program in India to answer these questions. We find that private financing does indeed respond to changes in productive opportunities resulting from connectivity. Financing flows disproportionately

¹⁴ Districts with better credit markets are defined based on whether the total (private plus state-owned banks) rural credit per capita in a given year is above (below) the median district.

to villagers who lack collateralizable assets and traditionally have been among the most disadvantaged. Our results have important implications for understanding trickle down benefits of building infrastructure and its distributional consequences.

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