Educational Investment Responses to Economic Opportunity: Evidence from Indian Road Construction

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Education and Economic Opportunity

Access to international markets may be important for educational decisions, which are central to supporting long-run growth (Edmonds and Pavcnik 2006; Edmonds et al. 2010; Shastry 2012)

- Most rural poor not well-connected to international markets
- Large number of the poorest still thinly connected even to regional markets

Rural poor depend on domestic linkages to nearby towns/markets

- 31% of the world's rural population live more than 2 km from a paved road (World Bank 2015)

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How does schooling respond to domestic market integration?

- $\uparrow$ if $\uparrow$ returns to education, $\uparrow$ household income, or $\downarrow$ liquidity constraints
- $\downarrow$ if $\uparrow$ opportunity costs of schooling
- Mixed evidence on schooling impacts of economic opportunity

This Study

Are educational decisions influenced by increased exposure to economic opportunity through connections to new markets?

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How does rural road construction impact educational outcomes?

- School participation, measured by enrollment
- Impacts on achievement

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How are these impacts on schooling decisions affected by local labor market conditions outside of the village?

- Opportunity cost of schooling
- Returns to education
- Income or liquidity constraints

Estimating Impacts of Road Construction on Education

Indian national program to connect unconnected villages with paved roads

- Village-level variation in road construction
- Annual census of Indian schools (DISE), administrative data on road program (PMGSY), other census data

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Panel differences-in-differences empirical strategy

- Places with roads may differ from places without roads
- Use timing of road completion in each village by estimating a panel regression with village and state-year fixed effects
- Allows for comparison of educational outcomes in villages before and after road is built, controlling for regional shocks and static differences between early- and late-treated villages

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Exploring mechanisms

- Identify villages where each factor would be expected to be particularly important, test for treatment heterogeneity

Preview of Results

On average, improved access to regional markets via road construction motivates increases in schooling

- Increases in adolescent schooling (6-7%)
- Increases in middle-school exam performance

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Treatment heterogeneity consistent with classical human capital model

- Positive effects (statistically detectable) in 59% of sample
  - $\downarrow$ Opportunity cost, $\uparrow$ Returns to education

Effects do not seem to be driven by changes in:

- Migration, other government programs, access to school, changes in school quality

Outline

- Literature and Conceptual Framework
- Background on Road Construction Program
- Data from Census of Indian Schools, Road Construction
- Methodology: Panel Differences-in-Differences
- Main Results
- Exploring Treatment Heterogeneity
- Summary

Literature

Raising school enrollment in developing countries:

- Evans and Popova (2016); Glewwe and Muralidharan (2016); ...

Labor demand shocks and education:

- Adukia (2022); Atkin (2016); Cascio and Narayan (2020); Islam and Sivasankaran (2015); Jensen (2012); Li and Sekhri (2020); Oster and Steinberg (2013); Shah and Steinberg (2013, 2021); ...

Child labor:

- Edmonds and Pavcnik (2006); Edmonds, Pavcnik, and Topalova (2010); Shastry (2012); ...

Impacts of rural roads:

- Aggarwal (2013); Asher and Novosad (2016); Hine et al. (2016); Khandker, Bakht and Koolwal (2009); Shamdasani (2016); ...

Theory

Standard human capital model

- Key tradeoff: Short-run wages vs. long-term human capital accumulation

Model sketch

- Two periods, unitary household
  1. Period 1: Work or stay in school and consume endowment
  2. Period 2: Work only, wage determined by education
- Can save, may or may not be able to borrow
- Agent consumes in both periods, drawing from initial endowment and wages earned in each period worked
- Initial endowment can reflect household wealth or wages of household adults who have completed schooling
- Education may also be a normal good, valued independently of any impact on future wages

Theory

Roads → regional factor price equalization

- 1st-order effect of reduced transport costs: change in prices
- In equilibrium, urban areas have ↑ wages (credentialed and uncredentialed); ↑ returns to education than rural areas Urban
- Thus, connecting a village to its external market will likely:
  - Increase the uncredentialed wage
  - Increase the return to education

Comparative statics:

- Uncredentialed wage rises → Opportunity cost effect → Schooling ↓
- Skill/credential premium rises → Returns-to-education effect → Schooling ↑
  - Each effect should be largest when the regional wage/return is much larger than village wage/return

Other road effects not in model

- Changes supply of schooling?
- Changes access to school?

Literature through Conceptual Framework

Literature supports theory:

- Call centers expand school enrollment (Jensen 2012; Oster and Steinberg 2013)
- Uncredentialed wage work decreases school enrollment (Adukia 2022; Atkin 2016; Cascio and Narayan 2017; Shah and Steinberg 2013; ...)

This paper:

- Labor demand shock across all regional industries
- Treatment heterogeneity captures all of above dynamics
- Indirectly describes village labor markets relative to broader region

Pradhan Mantri Gram Sadak Yojana (PMGSY)

Prime Minister's Road Construction Program: Launched in 2000 to connect all villages to road network

By 2015:

- 113,000 roads constructed (400,000 km)
- 107,000 previously unconnected villages benefited
- $>$ \$37 billion disbursed

Funded centrally, construction administered by states

Transparent, systematic electronic recordkeeping

- Details of every road built

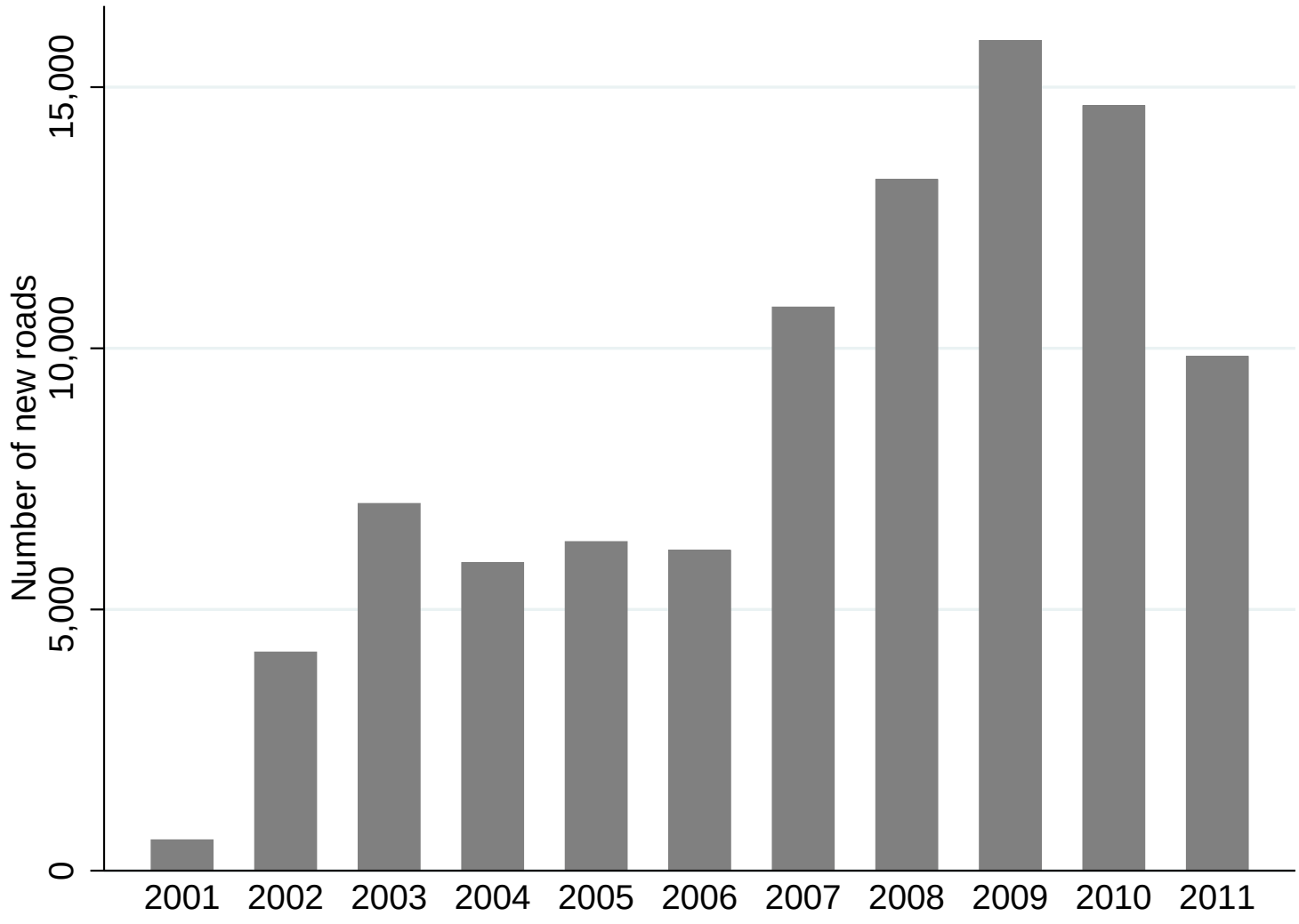
Objective eligibility rules (followed in some states)

- Prioritization to villages over 1000, 500 population

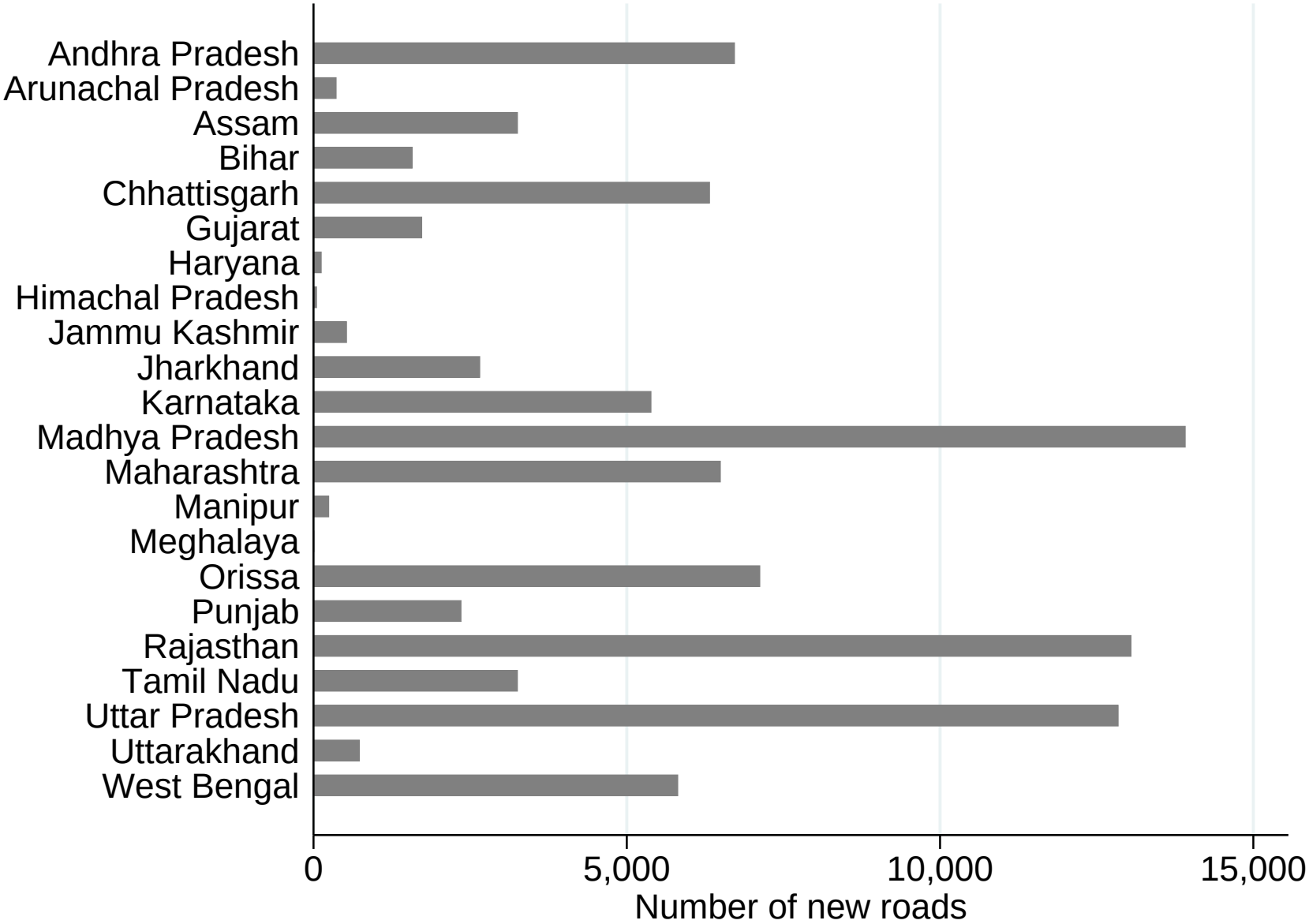
What is a rural road?

- Paved, all-weather road
- Median length 4.4 km
- Connects village to paved road network
- Village is terminus 71% of cases
- Not major artery to other regions

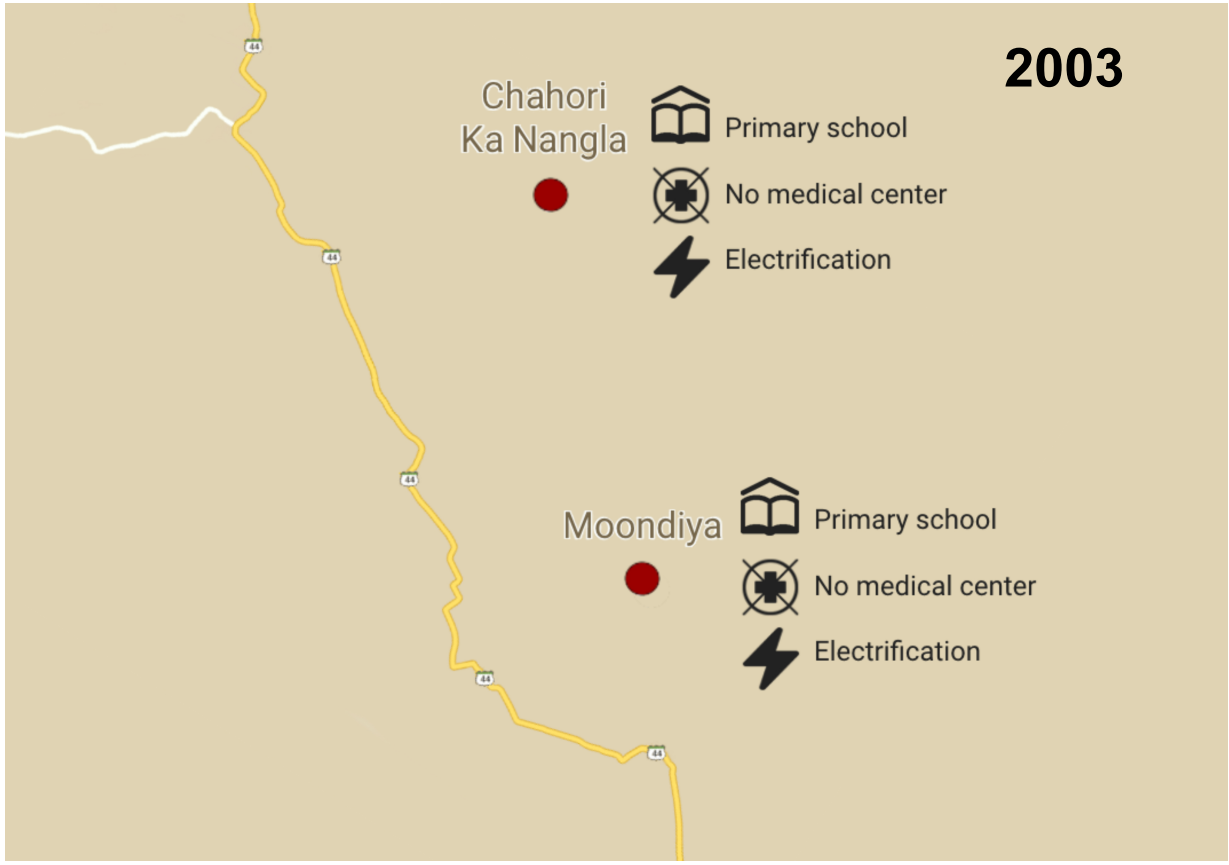
Program Timeline: Number of New Roads by Year



Program Timeline: Number of New Roads by State



Example: Before Road Construction



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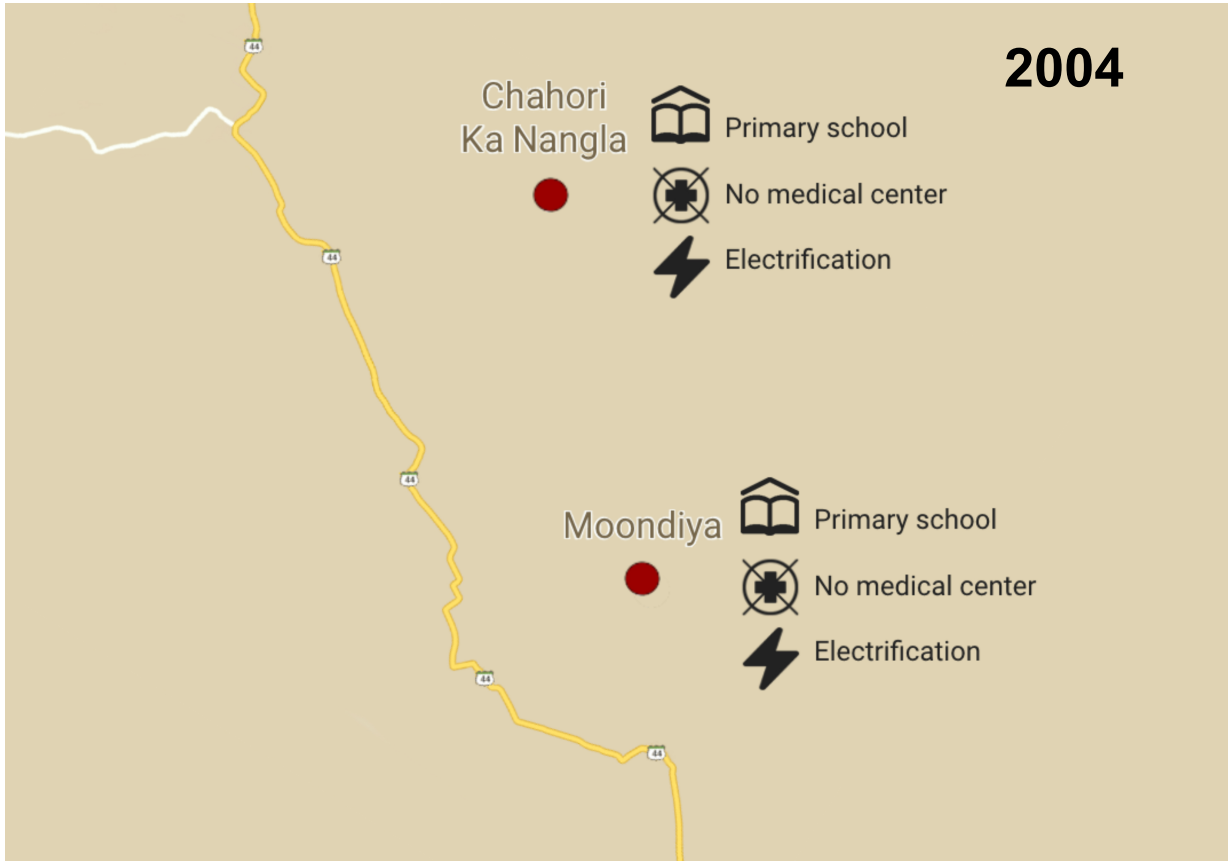

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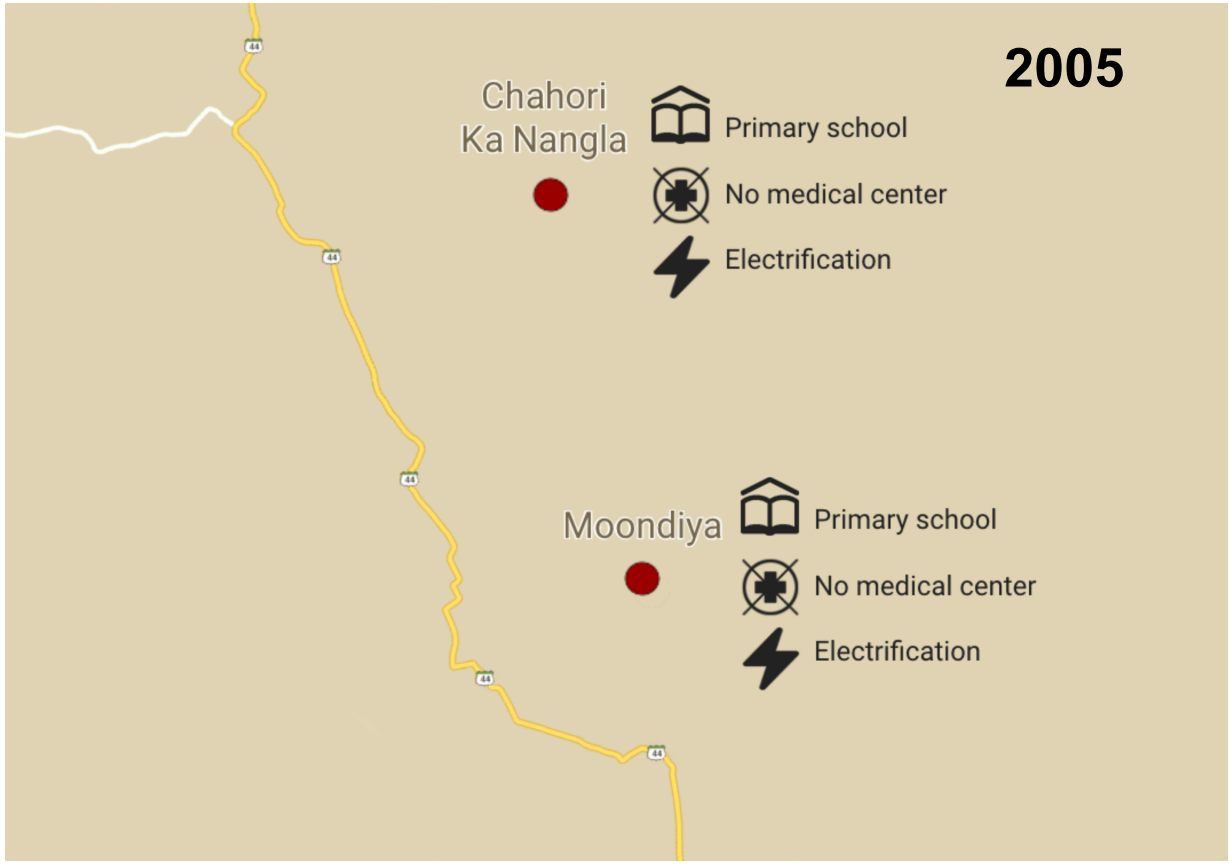

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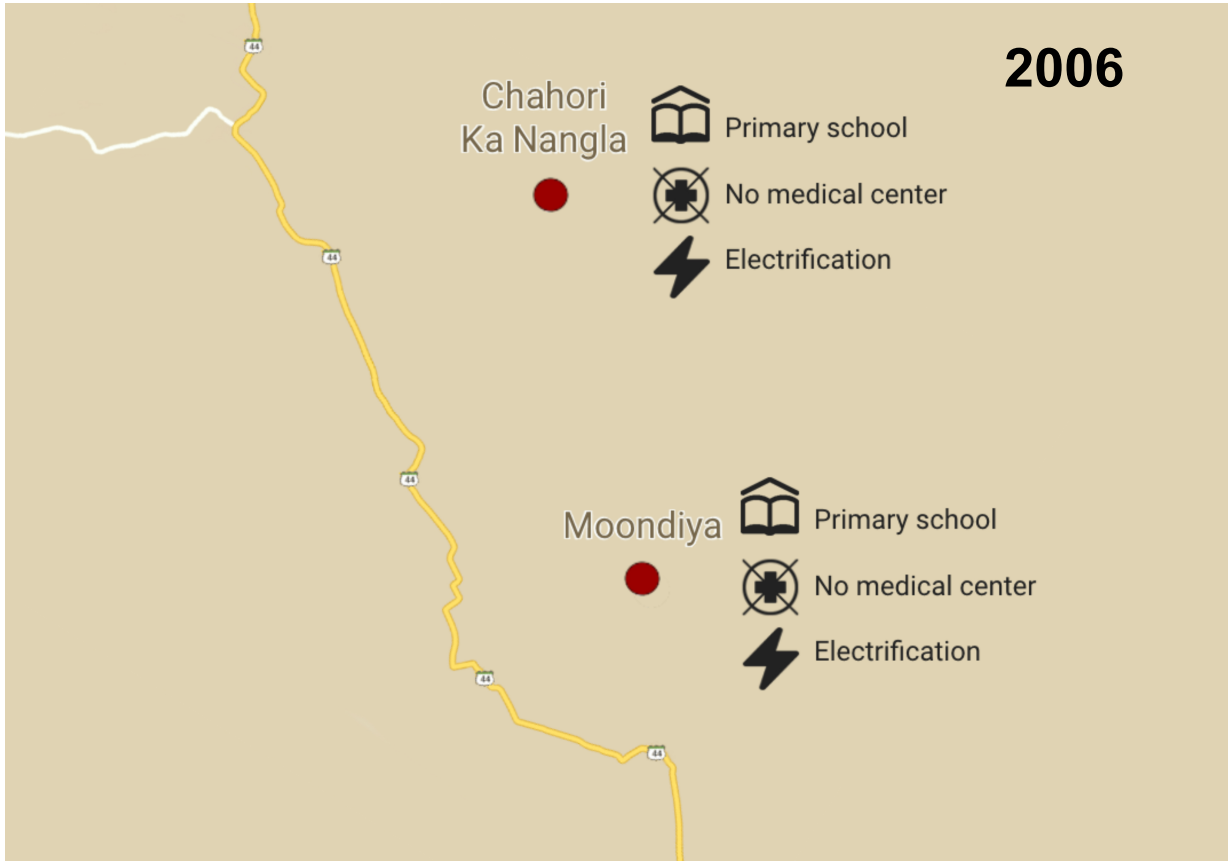
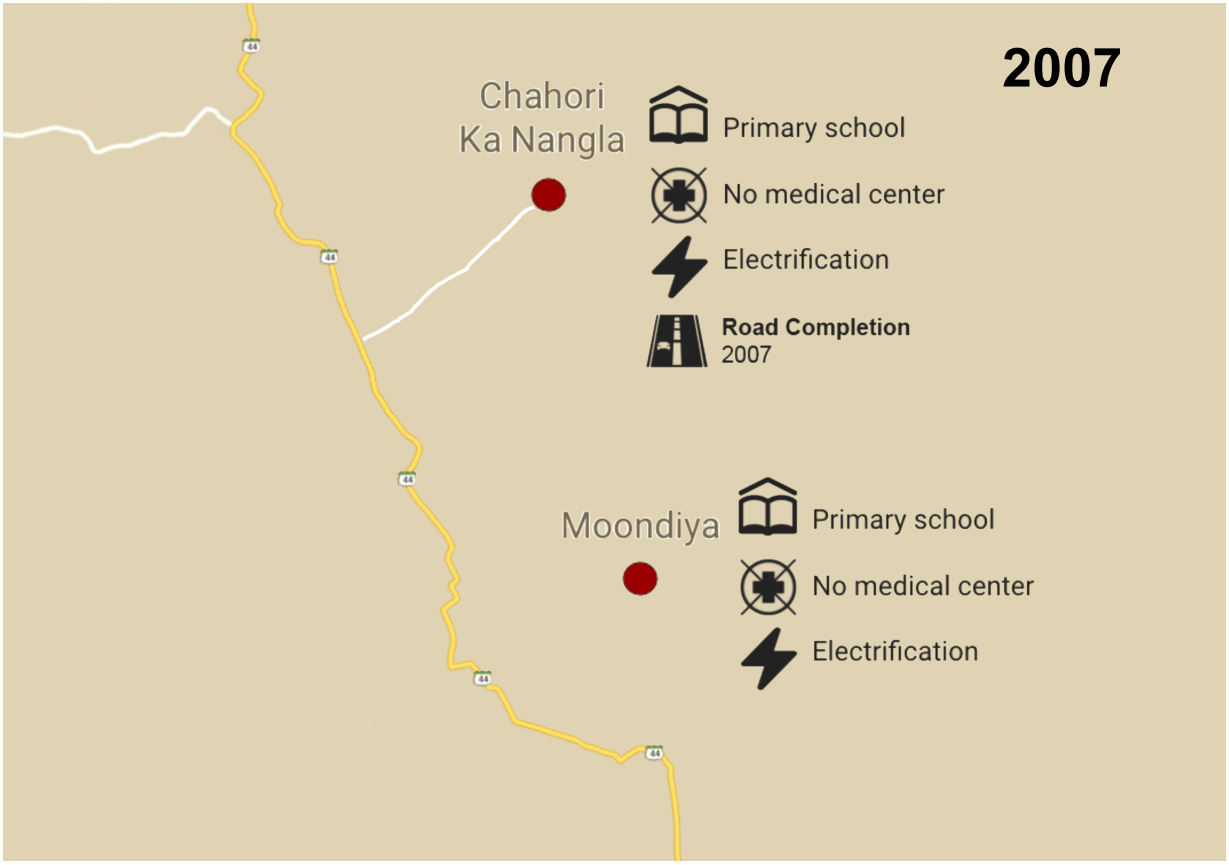


Example: After Road Construction



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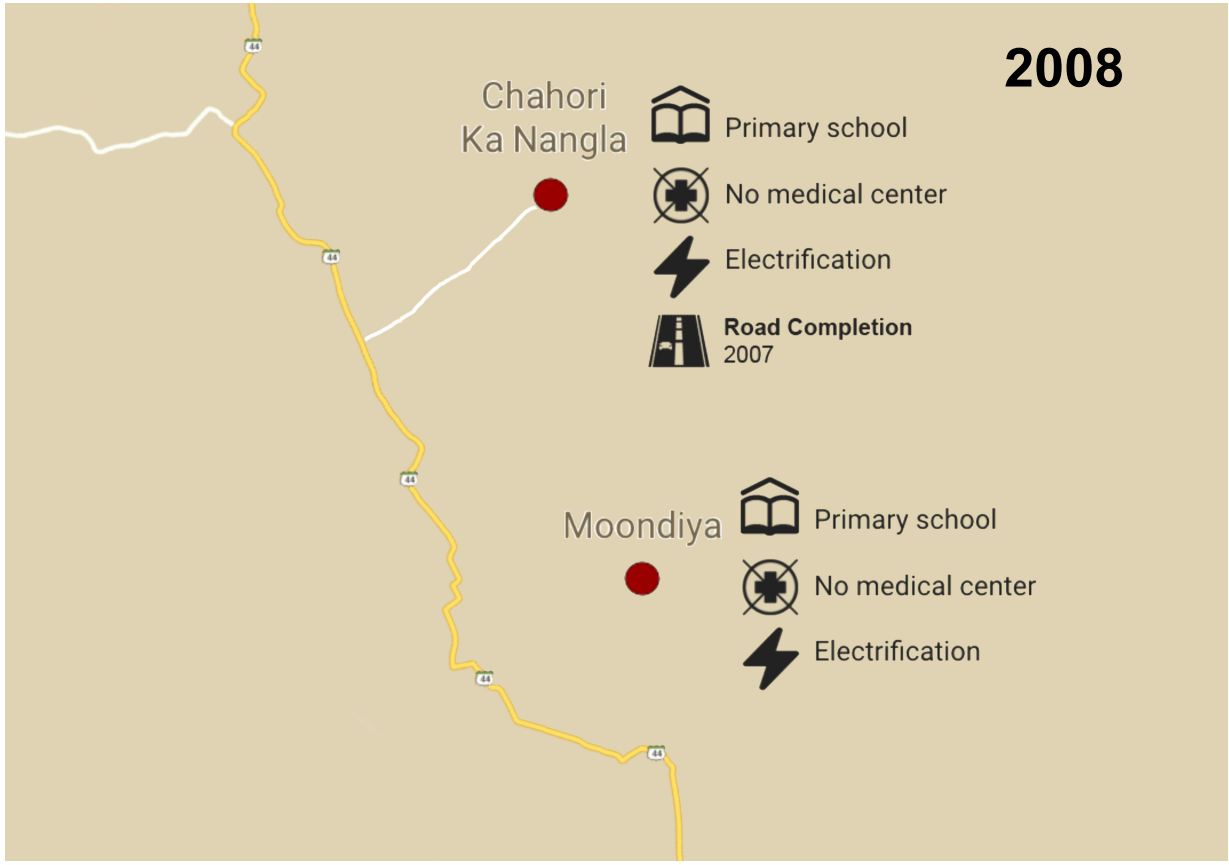

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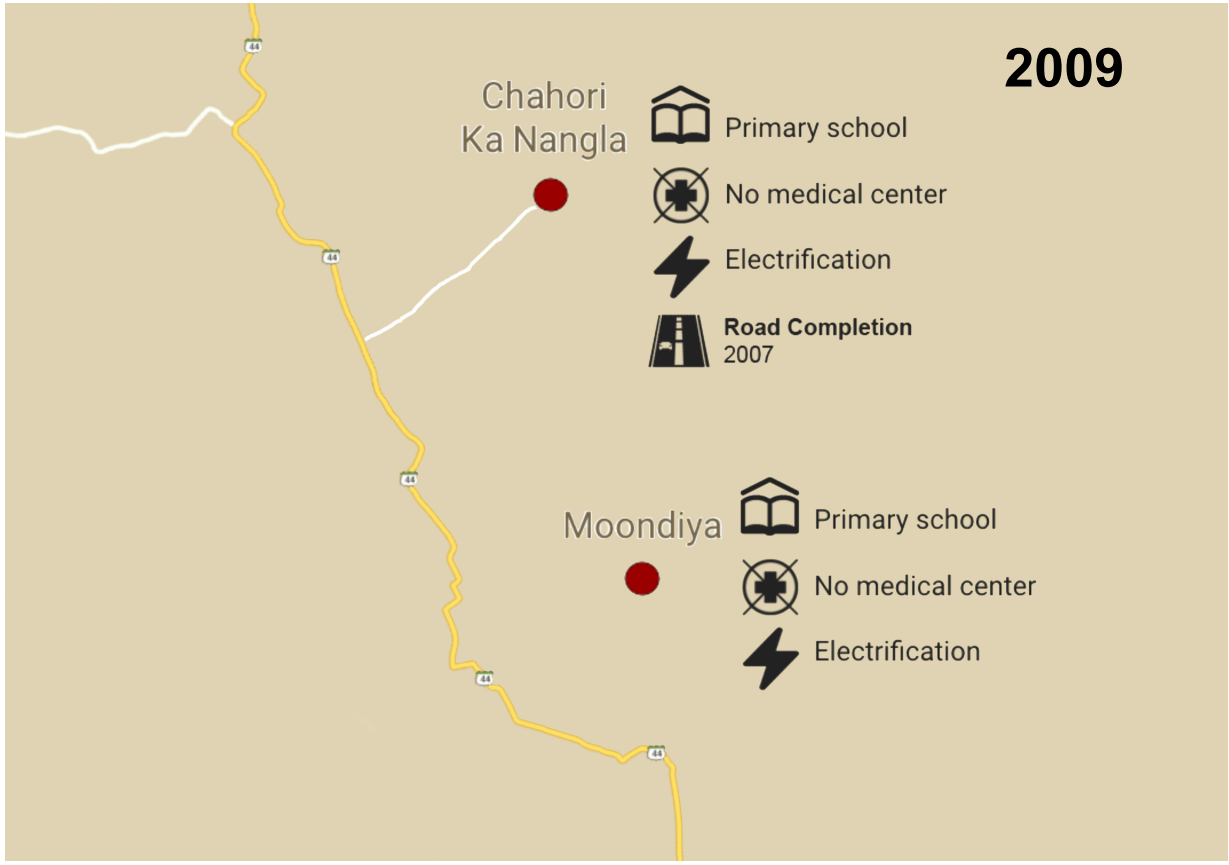

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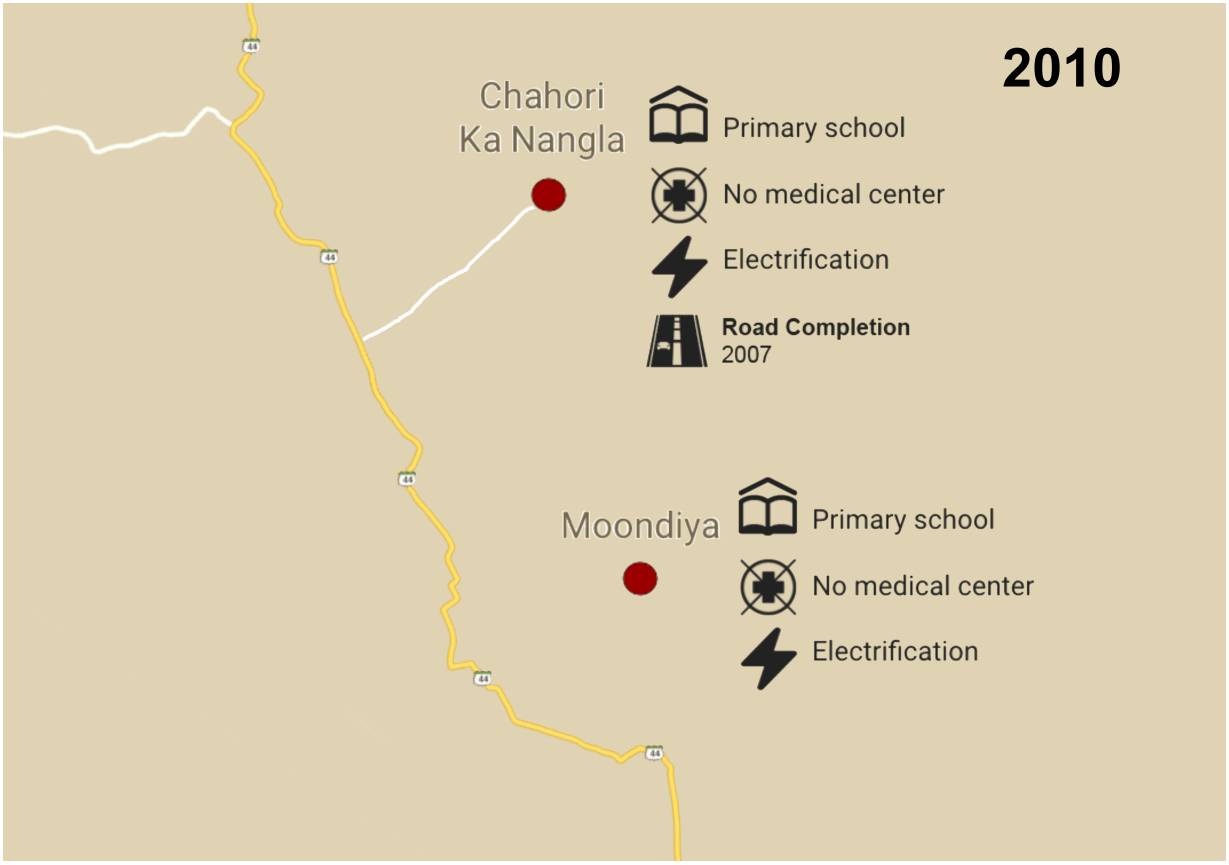


Data

Education: District Information System for Education (DISE)

- Annual school-level census data
- Student enrollment, by class, age, sex, grades 1-8 (2002-11)
- Middle-school exam completion and passing (2004-09)
- School infrastructure
- Data aggregated to village level
  - Fuzzy-matched, geocoded names of 300,000+ villages

Roads: Road-construction administrative data

- Matched by name, geocoded to villages
- Road completion date

Sample

Data: Other Characteristics

National Sample Survey (NSS): Employment and Unemployment Survey (1999-2000, 55th Round)

- District-level rural and urban wages

Population Censuses of India (1991, 2001, 2011)

- Village population, demographic data

Economic Census (1998)

- All non-farm establishments, sectors, number of workers (village-level)

Below Poverty Line (BPL): Census of rural assets (2002)

- Baseline village characteristics (household assets, individual education and occupation)

Empirical Strategy

Panel diff-in-diff, exploiting timing of road construction:

$$Y_{vst} = \beta \cdot ROAD_{vst} + \gamma_{st} + \eta_v + \epsilon_{vst}$$

- Outcome Y : Enrollment, exam completion and passing
- Village v, State s, Year t
- Presence of a road $ROAD_{vst}$
- Village fixed effects: unobserved village factors that may have influenced timing of road construction; remove level differences between early- and late-treated places
- State-by-year fixed effects: time-variant state-level policies and regional economic changes; state policies, regional shocks

β reports the average effect of a village having a road on outcomes across all students

- Unbiased if timing of other programs not highly correlated with timing of road construction

Estimation Notes

Empirical Challenges: Endogenous road placement

- Political and economic determinants of investment
- OLS biased if roads targeted to growing or lagging villages

Regression Details

- Data aggregated to village level
- Balanced panel of villages
- Limit sample to places with no road in 2002, road in 2011
- Cluster standard errors at village level

Robustness: Regression Discontinuity

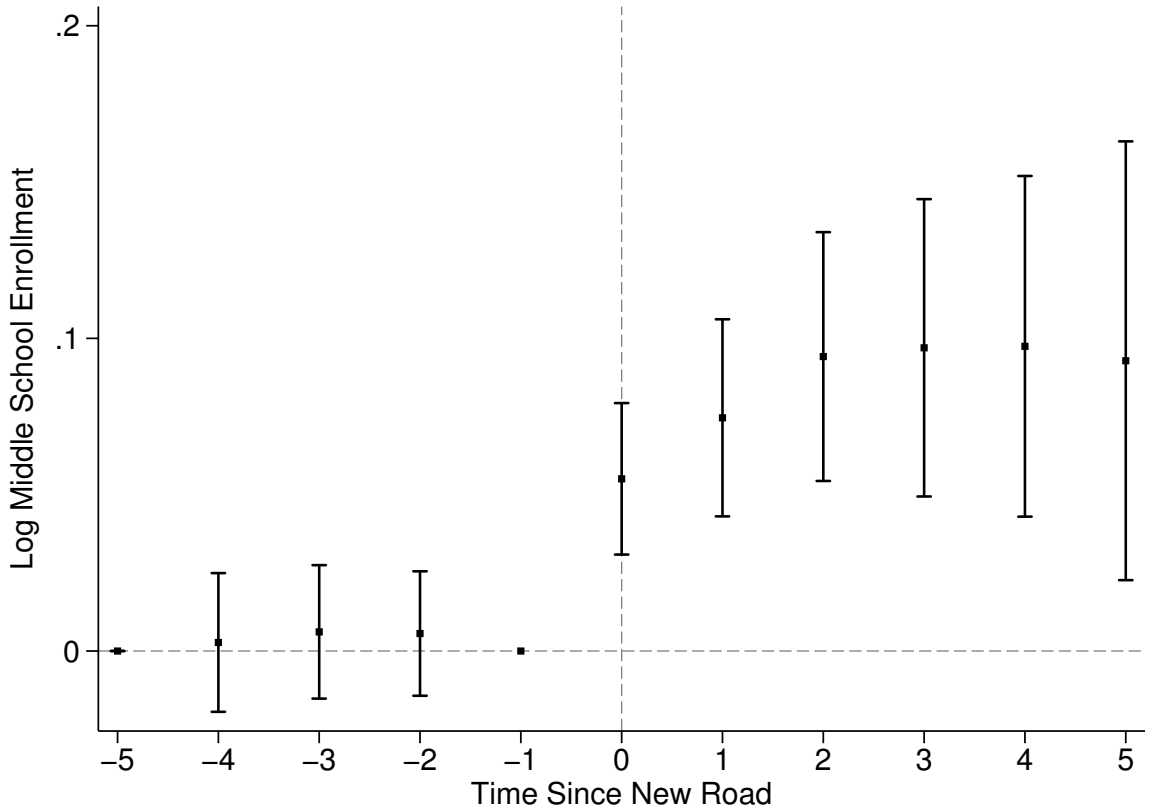
- Valid under fewer assumptions, but underpowered

Summary Statistics

	Mean (SD)
Population (2001 Census)	1,291.4 (998.3)
Non-farm Employment (1998 Economic Census)	60.1 (173.8)
Number of Schools	1.7 (2.0)
Total Enrollment (Grades 1-8)	217.1 (389.0)
Total Primary Enrollment (Grades 1-5)	178.0 (286.8)
Total Middle Enrollment (Grades 6-8)	39.1 (125.6)
Middle-School Exam Passers (2005)	7.3 (15.4)
Exam Passers with Distinction (2005)	1.5 (5.4)

Results

Log Middle-School Enrollment, Relative to Year of Road Construction



The figure shows coefficient estimates from a panel regression of log middle-school enrollment on a set of indicator variables noting the number of years before or since a road was constructed, along with a set of state-by-year fixed effects and village fixed effects. Year 0 is the first year in which a road was present when enrollment data were collected on September 30. Years t = -1 and t = -5 are omitted, following Borusyak and Jaravel (2017).

Results: Increased Middle-School Enrollment

	Log Enrollment (1)	Level Enrollment (2)
New Road	0.070***	2.558***
	(0.015)	(0.537)
State-Year F.E.	Yes	Yes
Village F.E.	Yes	Yes
Panel Sample	Balanced	Balanced
N	146,678	146,678
R2	0.80	0.79

$^{*}p < 0.10,^{**}\ p < 0.05,^{***}\ p < 0.01$

Results: Increased Enrollment, by Student Gender

Dependent Variable	Girls (log)	Boys (log)	Girls (levels)	Boys (levels)
	(1)	(2)	(3)	(4)
New Road	0.060***	0.056***	1.331***	1.227***
	(0.012)	(0.013)	(0.287)	(0.284)
N	146,678	146,678	146,678	146,678
R2	0.81	0.80	0.77	0.78

$^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$

Results: Log Middle-School Enrollment, Robustness

	(1)	(2)	(3)	(4)	(5)	(6)
New road	0.058	0.058	0.086	0.078	0.053	0.041
	(0.012)	(0.014)	(0.013)	(0.013)	(0.013)	(0.009)
State-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Village fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Village time trends	Yes	No	No	Yes	No	No
Baseline variables × year dummies	No	Yes	No	No	No	No
Panel sample	Balanced	Balanced	Unbalanced	Unbalanced	Balanced post-2004	4 years pre/post
Observations	146,678	142,748	237,281	237,281	115,247	148,910
R^2	0.91	0.83	0.76	0.88	0.87	0.84

- Village time trends may pick up effects of the new road over time (Wolfers 2006)
- Time-varying village characteristics not driving effects
- Unbalanced sample includes villages with missing data in ≥ 1 year

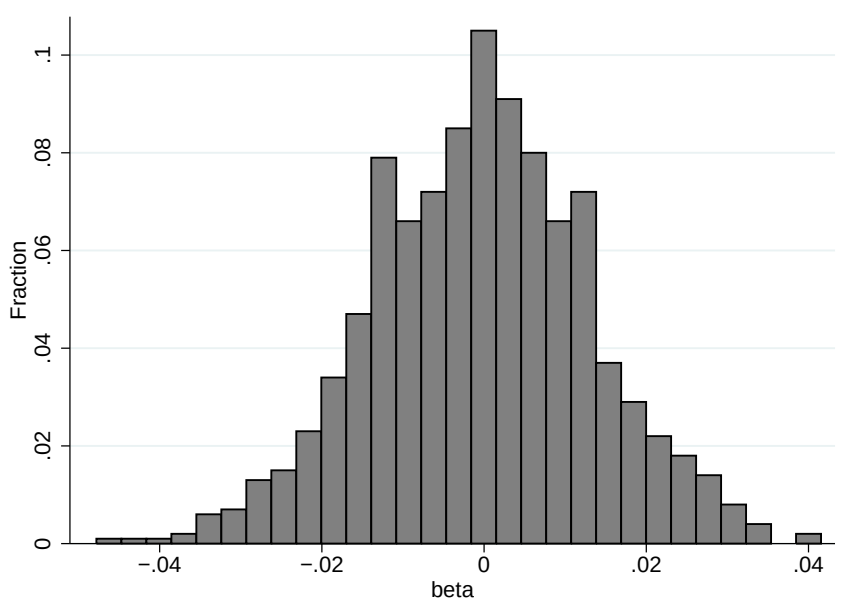
Permutation Test

Run a randomization test to verify that p-values are estimated correctly

- In the spirit of a Fisher Randomization Test
  - Randomly relabel treatment status of observations and recalculate test statistics
  - Calculates distribution of estimated treatment effects under the null hypothesis

- Analogous application to road treatment
  - Randomly generate a placebo year of completion for each village
  - Calculate test statistic as in main specification, 1000 times
    - Estimate equation as if placebo year were the treatment year

Permutation Test



- Distribution of $\beta_{placebo}$: the placebo impacts of a new road
  - Non-parametric distribution of test statistics under H_0, with existing data
- Placebo estimates centered around zero
- None of the thousand estimates attains our main estimate (7% increase)
- Consistent with our finding of a p-value of less than 0.001 for the main estimate

Robustness: Regression Discontinuity

Robustness: Regression Discontinuity (RD)

Exploit eligibility criteria of program: 1000, 500 population cutoffs

- Identification comes from discontinuities in treatment probabilities
- Fuzzy RDD due to other rules, imperfect compliance
  - Politicians could request getting a road
  - Villages that were close to each other could clump together to have a larger population
  - Left-Wing Extremist areas were supposed to be given priority
- Estimate local average treatment effect for compliers
- Pool cutoffs for power

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- Sample: Limit to states that followed the rules:
  - Chhattisgarh, Gujarat, Madhya Pradesh, Maharashtra, Odisha, Rajasthan

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- Sample: Limit to states that followed the rules:
  - Chhattisgarh, Gujarat, Madhya Pradesh, Maharashtra, Odisha, Rajasthan
- Placebo test of states that did not follow program guidelines:
  - Andhra Pradesh, Assam, Bihar, Jharkhand, Uttar Pradesh, Uttarakhand

Validity of regression discontinuity design

Perform standard tests to establish validity of methodology

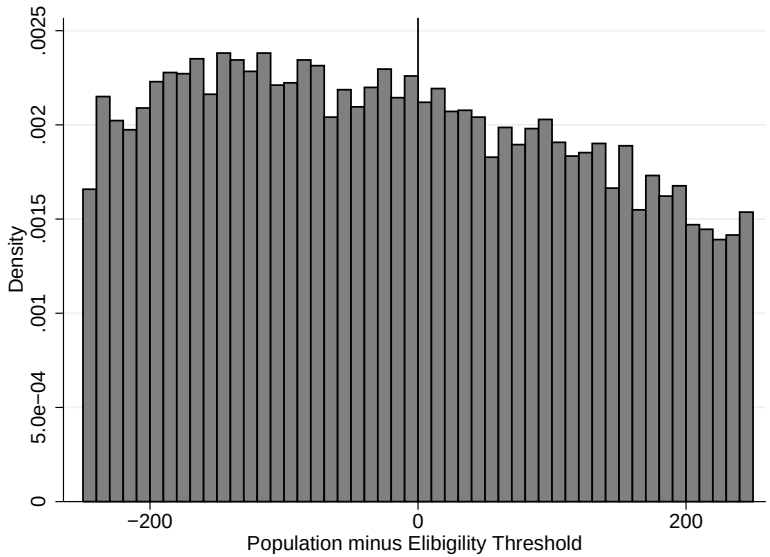
- No discontinuity in running variable at threshold
- Strong discontinuity in treatment probability

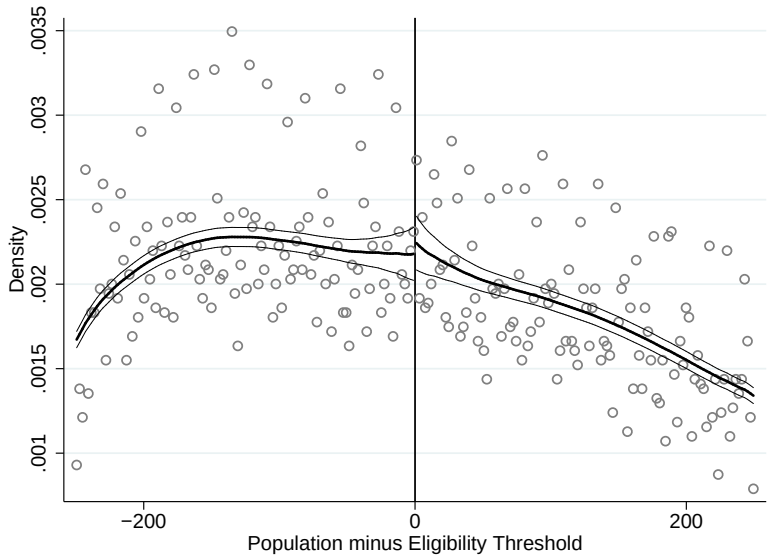
Notes on RD approach

- Reliance on few assumptions for causal inference
- Restricts sample to villages close to threshold in compliant states
  - Lower precision of estimates
  - Estimates less representative of impacts across India

Running variable smoothness

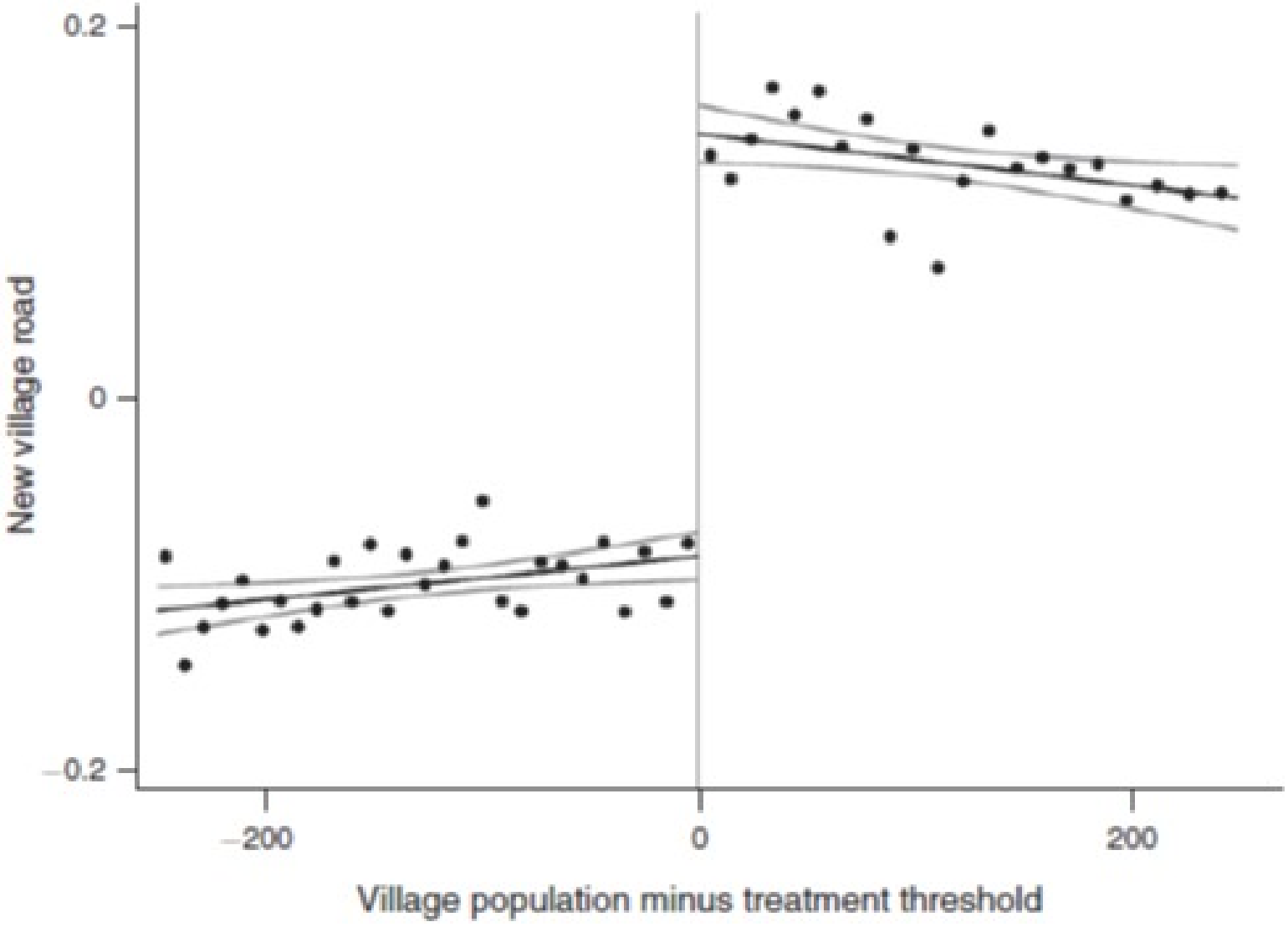
- RD test: density of running variable should be continuous at treatment threshold



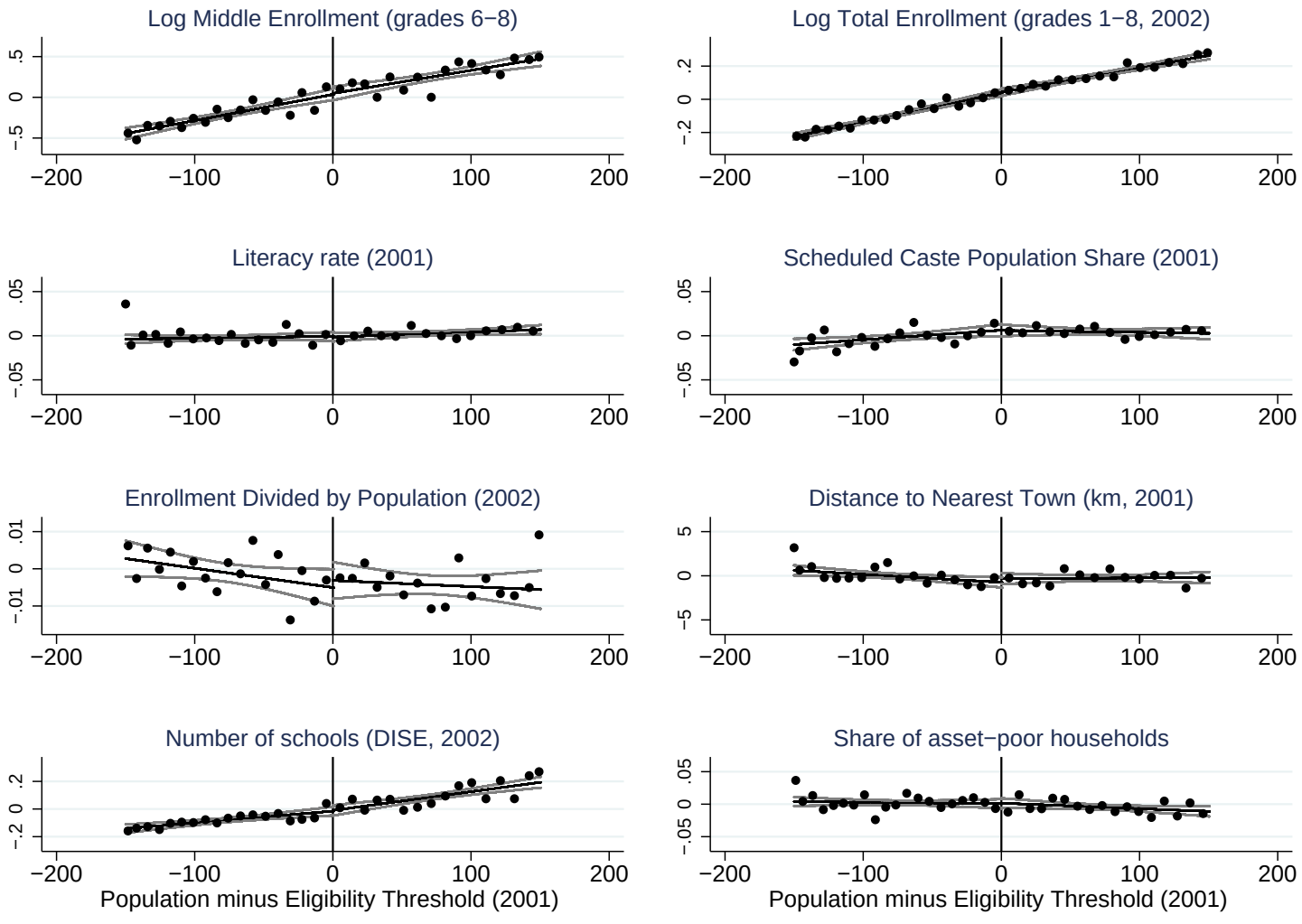


Regression Discontinuity First Stage

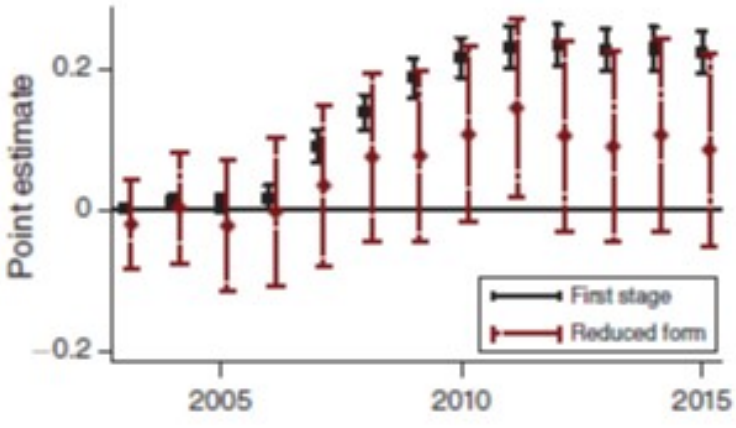
- Strong discontinuity in treatment probability

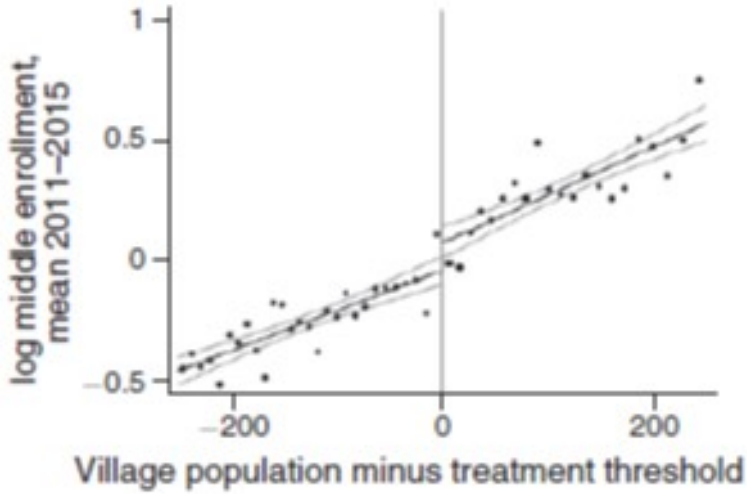


Validity of Regression Discontinuity



RD Reduced Form and First Stage: Log Middle-School Enrollment





RD Results: Log Middle-School Enrollment

Panel A: RD Estimates

	First Stage	Reduced Form	IV
	(1)	(2)	(3)
Above Population Threshold	0.239***	0.108*	
	(0.015)	(0.066)	
New Road by 2011			0.450
			(0.276)
N	55,271	55,271	55,271
R2	0.26	0.28	0.28

Panel B: Placebo RD Estimates

	First Stage	Reduced Form
	(1)	(2)
Above Population Threshold	0.014	0.009
	(0.011)	(0.059)
N	56,219	56,219
R2	0.27	0.25

$^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$

Results: Exam Performance

	Exam Taken	Exam Passed	High Exam Score
	(1)	(2)	(3)
New Road	0.060***	0.058***	0.035***
	(0.019)	(0.019)	(0.014)
State-Year F.E.	Yes	Yes	Yes
Village F.E.	Yes	Yes	Yes
Panel Sample	Balanced	Balanced	Balanced
N	32,239	32,239	32,239
R2	0.73	0.72	0.61

$^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$

- Dependent variable: log number of students with outcome (in heading)
- Education interventions that increase enrollment often do not impact test scores (Miguel and Kremer 2004; Behrman et al. 2008; Adukia 2017)
- Reflect net impact of achievement
  1. Marginal students (those induced to stay in school) pass exams at same rate as non-marginal students (but receive slightly fewer top grades). No effect on non-marginal students.
  2. Marginal students do worse on exams (perhaps due to negative selection in terms of ability). Inframarginal students perform better (perhaps due to perceived increase in education value)

Treatment Heterogeneity

Treatment Heterogeneity

Does the model predict treatment heterogeneity?

Strategy

- Identify districts where each comparative static is expected to be especially prominent, based on regional characteristics
- Underlying assumption: Factor price equalization
  - District-level rural-urban gaps predict wage / return to education will change when a road is built

Treatment Heterogeneity: Predictions

1. High uncredentialed urban-rural wage gap:
   - New road increases uncredentialed rural wages $\rightarrow$
   - Opportunity cost effect will be large $\rightarrow$
   - Smaller enrollment effect

2. High urban-rural returns-to-education gap:
   - New road increases returns to education $\rightarrow$
   - Returns to education effect will be large $\rightarrow$
   - Larger enrollment effect

Treatment Heterogeneity in Effects of New Roads

	(1)	(2)	(3)	(4)
New Road	0.075***	0.121***	0.059**	0.105***
	(0.017)	(0.025)	(0.024)	(0.030)
New Road * High Opportunity Cost		-0.090***		-0.090***
		(0.034)		(0.034)
New Road * High Return to Ed			0.031	0.032
			(0.034)	(0.034)
State-Year F.E.	Yes	Yes	Yes	Yes
Village F.E.	Yes	Yes	Yes	Yes
Panel Sample	Balanced	Balanced	Balanced	Balanced
N	113,960	113,960	113,960	113,960
R2	0.80	0.80	0.80	0.80

$^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$

Unbalanced Panel Quartile Results

Treatment Heterogeneity in Effects of New Roads

Opportunity Cost Effect (Urban minus Rural Uncredentialed Wage Gap)	Returns-to-Education Effect (Urban minus Rural Skill/Credential Premium)	Treatment Effect	Number of Villages
High	Low		2319
High	High		2320
Low	Low		4435
Low	High		2322

*p<0.10, **p<0.05, ***p<0.01

Treatment Heterogeneity in Effects of New Roads

Opportunity Cost Effect (Urban minus Rural Uncredentialed Wage Gap)	Returns-to-Education Effect (Urban minus Rural Skill/Credential Premium)	Treatment Effect	Number of Villages
High	Low	0.010	2319
		(0.033)	
High	High	0.051	2320
		(0.033)	
Low	Low	0.110***	4435
		(0.035)	
Low	High	0.132***	2322
		(0.034)	

*p<0.10, **p<0.05, ***p<0.01

Other Potential Mechanisms

School Quantity, Quality

- Could changes in school quality or quantity be influencing the changes in enrollment?
- Similar estimates when weighting variables by number of enrolled students

Dependent Variable	Balanced Panel	Unbalanced Panel	RD
Piped Water	0.001	0.002	0.005
	(0.004)	(0.003)	(0.007)
Toilet	0.003	0.016***	0.000
	(0.005)	(0.004)	(0.008)
Electricity	0.003	0.004**	-0.002
	(0.002)	(0.002)	(0.006)
Library	0.000	0.006	0.004
	(0.005)	(0.004)	(0.009)
Computer	-0.004**	-0.002	0.001
	(0.002)	(0.002)	(0.004)
Perimeter Wall	0.001	0.002	0.005
	(0.004)	(0.003)	(0.009)
Playground	0.009**	0.007*	0.011
	(0.004)	(0.004)	(0.009)
Log Number of Schools	0.000	0.001	0.006
	(0.000)	(0.002)	(0.005)

*p<0.10, **p<0.05, ***p<0.01

Testing for out-migration

Concern

- Estimates are gross enrollment, hence include migration

Approach

- Estimate impact on primary-school enrollment
- Estimate impact on village population shifts

Primary School Effects

- Migration effects should equally affect families with primary-school-aged children
- No impact on enrollment of younger children

		Panel		Reduced Form	IV
	Main Specification	Village Time Trends	Unbalanced Panel		
	(1)	(2)	(3)	(4)	(5)
New Road	-0.005	-0.004	-0.005		0.033
	(0.004)	(0.003)	(0.005)		(0.086)
Above Population Threshold				0.008	
				(0.020)	
N	146,678	146,678	237,281	66,663	66,663
R2	0.87	0.92	0.88	0.30	0.30

$^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$

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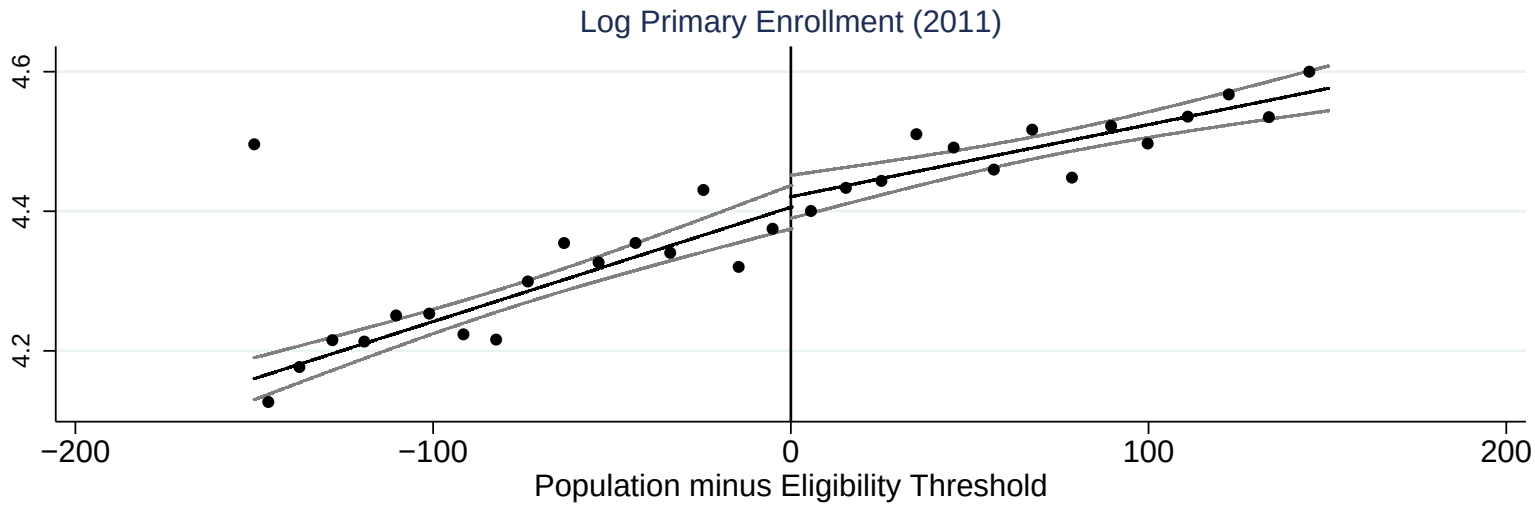
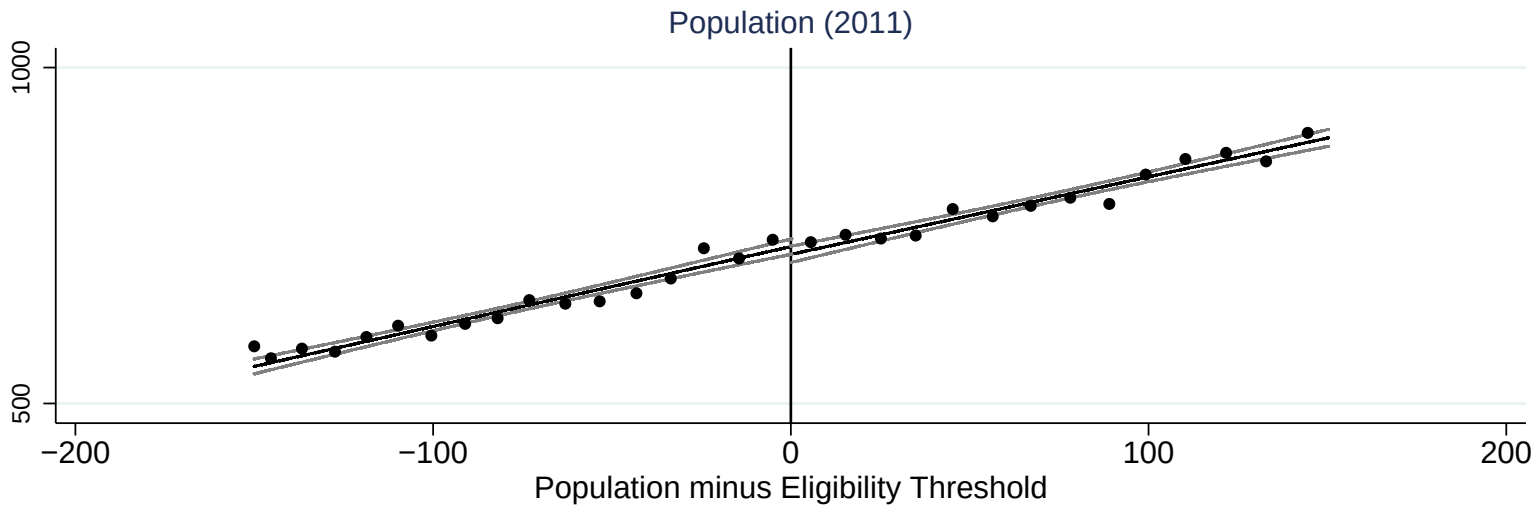
- Note: small increases in primary school performance, suggesting that students may be increasing effort on the intensive margin

Primary-School Effects, Migration Effects (RD)





- Rule out net entry/exit of > 4 people from a treated village

Not Driven By Other Government Programs

- Does not appear that other public goods or services (e.g. schools, electricity, health centers, banks) were delivered simultaneously with new roads

Dep. Var.	Prim. School	Mid. School	Sec. School	Electricity	Health Center	Bank
	(1)	(2)	(3)	(4)	(5)	(6)
Above Population	-0.008	0.012	-0.001	0.016	0.002	0.002
Threshold	(0.005)	(0.013)	(0.006)	(0.013)	(0.002)	(0.002)
N	16,973	16,973	16,973	16,973	16,973	16,973
R2	0.37	0.32	0.15	0.36	0.09	0.08

$^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$

Spatial Effects

Are roads just displacing students from other schools?

- Estimate impact on enrollment close to village, *excluding* treated village
- If true, expect enrollment increases in treated village to be counterbalanced by enrollment declines in nearby villages

	Spillovers	
	3km	5km
New Road	0.006	0.013
	(0.017)	(0.014)
N	114,240	117,270
R2	0.90	0.89

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School Accessibility

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- More dispersed villages (village surface area)
  - Children in more dispersed villages have further to walk to school and may be expected to benefit more
- Villages close to other villages without middle schools
  - The road could make it easier for children in a different village to access middle school

	Village Area		Nearby Eligible Kids	
	Low	High	Low	High
New Road	0.072***	0.063***	0.057***	0.059***
	(0.015)	(0.014)	(0.016)	(0.015)
N	90,358	86,566	76,513	76,515
R2	0.76	0.76	0.75	0.75

$^{*}p < 0.10,^{**}\ p < 0.05,^{***}\ p < 0.01$

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Conclusion

Educational investment responses to domestic market integration can have important implications for long-run economic growth

Theory ambiguous

- Positive labor demand can increase or lower schooling

On average, improved access to regional markets via rural road construction motivates increases in schooling

- Increases in adolescent schooling (6-7%)
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Treatment heterogeneity consistent with classical human capital model

- Positive effects (statistically detectable) in 59% of sample
  - $\downarrow$ Opportunity cost, $\uparrow$ Returns to education

Effects do not seem to be driven by changes in:

- Migration, other government programs, access to school, changes in school quality

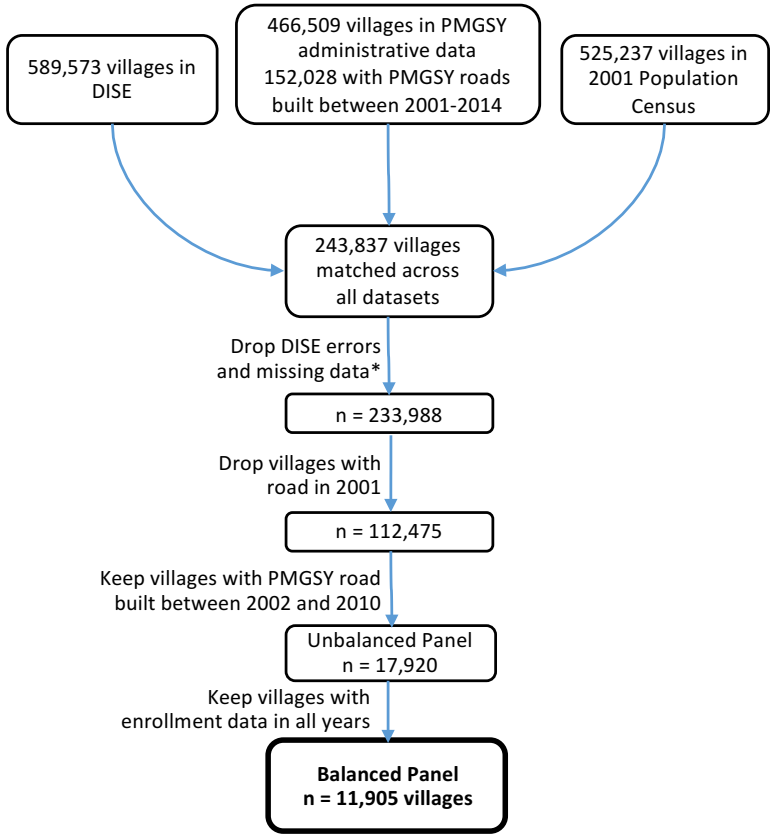
Thank you!

Urban vs. Rural Wages and Mincerian Returns to Education

	Rural	Urban
Uncredentialed Wage	43.6	73.3
	(0.2)	(0.5)
Credentialed Wage	114.3	166.0
	(0.9)	(0.8)
Return to Education	0.068	0.080
	(0.001)	(0.001)
Sample Size	46,120	34,024

The table shows mean wages and returns to education from the 55th round of the NSS Employment and Unemployment Survey (1999-2000), separately for urban and rural areas. Wages are daily wages in Indian Rupees (in 1999, approximately 59 INR = 1 USD); the Mincerian return is a regression of log wages on age, age squared, and log of household land. An individual is considered credentialed if he or she has attained middle school or higher. Standard errors of means are shown in parentheses. Theory

Sample Construction



The figure shows how we arrived at our final number of observations from the original datasets. DISE = District Information System for Education. PMGSY = Prime Minister's Road Building Program. All observation counts indicate number of villages at each stage.
*Observations were dropped if DISE reported grade one to eight enrollment greater than 60% of village population (99^{th} percentile), year-on-year enrollment growth outside of interval (-73%, +270%) (99^{th} percentile), or zero enrollment in all years. State-years were dropped if DISE reported enrollment for fewer than 25% of villages (Jharkhand 2005, Karnataka 2005, Uttarakhand 2006).

Treatment Heterogeneity: Unbalanced Panel

	(1)	(2)	(3)	(4)
New Road	0.075***	0.130***	0.060***	0.115***
	(0.014)	(0.020)	(0.020)	(0.025)
New Road * High Opportunity Cost		-0.106***		-0.106***
		(0.028)		(0.028)
New Road * High Return to Ed			0.030	0.029
			(0.028)	(0.028)
State-Year F.E.	Yes	Yes	Yes	Yes
Village F.E.	Yes	Yes	Yes	Yes
Panel Sample	Unbalanced	Unbalanced	Unbalanced	Unbalanced
N	171,637	171,637	171,637	171,637
R2	0.76	0.76	0.76	0.76

$^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$

Main Treatment Heterogeneity

Treatment Heterogeneity: Quartile Results

Panel A: Wage Gap Quartiles

	(1)	(2)	(3)	(4)
New Road	0.144***	0.100***	0.033	0.015
	0.026	0.021	0.021	0.024
N	28,190	27,990	31,550	26,230
R2	0.76	0.82	0.81	0.79

Panel B: Return Gap Quartiles

	(1)	(2)	(3)	(4)
New Road	0.060***	0.092***	0.080***	0.101***
	0.021	0.026	0.022	0.023
N	29,910	27,070	27,900	29,080
R2	0.81	0.77	0.81	0.80

$^{*}p < 0.10,^{**}\ p < 0.05,^{***}\ p < 0.01$

Main Treatment Heterogeneity