Cheaper and better? Explaining a newborn mortality advantage at public versus private hospitals in India

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Abstract

Public provision of healthcare may reduce efficiency but can also correct market failures. I study two large Indian states where high mortality rates make effects detectable and weak regulation allows provider incentives to operate without constraint. In these states, public (government-run) clinics and hospitals charge less for care and have poorer patients than private clinics and hospitals—yet, puzzlingly, births there survive at much higher rates. I show that these public facilities reduce the rate of newborn death by over 25 per thousand births, over half the rate of death for private-facility births. I use two complementary empirical strategies. First, to address selection, I estimate the slope of village neonatal mortality with respect to the public–private birth share; if facility type didn't matter, and the observed differences were entirely due to selection, then this slope would be zero. Second, I estimate the spatial discontinuity in mortality at district borders, which exogenously shift public use for otherwise-similar nearby births. I present evidence that the mortality gap operates through interventions that follow separation of mothers and babies, a pattern consistent with pay-per-service incentives that reward additional procedures in private facilities. These results suggest that if private providers treated patients identically to public providers, they would prevent over 37,000 children's deaths each year.

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1 Introduction

Healthcare during labor and delivery affects whether children survive.[1] What care gets provided depends on features such as training, resources, demand, regulation, and provider incentives[2]. Public and private health care differs across these dimensions. In many countries, private facilities typically deliver better outcomes, albeit at higher cost. Whether this pattern holds in developing countries—where regulation is weaker and market failures may be more severe—remains unclear. Observational evidence typically shows private facilities have similar or better outcomes than public facilities [3].

Recent research, including my own, has documented that private hospitals and clinics in rural India deliver worse outcomes for a higher price than public hospitals and clinics (Verma and Cleland 2022; Coffey et al. 2025). Mothers in this area who give birth in private facilities, on average, come from wealthier families and pay twenty times as much for care as those who choose public (government) facilities; however, 5.1% of babies born in private facilities die in their first month, rather than 3.2% born in public facilities.[4] This is consequential for human development: The two states I study have a population larger than the United States and a rate of newborn death exceeded only by Afghanistan, Pakistan, and Nigeria.

In this paper, I show that public facilities reduce newborn mortality compared to private facilities for births to women living in rural Uttar Pradesh and Bihar. The primary difficulty in identifying the public–private causal mortality effect is family-level selection. That is, families who expect a riskier birth may select into private facilities that they believe provide better care than public facilities. An additional challenge is village-level confounding, in which villages that have more births in private facilities may also have worse underlying health.

This paper addresses both identification challenges. First, I develop an econometric model that addresses the problem of family-level selection. The key observation of the model is that, if there is no difference in the effect of public versus private facilities, the mortality rate overall in the village does not depend on the allocation of its births into each facility type; however, in the presence of a causal effect, a village's mortality *does* depend on the fraction born in each facility type. This is clearest in the extreme case—a village would have a different mortality rate if all its births took place in public facilities than if they all took place in private. This model is similar to those presented by Geruso and Layton (2020), Gruber, Levine, and Staiger

[1] See Currie and Gruber (1996), Lazuka (2018), and Okeke (2023).
[2] See McGuire (2000) and Das, Holla, et al. (2016).
[3] See Das and Hammer (2014)
[4] These estimates are based on data from the National Family Health Survey, 2019–21.

(1999), Einav, Finkelstein, and Cullen (2010), and Chetty, Friedman, and Rockoff (2014) in its use of marginal changes in an average to overcome endogenous sorting and identify marginal effects. Using cross-sectional variation across villages to estimate the relationship between overall mortality and fraction born in private can identify the causal effect if family-level selection is the only concern, but not if village-level confounding is also present.

If village-level confounding is a problem, one might expect that villages with a higher fraction of births in private facilities would have characteristics—other than facility choice—that predict worse health outcomes. But I show that, in fact, markers like literacy, wealth, sanitation, energy access, and other characteristics tend to be better, not worse, in villages where private facilities are more often chosen. This is prima facie evidence against unobservables driving both higher mortality and higher private facility use.

To more systematically isolate effects that are purged of village-level confounding, I introduce a district borders regression discontinuity design that uses plausibly exogenous variation in public facility use. This design compares births on either side of the borders between districts that have different fractions born in each facility type. These otherwise-similar births were to mothers who, on average, differ only in the costs[5] of accessing the districts' public facilities. The district-level difference in public facilities predicts discontinuities in public facility use and in mortality right at district borders. I exploit this variation to identify a causal public–private mortality effect for families near district borders whose choice of facility was shaped by where the border happened to fall.

Overall, these two identification strategies point in the same direction: I find that public facilities dramatically reduce newborn mortality compared to private facilities. In regressions using cross-sectional variation to estimate the relationship between mortality and village-level public–private birth share—that is, the effect parameter of the econometric model—I find a mortality reduction of 25 per thousand births. The inclusion of various controls and fixed effects increases the estimate. Using the quasi-experimental regression discontinuity design to estimate the model, I find a mortality reduction of 123 per thousand. These estimates are statistically indistinguishable, but the point estimates increase as confounders are increasingly accounted for.

What explains these effects? I present evidence that private facility birth is more dangerous than public facility birth because private providers are more often performing harmful interventions. Patients don't know what medical care is best, and rely on the recommendations of health care providers. Private providers charge fees per service and so have incentive to recommend and perform interventions, regardless of their medical necessity. For many conditions and in

[5] "Costs" here are broadly construed to include, for example, the difficulties of travel.

many contexts, this type of provider-induced demand is merely wasteful; for newborns in rural India, these interventions are dangerous. To investigate this empirically, I can't use the full list of interventions performed by health care providers during each birth. No such data set exists. Instead, I look at the first step of any of these interventions: separation of the mother and baby.

I present a collage of evidence that separation of the mother and baby—or its correlates, the interventions taking place thereafter—explains the public–private mortality effect. First, private facilities more commonly separate the mother and baby than public facilities. Second, in villages where public and private facilities separate mothers and babies equally often, the gap in mortality disappears. Finally, if the public mortality advantage came from factors other than separation, then that advantage would persist whether or not babies were separated. But stratifying by whether separation occurred reverses the relationship between public facility use and village mortality—babies who were separated from their mothers are less likely to die in places with more private birth, as expected from their predictors of health. So too are babies who were not separated from their mothers. The reversal when conditioning on this single practice suggests it is responsible for the mortality effect.

The size and scale of this effect is very large. According to my central estimates, if private facilities provided public-type care, the yearly number of deaths in these areas would decrease by over 37,000. This change would reduce the neonatal mortality rate of the area by almost 5 per thousand births[6] and the rate of all of India by over 1.5 per thousand[7]. This alone would achieve nearly 20% of the progress India needed in 2020 to achieve the UN Sustainable Development Goal 3.2[8].

This paper contributes to several strands of the economics literature. First, it provides evidence on a fundamental question in public economics: Can the public provision of a private good improve outcomes compared to markets? There is extensive theoretical work (Hart, Shleifer, and Vishny 1997; Besley and Ghatak 2001) and empirical work (Megginson and Netter 2001; Galiani, Gertler, and Schargrodsky 2005) on this question as applied to different goods, markets, and governments. This paper is the first to provide causal evidence that public provision of health care reduces children's mortality compared to private health care in a developing country.

Second, it contributes to the literature on provider agency, which observes that patients and

[6] 25.6 deaths averted per thousand births in rural UP and Bihar × 19.4% born in private in rural UP and Bihar = 4.97 per thousand reduction in rural UP and Bihar; 4.97 × 7,700 thousand births in rural UP and Bihar = 37,453 deaths averted

[7] 25.6 deaths averted per thousand births in rural UP and Bihar× 19.4% born in private in rural UP and Bihar × 31.9% born of Indian births in rural UP and Bihar = 1.58 per thousand reduction in India

[8] 1.58 per thousand reduction in India / (20.18 per thousand rate in India in 2020 – 12 per thousand rate goal) = 19.4% of progress toward goal (all-India rate from the Inter-agency Group for Mortality Estimation)

health care providers are in a principal–agent problem (McGuire 2000). Patients who don't know what medical services would serve them best rely on recommendations from providers whose incentives don't perfectly align with the preferences of their patients (Clemens and Gottlieb 2014; Einav, Finkelstein, and Mahoney 2018; Donato et al. 2017; Lagarde and Blaauw 2022; Gruber, Kim, and Mayzlin 1999; Currie, Lin, and Meng 2014; Cohen, Dupas, and Schaner 2015; Alexander 2020). However, the question of whether this type of provider-induced demand is harmful in practice, not merely wasteful, is unclear (Garber and Skinner 2008; Doyle et al. 2015). This paper provides the first causal evidence that provider-induced demand for health care increases children's mortality.

Third, this paper contributes to the literature on the effects of giving birth in *any* health facility. Prior research finds mixed evidence on the mortality effects of facility birth. Several studies find reductions in mortality (Gruber, Hendren, and Townsend 2014; Cesur et al. 2017; Okeke 2023). Others find no reduction in mortality (Godlonton and Okeke 2016); in the context of India, Powell-Jackson, Mazumdar, and Mills (2015) and Andrew and Vera-Hernández (2024) show no causal evidence of a reduction in neonatal mortality and even an increase in perinatal mortality. This paper rationalizes these disparate effects by showing that what happens in facilities matters, not merely whether births happen in facilities at all.

2 Puzzle: Richer mothers pay more for natal care with worse outcomes

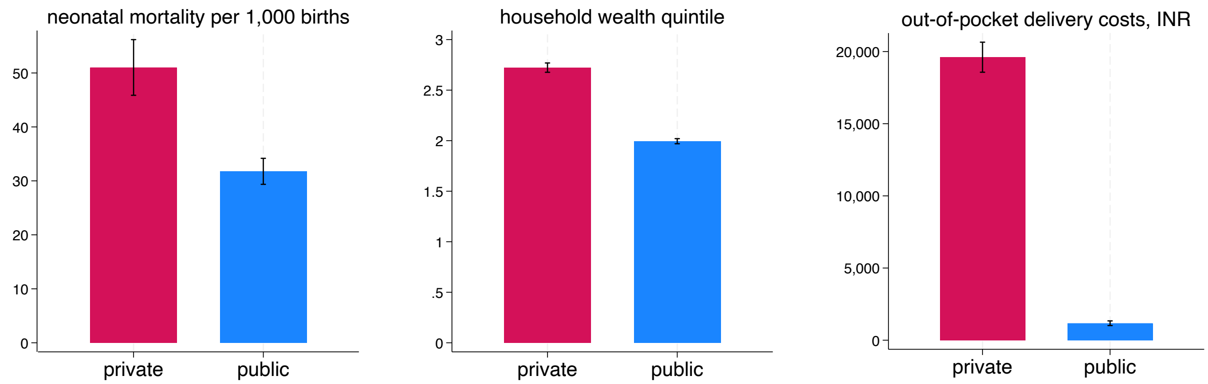
Figure 1 presents the puzzle that motivates this paper. Neonatal mortality—death in the first month of life—is much more common in private than in public facilities in rural Uttar Pradesh and Bihar. This is even though, as Panels b and c show, babies born in private facilities in this context come from richer households and the costs for their natal care are twenty times as high, on average.

Patients at public and private facilities are different in ways beyond their mortality rates and wealth. Panel a of Appendix Table A1 shows the same pattern persists: mothers of babies born in private facilities are younger, taller, less underweight, though more anemic. They have fewer children and are more likely to be literate. They are less likely to live in a household that is part a marginalized social group or reports practicing open defecation.

How else are public and private facilities different, beyond their mortality rates and the costs to their patients? While not a representative sample of health facilities, the India Human Development Survey-II (IHDS-II) sheds some light on this question. The IHDS-II surveyed

Figure 1: The motivating puzzle—neonatal mortality is significantly lower and care is cheaper in public facilities

(a) Births in private facilities die more, (b) come from richer households, (c) and have higher-cost natal care



Notes: The figure displays bar charts of means and standard errors outlining a mortality puzzle. Data are from the NFHS-5. Observations are births aged 1–59 months that were born in a health facility to women living in rural areas of Uttar Pradesh and Bihar at the time of interview. Means and standard errors are calculated according to the survey design: survey-weighted and clustered at the village level.

approximately one public and one private primary health care facility from each primary sampling unit, yielding 385 facilities surveyed in rural UP and Bihar in 2010–11. Of those, 67% of the public facilities and 9% of the private facilities report providing childbirth services. Panel b of Appendix Table A1 shows that, among those that provide natal care, private facilities tend to have less staff, less-educated staff, and fewer resources than public facilities.

Thus, observationally, public care has better outcomes than private care, but the question of whether public facilities causally reduce neonatal mortality compared to private facilities remains open. I explore this question in the rest of the paper.

3 Data

This paper primarily uses data from the two most recent Demographic and Health Surveys of India. These nationally representative surveys of India are known as the National Family Health Survey 2015–2016 (NFHS-4) and 2019–2021 (NFHS-5). They record responses from interviews with household members about the demographics and asset ownership of the household as well as health behavior and outcomes of women and children in those households. The surveyors also measure the location of each sample cluster they interview with a small random displacement within district. This study only uses observations from rural villages of Uttar Pradesh and Bihar.

I use NFHS sampling weights in all descriptive statistics and regressions to reflect unequal selection probabilities and nonresponse. At the birth level, I weight each summary statistic or regression by the survey weight given by each survey. If a design adds extra weights (kernel weights in the RD), I multiply them by the survey weights. When I aggregate to the village level, I compute village means as weighted averages of births and then run village-level regressions with the sum of the birth-level weights. I cluster standard errors at the primary sampling unit and keep this clustering level fixed across all specifications.

The primary outcome for this study is neonatal mortality, which I construct from mortality data based on comprehensive birth histories of women aged 15–49 at the time of survey. I define neonatal deaths as those reported during the first month of life[9], excluding those births that were born less than a month before or more than 59 months before the survey.

The primary explanatory variable I investigate in this paper is whether a birth took place in a public health facility or a private health facility. This is collected for births that took place within the five years preceding the survey. Facility birth is now the norm in India, but home birth is still practiced. I exclude these births from the analysis, though their inclusion does not substantially alter any of the empirical results of the paper.

I examine several care practices as possible mechanisms for the public–private differences in neonatal mortality. Specifically, I construct binary measures for (i) ambulance transport to the delivery facility, (ii) interaction with a community health worker during pregnancy, (iii) cesarean delivery, (iv) separation of the baby and mother immediately after birth, (v) immediate initiation of breastfeeding, and (vi) adequate antenatal care, defined as three or more antenatal care visits. Apart from cesarean delivery, these care practices have sample restrictions compared to the sample with facility type measured. The question for separation at birth was only asked in the NFHS-5, so it has about half the sample size of other variables; and the remaining indicators are only measured for the most recent birth.

This paper uses a border regression discontinuity design, with the distance to the district border as a running variable. I use village geographical coordinates and district administrative boundaries from the DHS Spatial Data Repository for each survey to construct the straight-line distance to the nearest point on the border. Some district boundaries changed from NFHS-4 to NFHS-5. Appendix A details the process for creating comparable areas in those cases.

[9] This differs from the standard definition of 28 days. Mortality risk decreases rapidly during the first month of life, with a marginal 1 to 3 days making little difference.

4 Econometric model: village-level mortality as a function of birth-mix

4.1 The model

The central empirical problem this paper addresses is whether the observed difference in neonatal mortality can be attributed to a public–private mortality effect rather than to selection of riskier births into one facility type. In this section, I outline the model I use to separately identify the causal effect and selection. I also discuss a graphical depiction of the model presented in Figure 2.

First, consider a village in which all births take place in facilities, with some fraction in private and the rest in public. As the fraction born in private increases from zero to one, it traces three mortality curves: the mortality rate for those born in public, the rate for those born in private, and the village-level mortality rate. Note that the facility-type mortality curves are subject to selection, but the overall mortality rate is not. A neonatal death is counted in the overall mortality rate, regardless of the delivery facility type.

In the absence of a public–private mortality effect, the village-level mortality rate should be constant across different fractions born in each facility type. This is easiest to see when considering the extreme points: If the mortality rate is the same when all births take place in public facilities and when all births take place in private facilities, then there can be no net mortality effect. However, if the mortality rate is higher when all births are private than when all are public, then there must be a public mortality advantage the size of that difference in mortality rates. For estimation purposes, it is useful to note that the public–private mortality effect is also the slope of the overall mortality line (rise: public–private mortality effect; run: 1).

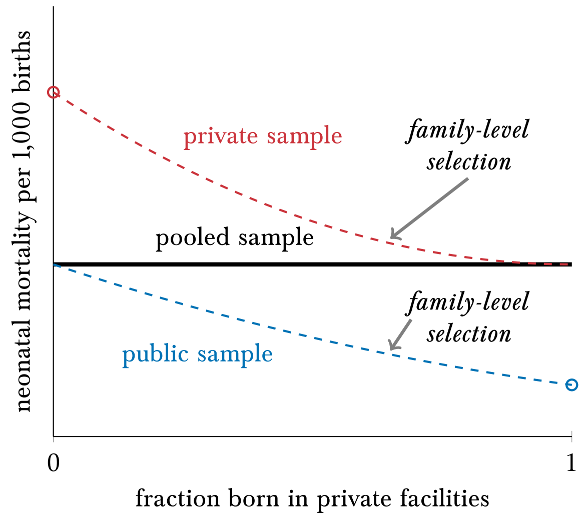
This same logic holds not just at the extreme points, but across all fractions born in private if the additive public–private mortality effect doesn't vary across marginal births. Consider the highest-risk births handled at a public facility. Assume that these frequently end in an infant death. If these high risk births were counterfactually moved from a public facility to a private facility then the death rate at the public facility would fall. The death rate at the private facility would also change[10], so average death rates at both facility types would change. But note that in the absence of a causal effect, the village-level death rate would not change by this reshuffling of risk across facilities. See Appendix B for a detailed proof.

Figure 2 depicts the model visually in graphs of neonatal mortality versus the fraction born

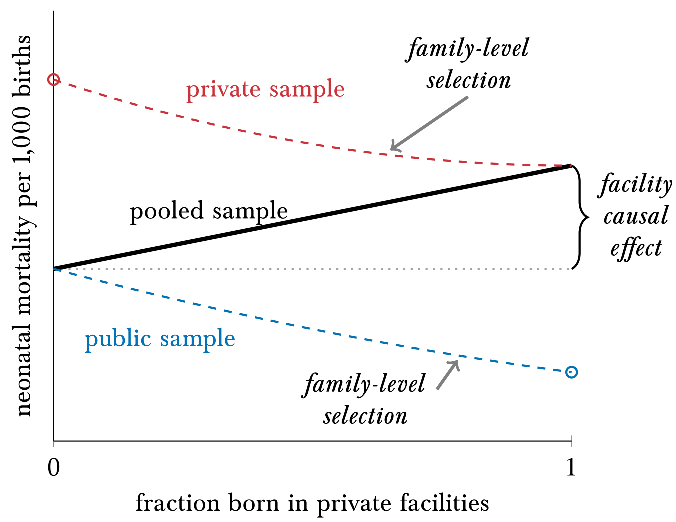
[10]The direction could be up or down, depending on whether the new tranche of births were higher or lower risk than the existing average.

Figure 2: Econometric framework—slope of overall mortality line identifies causal effect

(a) Selection, **without** facility-type causal effect



(b) Selection **with** facility-type causal effect



Notes: The figure displays graphical versions of the econometric model this paper employs to identify the causal effect of public versus private facility natal care. Each panel presents neonatal mortality as a function of the fraction of a village's births that take place in private facilities.
Panel a shows a scenario in which there is selection into facilities, since the blue "private births" line and the red "public births" line don't overlay the black "all births" line. However, there is no causal effect, since the black line has zero slope.
Panel b shows a scenario in which there is selection, but there is also a harmful mortality effect of being born in private facilities. The slope of the black line, or equivalently the difference between the mortality rates when all births are in private and when all births are in public, identifies the facility-type causal effect.

in private facilities. As the fraction born in private facilities increases, marginal births shift from public facilities to private facilities.

In Panel a, there is selection of higher-risk births into private facilities, as shown by the private mortality curve (blue dashed line) being higher than the all-births curve and the public births curve[11]. The marginal birth shifting into private has higher risk than the births remaining in public. In Panel b, there is again selection, but there is also a facility causal effect, as shown by the vertical distance between the mortality rate when all births take place in public facilities and the mortality rate when all are private. This is also equal to the slope of the all-births mortality curve.

4.2 Estimation with cross-sectional village-level variation

The model discussed above refers to a particular village's neonatal mortality rate as a function of the fraction of its births that take place in each facility type. If family-level selection is the only avenue of confounding, then I can use variation across villages to estimate the model. In fact, village-level confounding appears to work in the *opposite* direction of the effect I identify, as I later investigate empirically.

Figure 3 shows a local polynomial regression with the same structure as the explanatory Figure 2. On the horizontal axis is

$$\overline{private}_v = \frac{\text{count of births in } \textbf{private} \text{ facilities in the last 5 years}}{\text{count of births in facilities in the last 5 years in the baby's village}},$$

which is calculated for each village. On the vertical axis is the neonatal mortality rate per thousand births. The figure's red and blue dashed lines show that there is adverse selection into private facilities in these areas. More importantly, the black all-birth mortality curve slopes up. This identifies a public mortality advantage, squarely in line with panel b of Figure 2. However, there is no quantification of uncertainty in this graph.

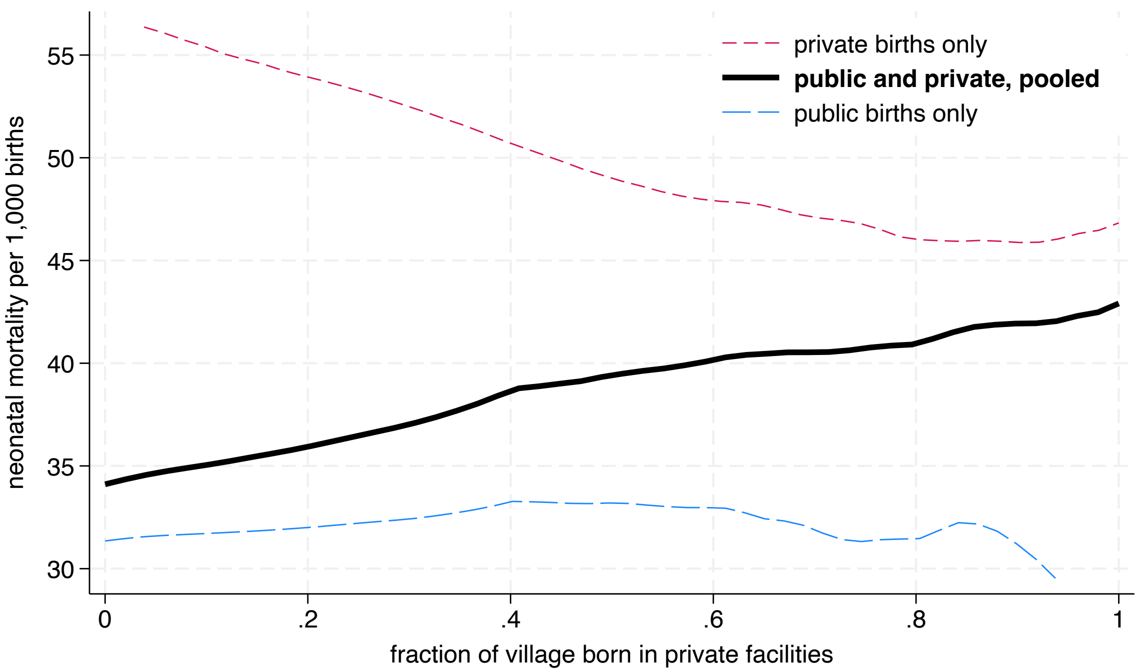
In order to test the statistical significance of the slope of the overall mortality curve, I estimate regressions of the form

$$y_{i,v} = \beta_0 + \beta_1 \overline{private}_v + f(X) + \epsilon_{i,r}, \tag{1}$$

where the unit of observation is a birth i in a village v, and $f(X)$ is a function of a vector

[11] If there were no selection or mortality effect, then all three curves would be horizontal lines at the mortality rate for the village.

Figure 3: **Main result 1**—Neonatal mortality is more likely in villages with more private facility birth, identifying a harmful private effect; UP and Bihar, NFHS-5



Notes: The figure displays the results of a splined local linear regression using an Epanechnikov kernel. It presents neonatal mortality as a function of the fraction of a village's births that take place in private facilities. In the absence of village-level confounding, it identifies the causal parameter from the econometric model developed in Section 4.1: the slope of the black pooled births line. Data are from the NFHS-5. Observations are births aged 1–59 months that were born in a health facility to women living in rural areas of UP and Bihar at the time of interview. Regressions are survey-weighted.

Table 1: Neonatal mortality is more likely for births in villages with a larger fraction born in private facilities, regressions with varying FEs and controls; UP and Bihar, NFHS-5

	Neonatal mortality per 1,000 births					
	(1)	(2)	(3)	(4)	(5)	(6)
fraction born in private	18.479**	25.629***	28.913***			
	(5.804)	(6.875)	(6.942)			
fraction born in private, excluding self				12.074*	17.107**	18.666**
				(5.611)	(6.630)	(6.641)
District-by-month FEs		Yes	Yes		Yes	Yes
Additional controls			Yes			Yes
Observations	33932	33932	33932	33899	33899	33899

Notes: The table displays OLS regression results using data from the NFHS-5. The parameter of interest is the causal parameter from the econometric model developed in Section 4.1: the slope of neonatal mortality as a function of the fraction born in private. Additional controls include the household wealth index, household electricity access, household caste and religion, mother's literacy, a quadratic of mother's height, a quadratic of mother's age at birth, mother's anemia, sex, singleton status, and birth order interacted with family size. Observations are births aged 1–59 months that were born in a health facility to women living in rural areas of UP and Bihar at the time of interview. Survey design weights are used and standard errors are clustered by PSU. *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

of controls. These controls can include sex of the child, household wealth index, toilet use, electricity use, caste status, and religion, mother's height, literacy, and number of children ever born, as well as district-by-month fixed effects. β_1 is the coefficient of interest. Observations are survey-weighted and standard errors are clustered at the village level, which is the primary sampling unit of the surveys.

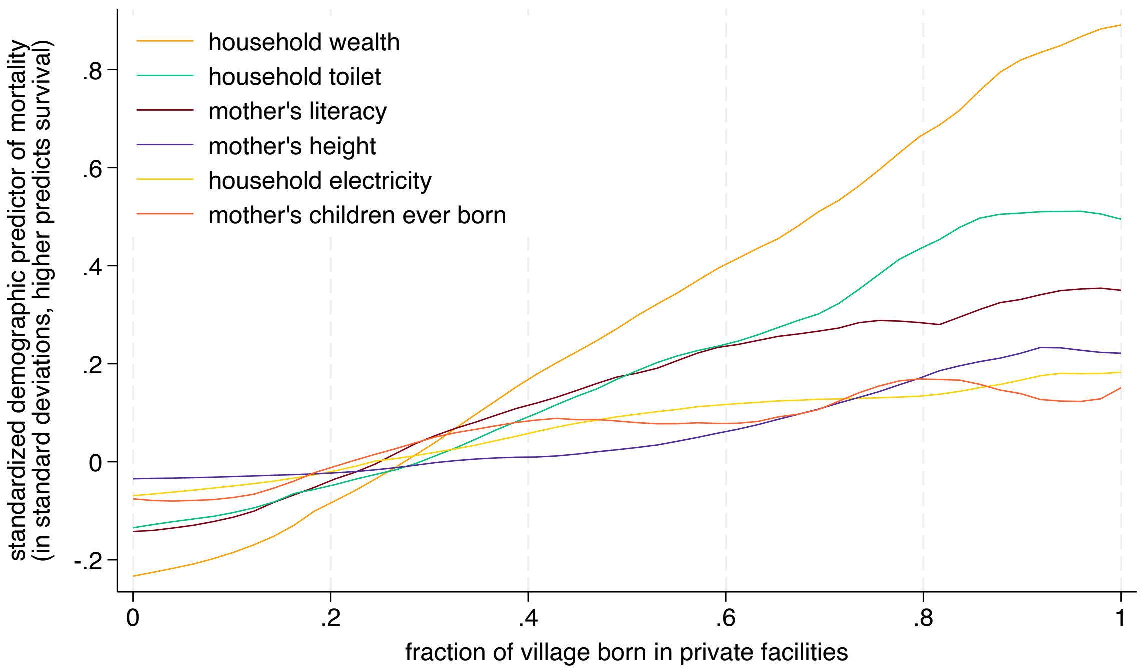
The sample size of births in a village may be small, and so the fraction born in private may hinge importantly on a "marginal" birth. For that reason, I also calculate

$$\overline{private}_{v-i} = \frac{\text{count of births in private facilities in the last 5 years, excluding self}}{\text{count of births in facilities in the last 5 years in the baby's village, excluding self}},$$

which varies at both the village level, v, and the individual birth level, i, because the fraction is computed separately for each baby, to exclude it from the average among its neighbors.

In Table 1, I report coefficients from regressions with either $\overline{private}_v$ or $\overline{private}_{v-i}$ as the regressor of interest and varying controls. The results are strong and consistent: Private facilities significantly increase mortality relative to public facilities. The inclusion of additional controls only makes these results stronger. The estimates based on $\overline{private}_v$ range from 18.5 to 28.9 per thousand, and are all significant at the 1-percent level or less. The estimates based on $\overline{private}_{v-i}$ are smaller, ranging from 12.1 to 18.7 per thousand. These are still significant at no greater than the 5-percent level.

Figure 4: Potential confounders are *better* in villages with more private facility birth, against direction of estimated mortality effect; UP and Bihar, NFHS-5



Notes: The figure displays the results of splined local linear regressions using an Epanechnikov kernel. It presents various standardized predictors of mortality as a function of the fraction of a village's births that take place in private facilities. Data are from the NFHS-5. Observations are births aged 1–59 months that were born in a health facility to women living in rural areas of UP and Bihar at the time of interview. Regressions are survey-weighted.

Figure 4 is in the spirit of verification that an instrument, randomization, or empirical strategy is balanced on observables. On the horizontal axis is again $\overline{private}_v$, and on the vertical axis are potential confounder: demographic predictors of mortality. Each variable is standardized with its mean and standard deviation in this sample for legibility, where higher predicts better survival. Six covariates are included: The asset wealth, sanitation use, and electrification of the baby's household and the literacy, height, and number of children born by the time of the survey of the baby's mother. For each of these markers of socioeconomic status, babies from villages with a greater fraction born in public facilities are more disadvantaged, on average.

This pattern runs counter to the mortality effect estimate, and clarifies why the inclusion of additional controls in Table 1 increases the size of the estimate. To address the potential confounders not present in my data set, I next identify the mortality effect with a regression discontinuity design.

5 Identification strategy: district borders regression discontinuity

5.1 Identifying variation

The effect of being born in a public facility in Uttar Pradesh and Bihar is a challenge to measure. Facility of birth is a choice made by mothers and their families. Many factors influence this choice, most of which are not observed in any data set. These factors can include objects that are themselves equilibrium outcomes, such as the locations of health facilities in the choice set and the prices of the care the provide. One benefit of a spatial regression discontinuity design is that, in expectation, it holds equal any such factors that don't change discontinuously at district borders.

One thing that does change discontinuously at district borders is the district government and, thereby, the costs of seeking care from public health facilities. For each village, I construct a measure of the choice-worthiness of the accessible public health facilities relative to private health care: the proportion of institutional deliveries in a district that took place in a public facility, leaving out the village's own deliveries. That is, I calculate the fraction

$$\overline{public}_{d-v} = \frac{\text{count of births in public facilities in own district in last 5 years, excluding own village}}{\text{count of births in facilities in own district in last 5 years, excluding own village}}$$

and compare it to the fraction in the village's neighboring districts.

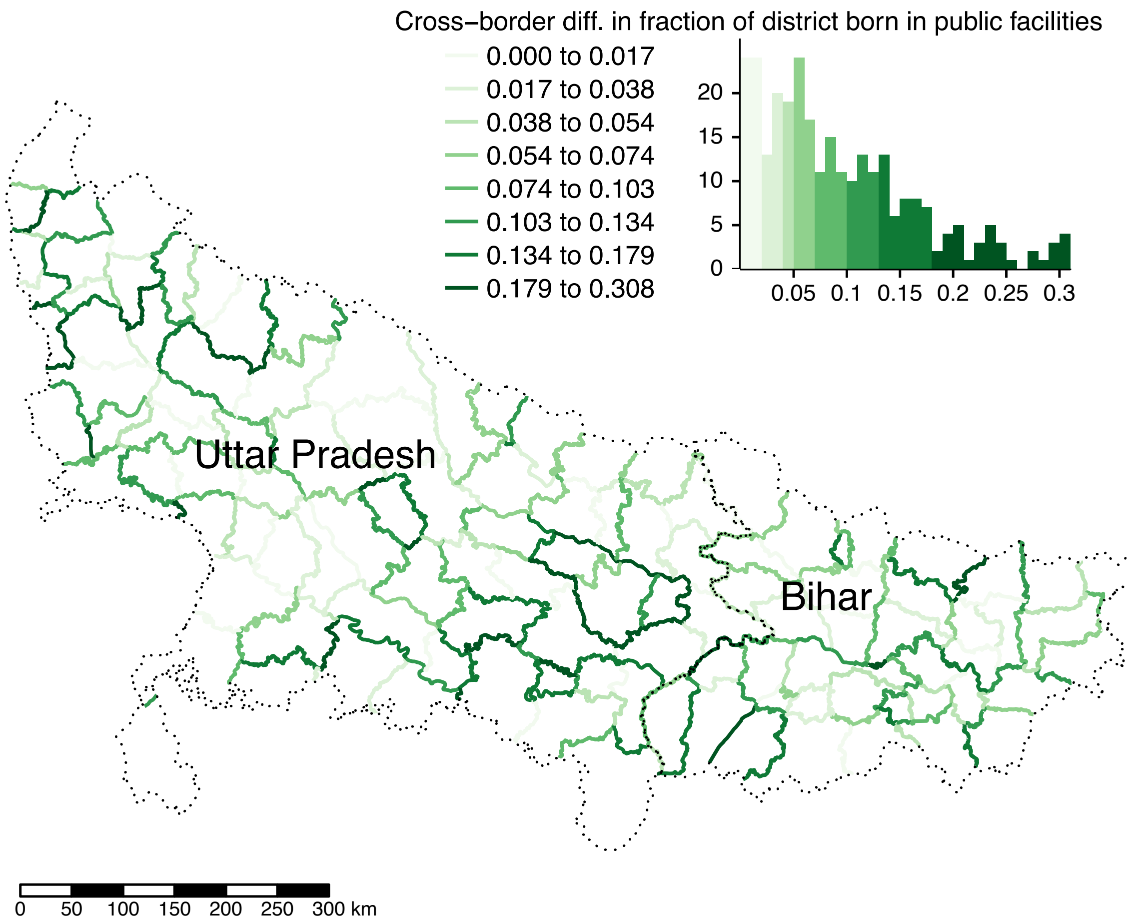
I then use this cost-shifter-type variation across neighboring districts to assign the groups that the regression discontinuity compares. If the village's own district has a higher public birth fraction than a neighbor district, it is on the positive side of the regression discontinuity cutoff. If the village's district has a lower public birth fraction, then it is on the negative side of the cutoff.

Figure 5 shows that the public fraction of facility births can vary substantially from district to district, even between neighboring district pairs. Panel a shows this spatially, restricted to only those districts in the states of interest. Panel b shows the variation across borders as a histogram. The median difference in the use of public facilities is 7.8 percentage points.

Column 1 of Table 2 shows the averages of relevant variables for the sample of births this analysis uses. Columns 2 and 3 show the averages for the subsamples below the cutoff and above the cutoff of the regression discontinuity. Similarly to the evidence from the prior empirical strategy, births in districts with more public birth tend to have worse predictors of mortality. They come from less wealthy households, and their mothers are less likely to be literate.

Why do differences in district-level public facility use also predict differences in public facility

Figure 5: Setting and identifying variation; UP and Bihar, NFHS-5



Notes: The figure displays a map of district borders in the Indian states under study, Uttar Pradesh and Bihar. The border color shows how much the two adjacent districts differ in the share of their births that occur in public facilities. Larger differences are drawn in darker shades. The differences are grouped into eight quantile bins, one shade per bin. The distribution of the values of the borders is displayed in a histogram next to the legend. The fraction of the district born in public facilities is based on survey-weighted births aged 1–59 months that were born in a health facility to women living in rural areas of UP and Bihar at the time of interview.

Table 2: RD sample—summary statistics and balance test; UP and Bihar, NFHS-4 and -5

	Sample means			Balance		Continuity tests	
	Full sample	Less public	More public	Difference	*p*-value	RD estimate	*p*-value
Public fac. birth	0.744	0.709	0.781	0.072	0.00	0.086	0.00
Neonatal mortality	38.6	38.3	38.8	0.5	0.68	-9.8	0.03
Wealth index	0.00	0.04	-0.03	-0.07	0.00	-0.01	0.76
Mother's literacy	0.568	0.578	0.557	-0.021	0.00	-0.024	0.13
Mother's height (cm)	150.1	150.1	150.0	-0.0	0.36	-0.2	0.33
Mother's age at birth	26.9	26.8	26.9	0.1	0.01	0.1	0.52
Scheduled Caste	0.264	0.265	0.263	-0.002	0.63	0.002	0.92
Scheduled Tribe	0.023	0.023	0.024	0.001	0.54	-0.006	0.31
OBC	0.563	0.560	0.565	0.005	0.35	0.019	0.35
Muslim	0.137	0.138	0.135	-0.003	0.38	0.005	0.76
Children ever born	2.8	2.7	2.8	0.0	0.07	-0.0	0.97
Birth order	2.4	2.4	2.4	0.0	0.05	0.0	0.88
Male birth	0.524	0.525	0.524	-0.002	0.55	0.008	0.47
Singleton birth	0.982	0.982	0.982	0.000	0.68	-0.007	0.16
Skin-to-skin contact	0.756	0.737	0.777	0.040	0.00	0.048	0.05
Ambulance use for birth	0.309	0.280	0.340	0.060	0.00	0.047	0.00
Met community health worker during preg.	0.562	0.559	0.565	0.006	0.24	0.043	0.02
3+ antenatal care visits	0.495	0.499	0.491	-0.007	0.15	-0.013	0.51
Cesarean delivery	0.114	0.125	0.103	-0.022	0.00	-0.011	0.23
Immediate breastfeeding	0.310	0.314	0.305	-0.009	0.08	-0.015	0.41

Notes: The table displays summary statistics and local linear regression balance tests for the sample used for analyses in this paper. Data are from the NFHS-4 and NFHS-5. Observations are births aged 1–59 months that were born in a health facility to women living in rural areas of Uttar Pradesh or Bihar at the time of interview, restricted to a bandwidth of 8 kilometers from the nearest district border. I calculate means and p-values according to the survey design: survey-weighted and clustered at the village (primary sampling unit) level.

use at the border? One important reason is that administrative frictions make it harder, though not impossible[12], to seek care outside of one's own district. For example, local healthcare workers are paid by district or sub-district governments for each delivery they assist in making happen in a public health facility (Maternal Health Division 2006). These payments are often late or entirely missed (Wang et al. 2012). This problem is likely to be worse for local healthcare workers who are seeking payment from governments outside their usual remit—such as for a birth to a family from a neighboring district. Another example is that mothers using public health facilities are themselves eligible for payment through a conditional cash transfer program rewarding facility birth (Maternal Health Division 2006). Navigating the reimbursement process is harder for mothers who live outside the district they delivered in (GfK MODE and Development Research Services 2009). Finally, referrals within the public health system are made within district.

5.2 Regression equations and identification assumptions

The unit of this analysis is a birth to a mother living in a rural area of Uttar Pradesh or Bihar in the five years prior to the survey. I pool together all the district borders such that the district with lower public facility use is on the negative side of the border cutoff and the district with higher public facility use is on the positive side. This means that villages in the same district may appear on different sides of the regression discontinuity, since district pairs are the basis of comparison. Furthermore, villages appear multiple times in each regression—once for each of its neighboring districts[13].

Pooling different borders together is necessary for statistical power, but the difference in the fraction born in public between one district and its neighbor can vary. This variation has important implications for the effect sizes we expect to see. Neighboring districts with only a small difference in public birth likely have a correspondingly small change at the border in public birth and in mortality. Neighboring districts with a larger difference likely have a larger discontinuity. The best way to handle that heterogeneity is unclear.

I present a variety of results that account for this heterogeneity. My primary results exclude district borders between districts with a difference in fraction born in public that is below a threshold level, to include only borders that actually cause variation. I chose this threshold to be the third tercile of cross-border difference, 9.5 p.p. I also present results that use a difference-in-discontinuities design to compare the borders with bottom-tercile differences to the

[12] Dupas and Jain (2024) note in their Table 3 that almost 30% of the female beneficiaries of a government health insurance program in the state of Rajasthan seek care outside their own district.

[13] Regressions including each village only once and assigning it to the nearest border find similar results. See Appendix C for details.

borders with top-tercile differences. Finally, I present results that treat the effect of borders as linearly related to the difference between the neighboring districts, as described by Calonico et al. (2025).

Following Cattaneo, Idrobo, and Titiunik (2019), I run local linear regressions with a triangular kernel function. That is, I restrict the regression to include only observations that fall within a particular bandwidth from each district border, and I weight the observations near the cutoff more heavily. For all results, I use the bandwidth that I estimate to minimize the mean squared error of a regression discontinuity with neonatal mortality as outcome, 22.1 kilometers[14].

I report first-stage, reduced-form, and continuity test estimates from the following regression discontinuity equation:

$$y_{i,r} = \beta_0 + \beta_1 d_{i,r} + T_{i,r}(\beta_2 + \beta_3 d_{i,r}) + f(X) + \epsilon_{i,r}, \tag{2}$$

where i is a birth with mother living in district pair r a distance d from the border, with sign dictated by the instrument. y is one of public facility birth, neonatal mortality, separation of mother and baby at birth, or a set of demographic variables to test for discontinuities. T is an indicator of being on the side of a district border that has a higher district-level fraction born in public, so the coefficient that identifies the border effect is β_2. Depending on the specification, the regressions may include controls captured by $f(X)$: a function of a vector of controls X including household wealth index, household electricity access, household caste and religion, household open defecation, mother's literacy, a quadratic of mother's height, a quadratic of mother's age at birth, sex, singleton status, birth order interacted with family size, year of birth, and survey, state, and district-pair fixed effects. In all regressions, I use survey weights[15] and cluster standard errors at the level of the primary sampling unit.

In order for the regressions I estimate to have a causal interpretation, the expectations of the potential outcomes at the cutoff must be continuous in the running variable. A possible violation of this continuity assumption is if there is sorting on the basis of district-level public health outcomes or their correlates. In Figure 6 and the final two columns of Table 2, I present falsifying RD estimates with a variety of covariates [16], which have no significant discontinuities. At the

[14] Appendix D shows that the results are robust to alternative bandwidth specifications.

[15] Because I also use a triangular kernel, I multiply the kernel weights by the survey weights.

[16] Of course, there are other possible characteristics of people that may be discontinuous at the border but are not present in my data. One that is troubling for some explanations of the identifying variation is that it is somewhat common in this context for women to stay in their natal village to give birth. Using the IHDS, a smaller and older dataset, I find this practice is more common among women whose last birth was in private facilities. Unfortunately the IHDS does not have geographical coordinates for me to explore this further in the borders RD design.

bottom of that table, I also include several possible mechanisms, which have discontinuities consistent with them acting as mechanisms.

I also present estimates from local linear regressions, following Cattaneo, Idrobo, and Titiunik 2024, using the following two-stage least squares specification:

$$\boldsymbol{pub}_{i,r} = \alpha_0 + \alpha_1 d_{i,r} + T_{i,r}(\alpha_2 + \alpha_3 d_{i,r}) + g(X) + \zeta_r + \delta_{i,r} \tag{3}$$

$$y_{i,r} = \beta_0 + \beta_1 d_{i,r} + \widehat{\boldsymbol{pub}}_{i,r}(\beta_2 + \beta_3 d_{i,r}) + f(X) + \eta_r + \epsilon_{i,r} \tag{4}$$

where $\widehat{\boldsymbol{pub}}$ is the predicted value of $\boldsymbol{pub}$ from the first-stage equation.

In a fuzzy regression discontinuity research design, four assumptions are sufficient for the estimate to have a causal interpretation (Hernan and Robins 2023). First, instrument relevance, which requires that the instrument be associated with the explanatory variable (and is directly tested in the first stage). Second, independence, which requires that the instrument and the outcome not share any causes. Third, the exclusion restriction, which requires that the instrument only affect the outcome through its potential effect on the explanatory variable. Lastly, monotonicity, which requires that the probability of the (binary) explanatory variable be weakly increasing in the instrument.

Independence may be violated if the medical system is strained as a result of greater use. Another possible violation is if public health facility users are more likely to engage in protective behavior for their children apart from choosing public health care. However, public health users are disadvantaged on many, as I discuss earlier in this paper. In each case, a violation of this type would likely favor private health care in this design. This is the opposite of the results I see.

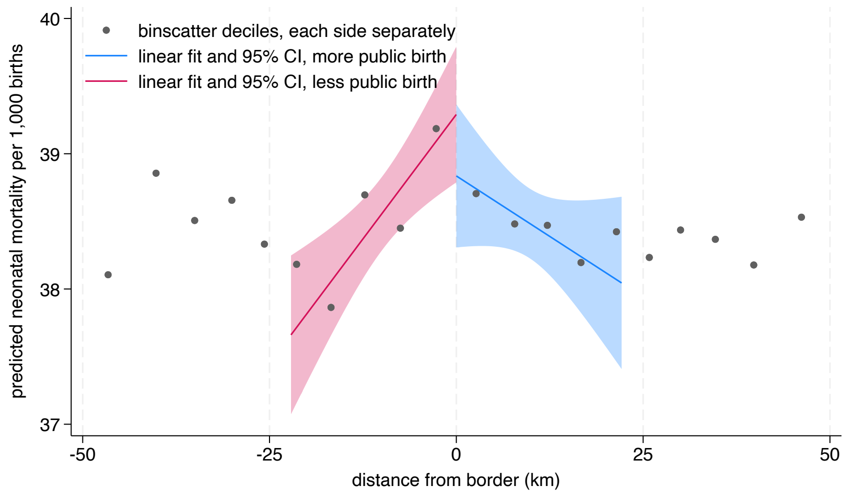
Monotonicity may be violated if some potential users of public health care decide not to on the basis of crowding at public health facility nearby, perhaps more likely if a greater proportion of births happen in those facilities. This sort of crowding is more likely to discourage those who are more sensitive to time and status costs. If these people are primarily those with higher wages and thus lower mortality, then this violation would also favor private health care.

As mentioned earlier, I also employ two further regression discontinuity designs that account for heterogeneity by the difference in the neighbor districts' fraction born in public facilities: difference-in-discontinuity regressions as described by Grembi, Nannicini, and Troiano (2016) and conditional average treatment effect regressions as described by Calonico et al. (2025)[17]. The

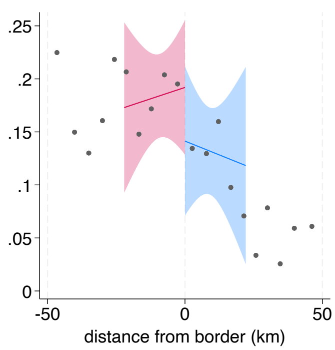
[17]These have additional identification assumptions that I discuss in Appendix E.

Figure 6: Continuity tests—potential confounders are not discontinuous at borders; UP and Bihar, NFHS-4 and NFHS-5

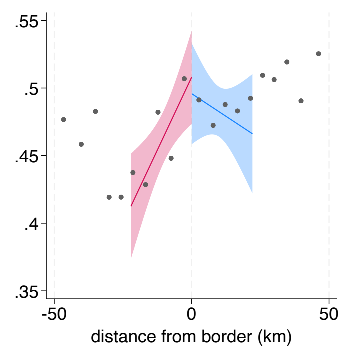
(a) Predicted neonatal mortality per 1,000 births



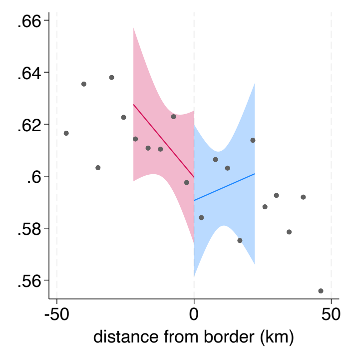
(b) Household wealth (SDs)



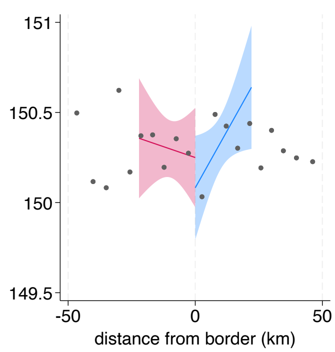
(c) Household open defecation



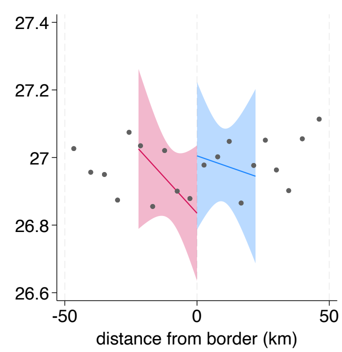
(d) Mother's literacy



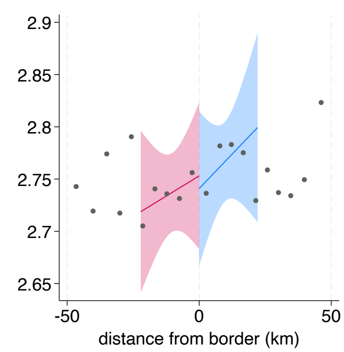
(e) Mother's height (cm)



(f) Mother's age (years)



(g) Children ever born



Notes: The figure displays the results of local linear regressions using a triangular kernel. It presents various demographic variables as a function of distance from the border. For every pair of adjacent districts, observations from the district with a lower fraction born in public are on the left side of the plots, with negative distances; the districts with a greater fraction born in public are on the right, with positive distances. The plotted points are a weighted binscatter of deciles on each side of the cutoff. Data are from the NFHS-4 and NFHS-5. Observations are births aged 1–59 months that were born in a health facility to women living in rural areas of UP and Bihar at the time of interview, with a top-tercile (9.5 p.p.) cross-border difference in the fraction of rural births in public facilities. Regressions are survey-weighted with standard errors clustered at the village level.

difference-in-discontinuities regression equation is

$$y_{i,r} = \delta_0 + \delta_1 d_{i,r} + T_{i,r}(\gamma_0 + \gamma_1 d_{i,r}) + S_{i,r}[\beta_0 + \beta_1 d_{i,r} + T_{i,r}(\alpha_0 + \alpha_1 d_{i,r})] + f(X) + \epsilon_{i,r}, \quad (5)$$

where S is an indicator of being in a district pair with a large difference in district-level fraction born in public facilities rather than a smaller difference. In the regressions I present in the paper, a difference greater than the median difference is "large", and a difference less than the median is "small". Thus, the coefficient of interest here is α_0, which identifies the change in the discontinuity from the small-difference borders to the large-difference borders.

The conditional average treatment effect regression equation is

$$y_{i,r} = \delta_0 + \delta_1 d_{i,r} + T_{i,r}(\gamma_0 + \gamma_1 d_{i,r}) + R_{i,r}[\beta_0 + \beta_1 d_{i,r} + T_{i,r}(\alpha_0 + \alpha_1 d_{i,r})] + f(X) + \epsilon_{i,r}, \quad (6)$$

where R is the difference between the fraction of institutional deliveries in the birth's own district that took place in a public facility (leaving out the village's own deliveries) and the neighboring district's fraction. The coefficient of interest here is again α_0, which identifies the change in the discontinuity as the border difference increases.

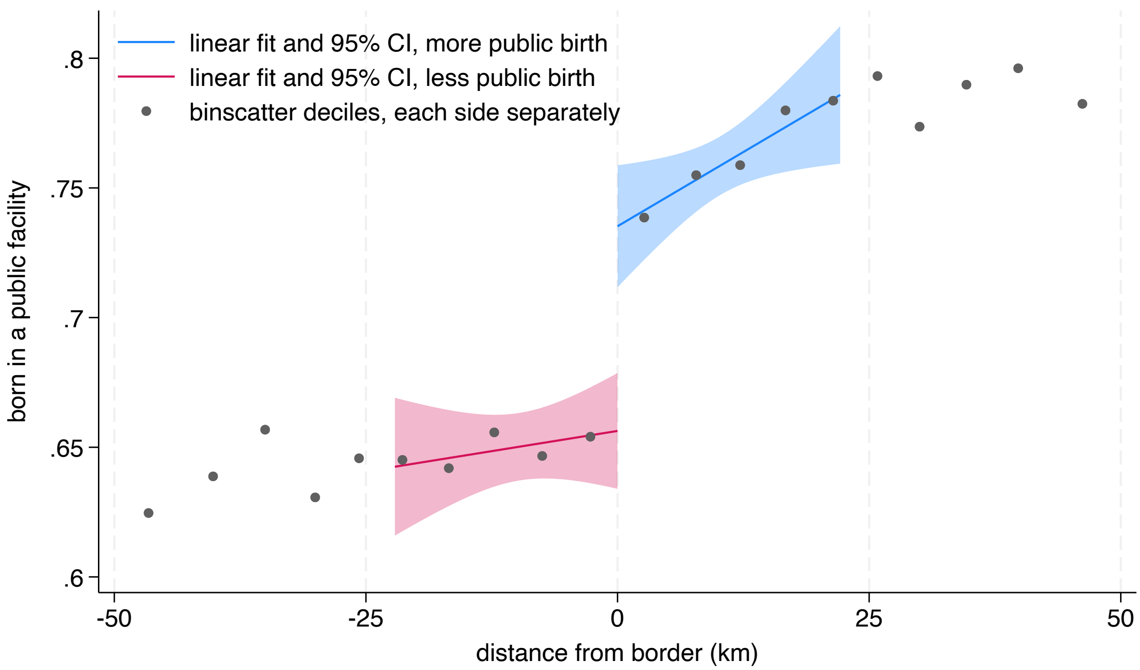
5.3 Results

Figure 7 and Table 3 show that residing just across the border in a district with more public health facility deliveries is predictive of public facility birth. The first stage estimate for the mean squared error optimal bandwidth is 8.2 percentage points (SE: 1.4).

Figure 8 and Table 4 present the main results of this paper, and they show that neonatal mortality jumps discontinuously at district borders. Panel a of Table 4 shows that being on the side of a district border with more public birth significantly reduces neonatal mortality by over 10 per thousand births. The reduction in mortality, if scaled by the proportion of births that "comply" in the first stage to give birth in public, is around 130 per thousand births. Panel b uses the border variation as an instrument for public birth. According to these estimates, public birth reduces neonatal mortality by 124–151 per thousand births. Figure 8 shows the result from Table 4, Panel a, Column 1 graphically. It presents neonatal mortality as a function of distance from the border. The plotted points are a weighted binscatter with deciles on each side of the border, and the lines on the graph represent a local linear regression as described in Section 5.2.

Columns 4–6 present additional results showing difference-in-discontinuity estimates that verify the prior estimates. In Panel a, the coefficient on "Own district's public birth is higher" measures the effect of going from the side of a border with less public facility birth to the side

Figure 7: First stage—public birth increases (7.7 p.p., SE: 1.7) crossing the border from districts with lower public facility use to higher; UP and Bihar, NFHS-4 and NFHS-5



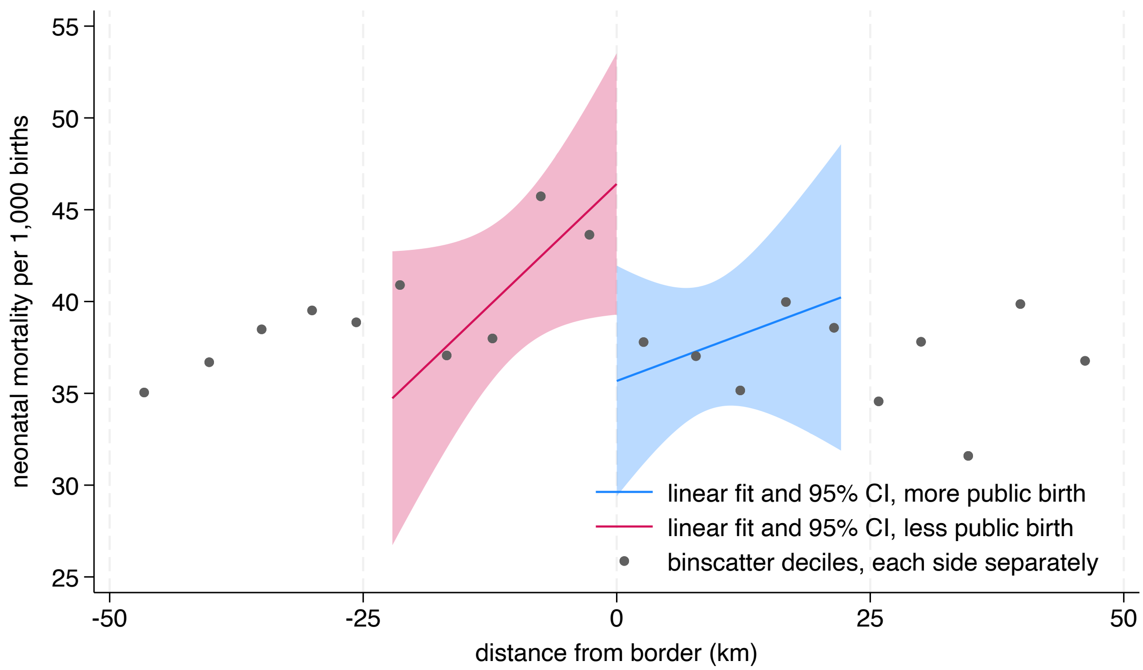
Notes: The figure displays the results of a local linear regression using a triangular kernel. It presents public birth as a function of distance from the border. For every pair of adjacent districts, observations from the district with a lower fraction born in public are on the left side of the plots, with negative distances; the districts with a greater fraction born in public are on the right, with positive distances. The plotted points are a weighted binscatter of deciles on each side of the cutoff. Data are from the NFHS-4 and NFHS-5. Observations are births aged 1–59 months that were born in a health facility to women living in rural areas of UP and Bihar at the time of interview, with a top-tercile (9.5 p.p.) cross-border difference in the fraction of rural births in public facilities. Regressions are survey-weighted with standard errors clustered at the village level.

Table 3: First stage—public birth jumps (7.2 p.p., SE: 1.7) crossing the border from a district with lower public facility use to a district with higher; UP and Bihar, NFHS-4 and NFHS-5

Outcome: born in a public facility	Regression discontinuity			Difference-in-discontinuities			Cond. ave. treatment effect		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Own district's public birth is higher	0.077***	0.083***	0.082***	0.011	0.020	0.017	0.022	0.011	0.004
	(0.017)	(0.015)	(0.014)	(0.015)	(0.012)	(0.012)	(0.014)	(0.011)	(0.011)
Own district's public birth is higher × difference at border is large				0.066**	0.064***	0.066***			
				(0.023)	(0.019)	(0.018)			
Cross-border difference in fraction born in public							0.300*	0.466***	0.499***
							(0.145)	(0.122)	(0.117)
Survey, state, and district-pair FEs		Yes	Yes		Yes	Yes		Yes	Yes
Additional controls			Yes			Yes			Yes
Observations	51208	51208	51208	105575	105575	105575	163047	163047	163047

Notes: The table displays the results of local linear regressions using triangular kernels and 8 km bandwidths. They measure the discontinuity in public birth at the border, crossing from a district with a lower district-level fraction of births in public facilities to a district with a higher public fraction. "Additional controls" include the household wealth index, household electricity access, household caste and religion, household open defecation, mother's literacy, a quadratic of mother's height, a quadratic of mother's age at birth, sex, singleton status, birth order interacted with family size, and year of birth fixed effects. Columns 1–3 include only observations from the top tercile of difference in public facility use between adjacent districts, greater than 9.5 p.p. Columns 4–6 compare observations from the top tercile ("large") to the bottom tercile. Columns 7–9 treat the discontinuity as linear in the difference between adjacent districts. Data are from the NFHS-4 and NFHS-5. Observations are births aged 1–59 months that were born in a health facility to women living in rural areas of UP and Bihar at the time of interview. Means and standard errors are calculated according to the survey design: survey-weighted and clustered at the village level. $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Figure 8: **Main result 2**—neonatal mortality drops (-11.6 per thousand, SE: 4.9) crossing the border from lower public facility use to higher; UP and Bihar, NFHS-4 and NFHS-5



Notes: The figure displays the results of a local linear regression using a triangular kernel. It presents neonatal mortality as a function of distance from the border. For every pair of adjacent districts, observations from the district with a lower fraction born in public are on the left side of the plots, with negative distances; the districts with a greater fraction born in public are on the right, with positive distances. The plotted points are a weighted binscatter of deciles on each side of the cutoff. Observations are births aged 1–59 months that were born in a health facility to women living in rural areas of UP and Bihar at the time of interview, with a top-tercile (9.5 p.p.) cross-border difference in the fraction of rural births in public facilities. Regressions are survey-weighted with standard errors clustered at the village level.

Table 4: Neonatal mortality, reduced-form and fuzzy RD—birth in a public facility reduces neonatal mortality relative to private; UP and Bihar, NFHS-4 and NFHS-5

Outcome: neonatal mortality per 1,000	Regression discontinuity			Difference-in-discontinuities			Cond. ave. treatment effect		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel a: Reduced-form regressions									
Own district's public birth is higher	-11.567*	-10.243*	-11.423**	6.164	5.456	4.632	7.876*	8.040*	7.045†
	(4.910)	(4.649)	(4.403)	(4.333)	(4.139)	(4.052)	(4.010)	(3.911)	(3.805)
Own district's public birth is higher × difference at border is large				-17.599**	-15.245*	-15.549**			
				(6.489)	(6.181)	(5.934)			
Cross-border difference in fraction born in public							-129.287**	-113.662**	-109.116**
							(42.610)	(39.462)	(37.917)
Panel b: Two-stage least squares regressions									
Born in a public facility	-150.967*	-123.676*	-139.940*	-266.708*	-237.401*	-236.518*	-109.115†	-79.733†	-96.169*
	(71.152)	(60.613)	(58.765)	(133.141)	(120.132)	(111.468)	(56.857)	(45.799)	(47.138)
Survey, state, and district-pair FEs		Yes	Yes		Yes	Yes		Yes	Yes
Additional controls			Yes			Yes			Yes
Observations	51208	51208	51208	105575	105575	105575	163006	163006	163006

Notes: The table displays the results of local linear regressions using a triangular kernel. Panel a presents results of regressions using Equation 2, and Panel b presents results of regressions using Equation 3. They measure the discontinuity in neonatal mortality at the border, crossing from a district with a lower district-level fraction of births in public facilities to a district with a higher public fraction. "Additional controls" include the household wealth index, household electricity access, household caste and religion, household open defecation, mother's literacy, a quadratic of mother's height, a quadratic of mother's age at birth, sex, singleton status, birth order interacted with family size, and year of birth fixed effects. Columns 1–3 include only observations from the top tercile of difference in public facility use between adjacent districts, greater than 9.5 p.p. Columns 4–6 compare observations from the top tercile ("large") to the bottom tercile. Columns 7–9 treat the discontinuity as linear in the difference between adjacent districts. Data are from the NFHS-4 and NFHS-5. Observations are births aged 1–59 months that were born in a health facility to women living in rural areas of UP and Bihar at the time of interview. Coefficients and standard errors are calculated according to the survey design: survey-weighted and clustered at the village level. $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

with more public birth, for those district borders separating two districts with a bottom-tercile difference (4.5 p.p.). None of these are significant. The coefficient on "Own district's public birth is higher × difference at border is large" measures the change in that effect for those district borders separating two districts with a top-tercile difference in public facility birth. These estimates are all significant. Panel b uses this border variation as an instrument for public birth, yielding estimates that agree with the causal estimates from Columns 1–3, though they are very large and more uncertain. According to these estimates, public birth reduces neonatal mortality by 237–267 per thousand births.

Columns 7–9 present the final results, assuming that the effect of the district border is linearly related to the size of the difference between the adjacent districts' fraction born in public facilities. In Panel a, the coefficient on "Own district's public birth is higher" measures the effect of crossing a border separating two districts with no difference in public birth. The coefficient on "Cross-border difference in fraction born in public" measures the effect of crossing a border when linearly scaled by the size of the difference at the border. These results show that crossing a border between districts with a median difference in fraction born in public, 7.8 p.p., causes a reduction in neonatal mortality of 8.5–10.1 per thousand births. Panel b uses this border variation once again as an instrument for public birth. These are noisier, with some only significant at the 10% level. According to these estimates, public birth reduces neonatal mortality by 80–109 per thousand births.

Together these estimates show that the local average treatment effect of public birth in the remote areas near district borders may be very large, much larger than the average treatment effects across the entire population could plausibly be. However, the large size of the effect estimate is matched by large standard errors: The main conclusion we may draw is that public health facilities reduce neonatal mortality compared to private health facilities.

6 Mechanism: Dangerous medical interventions, measured by separation of mother and baby, resolve the puzzle

In the previous sections, I show that private facility birth is more dangerous than public facility birth. In this section I give an account of why private is more dangerous.

I provide evidence that private providers are more commonly performing medical intervention that harm newborn babies. The case depends on the principal–agent problem that patients and health care providers are engaged in. Patients can't identify or carry out the medical care that they would benefit most from, so they employ health care providers to do this on their behalf.

Private health care providers charge fees per service, giving them incentive to recommend more services regardless of medical necessity. Public providers have no such incentive, since they are paid a salary independent of the services they provide.

For many conditions and in many contexts, this type of provider-induced demand is merely wasteful; for newborns in rural India, these interventions are dangerous. In this area of India, some potentially harmful interventions include washing the baby soon after birth, using electric warmers that may not have power or may be improperly set, using a tube to remove mucus or debris from near the baby's airways, feeding the baby before breastfeeding, and giving the baby unnecessary antibiotics. Each of these interventions can harm the baby's ability to survive by lowering the baby's body temperature, by damaging the baby's airways, by introducing pathogens, or by impairing the function of fragile organ systems.

To investigate this type of intervention empirically, I can't use the full list of interventions performed by health care providers during each birth. No such data exists. Instead, I look at the first step of all these interventions: separation of the mother and baby. This is measured in my data with questions about contact between the mother and baby immediately after birth.[18]

In the following subsections, I present a collage of evidence that separation of the mother and baby—or its correlates, the interventions taking place thereafter—explains the public–private mortality effect. First, with cross-sectional regressions and RD specifications, I show that private facilities more commonly separate the mother and baby than public facilities. Second, I show that in villages where public and private facilities separate mothers and babies equally often, the gap in mortality disappears. Finally, I use stratified regressions to show that the public mortality advantage does not come from factors other than separation or its correlates. If it did, then that advantage would persist whether or not babies were separated. But stratifying by whether separation occurred reverses the relationship between public facility use and village mortality—babies who were separated from their mothers are less likely to die in places with more private birth, as expected from their predictors of health. So too are babies who were not separated from their mothers.

[18]There is also evidence that skin-to-skin contact at birth, sustained for 60 to 90 minutes, can improve survival beyond the usual standard of care. Some who answer "yes" to these survey questions may experience this type of care, but it's unlikely to be driving the main effect.

6.1 Private facilities more commonly separate mothers and babies than public facilities

6.1.1 Regression discontinuity evidence

Using the regression discontinuity design detailed in Section 5.2 I show that, just as crossing the border increases the fraction born in public facilities and decreases mortality, crossing the border decreases the fraction of babies separated from their mothers immediately after birth.

Figure 9 and Table 5 present the mechanism results of the regression discontinuity, and they show that separation jumps discontinuously at district borders. Columns 1–3 of Panel a of Table 5 shows that being on the side of a district border with more public birth decreases separation by 4.8–8.4 percentage points. Not all of these are significant at the 5-percent level. Panel a of Figure 9 shows the result from Table 5, Panel a, Column 1 graphically. It presents separation as a function of distance from the border. The plotted points are a weighted binscatter with deciles on each side of the border, and the lines on the graph represent a local linear regression as described in Section 5.2. Panel b uses the border variation as an instrument for public birth. According to these estimates, public birth decreases separation by 46.5–75.8 percentage points.

The remaining panels of Figure 9 show that there are no significant differences in other measured care practices. Cesarean delivery is perhaps less common on the more-public side of the border, but is only marginally significant at the 10% level.

6.1.2 Village composition strategy

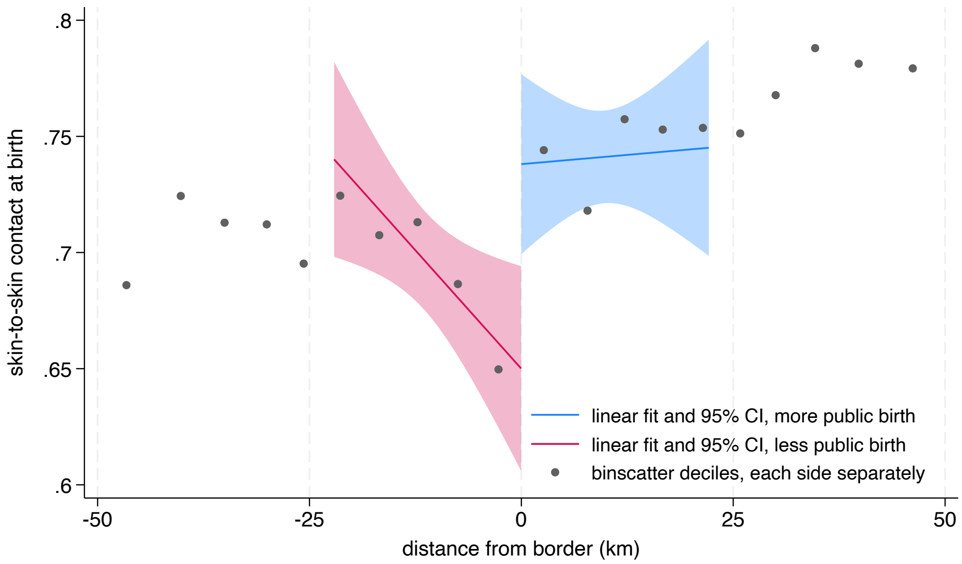
Next, I look at how the fraction of babies separated from their mothers changes across villages with different fractions born in private. The logic of this is explained thoroughly in Section 4.1. The key idea is that, if there is no difference in the separation practices of public versus private facilities, then separation overall in the village does not depend on the allocation of its births into each facility type; however, in the presence of a public–private difference in care practices, the fraction of village's births that are separated *does* depend on the fraction born in each facility type. The dimension of variation is $\overline{public}_{v-i}$, which reflects the baby's village-level context:

$$\overline{public}_{v-i} = \frac{\text{count of births in } \textbf{public} \text{ facilities in the last 5 years}}{\text{count of births in facilities in the last 5 years in the baby's PSU, excluding self}}.$$

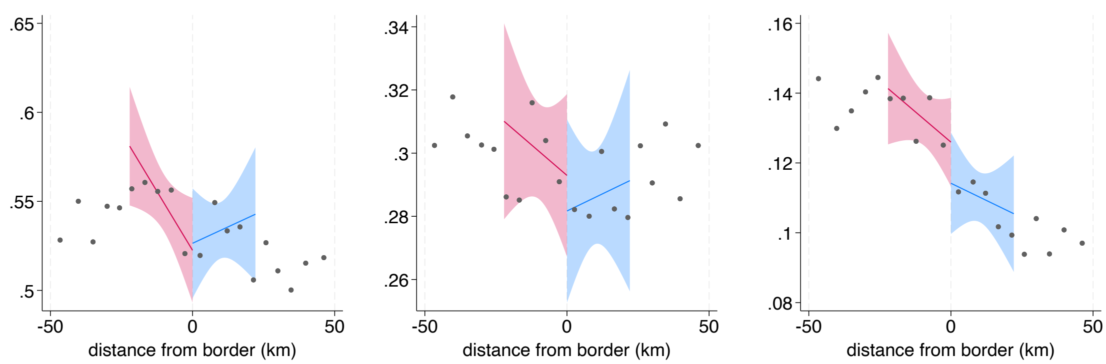
$\overline{public}_{v-i}$ varies at both the PSU level, v, and the individual birth level, i, because the fraction is computed separately for each baby, to exclude it from the average among its neighbors. This exclusion avoids the problem of endogeneity introduced by mothers selecting into public or

Figure 9: Mechanism—separation of mothers and babies decreases on the side of the border with more public birth; UP and Bihar, NFHS-4 and NFHS-5

(a) Non-separation (coef: 8.4 p.p., SE: 3.0; NFHS-5 only)



(b) 3+ antenatal care visits (c) Immediate breastfeeding (d) Cesarean delivery



Notes: The figure displays the results of a local linear regression using a triangular kernel. It presents obstetric care practices as a function of distance from the border. For every pair of adjacent districts, observations from the district with a lower fraction born in public are on the left side of the plots, with negative distances; the districts with a greater fraction born in public are on the right, with positive distances. The plotted points are a weighted binscatter of deciles on each side of the cutoff. Data are from the NFHS-5. Observations are births aged 1–59 months that were born in a health facility to women living in rural areas of UP and Bihar at the time of interview, with a top-tercile (9.5 p.p.) cross-border difference in the fraction of rural births in public facilities. Regressions are survey-weighted with standard errors clustered at the village level.

Table 5: Mechanism, reduced-form and fuzzy RD—birth in a public facility decreases separation of mother and baby relative to private

Outcome: non-separation	Regression discontinuity			Difference-in-discontinuities			Cond. ave. treatment effect		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel a: Reduced-form regressions									
Own district's public birth is higher	0.084**	0.048†	0.050*	0.024	0.036†	0.033	0.016	0.031	0.027
	(0.030)	(0.025)	(0.024)	(0.023)	(0.021)	(0.021)	(0.023)	(0.020)	(0.020)
Own district's public birth is higher × difference at border is large				0.060	0.006	0.011			
				(0.038)	(0.032)	(0.032)			
Cross-border difference in fraction born in public							0.485†	0.131	0.159
							(0.251)	(0.208)	(0.207)
Panel b: Two-stage least squares regressions									
Born in a public facility	0.758**	0.465†	0.475*	0.588	0.083	0.128	0.871***	0.515**	0.508**
	(0.292)	(0.242)	(0.233)	(0.380)	(0.424)	(0.379)	(0.255)	(0.176)	(0.183)
State, and district-pair FEs		Yes	Yes		Yes	Yes		Yes	Yes
Additional controls			Yes			Yes			Yes
Observations	25502	25502	25502	53043	53043	53043	81042	81042	81042

Notes: The table displays the results of local linear regressions using a triangular kernel. Panel a presents results of regressions using Equation 2, and Panel b presents results of regressions using Equation 3. They measure the discontinuity in reported separation of mother and baby after birth at the border, crossing from a district with a lower district-level fraction of births in public facilities to a district with a higher public fraction. "Additional controls" include the household wealth index, household electricity access, household caste and religion, household open defecation, mother's literacy, a quadratic of mother's height, a quadratic of mother's age at birth, sex, singleton status, birth order interacted with family size, and year of birth fixed effects. Columns 1–3 include only observations from the top tercile of difference in public facility use between adjacent districts, greater than 9.5 p.p. Columns 4–6 compare observations from the top tercile ("large") to the bottom tercile. Columns 7–9 treat the discontinuity as linear in the difference between adjacent districts. Data are from the NFHS-5. Observations are births aged 1–59 months that were born in a health facility to women living in rural areas of UP and Bihar at the time of interview. Coefficients and standard errors are calculated according to the survey design: survey-weighted and clustered at the village level. $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

private care on the basis of the separation of their baby.

Figure 10 Panel a is similar to Figure 4, but it uses $\overline{public}_{v-i}$ on the horizontal axis and includes an additional line for separation of the baby at birth. Each variable is standardized with its mean and standard deviation in this sample for legibility. Six covariates are included: The asset wealth, sanitation use, and electrification of the baby's household and the literacy, height, and number of children born by the time of the survey of the baby's mother. For each of these markers of socioeconomic status, babies with more neighbors born in public facilities are more disadvantaged, on average. One line slopes up, which indicates that babies with greater $\overline{public}_{v-i}$ are less likely to be separated from their mothers.

Panel b focuses in particular on characteristics of obstetric care beyond just separation of mothers and babies. Ambulance use, cesarean birth, community health worker access, 3 or more antenatal care visits, immediate breastfeeding are additionally included. Ambulance use and vaginal birth are the only characteristics that, like non-separation, are greatly less common in villages with more private birth. These are investigated as alternative mechanisms below.

6.2 Births in places with more private birth are better off after accounting for separation

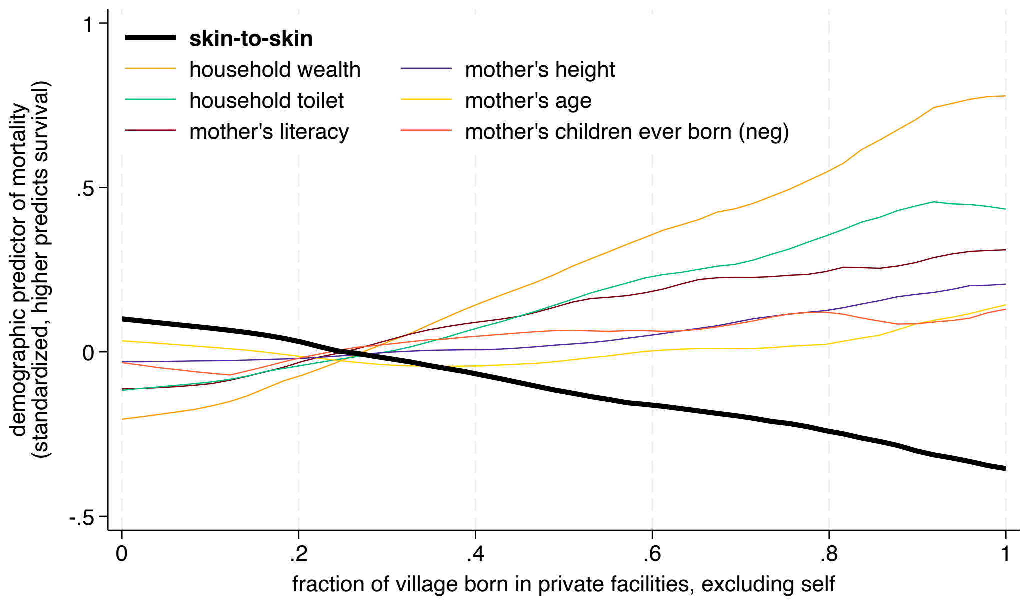
If the public-private mortality effect were driven by factors unrelated to separation of the baby and mother, then mortality would not depend on whether the baby was separated. This analysis tests whether this is the case.

Figure 11 relates $\overline{public}_{v-i}$ to neonatal mortality as in Section 4.2. The solid black line shows that babies with greater $\overline{public}_{v-i}$ are more likely to survive neonatancy. In Panel a, the other two lines show that this survival advantage for births with more neighbors born in public facilities can be accounted for by separation. They split the same sample according to whether the baby was separated from the mother, the two dashed lines. Within both of these subsets, there is no longer a negative association between $\overline{public}_{v-i}$ and neonatal mortality. In fact, there is the positive association that observables would predict. The fact that the $\overline{public}_{v-i}$ survival advantage can be so completely accounted for by separation suggests that it is because of it; if the advantage came from factors other than separation, then that advantage would persist whether or not babies were separated.

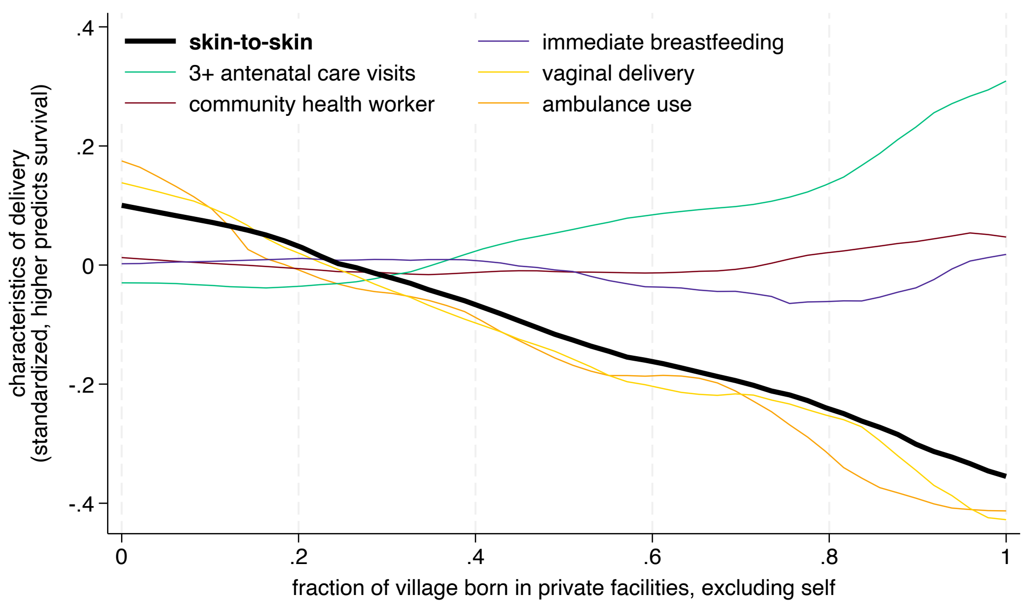
I show in Panels b–d of Figure 11 that the result is robust to splitting the sample according to other indicators of the health and care of the mother and pregnancy. Unlike splitting the sample by separation of the baby, these splits do not reverse or eliminate the association between neonatal death and $\overline{public}_{v-i}$. Neither ambulance use, cesarean birth, nor breastfeeding account

Figure 10: Separation of mothers and babies at birth, cesarean delivery, and ambulance use are candidates to explain the mortality advantage; UP and Bihar, NFHS-5

(a) Non-separation of mothers and babies is common in villages with a higher fraction born in private facilities, counter to neonatal mortality and demographic predictors of survival



(b) Non-separation, vaginal delivery, and ambulance use are more common in villages with a lower fraction born in private facilities



Notes: The figure displays the results of splined local linear regressions using an Epanechnikov kernel. It presents various standardized predictors of mortality as a function of the fraction of a village's births (excluding self) that take place in private facilities. Data are from the NFHS-5. Observations are births aged 1–59 months that were born in a health facility to women living in rural areas of UP and Bihar at the time of interview. Regressions are survey-weighted.

for the $\overline{public}_{v-i}$ survival advantage so fully as separation. The consistent patterns of these results is evidence, collectively, that properties of the pregnancy are not confounding the separation result.

6.3 Where private facilities don't separate mothers and babies more than public, mortality is no higher

If separation and its correlated interventions are responsible for the public–private mortality advantage, then there should be no mortality advantage in places where public and private facilities separate mothers and babies at similar rates. I investigate this here.

Figure 12 collapses the data to the village level. Where Figures 10 and 11 compare across villages, learning from the differences in outcomes between babies born in different villages, Figure 12 compares babies born in public and private facilities within the same village. The horizontal axes measures the extent to which different care practices are more common in public rather than private facilities, for births to families living in a given village. The vertical axis measures the extent to which neonatal mortality is greater for births in public rather than private facilities, for births in the same village. The dots are averages of the public–private difference in separation of mothers and babies, within approximately-equal-mass quantiles of villages according to the horizontal axis.

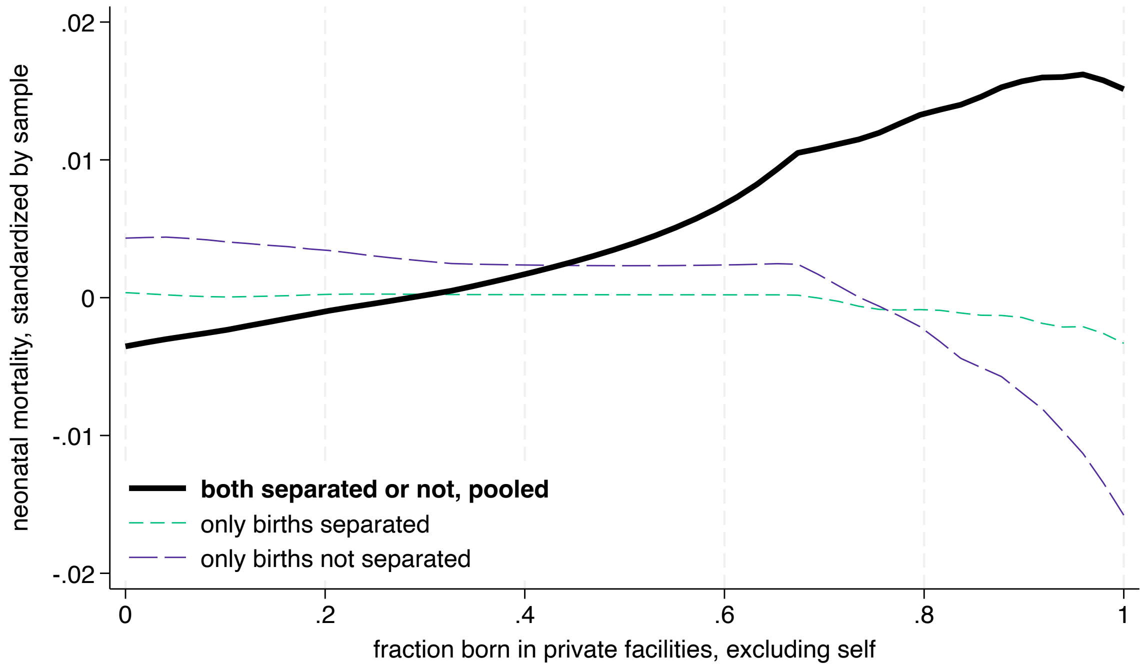
The downward sloping regression line in Figure 12, Panel a says that the public–private mortality advantage is greater for villages where the public–private separation advantage is greater. Moreover, in the minority of villages where there is not a public–private separation advantage, there is also not a public–private neonatal mortality advantage. The same is not true of the other care practices in Panels b–d, where the gap in ambulance use, cesarean delivery, and immediate breastfeeding appear to be unrelated to the village's mortality gap.

Table 6 confirms and quantifies the separation associations in these figures with regression. Table 6 also reports robustness checks (proceeding from Panel A through Panel C) that restrict the sample to relatively less vulnerable births, in order to further rule out that my results are due to fragile births being endogenously sorted into or out of separation.

The panels of Table 6 show a consistent pattern. In Column 1, $\overline{public}_{v-i}$ is associated with decreased neonatal mortality. Controlling for own facility at birth, Column 2, eliminates this association, but why? The answer is that the association between $\overline{public}_{v-i}$ and neonatal mortality is accounted for or eliminated once the analysis considers whether the baby was separated from the mother—incorporating this information either as a regression control, in Column 3, or by splitting the sample, in Columns 4 and 5.

Figure 11: The public mortality advantage reverses after accounting for separation of the mother and baby, but not for other practices; UP and Bihar, NFHS-5.

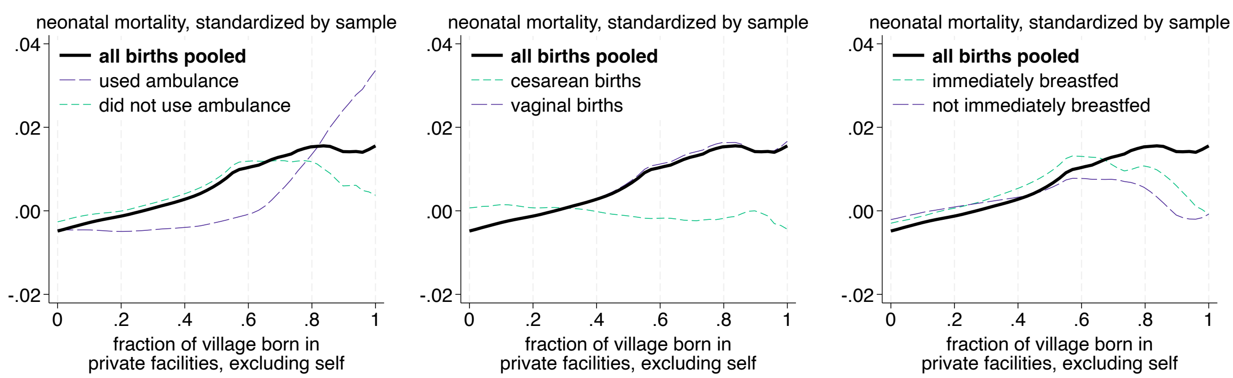
(a) Stratifying by separation reverses the mortality relationship for both groups



(b) But not by ambulance use,

(c) nor by cesarean delivery,

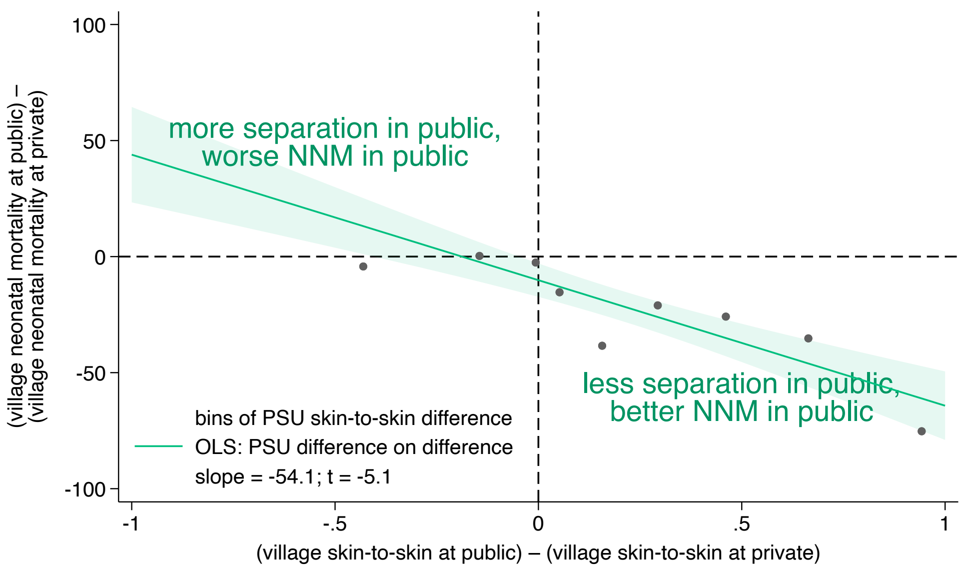
(d) nor by immediate breastfeeding



Notes: The figure displays the results of splined local linear regressions using an Epanechnikov kernel. It presents neonatal mortality as a function of the fraction of a village's births (excluding self) that take place in private facilities, stratified by various obstetric care practices. If the public mortality advantage is not explained by a practice, then the advantage should persist whether or not babies experienced that practice. A flattening or reversal of the mortality advantage in samples stratified by the practice suggests that the practice is mediating the effect. Data are from the NFHS-5. Observations are births aged 1–59 months that were born in a health facility to women living in rural areas of UP and Bihar at the time of interview. Regressions are survey-weighted.

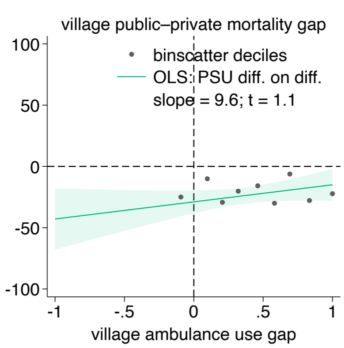
Figure 12: At the village level, where public and private facilities separate mothers and babies equally often, the gap in mortality disappears; UP and Bihar, NFHS-5

(a) Villages with no separation gap have no mortality advantage,

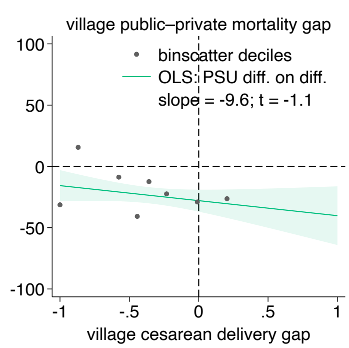


but the public mortality advantage persists across all sizes of public–private gaps in

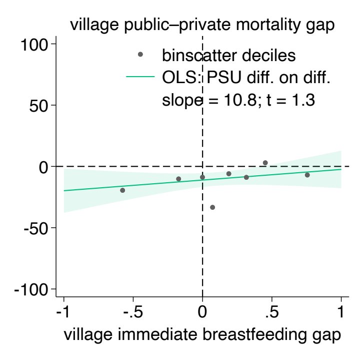
(b) ambulance use,



(c) cesarean delivery,



(d) and immediate breastfeeding



Notes: The figure displays regressions of village public–private mortality gap, (village neonatal mortality at public) – (village neonatal mortality at private), on village public–private gap in obstetric care practices, (village fraction receiving care practice at public) – (village fraction receiving care practice at private). Data are from the NFHS-5. Observations are rural villages in UP and Bihar. Village-level average values are survey weighted. Regressions use the weights based on the sum of the underlying birth-level weights.

Table 6: A greater fraction of neighboring births in public facilities predicts better chances of neonatal survival, but not after accounting for separation of mother and baby

	Neonatal mortality per 1,000 births					
	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	all	all	all	skin-to-skin only	not skin-to-skin only	all
Estimate:	OLS	OLS	OLS	OLS	OLS	IV
Village fraction at public	-10.644*	1.787	1.437	1.568	1.082	
(excluding self)	(5.157)	(5.449)	(5.161)	(4.736)	(13.527)	
Own birth at public		-19.931***				
		(3.091)				
Received skin-to-skin			-55.504***			-48.903*
			(3.566)			(23.618)
Constant	44.426***	50.117***	77.842***	22.236***	78.094***	73.868***
	(4.112)	(4.274)	(4.980)	(3.735)	(10.083)	(18.191)
Observations:	39,045	39,045	39,045	30,048	8,997	39,045

Notes: NFHS-5. PSU = primary sampling unit, a local area which is often a village, which we sometimes call "neighborhood" for simplicity. "PSU fraction at public (excluding self)" is the fraction of the observations in an observation's primary sampling unit, other than that observation itself, that happened in a public health care facility, among those that happened in a public or private health care facility. Each observation is a birth within the 60 months before the survey, to a family in a rural area of Uttar Pradesh or Bihar at the time of interview. Survey design weights are used and standard errors are clustered by PSU. †$p < 0.10$, *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

7 Discussion and conclusion

According to the UN World Population Prospects, there were 23.5 million births in India in 2020. According to the NFHS-5, about one third were in rural Uttar Pradesh and Bihar. This amounts to 7.7 million births, or 5.7 percent of all births globally in 2020. Among these births in rural Uttar Pradesh and Bihar, 271,000 died in the first month of life, suffering a neonatal mortality rate of 35 per 1,000.

Most births in India now occur at health facilities, rather than at home. However, health policies and programs are needed to improve the quality of care (Semrau et al. 2017). Among births in health facilities in rural UP and Bihar, 23.3 percent were separated from their mothers at birth. Neonatal mortality among this group was 79 per 1,000. That means 1.5 million babies born in rural UP and Bihar in 2020 were separated from their mothers. 115,000 of them died neonatal deaths.

Surely not all of these separations preceded harmful medical interventions. Nevertheless, the public-private mortality gap appears where facilities differ in separation rates and disappears where they do not. Private facilities separate more babies from mothers than public facilities do. Private providers charge per service; public providers receive salaries. The interventions that follow separation—washing, warming devices, airway suctioning, pre-breastfeeding feeds, antibiotics—can harm newborns in settings where temperature control, sterility, and proper dosing are uncertain.

Regulation of private facility practices, payment structures that reduce per-service incentives, or expanded public facility capacity could each reduce neonatal mortality. Each faces obstacles. Future research should measure specific interventions directly, test responses to different regulatory and payment structures, and assess whether these patterns extend beyond rural UP and Bihar.

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