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

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Abstract

In two large Indian states, rural mothers who give birth in public (government-run) clinics and hospitals are poorer and pay less for natal care than at private clinics and hospitals—yet, puzzlingly, their newborns survive at much higher rates. I show that these public facilities reduce the risk of newborn death, by over 25 per thousand births or over. I use two complementary empirical strategies: (i) a strategy that addresses selection by relating village-level neonatal mortality to the fraction of village births that occur in public facilities, and (ii) a spatial regression discontinuity that compares births in districts with a smaller fraction born in public facilities to nearby and otherwise-similar births in districts with a larger fraction born in public. I present evidence that skin-to-skin contact at birth, recommended by the World Health Organization but often eschewed by private facilities, is responsible for this mortality advantage. These results suggest that private providers cause over 37,000 children's deaths each year, due either to incompetence or to incentives that conflict with providing high-quality care.

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1 Introduction

In some contexts—especially in the developing world—the choice of care during labor and delivery can determine whether a child lives or dies[1]. Even so, the question of which facility to choose for labor and delivery may be complex. It can incur large financial costs. It is made without full understanding of the effects of care on the mother's or child's health. And it is made without full knowledge of the services different practitioners will provide[2].

Prior work, including my own, has shown a puzzle: Millions of families in rural Uttar Pradesh and Bihar are choosing riskier facilities and paying a premium for it (Verma and Cleland 2022; Coffey et al. 2025). These two Indian states have a population larger than the United States and a rate of newborn death exceeded only by Afghanistan, Pakistan, and Nigeria. Mothers in this area who give birth in private clinics and hospitals, on average, come from wealthier families and pay five times as much for care as those who choose government clinics and hospitals; however, 51 babies die in their first month per thousand births in private facilities, rather than 32 per thousand in public facilities[3].

In this paper, I show that public facilities reduce newborn mortality compared to private facilities for births to women living in this area. 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, in the absence of a causal effect, a village's mortality rate 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 identification strategy is similar to those used by Geruso and Layton (2020), Gruber, Levine, and Staiger (1999), Einav, Finkelstein, and Cullen (2010), and Chetty, Friedman, and Rockoff (2014) in its use of marginal changes in an average to identify marginal effects in the presence of endogenous sorting. Using cross-sectional variation across villages to estimate this model identifies the causal effect if family-level selection is the only concern, but

[1] See Currie and Gruber (1996), Lazuka (2018), and Okeke (2023).

[2] See McGuire (2000) and Das et al. (2016).

[3] These estimates are based on data from the National Family Health Survey, 2019–21.

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 that, on average, differ only in the costs[4] 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.

I find that public facilities dramatically reduce newborn mortality compared to private facilities. In regressions using across-village variation to estimate the effect parameter of the econometric model, I find a mortality reduction of 11–32 per thousand, robust to (indeed, strengthened by) the inclusion of various controls and fixed effects. Using the quasi-experimental regression discontinuity design to estimate the model, I find a mortality reduction of 116-223 per thousand. None of these effect estimates are significantly different from one another, but all that include basic controls are different from zero.

What explains these effects? I present a collage of evidence that skin-to-skin contact at birth is the primary protective service that public facilities are providing at higher rates than private facilities. Babies who are put in skin-to-skin contact with their mothers' chests for an hour after birth have more stable respiration and cardiac activity, and they are more likely to successfully initiate breastfeeding. The World Health Organization's guidelines for high-risk infants recommend putting all but those who are in shock or require mechanical ventilation in skin-to-skin contact with the mother's chest immediately following birth (World Health Organization 2022). This recommendation is supported by evidence from a randomized trial of immediate skin-to-skin contact among vulnerable infants in five countries (WHO Immediate KMC Study Group 2021) as well as by increasing understanding that the separation of mothers

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

and infants could exacerbate the physiological instability that keeping newborns in neonatal care wards was intended to treat (Bergman, Linley, and Fawcus 2004).

Using the regression discontinuity design, I show that public birth increases skin-to-skin care at the border, by over 90 percentage points. Additionally, I show that stratifying the sample into those that received skin-to-skin care and those that did not reverses the mortality pattern by birth-mix. Finally, I show that villages with a smaller difference in rates of skin-to-skin care between public and private births have a smaller mortality advantage. The data I use cannot shed light on the underlying reasons for this difference in care, but it is consistent with private facilities trading quality off against responding to other incentives, in an environment where patients cannot easily detect quality of care. That is, skin-to-skin care might not look to private-facility customers like active care worth paying for. Another possibility is that private providers are simply not competent to identify and provide life-saving care[5].

The size and scale of this effect is very large. According to the 25-per-thousand estimate, each year these public health facilities save the lives of over 100,000 of the births delivered there, on net[6]. Furthermore, if public facilities could provide this care to all private facility births in the region, then the 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[7] and the rate of all of India by over 1.5 per thousand[8]. This alone would achieve nearly 20% percent of the progress India needed in 2020 to achieve the UN Sustainable Development Goal 3.2[9].

This paper contributes to several strands of the economics literature. First, it contributes to the literature on provider agency and health care as a credence good. There is a robust literature showing that physicians do not perfectly follow the preferences of their patients with incomplete information McGuire (2000). However, none have shown a significant mortality effect (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). This paper provides the first evidence of a mortality effect

[5]And, of course, no extant quantitative data allows researchers to fully rule out that private providers are causing affirmative harm in difficult-to-observe ways.

[6]25.6 deaths averted per thousand births × 23.5 million births in India in 2020 (UN World Population Prospects) × 32% of India's births in rural UP and Bihar in 2020 (WHO) × 60% of births in public in rural UP and Bihar in NFHS-5 = 118,272 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 = 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

[8]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

[9]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)

consistent with this theoretical framework.

Additionally and most directly, this paper contributes to the literature on the effects of skilled natal care. Researchers have found that increases in health care supply and demand have uncertain effects on mortality. Several studies find that an increase in skilled natal care reduces infant mortality rates (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) show no causal evidence of a reduction in neonatal mortality after the introduction of a conditional cash transfer program increased facility birth in India, and Andrew and Vera-Hernández (2024) even show an increase in perinatal mortality from the same program. However, these papers study short-run changes in mortality from a program paying women to give birth in health facilities that struggled to keep up with the increased demand for care. The source of variation I use is not a shock to supply or demand, but long-standing administrative boundaries that give markets and policy time to reach a steady state. With that alternative source of variation, I show that public facility use improves neonatal mortality, even relative to private facilities.

2 Puzzle: Richer mothers pay more for riskier natal care

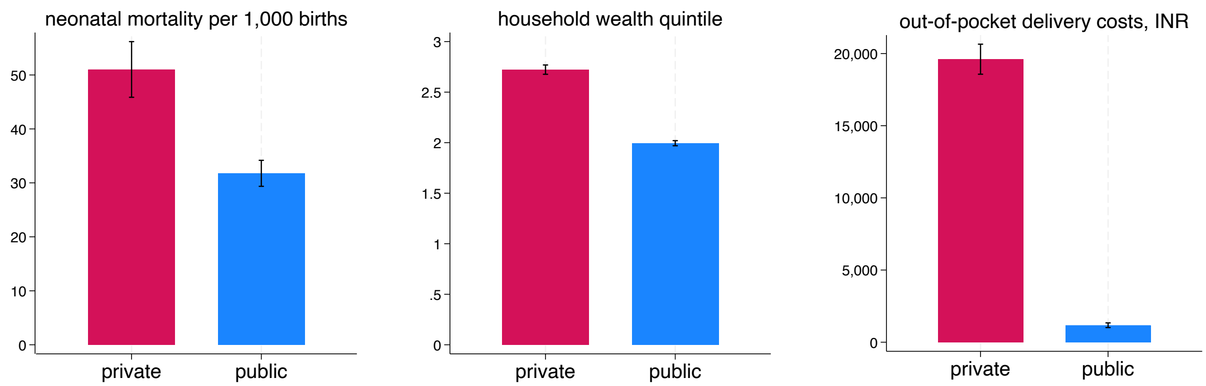
Figure 1 presents the puzzle that motivates this paper. Neonatal mortality—death in the first month of life—is much more common in private than in public facilities in rural Uttar Pradesh and Bihar. This is even though, as Panels b and c show, babies born in private facilities in this context come from 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 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 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

Figure 1: The motivating puzzle—a public-facility survival advantage in rural Uttar Pradesh and Bihar; NFHS-5

(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 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.

of Appendix Table A1 shows that, among those that provide natal care, private facilities tend to have lower numbers of staff who are less educated and worse resourced than at public facilities.

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[10], 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 drastically 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) skin-to-skin contact 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 skin-to-skin birth was only asked in the NFHS-5, so it has about half the sample size as 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.

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

[10]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.

births into one facility type. In this section, I outline the model I use to separately identify the causal effect and selection.

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[11], 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 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[12]. 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

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

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

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

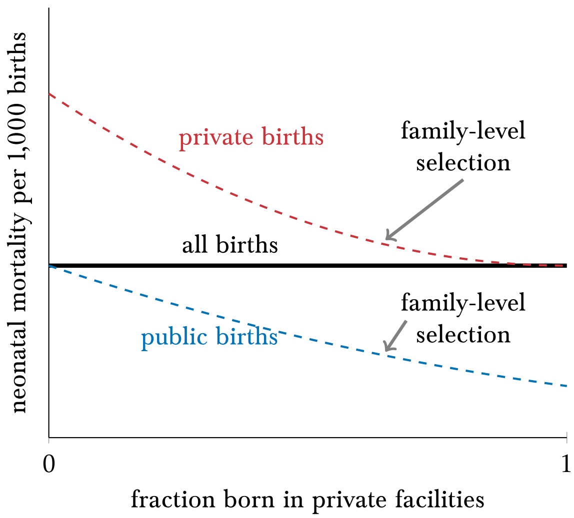
$$y_{i,v} = \beta_0 + \beta_1 \overline{private}_v + f(X) + \epsilon_{i,r}, \tag{1}$$

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

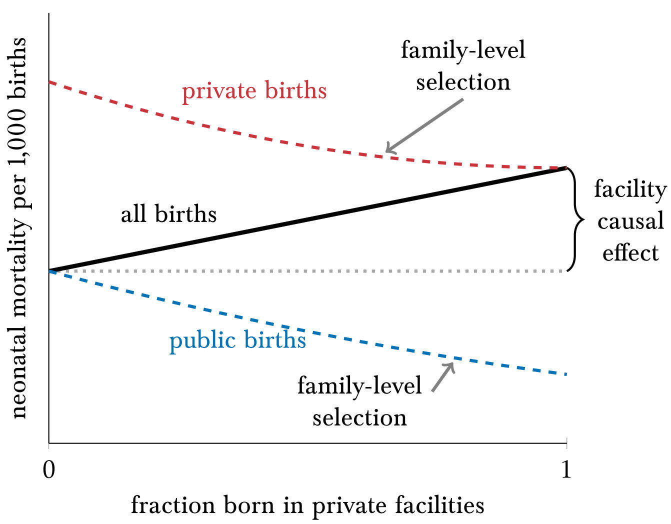
The sample size of births in a village may be small, and so the fraction born in private may

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

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

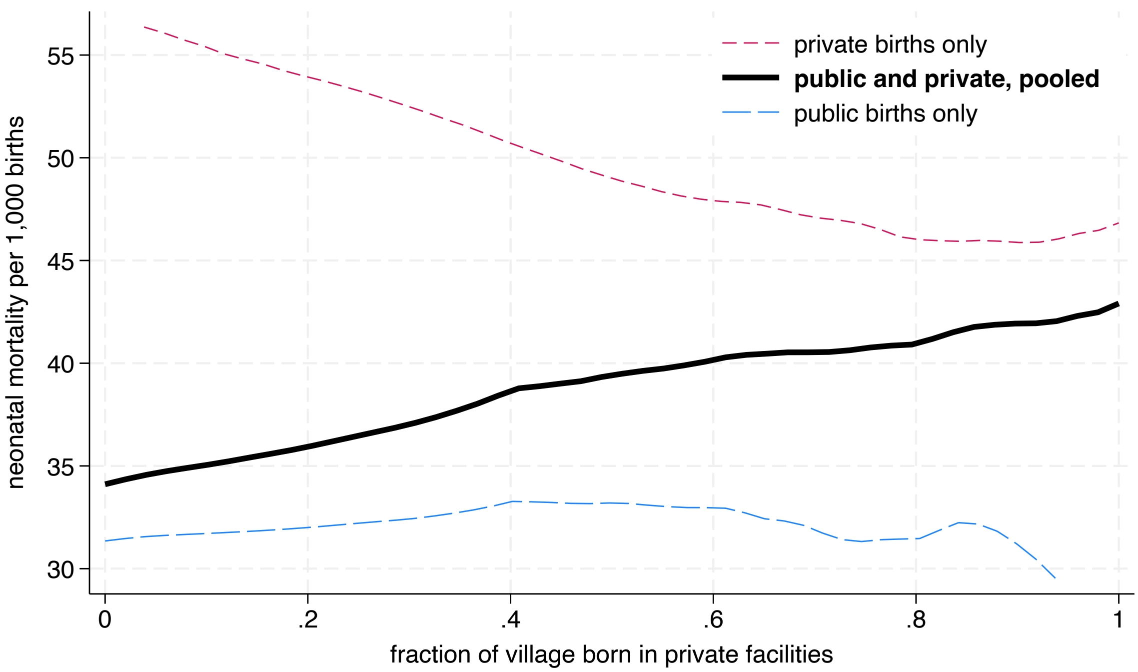


(b) Selection **with** facility-type causal effect



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.

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. *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

hinge importantly on a "marginal" birth. For that reason, I also calculate

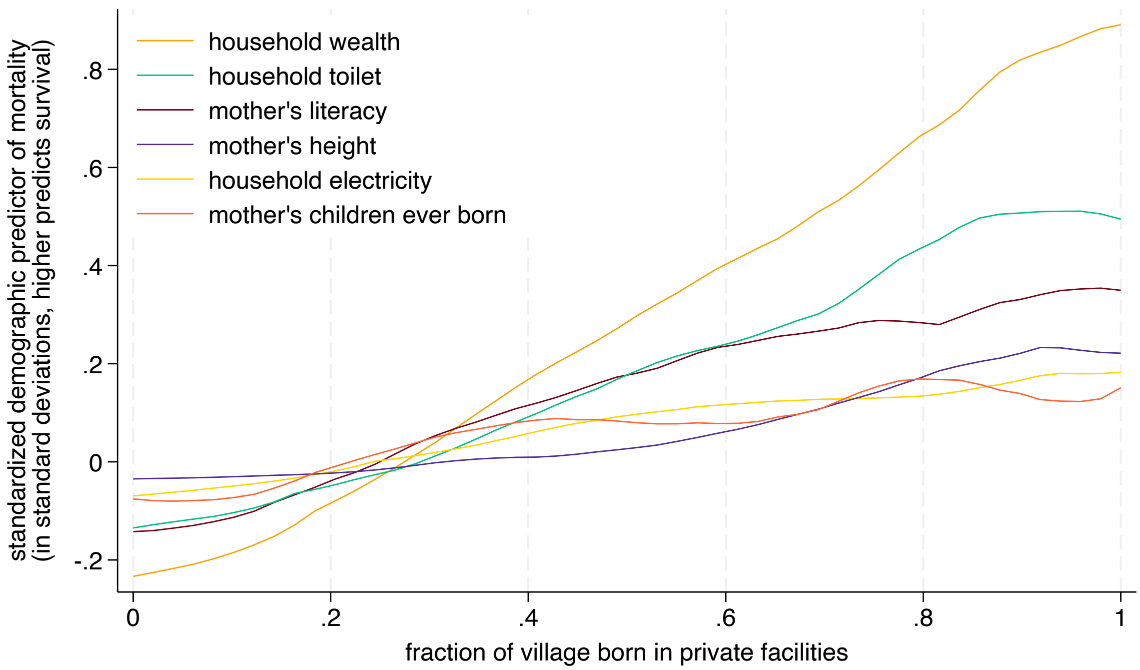
$$\overline{private}_{v-i} = \frac{\text{count of births in private facilities in the last 5 years, excluding self}}{\text{count of births in facilities in the last 5 years in the baby's village, excluding self}},$$

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 18.5–28.9 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 12.1–18.7 per thousand. These are still significant at no greater than the 5-percent level.

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

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.

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.

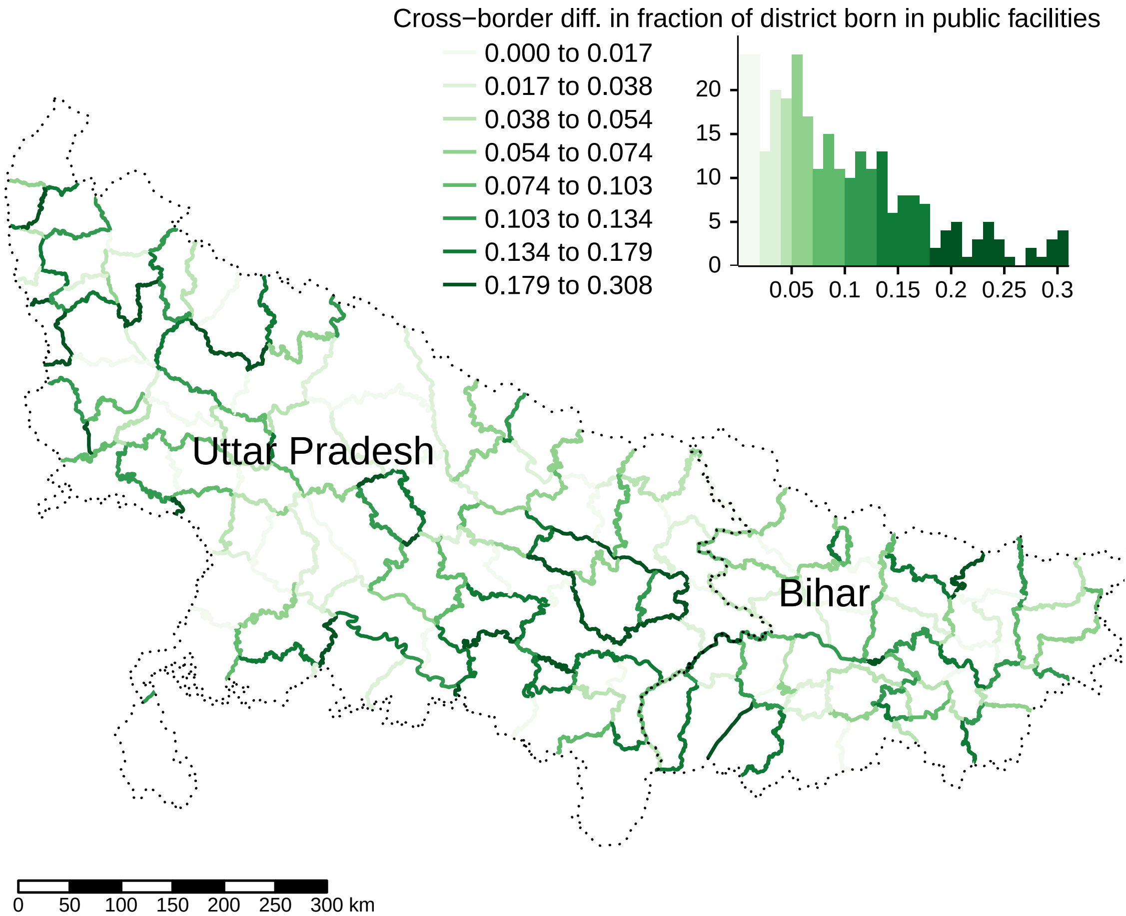
I then use this cost-shifter-type variation across neighboring districts to assign the groups that the regression discontinuity compares. If the village's own district has a higher public birth fraction than a neighbor district, it is on the positive side of the regression discontinuity cutoff. If the 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[13], 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 that are seeking payment from governments outside their usual remit. 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 likely 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[14].

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

[13] 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.

[14] 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[15].

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, skin-to-skin contact 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[16] 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 [17], which have only one significant discontinuity: There

[15] Appendix **??** shows that the results are robust to alternative bandwidth specifications.

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

[17] 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.

are more Other Backward Caste[18] people in districts with more public birth and fewer people without legal protections. While a discontinuity in demographic variables may be concerning, this would predict increased mortality on the side of the border on which I identify reduced mortality—fighting against the effect I estimate.

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.

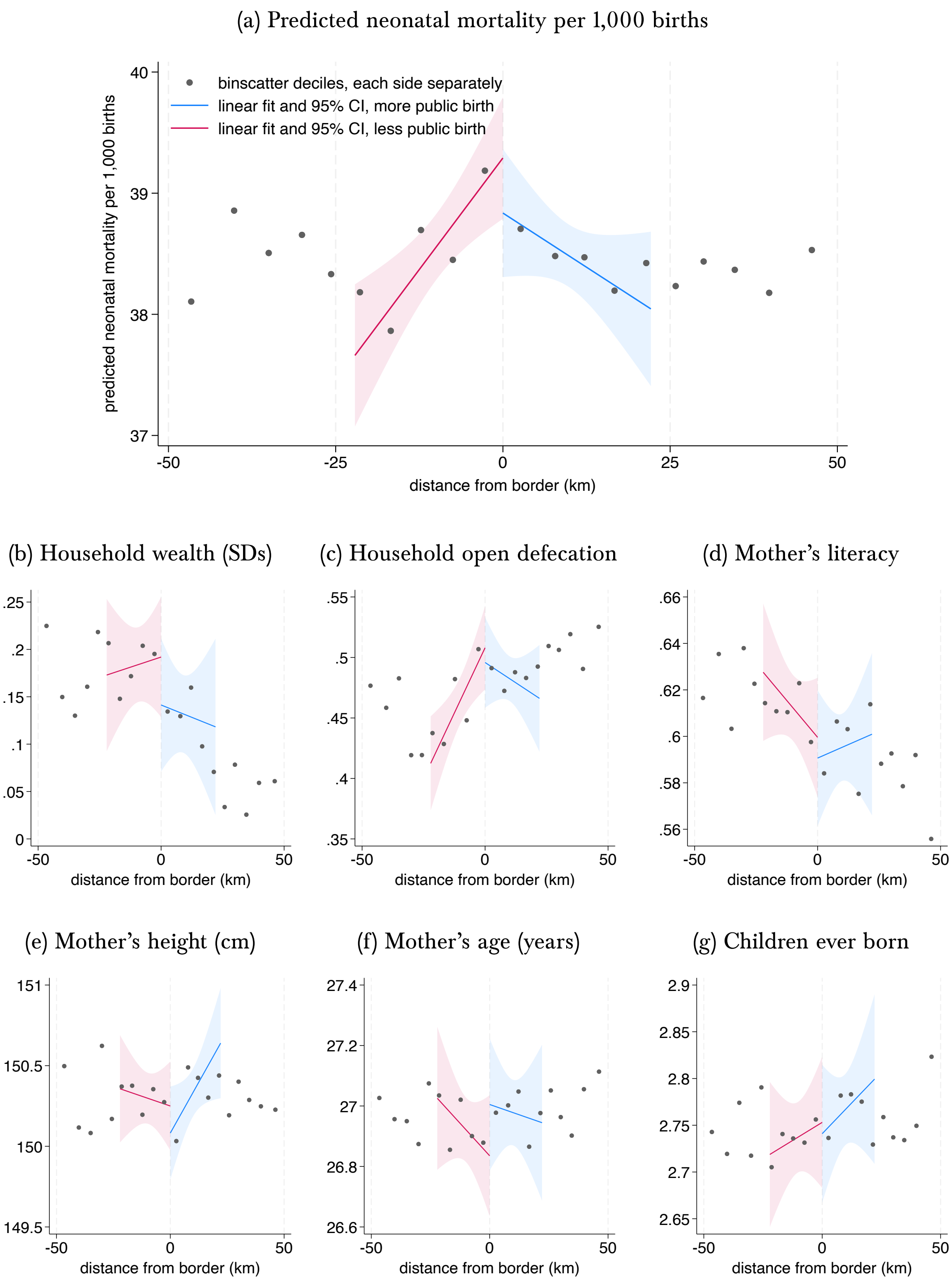
Independence may be violated if the medical system is strained as a result of greater use. Another possible violation is if public health facility users are more likely to engage in protective behavior for their children apart from choosing public health care. However, public health users are disadvantaged 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)

[18]This is a group protected by Indian law that was historically marginalized, though less so than Scheduled Caste or Scheduled Tribe groups.

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



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.

and conditional average treatment effect regressions as described by Calonico et al. (2025)[19]. The difference-in-discontinuities regression equation is

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

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

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

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 2 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.

[19] These have additional identification assumptions that I discuss in Appendix D.

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 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 $\times$ 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.

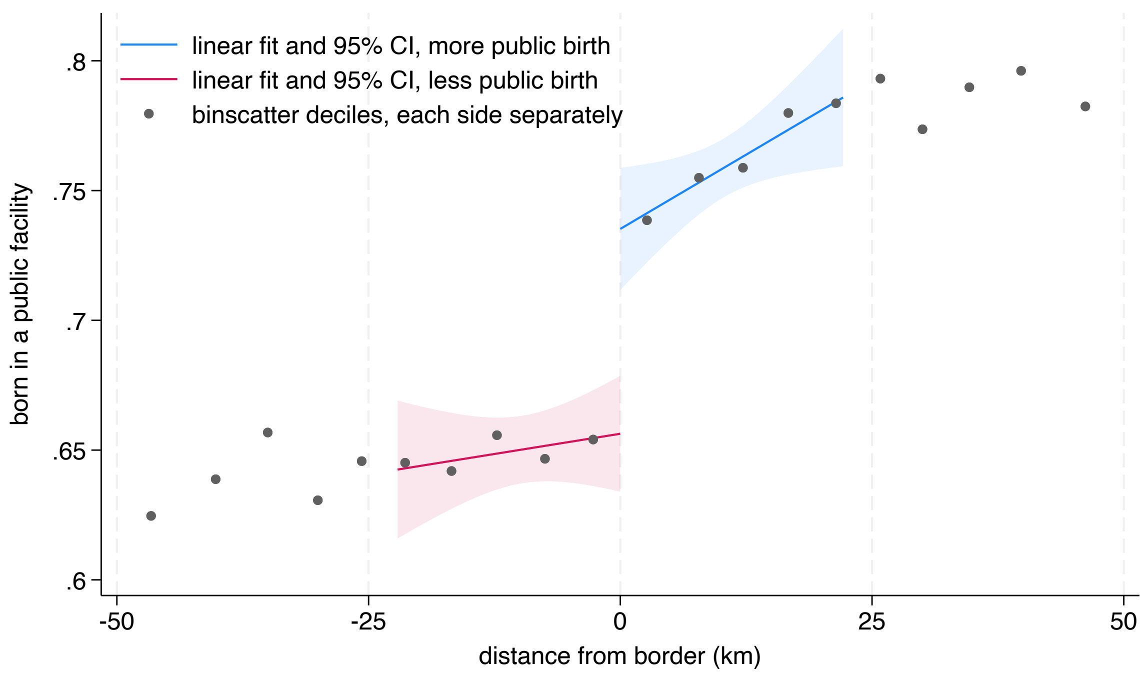
Together these estimates show that the local average treatment effect of public birth in the remote areas near district borders may be very large, much larger than the average treatment effects across the entire population could plausibly be. However, the large size of the effect estimate is matched 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: Skin-to-skin care resolves the puzzle

6.1 Regression discontinuity estimates

At least since the 1970s, medical researchers have advocated skin-to-skin contact between healthy newborns and their mothers immediately following birth (Château 1976; Thomson, Hartsock, and Larson 1979). Babies who are put in skin-to-skin contact with their mothers' chests have more

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



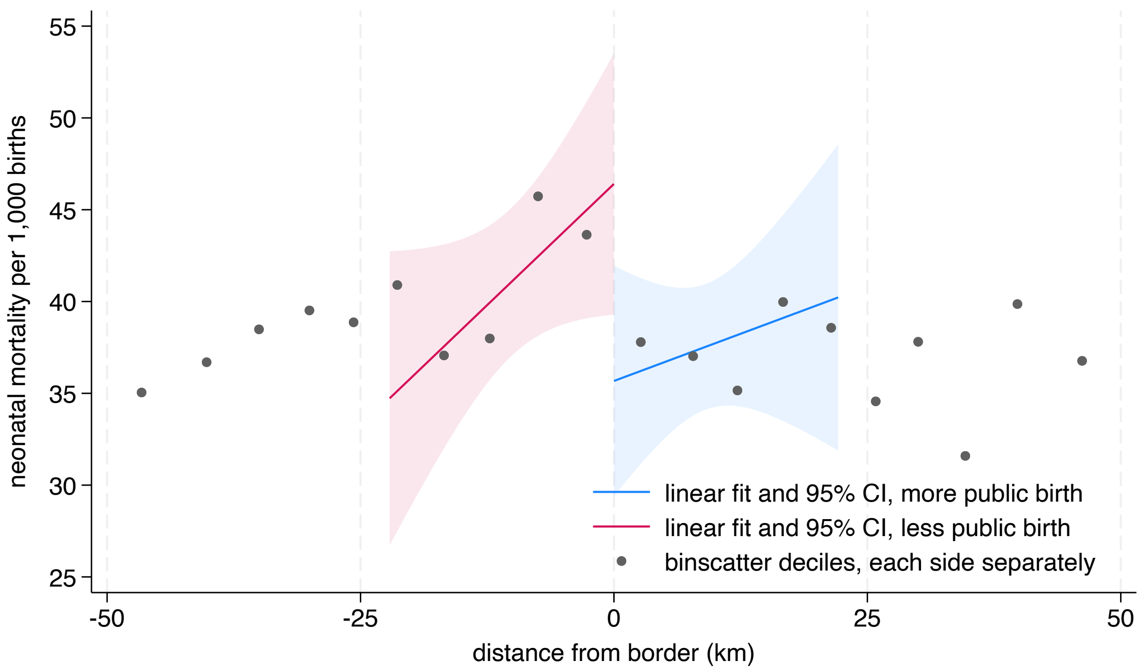
Notes: The figure displays the results of a local linear regression using a triangular kernel. It presents public birth as a function of distance from the border. For every pair of adjacent districts, observations from the district with a lower fraction born in public are on the left side of the plots, 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 5–9 treats 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 5–9 treats 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. $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

stable respiration and cardiac activity, and are more likely to successfully initiate breastfeeding. The first hour after birth is thought to be a "mutual early sensitive period" for mother-infant bonding and establishing breastfeeding behavior (Widström et al. 2019).

In 2022, the World Health Organization (WHO) released new guidelines that updated the existing medical practice of separating low birth weight and premature infants from their mothers so that they could receive care in specialized newborn units. Instead of moving these high-risk infants to incubators or radiant warmers after birth, the new guidelines recommend putting all but those who are in shock or require mechanical ventilation in skin-to-skin contact on the mother's chest immediately following birth (World Health Organization 2022). This recommendation was supported by evidence from a randomized trial of immediate skin-to-skin contact among vulnerable infants in five countries (WHO Immediate KMC Study Group 2021) as well as by increasing understanding that the separation of mothers and infants could exacerbate the physiological instability that keeping newborns in neonatal care wards was intended to treat (Bergman, Linley, and Fawcus 2004).

First, using the regression discontinuity design detailed in Section 5.2, I find a significant discontinuity in skin-to-skin care at birth in the same places where public birth increases discontinuously and neonatal mortality decreases discontinuously.

Figure 9 and Table 6 present the mechanism results of the regression discontinuity, and they show that skin-to-skin contact jumps discontinuously at district borders. Columns 1–3 of Panel a of Table 6 shows that being on the side of a district border with more public birth increases skin-to-skin contact 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 6, Panel a, Column 1 graphically. It presents skin-to-skin contact 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 increases skin-to-skin contact by 46.5–75.8 percentage points.

Columns 4–6 present additional results showing difference-in-discontinuity estimates that question 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 with more public birth, for those district borders separating two districts with a bottom-tercile difference (4.5 p.p.). The regression in Column 5 is significant at the 10% level. 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 greater-than-median difference in public facility birth. While these estimates are all positive, none are significant even at the 10% level. Panel b uses this border variation as an instrument for public birth, yielding point estimates that agree with the causal estimates from Columns 1–3, but none are significant.

Columns 7–9 present 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 no significant difference but are all directionally positive. Interestingly, when Panel b uses this border variation once again as an instrument for public birth, the findings are large and strongly significant. According to these estimates, public birth increases skin-to-skin contact by 50.8–87.1 percentage points.

While these estimates are certainly noisier than the prior findings, I am able to investigate the effects of skin-to-skin care and other obstetric care practices in alternative research designs.

6.2 Village neighbors empirical strategy

Next, I turn to another geographic empirical strategy that is not plausibly confounded by the health or socioeconomic status of a birth or its family. The core of this strategy is a variable, $\overline{public}_{v-i}$, which reflects the baby's village-level context:

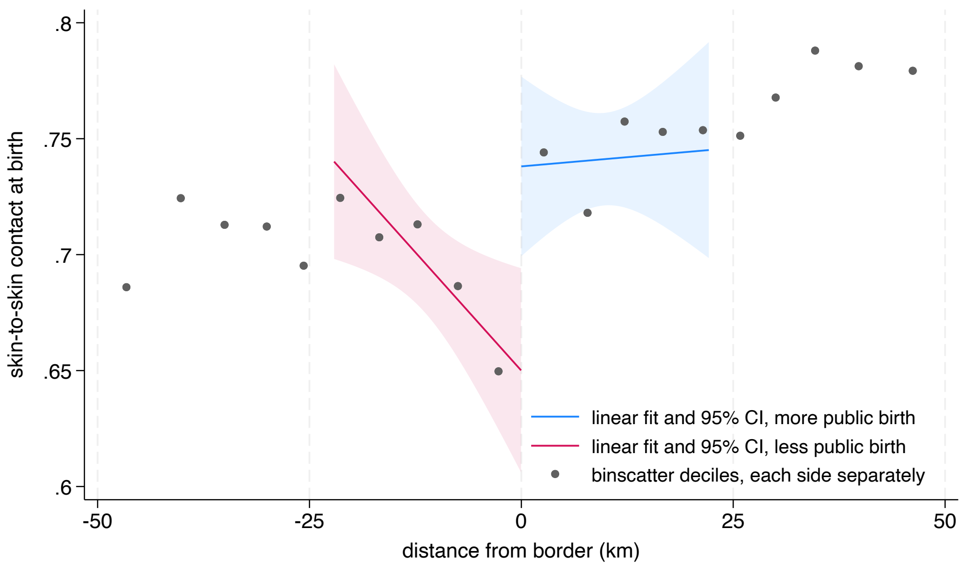
$$\overline{public}_{v-i} = \frac{\text{count of births in } \textbf{public} \text{ facilities in the last 5 years}}{\text{count of births in facilities in the last 5 years in the baby's PSU, excluding self}}.$$

$\overline{public}_{v-i}$ 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 (and rule out a mechanical correlation with itself). In the supplementary online appendix we plot the histogram of $\overline{public}_{v-i}$; it is asymmetrically skewed because in rural Uttar Pradesh and Bihar birth in public facilities is more common than birth in private facilities.

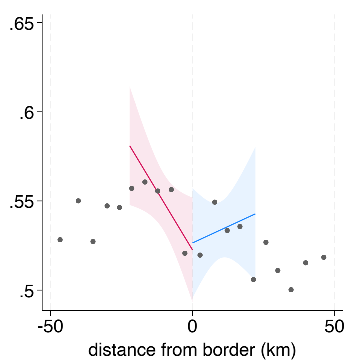
Figure 10 Panel a is similar to Figure 4, but it uses $\overline{public}_{v-i}$ on the horizontal axis and includes an additional line for skin-to-skin care at birth. Each variable is standardized with its mean and standard deviation in this sample for legibility. Six covariates are included: The asset 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 $\overline{public}_{v-i}$

Figure 9: Mechanism—skin-to-skin and ambulance use are the only discontinuous obstetric care practices at borders; UP and Bihar, NFHS-4 and NFHS-5

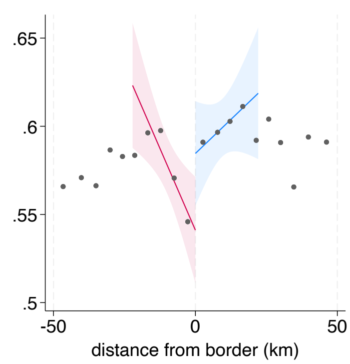
(a) Skin-to-skin contact (coef: 8.4 p.p., SE: 3.0; NFHS-5 only)



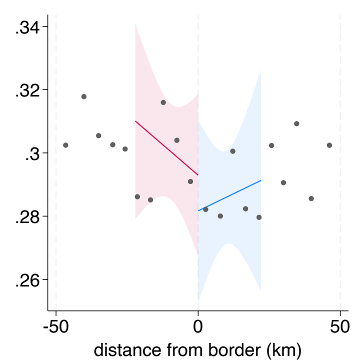
(b) 3+ antenatal care visits



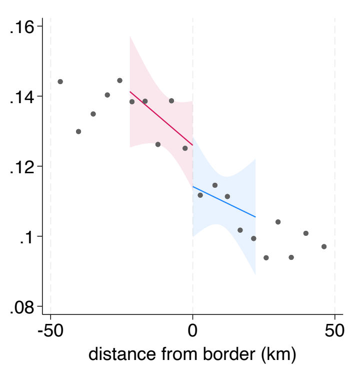
(c) Contact with health worker



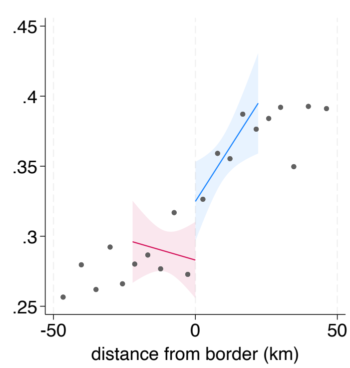
(d) Immediate breastfeeding



(e) Cesarean delivery



(f) Ambulance use



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 increases skin-to-skin contact relative to private

Outcome: skin-to-skin contact	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 skin-to-skin contact 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 5–9 treats 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. $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

are more likely to receive skin-to-skin care at birth. This positive slope can be interpreted as a first stage of this empirical strategy.

Panel b focuses in particular on characteristics of obstetric care beyond just skin-to-skin care. Ambulance use, out-of-pocket spending, 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 skin-to-skin care, are greatly less common in villages with more private birth. These are investigated as alternative mechanisms below.

Figure 11 relates $\overline{public}_{v-i}$ to neonatal mortality. 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 skin-to-skin care. They split the same sample according to whether the baby received skin-to-skin care at birth, the two dashed lines. Within both of these subsets, there is no longer a positive association between $\overline{public}_{v-i}$ and neonatal survival. In fact, there is the negative association that observables would predict. The fact that the $\overline{public}_{v-i}$ survival advantage can be so completely accounted for by skin-to-skin care suggests that it is because of it.

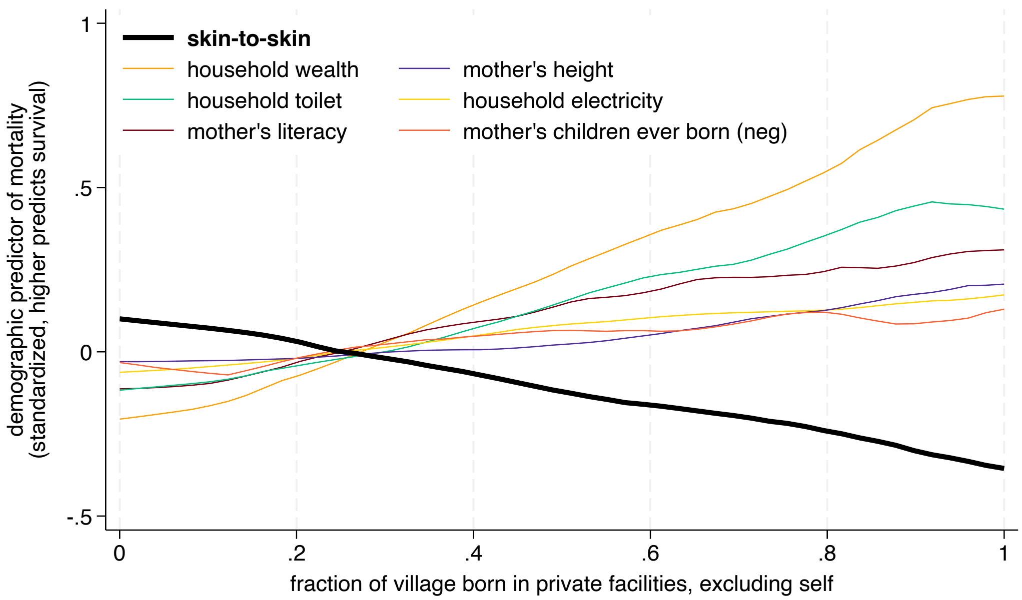
I show in Panels b–d,of Figure 11 that the result is robust to splitting the sample according to other indicators of the health and care of the mother and pregnancy. Unlike splitting the sample by skin-to-skin care of the baby, these splits do not reverse or eliminate the association between neonatal death and $\overline{public}_{v-i}$. This is important because this finding would be confounded by any difference in unobserved obstetric care, if it were sufficiently highly correlated with skin-to-skin pediatric care. Qualitative research by Srivastav et al. 2023 finds that private providers in this context are more likely to perform unnecessary labor inductions, for example. Neither ambulance use, cesarean birth, nor breastfeeding account for the $\overline{public}_{v-i}$ survival advantage so fully as skin-to-skin care. The consistent patterns of these results is evidence, collectively, that properties of the pregnancy are not confounding my main result.

6.3 Village-level difference-on-difference regressions

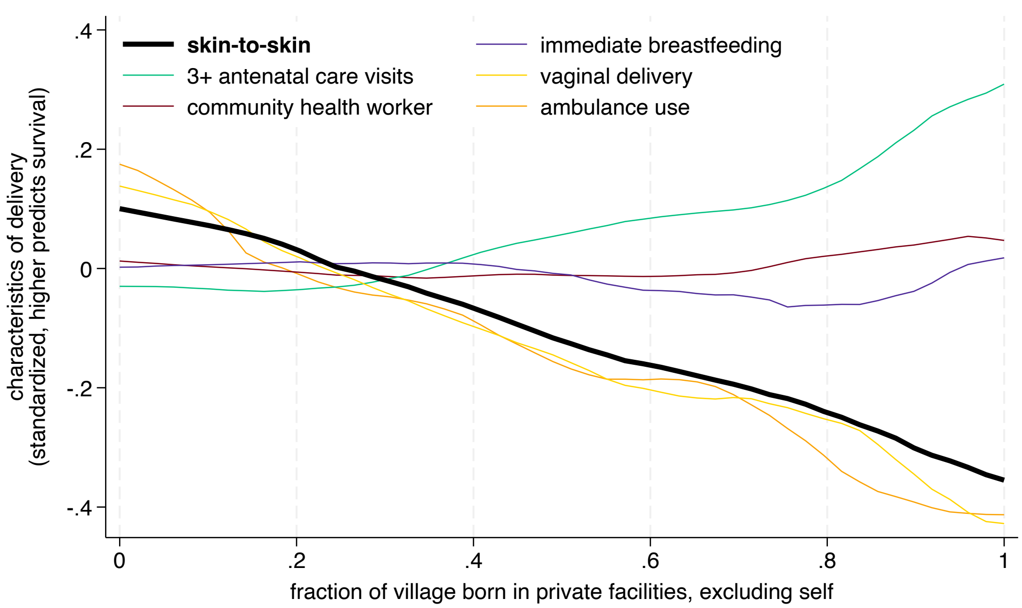
Finally, Figure 12 collapses the data to the PSU level. Where Figures 10 and 11 compare across PSUs, learning from the differences in outcomes between babies born in different villages, Figure 12 compares babies born in public and private facilities within the same village. The horizontal axes measures 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 skin-to-skin care

Figure 10: Skin-to-skin care, cesarean delivery, and ambulance use are candidates to explain the mortality advantage; UP and Bihar, NFHS-5

(a) Skin-to-skin care is more common in villages with a lower fraction born in private facilities, unlike neonatal mortality and demographic predictors of survival



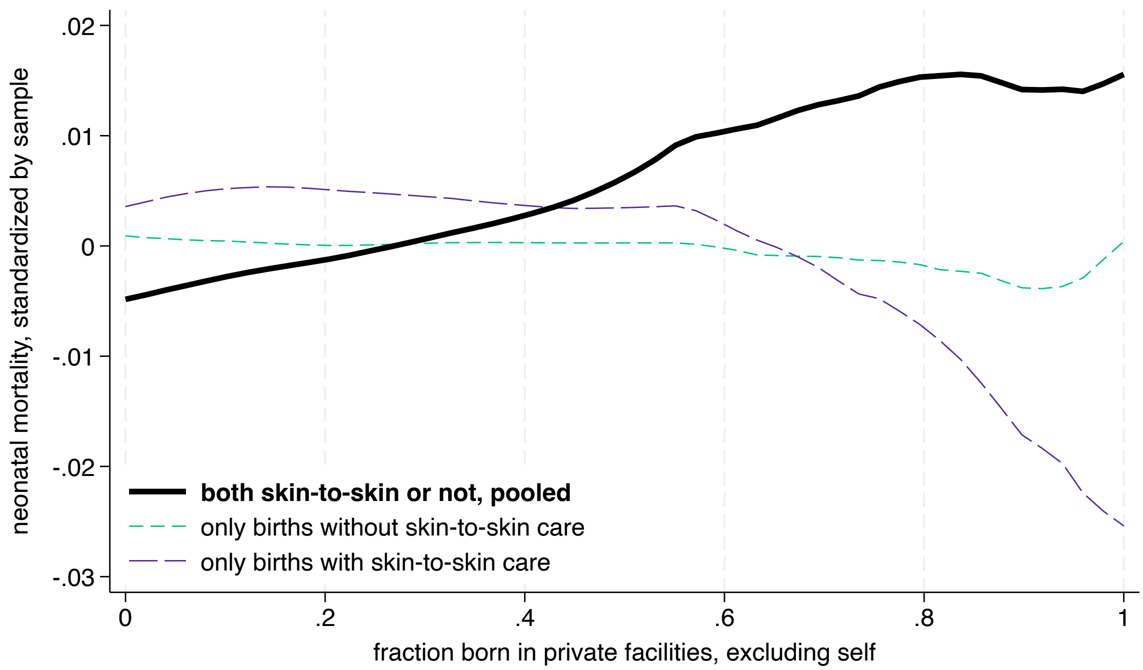
(b) Skin-to-skin care, vaginal delivery, and ambulance use are more common in villages with a lower fraction born in private facilities



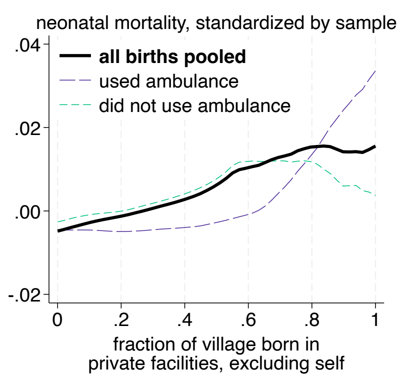
Notes: The figure displays the results of splined local linear regressions using an Epanechnikov kernel. It presents various standardized predictors of mortality as a function of the fraction of a village's births (excluding self) that take place in private facilities. Data are from the NFHS-5. Observations 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.

Figure 11: Stratified regressions show that skin-to-skin care accounts for the public mortality advantage, but other obstetric care practices do not; UP and Bihar, NFHS-5.

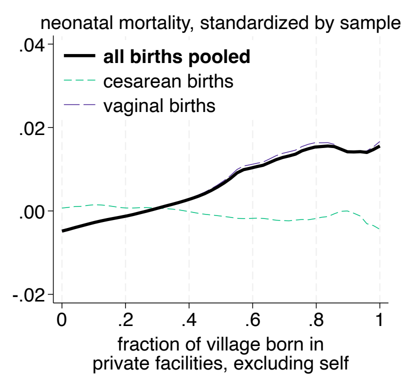
(a) Stratifying by skin-to-skin contact reverses the mortality relationship for both groups



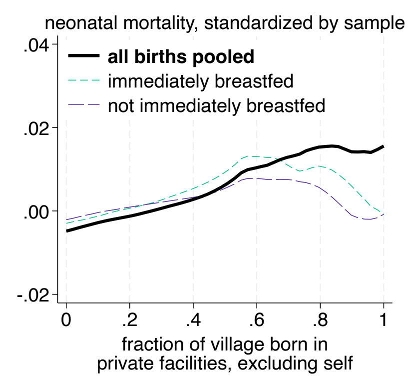
(b) But not by ambulance use,



(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. 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.

difference, 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 survival advantage is greater for villages where the public–private skin-to-skin advantage is greater. Moreover, in the minority of villages where there is not a public–private skin-to-skin advantage, there is also not a public–private neonatal survival advantage. The same is not true of the other care practices in Panels b–d, where the gap in ambulance use, cesarean delivery, or immediate breastfeeding appears to be unrelated to the village's mortality gap.

Table 6 confirms and quantifies the skin-to-skin 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 skin-to-skin care.

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 received skin-to-skin care—incorporating this information either as a regression control, in Column 3, or by splitting the sample, in Columns 4 and 5.

7 Discussion and conclusion

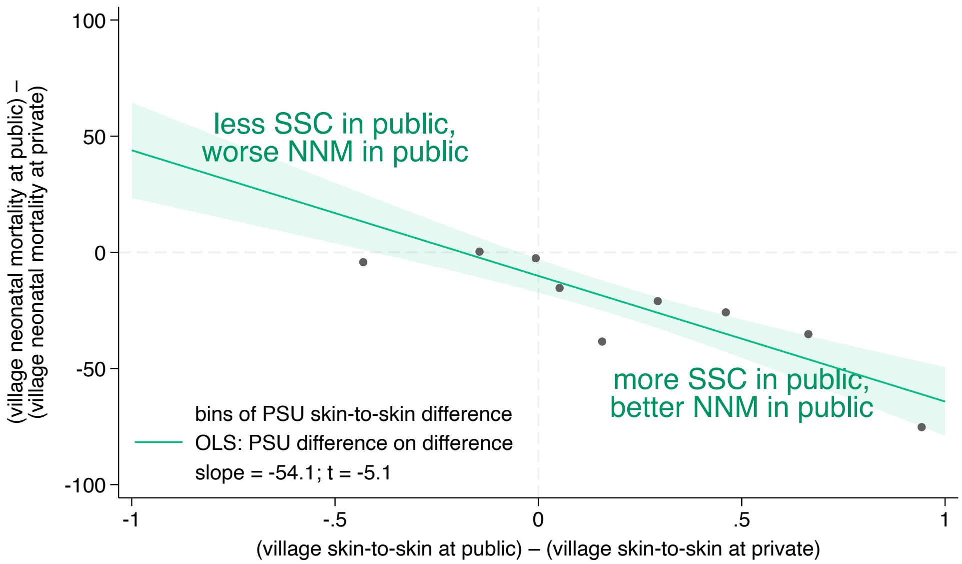
According to the UN World Population Prospects, there were 23.5 million births in India in 2020. According to the NFHS-5, about one third were in rural Uttar Pradesh and Bihar. This amounts to 7.7 million births, or 5.7 percent of all births globally in 2020. Among these births in rural Uttar Pradesh 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, 76.7 percent received skin-to-skin care at birth. Neonatal mortality among those who did not was 79 per 1,000. That means 1.5 million babies born in rural UP and Bihar in 2020 did not receive skin-to-skin care, despite being born in a health facility. 115,000 of them died neonatal deaths.

Two empirical strategies—the within-village difference comparisons of Figure 12 and the across-village instrumental variables estimate in Table 6—suggest that skin-to-skin contact reduces neonatal mortality, in this population, by about five percentage points. Interpreting all of

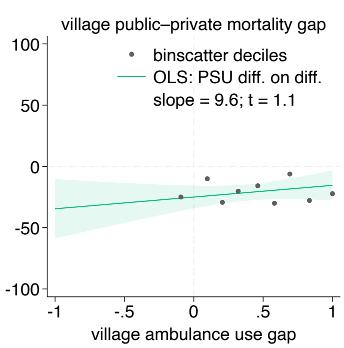
Figure 12: At the village level, the public–private gap in skin-to-skin contact is the only care practice that predicts the public–private gap in neonatal mortality; UP and Bihar, NFHS-5

(a) Villages with no skin-to-skin care gap have no mortality advantage,

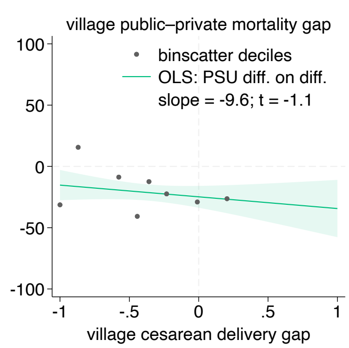


but the public mortality advantage persists across all sizes of public–private gaps in

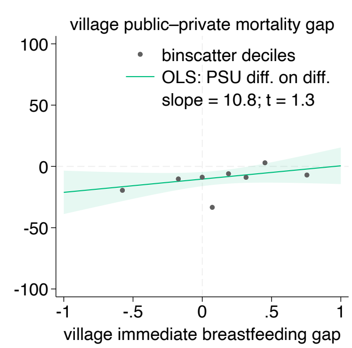
(b) ambulance use,



(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 skin-to-skin contact

	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. †$p < 0.10$, *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

these estimates linearly and literally and ignoring other heterogeneities, this suggests that there would have been 71,000 fewer neonatal deaths in 2020 if all babies born in facilities in rural Uttar Pradesh and Bihar received skin-to-skin care. To put these 71,000 fewer neonatal deaths in context, UNICEF estimates that there were 32,213 neonatal deaths in 2020 in the United States, Canada, and Europe combined.

This estimate may be too large. One reason is that my empirical strategy captures the public–private morality gap and effectively awards the credit to skin-to-skin care. If there are other important differences in the quality of care—for hypothetical example, if the activities of private facilities increase mortality risk beyond obstructing skin-to-skin care—then my strategy would overstate the quantitative benefits of skin-to-skin care.

Nevertheless, the combined pattern of my results—including that there is no advantage of neighbors being born in public facilities once skin-to-skin care is accounted for, nor where there is no public-facility advantage in skin-to-skin care—strongly suggests my interpretation that there indeed exists a protective effect of skin-to-skin care against neonatal death, even as implemented at scale, in public healthcare facilities, in these disadvantaged, populous, rural states of north India.

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A District merging

This section details how I merge districts to areas comparable across the NFHS-1, -2, -4, -5.

Official DHS employee responses to a request on the DHS Program User Forum, documented at https://userforum.dhsprogram.com/index.php?t=msg&th=232&start=0&, provide a set of district names for the codes supplied in the NFHS-1 and -2 data files. The NFHS-4 and -5 supply names for the district codes in the labels of the data files.

The number of districts covered by the surveys increased over the survey rounds from 394 districts in the NFHS-1 to 641 districts in the NFHS-4. Some of this increase is due to later surveys covering more states and union territories than previous surveys, but most of the increase is due to changes to the administrative boundaries of the Indian state.

No convenient and official list of changes to district boundaries exists. For this purpose, I refer to the list of changes at https://web.archive.org/web/20221113192534/http://www.statoids.com/yin.html. In the replication files, I have included a list of other sources that confirm the changes listed on that site for all the NFHS-4 to -5 changes.

For our analysis, I collapse districts to create comparable areas for which the outer boundary does not change across rounds. In some cases this is merely collapsing multiple districts split from a single district. In other cases, districts were created with land from multiple districts prior to the split. In such cases, I collapse the "old" districts and the "new" districts.

I give three examples to clarify this process:

1. Unchanged district: Bihar's Muzaffarpur district did not change, so we give this district a shared code across the four surveys.

2. District split: Bihar's Jehanabad district in the NFHS-1 and -2 split into Jehanabad and Arwal districts in the NFHS-4 and -5. So we give the district in the NFHS-1 and -2 and the districts in the NFHS-4 and -5 a single "district" code.

3. District "carving": Uttar Pradesh's Rae Bareli and Sultanpur districts in the NFHS-1, -2, and -4 split into Rae Bareli, Sultanpur, and Amethi in the NFHS-5. Amethi district was "carved" from parts of Rae Bareli and Sultanpur. So we give the the districts in the NFHS-1, -2, and -4 and the districts in the NFHS-5 a single "district" code.

Four new states were created during this period as well: Telangana (from Andhra Pradesh), Jharkhand (from Bihar), Chhattisgarh (from Madhya Pradesh), and Uttarakhand (from Uttar Pradesh). These were created from districts of the older states, so administrative boundaries did not change. I treat districts that changed state but did not otherwise change their administrative boundary as the same district across survey rounds.

B Econometric framework

I adapt this framework from Geruso and Layton (2020).

Let $m_i^{pub} = \hat{m}_i$ be birth i's neonatal mortality outcome if they were delivered in a public facility. Let $m_i^{pvt} = \hat{m}_i + \mu_i$ be that birth's mortality outcome were they delivered in a private facility, where μ_i is -1 if the birth would not die after a private facility delivery but would after a public delivery, 0 if the mortality outcome wouldn't change based on facility of birth, and 1 if the birth would die after a private but not after a public delivery. Then, defining an indicator function $\mathbb{1}[pvt_i]$ as equal to 1 if the birth occurs in private and 0 if it occurs in public, the realized mortality of each birth is $m_i(\mathbb{1}[pvt_i]) = \hat{m}_i + \mathbb{1}[pvt_i](\mu_i)$.

These individual-level mortality functions can be aggregated to the level of the village, as a function of the fraction of births taking place in a private facility, θ^{pvt}. Suppose that there's a continuum of births, normalized to be of unit length. If they can be sorted by their propensity to choose private over public facilities, given some underlying dimension of compensation that induces changes in θ^{pvt}, then the population's mortality rate at a given θ^{pvt} can be expressed as

$$\bar{m}(\theta^{pvt}) = \int_0^{\theta^{pvt}} (\hat{m}(t) + \mu(t))dt + \int_{\theta^{pvt}}^1 \hat{m}(t)dt \quad (7)$$

$$= \bar{\hat{m}} + \int_0^{\theta^{pvt}} (\bar{\mu} + \epsilon(t))dt \quad (8)$$

where I define

- the village-level neonatal mortality rate if everyone were born in a public facility, $\bar{\hat{m}} = \int_0^1 \hat{m}(t)dt$,
- the increase in the village-level neonatal mortality rate if everyone were born in a private facility, $\bar{\mu} = \int_0^1 \mu(t)dt$, and
- the difference between $\bar{\mu}$ and the increase in the neonatal mortality rate from a given fraction of births being delivered in private $\bar{\epsilon}(\theta^{pvt}) = \int_0^{\theta^{pvt}} (\mu(t) - \bar{\mu})dt$,[20] which can be used with the Leibniz rule to yield that difference for the marginal group of births, $\epsilon(\theta^{pvt}) = \frac{d\bar{\epsilon}(\theta^{pvt})}{d\theta^{pvt}} = \mu(\theta^{pvt}) - \bar{\mu}$.

If selection only operated at the level of individual families—such as mothers with more-complicated pregnancies choosing private facilities—then the slope of the black line would exactly identify the causal effect of public facilities. To isolate the effect of private facilities, we can apply the Leibniz rule to find

$$\frac{d\bar{m}(\theta^{pvt})}{d\theta^{pvt}} = \bar{\hat{m}} + \int_0^{\theta^{pvt}} (\bar{\mu} + \epsilon(t))dt \tag{9}$$

$$= \bar{\mu} + \epsilon(\theta^{pvt}) \tag{10}$$

That is, the slope of the overall neonatal mortality rate at a particular fraction of private birth is identical to the average causal effect of private facilities on neonatal mortality plus the average causal effect for the group of births on the margin. If the average effect were constant across the variation in fraction born in public, then the $\epsilon(\theta^{pvt})$ term would be zero and the slope of the black line would be $\bar{\mu}$.

If there were also confounding in the form of a correlation between average underlying health in a village and the fraction of mothers choosing public facilities in the village, then the slope of the black line would not identify the causal effect of public facilities. In particular, bias would arise that could explain the pattern in the figure if districts in which fewer people choose private facilities are districts that are less healthy on average. In Section 4.2, I show that this direction of bias is unlikely to be operating here: On almost all metrics, districts with more private facility births tend to have better indicators of underlying health.

[20] So $\bar{\epsilon}(1) = 0$.

C RD with other sample definitions

D Identifying assumptions for other RD designs

Table A1: Puzzle summary statistics

	Public sample			Private sample			Difference in means	
	mean	std. dev.	count	mean	std. dev.	count	pub.–pvt.	p-value
Panel a: unit of observation—births to rural households in UP and Bihar in past five years, NFHS-5								
neonatal mortality per 1,000	31.8	175.4	30034	51.0	220.1	9375	-19.2	0.00
household wealth index	-3.41	8.88	30034	2.41	10.17	9375	-5.82	0.00
household open defecates	0.52	0.50	27767	0.35	0.48	8475	0.17	0.00
household has electricity	0.91	0.28	27804	0.95	0.21	8486	-0.04	0.00
Scheduled Caste household	0.30	0.46	29847	0.21	0.41	9304	0.09	0.00
Scheduled Tribe household	0.02	0.15	29847	0.02	0.13	9304	0.01	0.01
OBC household	0.55	0.50	29847	0.56	0.50	9304	-0.01	0.10
Muslim household	0.14	0.35	30034	0.17	0.37	9375	-0.03	0.00
mother literate	0.56	0.50	30034	0.72	0.45	9375	-0.16	0.00
mother's years of ed.	5.8	5.3	30034	8.3	5.5	9375	-2.4	0.00
mother's height (cm)	150.1	6.0	29315	150.7	6.3	9034	-0.6	0.00
mother underweight	0.23	0.42	29274	0.15	0.36	9017	0.07	0.00
mother anemic	0.60	0.49	28929	0.60	0.49	8858	0.01	0.34
mother's age at birth	26.8	4.9	30034	26.5	4.5	9375	0.3	0.00
children ever born	2.8	1.5	30034	2.4	1.3	9375	0.4	0.00
birth order	2.4	1.5	30034	2.1	1.3	9375	0.4	0.00
male birth	0.51	0.50	30034	0.54	0.50	9375	-0.03	0.00
singleton birth	0.987	0.115	30034	0.972	0.166	9375	0.015	0.00
skin-to-skin contact	0.82	0.38	29908	0.60	0.49	9209	0.22	0.00
ambulance use for birth	0.50	0.50	21932	0.10	0.30	7246	0.40	0.00
met CHW during preg.	0.68	0.47	21932	0.61	0.49	7246	0.07	0.00
3+ antenatal care visits	0.57	0.50	20752	0.64	0.48	6873	-0.08	0.00
cesarean delivery	0.04	0.20	30034	0.39	0.49	9375	-0.35	0.00
immediate breastfeeding	0.31	0.46	28555	0.22	0.42	8694	0.09	0.00
Panel b: unit of observation—facilities used by rural households for natal care in UP and Bihar, IHDS-II								
number of staff	21.1	18.3	128	3.5	4.5	17	17.6	0.00
fraction with MBBS or RN	0.45	0.27	127	0.12	0.27	17	0.33	0.00
number of vacancies	4.0	5.5	118	0.3	1.0	15	3.8	0.01
has thermometer	0.95	0.23	128	1.00	0.00	17	-0.05	0.33
has vaginal speculum	0.72	0.45	128	0.35	0.49	17	0.37	0.00
has ultrasound	0.12	0.32	128	0.06	0.24	17	0.06	0.47
has delivery kit	0.92	0.27	128	0.59	0.51	17	0.33	0.00
has forceps	0.81	0.39	128	0.59	0.51	17	0.22	0.03
has partograph	0.62	0.49	128	0.24	0.44	17	0.38	0.00
has separate exam room	0.65	0.48	128	0.31	0.48	16	0.34	0.01
has clean floors	0.84	0.37	127	0.88	0.34	16	-0.03	0.74
has clean walls	0.85	0.36	127	0.81	0.40	16	0.04	0.69
has sink for handwashing	0.79	0.41	128	0.38	0.50	16	0.41	0.00
has exam table	0.88	0.33	128	0.69	0.48	16	0.19	0.05
has ad for illegal sonogram	0.20	0.40	127	0.47	0.51	17	-0.27	0.01

Notes: