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 drastically reduce the risk of newborn death, by over 25 per thousand births, with two complementary empirical strategies: (i) a strategy that relates village-level neonatal mortality to the share of neighboring births that occur in public facilities, and (ii) a spatial regression discontinuity that compares births on either side of borders separating districts with different public-birth shares. 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 difficult. It can incur large financial costs. It is made without full understanding of the effects of care on the 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[3]. 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[4].

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 family-level selection. The key observation of the model is that, in the absence of a causal effect, the village-level mortality rate does not depend on the fraction of its births that take place in each facility type; however, with a causal effect present, a village would have a different mortality rate if all its births took place in public facilities versus if they all took place in private. This identification strategy is similar to those used by Geruso and Layton (2020), Jonathan 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. Estimating this model with variation across villages identifies the causal effect if family-level selection is the only concern, but not if village-level confounding is also present.

If village-level confounding is a problem, one might expect that villages with a higher fraction

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

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

[3]Verma and Cleland (2022) first documented this puzzle, and Coffey et al. (2025) show its robustness to demographic standardization and decomposition.

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

of births in private facilities would have characteristics—apart from 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 further 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 to address the problem of village-level confounding. This design compares births on either side of the borders between districts that have different fractions born in each facility type. These otherwise-similar children live (or died) in villages that, on average, differ only in the costs (broadly defined to include, for example, the difficulties of travel) 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 who 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 have more stable respiration and cardiac activity, and 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 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 skin-to-skin care increases 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. Another possibility is that private providers are simply not competent to identify and provide life-saving care.

The size and scale of this effect is very large. Using the smallest of these estimates, each year these public health facilities save the lives of over 100,000 of the births delivered there, on net[5]. 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[6] and the rate of all of India by over 1.5 per thousand[7]. This alone would achieve nearly 20% percent of the progress India needed in 2020 to achieve the UN Sustainable Development Goal 3.2[8].

This paper contributes to several strands of the economics literature. First, it 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; Jon 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 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

[5] 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

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

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

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

reduces infant mortality rates (Jonathan Gruber, Hendren, and Townsend 2014; Cesur et al. 2017; Edward N Okeke 2023). Others find no reduction in mortality (Godlonton and Edward N. 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 studies are based on demand-side subsidies for supply-constrained care in the short run. With an alternative source of variation in a steady-state mode, I show that public facility use improves neonatal mortality, even relative to private facilities.

2 Data and puzzle: Richer mothers pay more for riskier natal care

This paper 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 and health behavior and outcomes of women and children members. The surveyors also measure the location of each village they interview. This study only uses observations from rural parts of Uttar Pradesh and Bihar.

The primary outcome for this study is neonatal mortality, which I construct from mortality data collected for all births. I define neonatal deaths as those reported during the first month of life as neonatal deaths, excluding those births that were born less than a month more than 59 months before the survey.

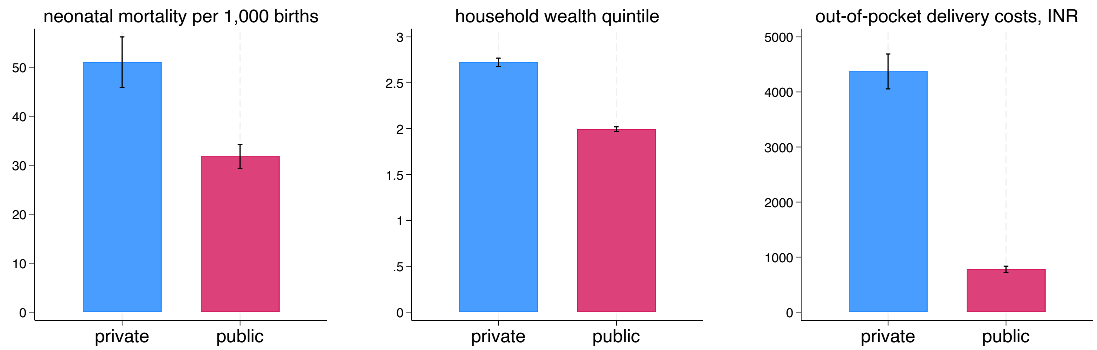
This paper uses a border regression discontinuity design, with the distance to the district border as a running variable. I use village geographical coordinates and district administrative boundaries published by the DHS to construct the straight-line distance to the nearest point on the border.

The measure I use to construct both the identifying variation and the explanatory variable is the type of birth facility: public health facility, private health facility, or home. This is collected for each birth to a mother respondent in the five years prior to the interview.

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 public facilities in this

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

(a) Private newborns die more, (b) come from richer households, (c) and have higher-cost natal care



Notes: The figure displays bar charts of summary statistics outlining a mortality puzzle: Private births have higher neonatal mortality, but wealthier families and higher-cost natal care. 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.

context come from poorer households and the costs for their natal care are five times as high.

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

3.1 The model

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

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, relative to them taking place in the public facility, the death rate at the public facility would fall. The death rate at the private facility would also change[9], 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 A 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[10]; The marginal birth shifting into private has higher risk than the births remaining in public. In Panel b, there is again selection, but there is also a facility causal effect, as shown by the vertical distance between the mortality rate when all births take place in public facilities and the mortality rate when all are private. This is also equal to the slope of the all-births mortality curve.

3.2 Estimation with variation across villages

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

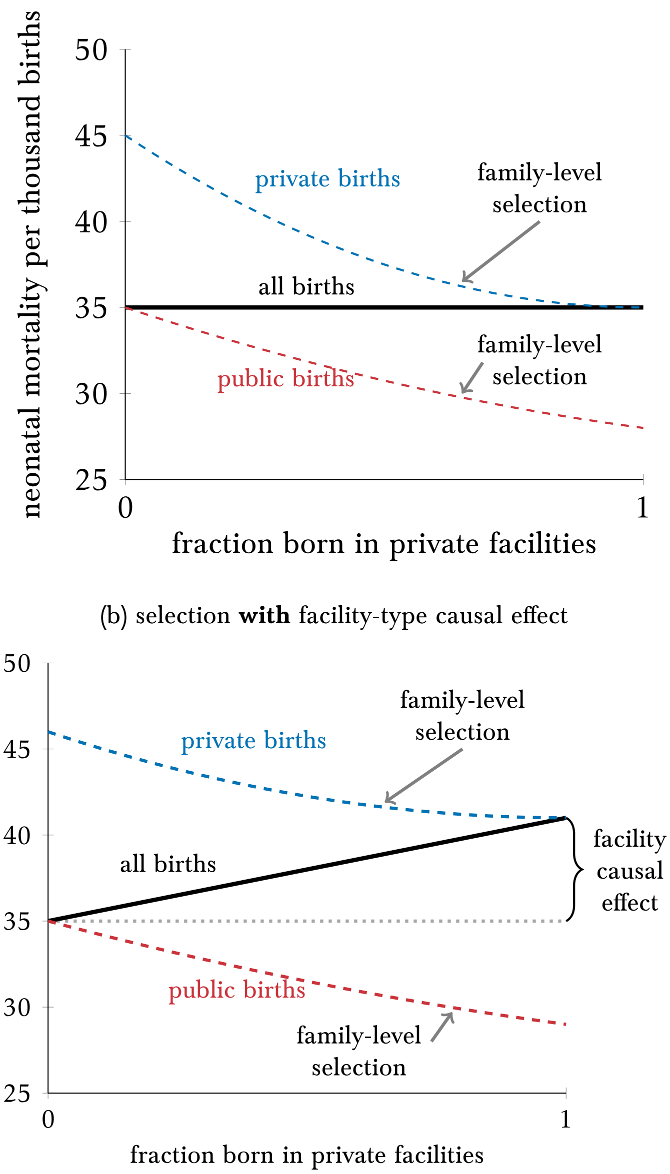
[9]The direction could be up or down, depending on whether the new tranche of births were higher or lower risk than the existing average.

[10]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.

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

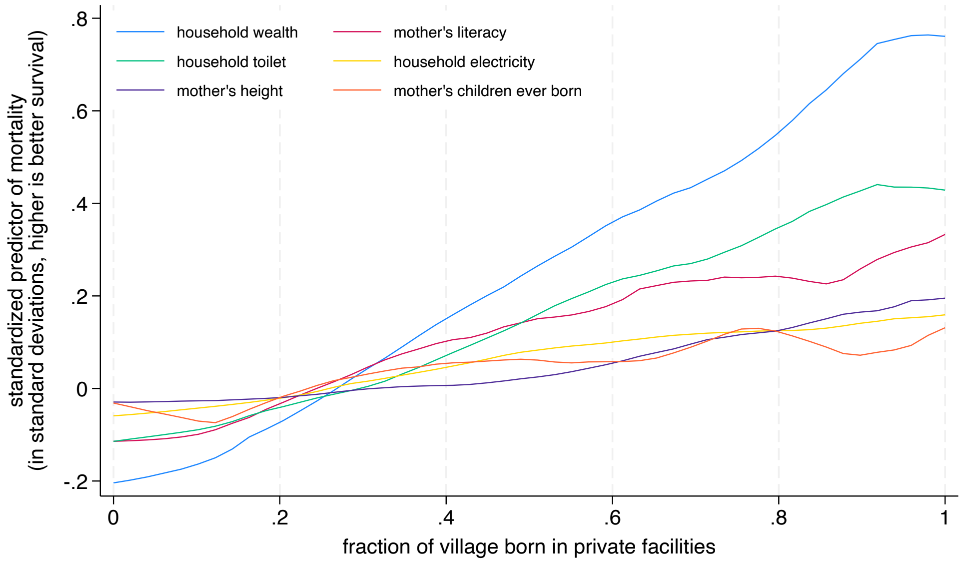


Notes: The figure displays graphical versions of the econometric model this paper employs to identify the causal effect of public versus private facility natal care. Each panel presents neonatal mortality as a function of the fraction of a village's births that take place in private facilities.

Panel a shows a scenario in which there is selection into facilities, since the blue "private births" line and the red "pubic 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: Demographic predictors of survival vs. fraction born in private; 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. It identifies a demographic advantage associated with villages with more births 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. Coefficients and standard errors are calculated according to the survey design: survey-weighted and clustered at the village level.

avenue of confounding, then I can use variation across villages to estimate the model. Empirically, however, village-level confounding appears to work in the *opposite* direction of the effect I identify.

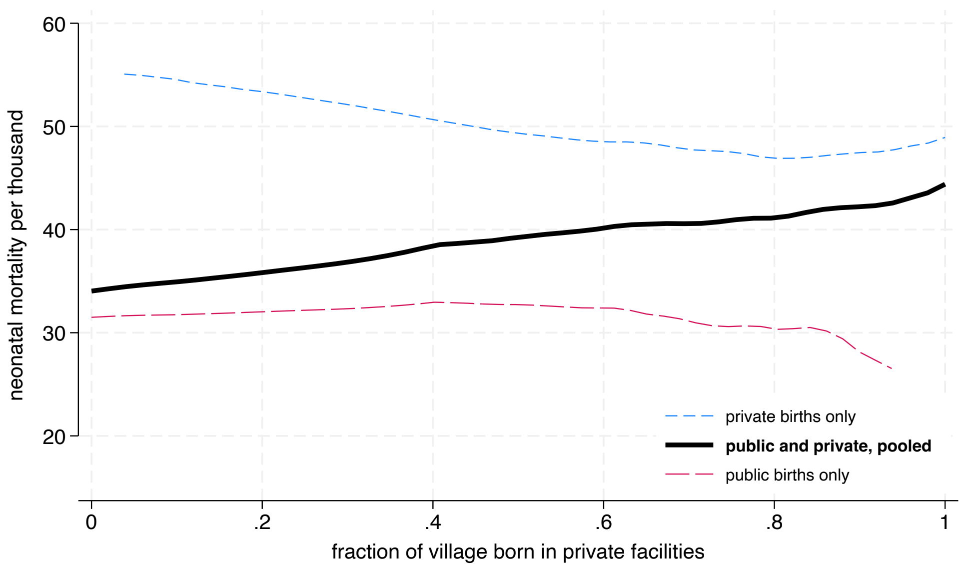
Figure 3 is in the spirit of verification that an instrument, randomization, or empirical strategy is balanced on observables. On the horizontal axis is

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

which is calculated for each village. Each variable is standardized with its mean and standard deviation in this sample for legibility, where higher predicts better survival. Six covariates are included: The asset wealth, sanitation use, and electrification of the baby's household and the literacy, height, and number of children born by the time of the survey of the baby's mother. For each of these markers of socioeconomic status, babies with more neighbors born in public facilities are more disadvantaged, on average.

Figure 4 shows a local polynomial regression that mimics the explanatory graphs from the previous section. On the horizontal axis is again $\overline{private}_v$, and on the vertical axis is the neonatal

Figure 4: Neonatal mortality is more likely for births in villages with a larger fraction born in private facilities



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. It identifies the causal parameter from the econometric model developed in Section 3.1: the slope of the black 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. Coefficients and standard errors are calculated according to the survey design: survey-weighted and clustered at the village level.

mortality rate per thousand births. The figure's red and blue dashed lines shows 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

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

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

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

	Neonatal mortality per 1,000 births					
	(1)	(2)	(3)	(4)	(5)	(6)
fraction born in private	17.612**	25.561***	31.733***			
	(5.689)	(6.760)	(6.905)			
fraction born in private, excluding self				11.378*	17.454**	21.118**
				(5.508)	(6.526)	(6.582)
District-by-month FEs		Yes	Yes		Yes	Yes
Additional controls			Yes			Yes
Observations	34444	34444	34444	34410	34410	34410

Notes: The table displays OLS regression results using data from the NFHS-5. The parameter of intereste is the causal parameter from the econometric model developed in Section 3.1: the slope of the mortality regressed on the fraction born in private. 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. $^*p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

survey sampling unit.

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

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

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

In Table 1, I report coefficients from regressions with either $\overline{private}_v$ or $\overline{private}_{v-i}$ as the regressor of interest and varying controls. The results are strong and consistent: Private facilities significantly increase mortality relative to public facilities. The inclusion of additional controls only makes these results stronger, as one would predict given the earlier balance test results.

4 Identification strategy: district borders regression discontinuity

4.1 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

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

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.

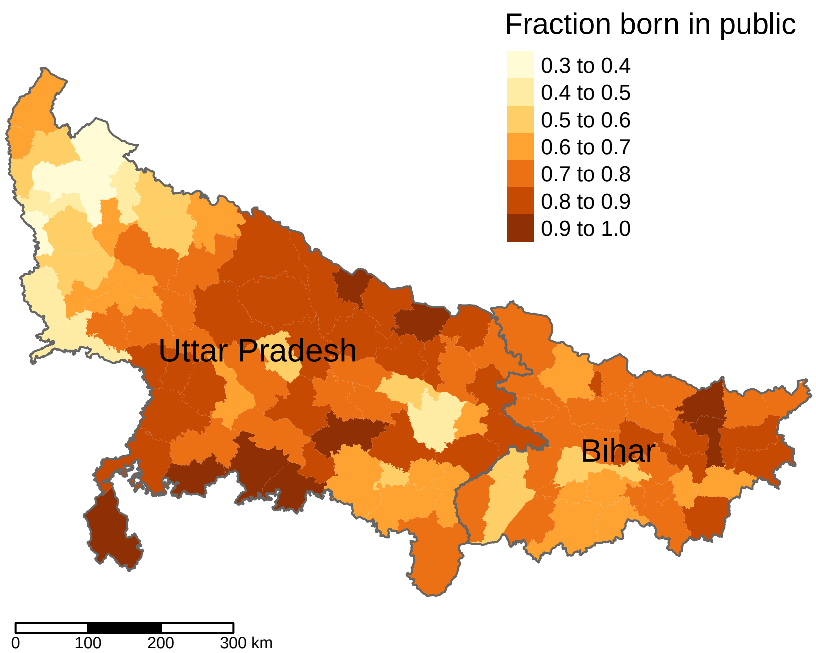
Figure 5 shows that the public fraction of facility births can vary substantially from district to district, even between neighboring district pairs. Panel a shows this spatially, restricted to only those districts in the states of interest. Panel b shows the variation across borders as a histogram.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

(a) Study area and variation in district-level fraction born in public



(b) Distribution of identifying variation in fraction born in public

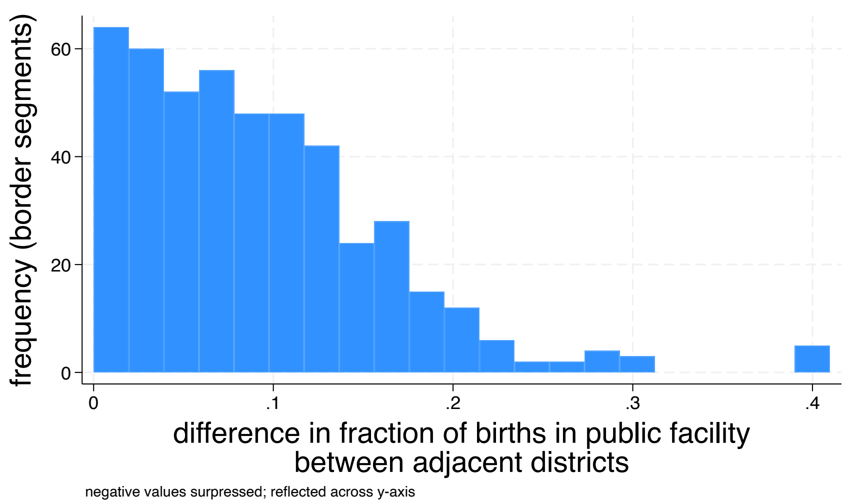


Table 2: RD sample: Summary statistics and balance test

	Full sample	Less public	More public	Difference	p-value	RD estimate	p-value
Public fac. birth	0.720	0.682	0.764	0.082	0.00	0.090	0.00
Private fac. birth	0.280	0.318	0.236	-0.082	0.00	-0.090	0.00
Neonatal mortality	38.148	40.896	34.978	-5.918	0.04	-11.922	0.03
Wealth index	0.1	0.2	0.0	-0.1	0.00	-0.0	0.44
Mother's literacy	0.574	0.583	0.563	-0.020	0.11	-0.020	0.32
Mother's height (cm)	150.126	150.175	150.071	-0.103	0.41	-0.424	0.07
Scheduled Caste	0.270	0.269	0.272	0.004	0.76	-0.028	0.22
Scheduled Tribe	0.020	0.023	0.018	-0.005	0.17	-0.001	0.88
OBC	0.557	0.550	0.566	0.016	0.25	0.070	0.01
Muslim	0.1	0.1	0.1	-0.0	0.35	-0.0	0.32

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[11], 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.

4.2 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

[11] Dupas and Jain (2024) note in 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.

comparison. Furthermore, villages appear multiple times in each regression—once for each of its neighboring districts[12].

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 district 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 also present results that use a difference-in-discontinuities design to compare the borders with below-median differences to the borders with above-median 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 most results, I use the bandwidth that I estimate to minimize the mean squared error of a regression discontinuity with neonatal mortality as outcome. Skin-to-skin contact was only measured in the NFHS-5, so has a restricted sample compared to other outcomes I study. For this reason, I use a different bandwidth for that outcome, one that minimizes the mean squared error of a regression discontinuity with skin-to-skin contact as outcome.

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

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

regressions may include controls captured by $f(X)$: a function of a vector of controls X including caste status, being Muslim, a wealth index, and a district-pair fixed effect. In all regressions, I use survey weights[13] 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, which have only one significant discontinuity: There are more Other Backward Caste[14] 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.

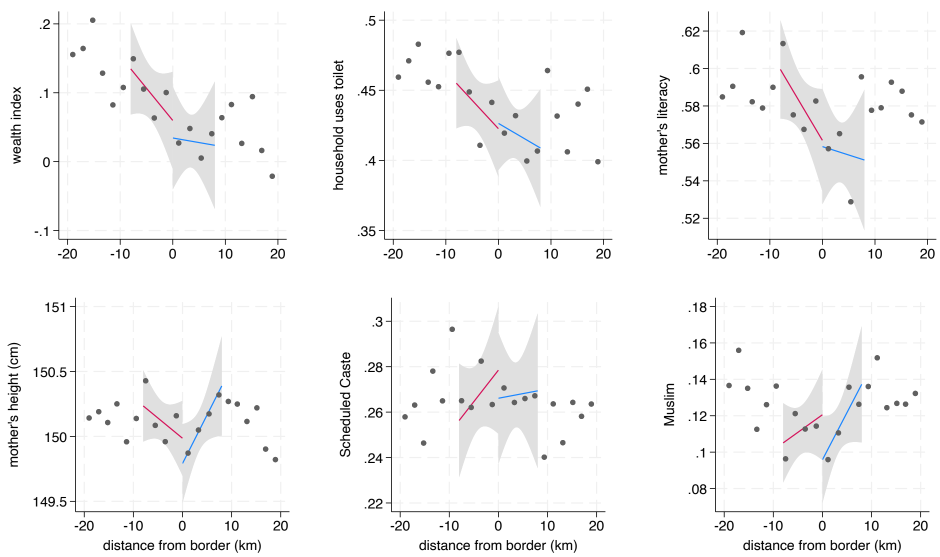
In a fuzzy regression discontinuity research design, four assumptions are sufficient for the estimate to have a causal interpretation (Hernan and Robins 2023). First, instrument relevance, which requires that the instrument be associated with the explanatory variable (and is directly tested in the first 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 dimensions (Coffey et al. 2025). In each case, a violation of this type would favor private health care in this design. This is the opposite of the results I see.

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

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

Figure 6: First stage—RD, demographic predictors of mortality vs. distance from district border; 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. The plotted points are a weighted binscatter of deciles on each side of the border, where on the left side of the graph are observations from the district with a lower fraction born in public and the right side are observations from the district with a higher public fraction. 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 difference between adjacent district-level fraction born in public of greater than the median, 7.8 p.p. Coefficients and standard errors are calculated according to the survey design: survey-weighted and clustered at the village level.

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

As mentioned earlier, I also employ two further regression discontinuity designs that account for heterogeneity by the difference in the neighbor districts' fraction born in public facilities: difference-in-discontinuity regressions as described by Grembi, Nannicini, and Troiano (2016) and conditional average treatment effect regressions as described by Calonico et al. (2025)[15]. 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.

4.3 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 5.5 percentage points (SE: 1.1).

Table 4 and Figure 8 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

[15] These have additional identification assumptions that I explore in Appendix ??.

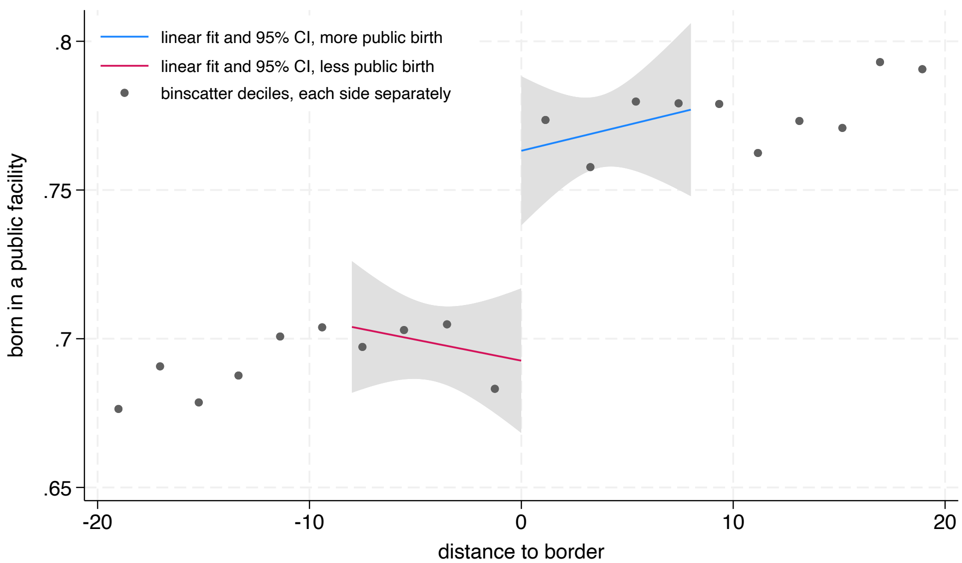
side of a district border with more public birth significantly reduces neonatal mortality by over 10 per thousand births. The reduction in mortality, if scaled by the proportion of births that "comply" in the first stage to give birth in public, is around 130 per thousand births. Figure 8 shows the result 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 4.2. Panel b uses the border variation as an instrument for public birth. According to these estimates, public birth reduces neonatal mortality by 133–188 per thousand births.

Columns 4–6 present additional results showing difference-in-discontinuity estimates that verify the prior estimates. In Panel a, the coefficient on "greater fraction born in public" 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 less-than-median difference (7.8 p.p.). None of these are significant. The coefficient on "greater fraction born in public, high vs. low difference" measures the change in that effect for those district borders separating two districts with a greater-than-median difference in public facility birth. These estimates are all significant at the 5% level. 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 noisier, only significant at the 10% level. According to these estimates, public birth reduces neonatal mortality by 218–224 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 "greater fraction born in public" measures the effect of crossing a border separating two districts with no difference in public birth. The coefficient on "difference in fraction born in public, for greater side" 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 7.5–8.3 per thousand births. They are significant only at the 10% level. Panel b uses this border variation once again as an instrument for public birth. These are also noisier, only significant at the 10% level. According to these estimates, public birth reduces neonatal mortality by 107–124 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

Figure 7: First stage—public birth jumps up (8.1 p.p., SE: 2.1) crossing the border from district 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. The plotted points are a weighted binscatter of deciles on each side of the border, where on the left side of the graph are observations from the district with a lower fraction born in public and the right side are observations from the district with a higher public fraction. 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 difference between adjacent district-level fraction born in public of greater than the median, 7.8 p.p. Coefficients and standard errors are calculated according to the survey design: survey-weighted and clustered at the village level.

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.

5 Mechanism: skin-to-skin care resolves the puzzle

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 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).

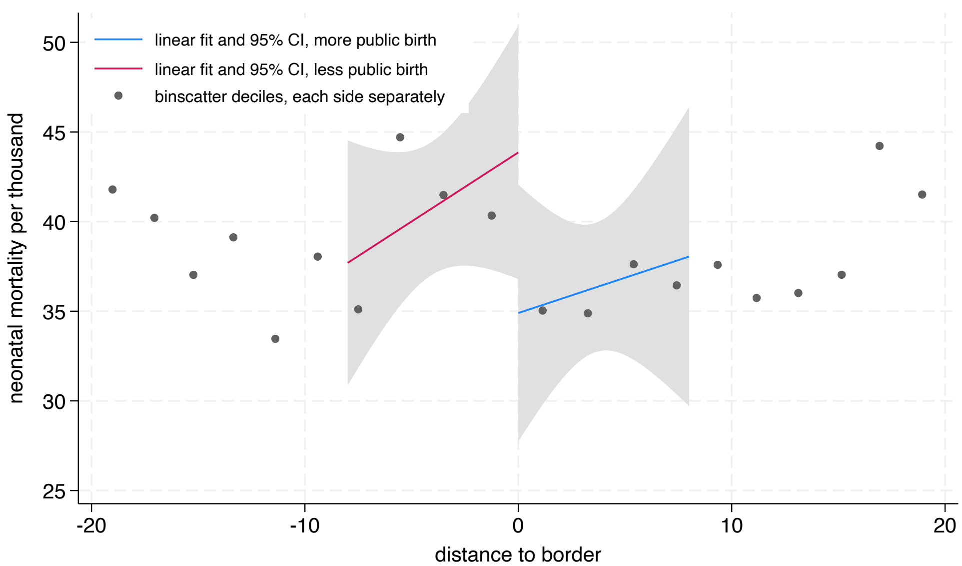
In 2022, the World Health Organization (WHO) released new guidelines that updated the

Table 3: First stage—public birth jumps up crossing the border from a district with lower public facility use to a district with higher

	Regression discontinuity			Difference-in-discontinuities			Cond. ave. treatment effect		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
greater fraction born in public	0.081***	0.091***	0.086***	0.040*	0.015	0.012	0.046†	0.006	0.001
	(0.021)	(0.018)	(0.018)	(0.020)	(0.017)	(0.017)	(0.024)	(0.021)	(0.021)
greater fraction born in public, high vs. low difference				0.040	0.075**	0.072**			
				(0.029)	(0.025)	(0.025)			
difference in fraction born in public, for greater side							0.130	0.531**	0.534**
							(0.210)	(0.189)	(0.188)
District pair FEs		Yes	Yes		Yes	Yes		Yes	Yes
Additional controls			Yes			Yes			Yes
Observations	29044	29044	29044	59585	59585	59585	59585	59585	59585

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. Regressions including "Additional controls" include as covariates asset wealth score, caste categories, and whether the household is Muslim. Columns 1–3 include only observations with a difference between adjacent district-level fraction born in public of greater than the median, 7.8 p.p. 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**—neonatal mortality drops (-15.1 per thousand, SE: 5.8) 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. The plotted points are a weighted binscatter of deciles on each side of the border, where on the left side of the graph are observations from the district with a lower fraction born in public and the right side are observations from the district with a higher public fraction. 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 difference between adjacent district-level fraction born in public of greater than the median, 7.8 p.p. Coefficients and standard errors are calculated according to the survey design: survey-weighted and clustered at the village level.

Table 4: **Main result**—birth in a public facility reduces neonatal mortality relative to private

	Regression discontinuity			Difference-in-discontinuities			Cond. ave. treatment effect		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel a: Reduced-form regressions									
greater fraction born in public	-15.142**	-12.145*	-11.972*	3.685	3.654	3.037	4.665	5.043	4.195
	(5.770)	(5.626)	(5.606)	(5.554)	(5.570)	(5.571)	(6.202)	(6.349)	(6.333)
greater fraction born in public, high vs. low difference				-18.827*	-16.408*	-15.587*			
				(7.850)	(7.843)	(7.826)			
difference in fraction born in public, for greater side							-107.060†	-102.569†	-95.965†
							(54.861)	(55.003)	(54.749)
Panel b: Two-stage least squares regressions									
public birth	-187.993*	-133.264*	-139.048*	-476.009	-223.665†	-218.991†	-106.690	-115.930†	-123.920†
	(86.549)	(66.237)	(69.965)	(390.870)	(128.415)	(131.152)	(74.520)	(62.983)	(66.822)
District pair FEs		Yes	Yes		Yes	Yes		Yes	Yes
Additional controls			Yes			Yes			Yes
Observations	28943	28943	28943	59362	59362	59362	59362	59362	59362

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. Regressions including "Additional controls" include as covariates asset wealth score, caste categories, and whether the household is Muslim. 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. $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

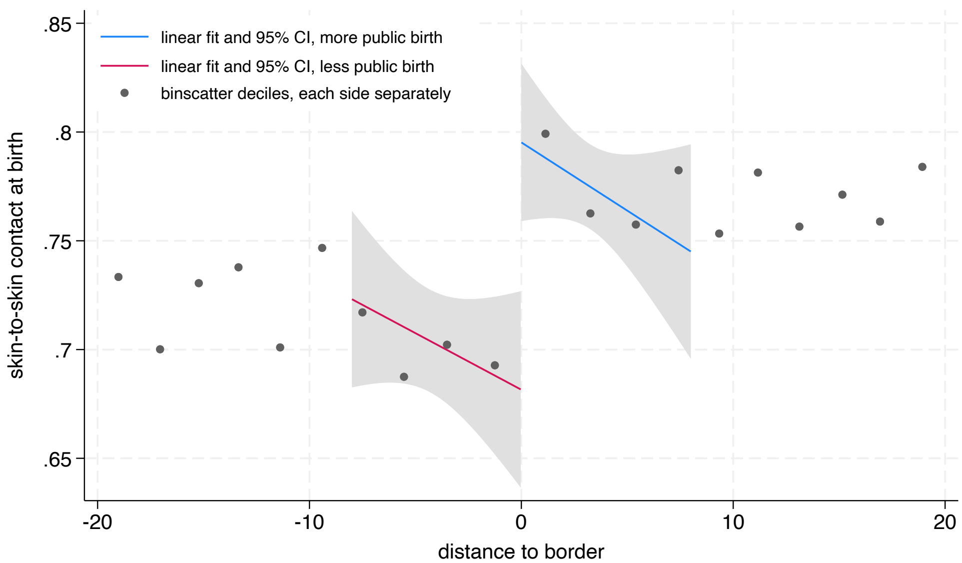
existing medical practice of separating low birth weight and premature infants from their mothers so that they could receive care in specialized newborn units. Instead of moving these high-risk infants to incubators or radiant warmers after birth, the new guidelines recommend putting all but those who 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 4.2, 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.

Table 6 and Figure 9 present the mechanism results of the regression discontinuity, and they show that skin-to-skin contact jumps discontinuously at district borders. Panel a of Table 6 shows that being on the side of a district border with more public birth significantly increases skin-to-skin contact by over 10 percentage points. 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 4.2. Panel b uses the border variation as an instrument for public birth. According to these estimates, public birth increases skin-to-skin contact by close to or over 100%. This is a limitation of the linear probability model I use, but a more sophisticated model will reveal little more than what this already shows: Public birth causes a great deal more skin-to-skin birth.

Columns 4–6 present additional results showing difference-in-discontinuity estimates that verify the prior estimates. In Panel a, the coefficient on "greater fraction born in public" 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 less-than-median difference (7.8 p.p.). The uncontrolled regression in Column 4 is significant at the 10% level. The coefficient on "greater fraction born in public, high vs. low difference" measures the change in that effect for those district borders separating two districts with a greater-than-median difference in public facility birth. These estimates are all positive and significant at the 5% level. 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 noisier, only significant at the 10% level. Once

Figure 9: Mechanism—skin-to-skin contact jumps up (14.7 p.p., SE: 3.4) crossing the border from lower public facility use to higher; UP and Bihar, NFHS-5



Notes: The figure displays the results of a local linear regression using a triangular kernel. It presents reported skin-to-skin contact as a function of distance from the border. The plotted points are a weighted binscatter of deciles on each side of the border, where on the left side of the graph are observations from the district with a lower fraction born in public and the right side are observations from the district with a higher public fraction. 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 difference between adjacent district-level fraction born in public of greater than the median, 7.8 p.p. Coefficients and standard errors are calculated according to the survey design: survey-weighted and clustered at the village level.

again, these estimates are greater than 100%, a limitation of the linear probability model I use.

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 "greater fraction born in public" measures the effect of crossing a border separating two districts with no difference in public birth. The coefficient on "difference in fraction born in public, for greater side" 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 percentage points, causes an increase in skin-to-skin contact by 4.9 percentage points. They are significant only at the 10% level. Panel b uses this border variation once again as an instrument for public birth. These are consistent with prior estimates and significant at the 1% level.

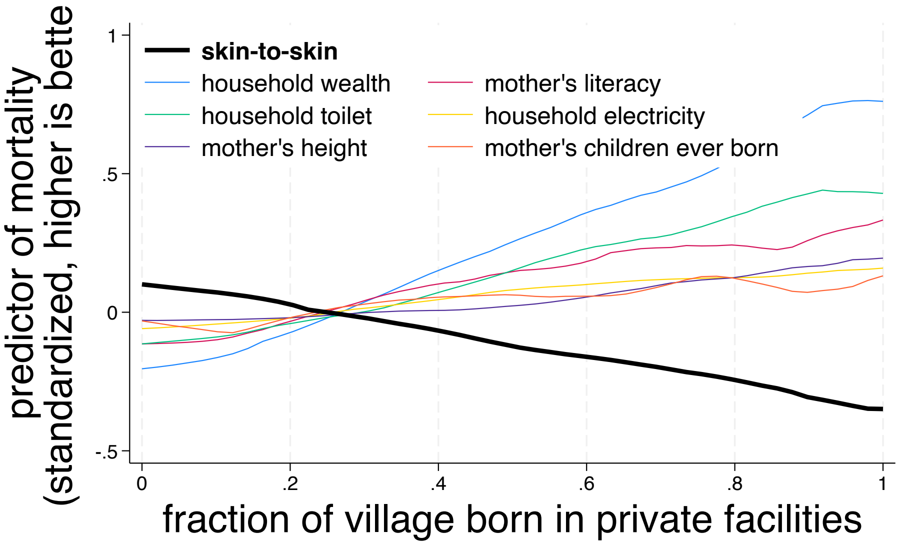
Next, I turn to another geographic empirical strategy that is not plausibly confounded by the

Table 5: Mechanism—birth in a public facility increases skin-to-skin contact relative to private

	Regression discontinuity			Difference-in-discontinuities			Cond. ave. treatment effect		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Panel a: Reduced-form regressions								
greater fraction born in public	0.147***	0.103***	0.102***	$0.054^{\dagger}$	0.005	0.005	0.040	-0.002	-0.002
	(0.034)	(0.030)	(0.030)	(0.031)	(0.031)	(0.031)	(0.038)	(0.036)	(0.036)
greater fraction born in public, high vs. low difference				0.093*	0.102*	0.102*			
				(0.046)	(0.043)	(0.043)			
difference in fraction born in public, for greater side							$0.632^{\dagger}$	$0.614^{\dagger}$	$0.620^{\dagger}$
							(0.340)	(0.322)	(0.320)
	Panel b: Two-stage least squares regressions								
public birth	1.534**	0.945**	0.919**	1.920	$1.111^{\dagger}$	$1.088^{\dagger}$	1.477**	0.927**	0.921**
	(0.560)	(0.311)	(0.302)	(1.779)	(0.591)	(0.574)	(0.499)	(0.317)	(0.313)
District pair FEs		Yes	Yes		Yes	Yes		Yes	Yes
Additional controls			Yes			Yes			Yes
Observations	14117	14117	14117	29578	29578	29578	29578	29578	29578

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. Regressions including "Additional controls" include as covariates asset wealth score, caste categories, and whether the household is Muslim. 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. $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Figure 10: Skin-to-skin care vs. fraction born in private; UP and Bihar, NFHS-5



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:

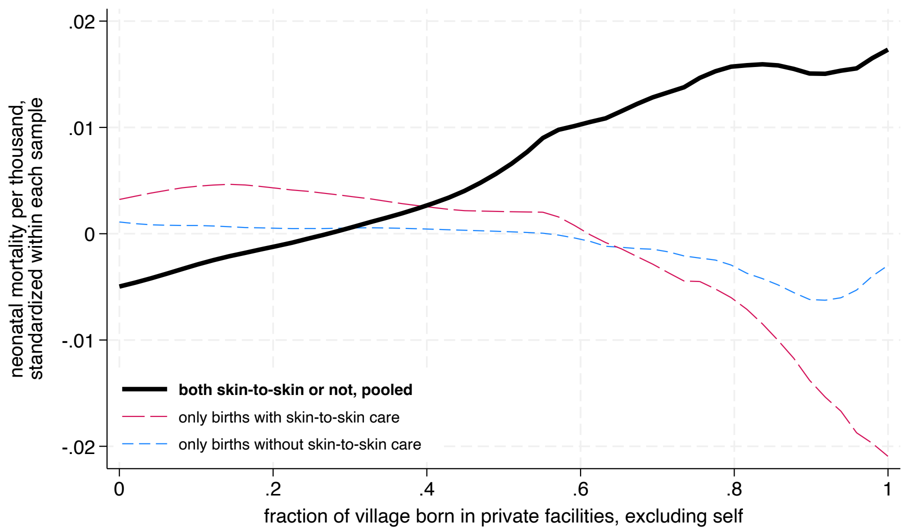
$$\overline{public}_{iv}^{PSU} = \frac{\text{count of births in } \textbf{public} \text{ facilities in the last 5 years}}{\text{count of births in facilities in the last 5 years in the baby's PSU, excluding self}}.$$

$\overline{public}_{v-i}$ 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 replicates Figure 3 with 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 $\overline{public}_{v-i}$ 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.

Figure 11 relates $\overline{public}_{v-i}$ to neonatal survival. The solid black line shows that babies with greater $\overline{public}_{v-i}$ are more likely to survive neonatancy. The other two lines show that this

Figure 11: NNM vs. fraction born in private, by skin-to-skin care; UP and Bihar, NFHS-5



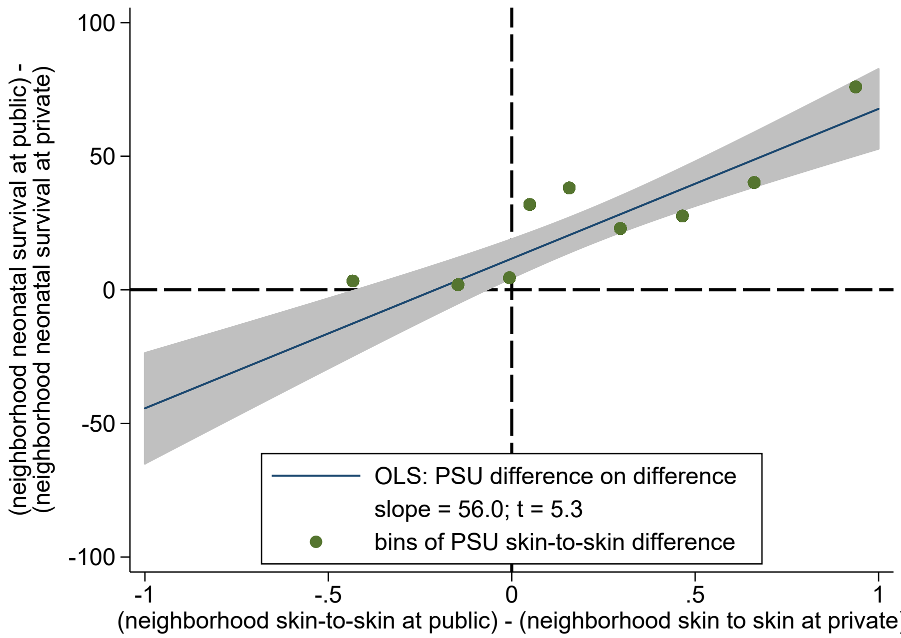
survival advantage for births with more neighbors born in public facilities can be accounted for by skin-to-skin care. They split the same sample according to whether the baby received skin-to-skin care at birth, the two dashed lines. Within both of these subsets, there is no longer a positive association 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.

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 axis measures the extent to which skin-to-skin care is more common, for births of families living in that village, for births in public rather than private facilities. The vertical axis measures the extent to which neonatal survival is greater for births in public rather than private facilities, for births in the same village. The dots are averages of approximately-equal-mass quantiles of villages according to the horizontal axis, the public-private skin-to-skin care difference.

The upward sloping regression line in Figure 12 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.

Table 6 confirms and quantifies the associations in these figures with regression. Table 6 also reports robustness checks (proceeding from Panel A through Panel C) that restrict the sample to

Figure 12: Villages with no skin-to-skin care disparity have no mortality advantage; UP and Bihar, NFHS-5



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.

In the supplementary online appendix, I present a further set of robustness checks. I show in Table **??** that our 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. However, none of these self-reported variables is measured without error. But the consistent patterns of these results is

Table 6: Results of neighborhood-based empirical strategy: A greater fraction of neighboring births in public facilities predicts better chances of neonatal survival, but not after accounting for skin-to-skin contact

	Dependent variable: Neonatal mortality (death in the first month)					
	(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
PSU 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. †$p < 0.10$, *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

evidence, collectively, that properties of the pregnancy are not confounding our main result.

6 Discussion and conclusion

According to the UN World Population Prospects, there were 23.5 million births in India in 2020. According 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 our 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 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

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

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

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 $\bar{\epsilon}(\theta^{pvt}) = \int_0^{\theta^{pvt}} (\mu(t) - \bar{\mu})dt$,[16] 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

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

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

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 3.2, 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.

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