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ARE BANKS BAD FOR BOYS? ESTIMATING THE EFFECT OF BANKS ON CHILD MORTALITY, EDUCATION, AND FERTILITY IN RURAL INDIA

Daniel Rosenblum

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155 College Street
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Are Banks Bad for Boys? Estimating the Effect of Banks on Child Mortality, Education, and Fertility in Rural India

Daniel Rosenblum[a,*,†]

[a] *Department of Economics, Dalhousie University*

Abstract

I investigate whether a large-scale bank expansion program affected parents' decisions to invest in the health and education of their children. From 1977 to 1990, the Indian government implemented a new licensing program to encourage the construction of banks in underserved rural communities. The timing of the bank expansion program is used as an instrument to account for the possible reverse causality of child mortality or education rates affecting bank expansion. An empirical analysis using large-scale Indian surveys finds that states with a more rapid expansion of rural banks did not have significantly lower child mortality overall. However, in households with a first-born daughter, in which discrimination against daughters and in favor of sons is exacerbated, excess female mortality declines with an increase in banks. This occurs through higher male mortality rather than lower female mortality. Similarly, an increase in banks has no effect on daughters' education, but it reduces sons' years of education. Both of these effects occur in a context of more banks causing lower fertility and a reduction in poverty rates, which if anything should lead to reduced child mortality and higher education levels.

JEL Classification: J13; K11; O12

Keywords: gender discrimination; child mortality; education; credit constraints; rural banking; India

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†E-mail: Daniel.Rosenblum@dal.ca. Address: Dalhousie University, Department of Economics, Halifax, NS B3H 3J5, Canada. Phone: (902) 494-8945. Fax: (902) 494-6917.

1 Introduction

Excess female mortality is a significant cause of missing women in India (Anderson and Ray, 2010), representing an extreme form, along with sex-selective abortion, of discrimination against women. The economic causes and solutions for this excess female mortality are still not well understood. In addition, there has long been a gap in India between male and female levels of education, with men having significantly higher literacy rates and years of education. This paper seeks to understand one potential mechanism, expanding access to banks, which could be used by policy makers to reduce such intrahousehold discrimination. Understanding how access to credit may affect discrimination is particularly relevant today with the recent rapid expansion of microcredit which in many cases aims to specifically empower and help women.

Indian policy created a large-scale expansion of banks in rural India between 1977 and 1990. This new access to banks could have benefited children in general by allowing parents to borrow to invest in income generating capital. I present a simple model of the Indian household and incentives for discrimination which shows that under some conditions access to credit may reduce intrahousehold discrimination that favors sons. However, this greater equality happens because the value of sons declines not because the value of daughters increases. In accord with the model, instrumental variables estimates show that although excess female mortality and the male-female education gap fell where there were more banks, this happened because of higher male mortality and lower levels of male education rather than lower female mortality and higher levels of female education.

The Indian banking expansion program is explained in Burgess and Pande (2005), and I summarize their description here: Banks in India were nationalized in 1969 and then liberalized in 1990. During nationalization, the Indian central bank sought to expand bank branches to the rural parts of India which for the most part did not have them. In 1977 the central bank only gave licenses to create a new bank in a location that already had access to banks if four new branches were opened in locations without banks (mostly rural locations). This policy ended

in 1990. Burgess and Pande (2005) show that up until 1977, states with greater initial financial development[1] had a relatively higher expansion of rural bank branches. This trend reverses between 1977 and 1990, when there was a substantial increase in banks in states with the least initial financial development. After 1990, there is little difference in bank expansion between states with different initial financial development.

Kochar (2011) argues that there are problems with the analysis of Burgess and Pande (2005), primarily that they do not take into account the Integrated Rural Development Program (IRDP), which provided subsidized credit to the rural poor (and possibly other investments in health and education that would also reduce poverty). Using a different empirical approach, Kochar (2011) finds that non-poor and high caste households benefited much more from the banking expansion than poor or low caste households. For the purposes of my paper, it does not matter whether the expansion of credit is coming from the the IRDP or the RBI's expansion rules. As long as an increase in banks per capita is a reasonable measure of an increase in access to credit (which was enhanced by the IRDP) and the timing of the bank policy changes are a reasonable instrument for the number of banks, then the estimates will be a valid measure of the effect of increased access to credit on child outcomes.

The timing of the bank expansion policy change is used as an instrument by Burgess and Pande (2005) and Burgess, Wong, and Pande (2005) to show that the banking expansion reduced poverty rates. Menon and Rodgers (2011) uses the same instrument to show that the banking expansion increased female self-employment. Mukherjee (2011) uses the the timing of the expansion for a differences-in-differences estimate to show that rural households in states that benefited the most from the banking expansion were more likely to adopt high yielding variety seeds. I use the instrumental variables approach to investigate the effects of banking expansion on child outcomes and gender discrimination.

As can be seen Figure 1, child mortality is high in India, but has been steadily falling. Although the child mortality rate is much lower in urban areas, the gap between rural and urban children has been shrinking over time. In addition, while female child mortality is substantially

[1] Burgess and Pande (2005) define initial financial development as the number of bank branches per 100,000 people in 1961.

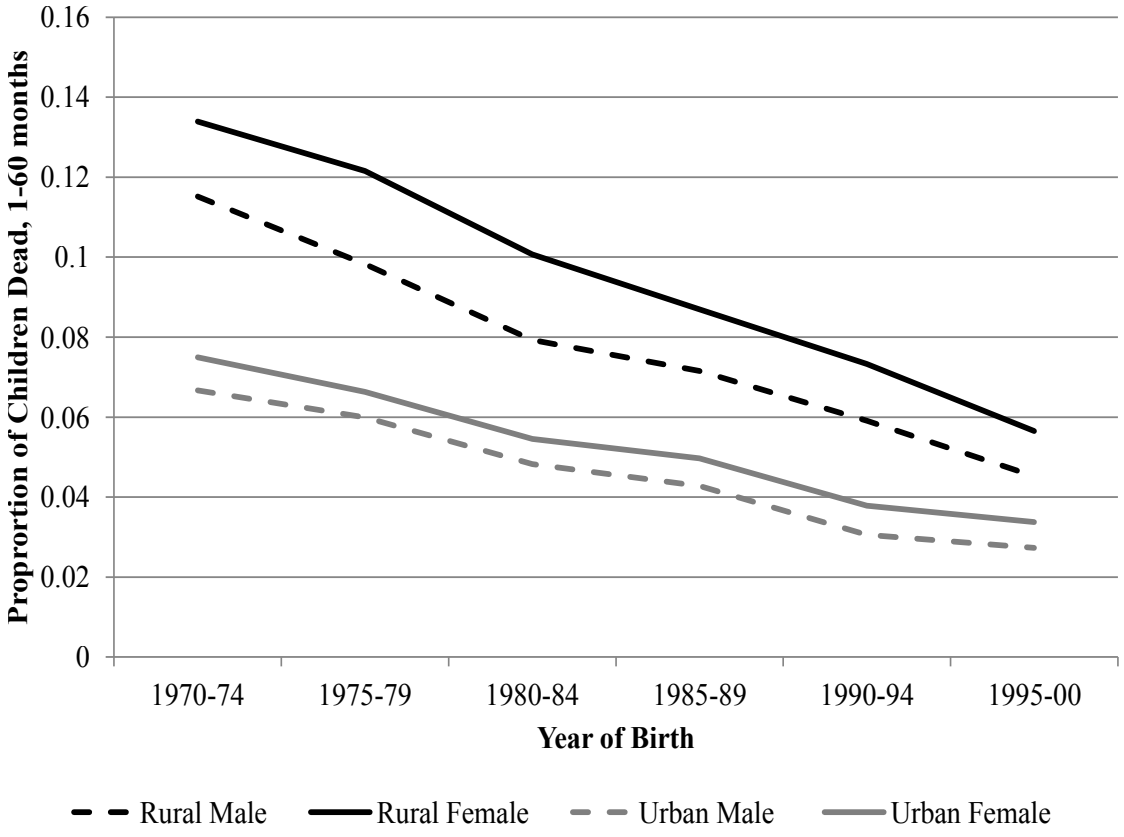

Figure 1: Trends in child mortality by male/female and urban/rural. Data source: Indian NFHS 1992-93, 1998-99 and 2005-06.

higher than male child mortality, the gap decreased substantially in rural India from about two percentage points in the 1970s to a little over one percentage point by the late 1990s. By contrast, the male-female mortality gap in urban areas has been steady at about 0.7 percentage points. The reason for this decline in excess female mortality in rural areas is still unexplained. The banking expansion program in rural India in the 1977 to 1990 time period could be one of the possible causes.

Similarly, the male-female gap in education is substantially higher in rural than urban India. For children aged 15 or older in the 1992-93 round of the Indian National Family Health Survey (NFHS), there is a 1.7 year gap in average years of schooling between male and female children in rural India versus a 0.5 year gap in urban India. By the 2005-06 round of the NFHS, the gap in rural India fell to about 0.8 years of schooling in rural India versus almost no gap in urban India.

Previous research has shown that the larger future economic returns of investing in sons over daughters can help to explain excess female mortality in India (Rosenzweig and Schultz, 1982; Carranza, 2014) and China (Qian, 2008). Giving parents greater access to credit may change the value of the expected economic returns of children, weakening the relative importance of sons over daughters. If women benefit more than men from the banking expansion,[2] this will reduce incentives for discrimination. If the banking expansion in India directly reduces poverty, as shown in Burgess and Pande (2005), families have more resources to invest in the health and education of their children, reducing mortality risks and raising education levels for all children. However, such reductions in poverty may especially help daughters. For example, Asfaw, Lamanna, and Klasen (2010) find that in India parents with tight budget constraints are more likely to give expensive medical care to sons than to daughters. A relaxation of these constraints could increase the likelihood of daughters receiving medical care with little effect on sons who are already receiving it. Surprisingly, I find the opposite: increased access to banks has no effect on girls and negative effects on boys.

[2]Menon and Rodgers (2011) shows, for example, that banking expansion in India increased the rate of self-employment for women, but not men.

2 Model of Credit Constraints and Son Mortality

Rosenblum (2013) models the relationship between the different economic returns of sons and daughters, parents' fertility decisions, and investment in children. One critical assumption of the model is that sons are the only asset that parents can invest in. If access to credit allows parents to invest in other productive assets that raise household income, then child mortality may fall or education levels may rise for all children (an income effect of access to credit). If parents invest in sons to increase old age income security, credit may provide the resources for more capital investments that will substitute for future son income, reducing the need for investment in sons (a substitution effect of access to credit). If access to credit and alternative investments lowers parents' desired number of sons, then there may also be a weakening of son-preferring fertility stopping rules. This lower fertility will make more resources available per child, strengthening the income effect. The net effect may be to reduce child mortality in general, but it could also be to increase male mortality if the substitution effect outweighs the income effect. The following model extends the one in Rosenblum (2013) by adding credit and a productive asset to the model. The model is explained with health investments in children, though the results would not change if it was reframed with education investments.

For simplicity, parents are assumed to be a unitary household. Assume that parents have completed their fertility and are only deciding how much to invest in their children's health and how much to borrow to invest in a capital asset (with the remaining income used for consumption). There are two time periods, when children are young (when health investments are most important) and when children grow up (when sons provide income for their parents and daughters cost parents in dowry and wedding expenses). Parents choose K_i in the first period, where K_B is health investment per son and K_G is per daughter. A is the amount borrowed to invest in non-child assets when children are young. R is the rate of interest for A plus 1 and $I(A)$ is a strictly concave function indicating the expected returns to investing in A. I assume for simplicity that the returns to investing in A is independent from investing in sons. B is the number of sons and G is the number of daughters in the household. D is the expected income

transfer from a surviving son to parents when sons are older and also the expected cost of a daughter when she is older. $p(K_i)$ is a concave survival function indicating the proportion of sons or daughters that are expected to survive to the second period conditional on childhood investment. Parents' exogenously determined income is assumed to be Y_1 in the first period and Y_2 in the second period. Parents derive utility from consuming in period 1 (U_1), consuming in period 2 (U_2), and the number of children that survive from period 1 to period 2 (U_S). Utility is assumed to be separable into these three components and each component is strictly concave. If there is no ability of the household to borrow, then the parents' utility maximization problem is as follows:

$$\max_{K_B,K_G} U_T(K) = U_1(C_1) + U_2(C_2) + U_S(Bp(K_B) + Gp(K_G)) \tag{1}$$

subject to the following constraints:

Period 1: $BK_B + GK_G + C_1 \leq Y_1$

Period 2: $GDp(K_G) + C_2 \leq Y_2 + BDp(K_B)$

If parents can borrow to invest in a capital asset, then the parents' utility maximization problem becomes one of choosing K_i and A subject to the new constraint in Period 2:

Period 2': $GDp(K_G) + C_2 \leq Y_2 + BDp(K_B) + I(A) - RA$

Note that allowing borrowing to invest in capital in this way is effectively the same as an exogenous increase in Y_2. If parents allocate an optimal quantity of K_B and K_G without borrowing, then the first order conditions are $U_1' = (DU_2' + U_S')(p'(K_B))$ and $U_1' + DU_2'p'(K_G) = U_S'p'(K_G)$. If parents are now allowed to borrow, they will borrow such that $I(A) - RA$ is maximized. Assuming that returns are larger than R for some amount A, freeing up credit constraints increases future expected income. That is, U_2' would decrease as credit constraints are relaxed. The only way to re-adjust K_B to attain an optimum investment in sons is is to lower K_B. Thus, relaxing credit constraints will lead to a reduction in health investments in sons as long as the returns from the capital assets are sufficiently high. Intuitively, if returns from investing in sons is lower than returns to investing in capital, then relaxing credit constraints will

cause parents to substitute capital investment for son investment. For daughters, the result is actually the opposite. The only way to adjust K_G to attain an optimum investment in daughters after credit constraints are relaxed is to increase K_G. Intuitively, if parents are richer in the future, and daughters create future expected net costs, then the higher future income from non-son investments will lessen the future utility cost of investing in daughters' health. Thus, relaxing credit constraints may reduce discrimination that favors sons and hurts daughters. The model can be extended to have a fertility component, where parents continue to have children until the expected utility gains from an additional child are negative. Access to credit would reduce the expected future income gains from an additional son (since they are less likely to live) and increase the expected future income costs of an additional daughter (since they are more likely to live) and, hence, reduce fertility.

The model in Rosenblum (2013) shows (and tests empirically) that the higher the proportion of children that are sons in a household, the less the discrimination. Given that the sex of a first-born child in India is random[3], siblings of a first-born son have higher male mortality and lower female mortality (i.e. less discrimination) compared to households with a first-born daughter. Thus, it may be in the sample of siblings of a first-born daughter where we find particularly strong reductions in discrimination.

This model is analogous to one where human capital investments in children are made instead of health investments. If a son's education is a substitute for physical capital, greater access to physical capital via increased credit may reduce a son's level of education. Indeed, Wydick (1999) finds that parents substitute physical for child human capital in the context of microlending in Guatemala.

One could imagine different models with different implications. For example, if parents could also use credit to consume and invest in children in the first period, then it would be possible for parents to have higher consumption levels in both periods and invest more in all children (the income effect may always outweigh the substitution effect in this case), reducing child mortality or raising education rates in general. Furthermore, in a general equilibrium

[3]There is no evidence of sex selective abortion at the first parity in India, while there is for higher order births.

framework, if increased access to credit raises the demand for and, thus, price of capital, access to credit may have negative effects on those who already had access to credit. In the context of rural India, if widespread access to capital increases the supply of agricultural goods, this could reduce the income of those without access to credit and those who would have had access to credit in any case. These effects may dampen or intensify the partial equilibrium effects of increased credit on discrimination in the above model. However, modeling these effects is outside the scope of this paper.

3 Data Description

This article uses the detailed fertility and child mortality data in the three rounds of the Indian NFHS (1992-93, 1998-99, and 2005-06). When combined, the three datasets contain information on 304,465 women and their 416,103 sons and 384,730 daughters. I restrict the sample to children in rural households born between 1970 and 2000 due to the low number of observations before 1970 and the lack of banking data after 2000. In addition, the sample is restricted to the 16 states for which banking data is available (these are generally larger states which together represent about 80 percent of the total sample). For child mortality (died between 1 and 60 months of age), observations are dropped if the child died before the first month of life or was under 5 years of age at the time of the survey. This reduces the number of children to 159,683 sons and 149,769 daughters. For years of education, the sample is restricted to children between 6 and 18 years of age who currently reside in the household,[4] giving a sample of 62,853 sons and 56,143 daughters. For the fertility estimates, the sample is restricted to rural households where the mother was aged 18 or over in 1970, providing a sample of 127,508 households.

Indian banking data and state-level variables used for the instrumental variables analysis are those used in Burgess and Pande (2005). I also use data from Desai (1969) for the state-level sex-ratio of the population in 1961.

[4]Education data is only available for current household members.

Table 1: Descriptive Statistics for Rural Households

Independent Variables	
Bank branches per 100,000	5.07
(Cumulative in rural, previously unbanked locations)	(2.89)
Mother's Age	30.31
	(7.86)
Mother's Years of Education	1.25
	(1.93)
Hindu	0.82
	(0.00)
Muslim	0.12
	(0.00)
Christian	0.018
	(0.018)
Sikh	0.03
	(0.00)
Other or No Religion	0.01
	(0.00)
Scheduled Caste	0.17
	(0.00)
Scheduled Tribe	0.11
	(0.00)
Other or No Caste	0.72
	(0.00)
Observations	127,508
Dependent Variables	
Child Mortality 1-60 months, Male	0.076
	(0.001)
Observations	159,683
Child Mortality 1-60 months, Female	0.094
	(0.001)
Observations	149,769
Total Years of Education, Male	3.87
	(3.26)
Observations	62,853
Total Years of Education, Female	3.09
	(3.22)
Observations	56,143
Total Children Born	3.06
	(2.10)
Observations	127,508

Notes: Standard errors in parentheses. The 1992-93 round of the NFHS divides caste into only 3 categories: schedules caste, scheduled tribe, and other. The definition of caste includes other backwards classes in the later surveys, but this is put into the "other" category for consistency across surveys. Similarly, the religion classifications are from the 1992-93 survey, which are Hindu, Muslim, Christian, Sikh, and other, with the additional categories from the later surveys (Buddhist/Neo Buddhist, Jain, Jewish, Zoroastrian/Parsi, Donyi-Polo, Sanamahi, and no religion) classified as other for consistency.

4 Empirical Strategy

Access to banks is not random and banks may selectively expand in locations where households have higher incomes and, thus, lower child mortality or higher levels of education. To avoid this reverse causality problem, I adopt the strategy from Burgess and Pande (2005) which uses the timing of the Indian bank expansion policy as instruments. The expansion started in 1969, with a new regulation in 1977 requiring a significant increase in bank branches in rural, unbanked locations, and ending in 1990 with the liberalization of the Indian banking sector. The instrument takes the form of linear trends in banking with trend breaks at 1977 and 1990, starting at initial conditions before the banking program was implemented. As long as the trend in banking reversed significantly between 1977 and 1990 (which it did) and other development programs are uncorrelated with the bank expansion during the same time period, the instrument should be valid.

The first stage, as in (Burgess and Pande, 2005, p.783), is as follows:

$$\begin{aligned} B_{it}^{R} = \alpha_i + \beta_t + \gamma_1(B_{i1961} * [t - 1961]) + \gamma_2(B_{i1961} * [t - 1977]) + \\ \gamma_3(B_{i1961} * [t - 1990]) + \gamma_4(B_{i1961} * P_{1977}) + \gamma_5(B_{i1961} * P_{1990}) + e_{ij} \end{aligned} \tag{2}$$

where B_{it}^{R} is the cumulative number of bank branches opened in underserved rural locations per 100,000 people in state i and year t. α_i and β_t are state and year fixed effects. B_{i1961} is the number of bank branches in state i in 1961 per 100,000 people (initial financial development), which is interacted with linear time trends with breaks at 1977 and 1990. P_{1977} and P_{1990} equal one for years starting in 1977 and 1990 respectively. Per capita state domestic product in 1961, state population density in 1961, and log rural locations per capita in each state in 1961 are included as additional initial conditions and enter the estimation equation just as B_{i1961}. I include one more initial condition, the male/female state-level population sex ratio in 1961, which captures the initial level of discrimination against women. Robust standard errors in all of the estimates are clustered at the state level.

The second stage is as follows:

$$Y_{ijt} = \delta \widehat{B_{it}^R} + \theta(B_i 1961 * [t - 1961]) + \omega_1 X_j + \omega_2 M_{jt} + \alpha_i + \beta_t + \varepsilon_{ijt} \quad (3)$$

where Y_{ijt} is a 0/1 variable indicating whether child j born in year t died between 1 and 60 months of life conditional on surviving to the age of one month. $\widehat{B_{it}^R}$ is the estimated number of bank branches in rural, underserved locations from the first stage. X_j are household level controls: mother's completed years of schooling and caste and religion dummies. YOB_j is the child's year of birth, and M_{jt} is the mother's age in year t. Per capita state domestic product in 1961, state population density in 1961, log rural locations per capita by state in 1961, and the male/female sex ratio by state in 1961 are included in the second stage exactly as they are in the first stage estimation. State and year of birth fixed effects are included as well. For the education estimates, the outcome is the years of education of a child (aged 6 to 18, conditional on being alive at the time of the survey), and the child's age at the time of the survey is added as a control variable. For the fertility estimates, the outcome is the total number of children born in household j and t is the year the mother turned eighteen. In addition, the estimates include the state-level policy and politics controls from Burgess and Pande (2005): cumulative land reform acts, the share of spending on health and education, the share of spending on other development programs, and the proportion of state legislature seats held by the different political parties.

To further test the the effects of the banking expansion on excess female mortality and the education gap, I also perform separate estimates on the samples of siblings of first-born males and siblings of first-born females. As shown in Rosenblum (2013), a first-born son causes higher child mortality for sons and lower child mortality for daughters, and, thus, are households where discrimination is less intense. An analogous effect for education can be found in the NFHS data used here, at least for males. A first-born son (relative to a first-born daughter) predicts 0.14 fewer years of education for younger male siblings at a 1 percent level of significance.[5] Thus, households with a first-born daughter are those where sons are particularly favored and in which we may find stronger effects of banks.

Similarly, to test the effects on son-preferring fertility stopping rules, I perform separate

[5]This effect on education has also been found by Makino (2012) in India using a different dataset.

estimates on mothers of first-born males and mothers of first-born females. Because of parents' strong son preference in India, a first-born son causes parents to have less children on average than parents with a first-born daughter. If son-preferring fertility stopping rules are weakened, we should find particularly strong effects of banks on fertility for households with a first-born daughter.

The empirical strategy has some limitations. First, I focus on the number of banks in the year of a child's birth or the year a mother turns eighteen, but the banks may have a lagged effect. That is, banks may only be able to help households if they have been around for several years. I have tested different lags and find consistent results (estimates not shown). In addition there could be some omitted variables that are the actual cause of the estimated effects. The estimates control for several state-level policies and the political composition of the state legislatures. However, omitted variables are still a concern. In addition, although the estimates can help us understand whether bank expansion caused better or worse outcomes for children or less discrimination, they cannot tell us the exact mechanism(s), such as through poverty reduction, increased capital investments, or lower fertility.

Another problem with the empirical approach could be the use of sex-selective abortion, particularly when looking at first-born male versus first-born female households. However, this would only affect the more recent births in the sample as sex selection did not become widely available until the mid 1990s (see Bhalotra and Cochrane (2010)). The number of banks does not predict the sex of the first-born child, except for older mothers whose children would have been born before sex selection was possible. This is likely due to recall or maternal survival bias as discussed in Rosenblum (2013) and Milazzo (2014). This bias will cause household with first-born daughters to look better-off (with lower child mortality and fertility) than they really are. That is, it would likely bias the estimates towards not showing discrimination.

5 Estimation Results

5.1 First Stage

The first stage results are presented in Table 2. I only show the first-stage for the male child mortality estimates and the household-level fertility estimates, as the first-stage is quite similar for all of the IV estimates. The results, consistent with Burgess and Pande (2005), show that the timing of the Indian banking reform is a good predictor and instrument for access to banking in rural, unbanked locations. A better initial financial development is positively correlated with bank expansion before 1977, then negatively related to the number of banks between 1977 and 1990, while the bank expansion policy was in effect. This reverses after 1990 when the bank expansion program ended.

Table 2: OLS: First Stage: The Effect of Policy Changes on the Cumulative Number of Bank Branches Per Capita in Rural, Unbanked Locations

	Child-Level	Household-Level
	(1)	(2)
Bank branches per 100,000	0.114***	0.114**
in 1961*(1961-2000) trend	(0.031)	(0.043)
Bank branches per 100,000	-0.258***	-0.258***
in 1961*(1977-2000) trend	(0.035)	(0.036)
Bank branches per 100,000	0.129**	0.136***
in 1961*(1990-2000) trend	(0.046)	(0.043)
Bank branches per 100,000	0.335	0.378
in 1961*Post-1976 dummy	(0.244)	(0.230)
Bank branches per 100,000	0.148	0.099
in 1961*Post-1989 dummy	(0.426)	(0.166)
Observations	159683	127508

Notes: Robust standard errors, clustered at state level, are reported in parentheses. The dependent variable is the cumulative number of bank branch openings in rural, unbanked locations per 100,000 people. The regression includes state and year fixed effects. Additional controls are per capita state domestic product, the male/female sex ratio in each state, log number of rural locations per capita, and the population density in each state, all in 1961. These are included in the regression just as the initial number of bank branches in 1961. Haryana enters the data in 1965. At the child-level, the first stage for males is reported (it is very similar for females). All estimates are for rural households

(* $p<0.1$, ** $p<0.05$, *** $p<0.01$)

5.2 Child Mortality

Table 3 shows the OLS and 2SLS estimates of the effects of banks on male and female child mortality. For female child mortality there is no statistically significant effect in any of the specifications. However, for male child mortality, there is a statistically significant increase in child mortality in the 2SLS estimates in columns (4) and (6). These estimates show that more banks caused higher male mortality, which was concentrated in siblings of first-born daughters. That is, in the households where parents discriminate substantially in favor of sons, more banks cause parents to discriminate less (rather than raising mortality rates for sons in households with first-born males, where sons and daughters are treated more equally). Given that the only effect on child mortality is through a worsening of male mortality rates, the substitution effect of more access to banks dominates any income effect. These results are robust to different measures of child mortality, in particular for child deaths between 0 and 60 months or between 0 and 12 months (estimates not shown). They are also robust for lagged effects of banks.

5.3 Education

For additional confirmation of the negative effects of banks on male children, I test whether banks cause a decline in a child's years of education. Unlike child mortality, education levels are only recorded if a child is currently in the household. In other words, education data is conditional on being alive and still residing with one's parents. Thus, older children who have joined or formed different households are not part of the education data. This is a particular problem for female children, as a very high proportion of daughters leave their parental household after marriage. (52 percent of children under age 18 who are present in the household are male compared to 66 percent who are aged 18 and over.) Thus, to get comparable estimates for male and female children, I restrict the sample for the education estimates to children aged between 6 and 18 years of age. Similar to the mortality estimates, the education estimates test whether the number of banks in a child's year of birth affects later education outcomes.

The results for males and females are presented in Table 4. The OLS estimates in column

Table 3: OLS and 2SLS: The Effect of Bank Expansion on Child Mortality, Rural India

	OLS			2SLS		
	All	FB	FG	All	FB	FG
	(1)	(2)	(3)	(4)	(5)	(6)
	Male Mortality					
$\widehat{B_{it}^R}$	0.0014	0.0027	0.0038*	0.0112*	0.0045	0.0239**
	(0.0023)	(0.0038)	(0.0018)	(0.0056)	(0.0105)	(0.0091)
Observations	159683	57602	58120	159683	57602	58120
	Female Mortality					
$\widehat{B_{it}^R}$	-0.0009	0.0024	-0.0024	0.0036	-0.0083	0.0058
	(0.0027)	(0.0043)	(0.0032)	(0.0057)	(0.0164)	(0.0100)
Observations	149769	54529	53071	149769	54529	53071

Notes: Robust standard errors, clustered at state level, are reported in parentheses. The dependent variable is child death between the ages of 1 and 60 months conditional on surviving to the first month of life. For the OLS estimates, the actual number of banks, not the instrumented number of banks, are used. FB indicates the sample of second order and higher children conditional on a first-born brother. FG indicates the sample of second order and higher children conditional on a first-born sister. The regression includes state and year fixed effects. Additional controls are per capita state domestic product, the male/female sex ratio in each state, log number of rural locations per capita, and the population density in each state, all in 1961. These are included in the regression just as the initial number of bank branches in 1961 in the first stage estimate in Table 2. The estimates also include controls for mother's age in year of birth, mother's years of schooling, religion, caste, and state-level policies and politics.

(* p<0.1, ** p<0.05, *** p<0.01)

(1) in both tables shows that banks are positively correlated with the years of male or female education, although more strongly correlated with female education. The 2SLS estimates find no statistically significant effect of banks on female education. However, there is a large and statistically significant negative effect of banks on male education as indicated in column (4). One additional bank causes a decrease of 0.44 years of education. Again, the substitution effect dominates any income effect of access to banks, making things worse for males. However, unlike the mortality estimates, the effect on education is particularly concentrated in siblings of first-born sons, where there is more equal treatment of sons and daughters. It is not clear why this should be the case. One possibility is that with child mortality, there is more of an ability for parents to reduce health investments for sons with an eldest sister (as these males have the lowest child mortality rates), but perhaps there is no additional incentive to re-adjust education levels given the lower health investments. Another possibility is that parents invest highly in the education of their first son regardless of birth order. Thus, parents are more willing to reduce their education investment in a male sibling of first-born sons rather than a male sibling of a first-born daughter who is likely to be a first male child.

5.4 Fertility

One of the possible mechanisms through which child mortality or education could be affected by banks is through changes in fertility. The OLS and 2SLS effect of rural banking expansion on fertility in rural India is shown in Table 5. Banks reduce fertility in all of the specifications except for column (6). In the 2SLS estimates, overall, an additional bank per 100,000 people reduces the total number of children born by 0.11 children. Comparing first-born son and first-born daughter households in Columns (5) and (6), it appears that banks only have a large negative effect on fertility if the first-born child is a son. That is, for households which already had a son, access to banks reduces parents' desire to have more children or strengthens the stopping rule of ending fertility after one son. One explanation for the lack of an effect on households with a first-born daughter is that having banks does not reduce the demand for a single son very much. Sons (at least in the vast majority of these states during the time period

Table 4: OLS and 2SLS: The Effect of Bank Expansion on Years of Education, Rural India

	OLS			2SLS		
	All	FB	FG	All	FB	FG
	(1)	(2)	(3)	(4)	(5)	(6)
	Male Education					
$\widehat{B_{it}^R}$	0.1365*	0.1310	0.1148	-0.4430***	-0.6443**	-0.0022
	(0.0685)	(0.0773)	(0.0760)	(0.1426)	(0.2589)	(0.1482)
Observations	62853	23464	24084	62853	23464	24084
	Female Education					
$\widehat{B_{it}^R}$	0.3098**	0.2969**	0.2689*	0.1004	0.1564	0.0072
	(0.1294)	(0.1121)	(0.1455)	(0.2106)	(0.2125)	(0.2030)
Observations	56143	21150	21023	53421	21150	21023

Notes: Robust standard errors, clustered at state level, are reported in parentheses. The dependent variable is total years of education. For the OLS estimates, the actual number of banks, not the instrumented number of banks, are used. FB indicates the sample of second order and higher children conditional on a first-born brother. FG indicates the sample of second order and higher children conditional on a first-born sister. The regression includes state and year fixed effects. Additional controls are per capita state domestic product, the male/female sex ratio in each state, log number of rural locations per capita, and the population density in each state, all in 1961. These are included in the regression just as the initial number of bank branches in 1961 in the first stage estimate in Table 2. The estimates also include controls for child's age, child's year of birth, mother's age in year of birth, mother's years of schooling, religion, caste, and state-level policies and politics. Sample restricted to children age 6 through 18.

(* p<0.1, ** p<0.05, *** p<0.01)

of interest) are the only ones who can inherit property and parents still need one son to provide old age income security.

For the child mortality outcomes, if anything lower fertility should cause lower mortality rates. This could be a mitigating factor in the above estimates. For first-born son households in which male mortality is not affected by banks, perhaps the positive effects of lower fertility cancel out the otherwise negative effects of access to banks. For male siblings of first-born daughters, there is no mitigating effect of lower fertility. For the education outcomes, lower fertility for first-born son households should have caused higher educational investments. Thus, the negative effect on male education in these households could have been even worse without the fall in fertility. However, given the lack of an effect of banks on fertility for first-born female households, there is no bias to explain why banks cause male siblings in these households to have higher mortality rates or do not effect their education. In addition, the fertility effects do not explain why there is no effect of banks on daughters, who presumably would be better off with fewer siblings.

Table 5: OLS and 2SLS: The Effect of Bank Expansion on Total Children Born, Rural India

	OLS			2SLS		
	All	FB	FG	All	FB	FG
	(1)	(2)	(3)	(4)	(5)	(6)
$\widehat{B_{it}^R}$	-0.1160***	-0.1082***	-0.1151***	-0.1103**	-0.1673***	-0.0637
	(0.0319)	(0.0312)	(0.0292)	(0.0478)	(0.0396)	(0.0452)
Observations	127508	58836	54855	127508	58836	54855

Notes: Robust standard errors, clustered at state level, are reported in parentheses. The dependent variable is the total number of children born to a mother. For the OLS estimates, the actual number of banks, not the instrumented number of banks, are used. FB indicates the sample of households with a first-born son. FG indicates the sample of households with a first-born daughter. The regression includes state and year-mother-turned-eighteen fixed effects. Additional controls are per capita state domestic product, the male/female sex ratio in each state, log number of rural locations per capita, and the population density in each state, all in 1961. These are included in the regression just as the initial number of bank branches in 1961 in the first stage estimate in Table 2. The estimates also include controls for mother's age in year of birth, mother's years of schooling, religion, caste, and state-level policies and politics.

(* p<0.1, ** p<0.05, *** p<0.01)

6 Conclusion

I have shown that access to the formal banking sector reduces discrimination in favor of sons, at least in terms of child mortality and education, but has no detectable effect on daughters. So, in some sense, the banking expansion program increased equality, although not in a way that a policy maker would desire. The unintended consequences of the expansion are consistent with investment in capital substituting for investment in sons. Given that other researchers have found that the bank expansion reduced poverty and that I find that it reduced fertility, it is surprising that there was no positive effect of this large scale banking expansion on child mortality or education. It was also unexpected that female outcomes were unaffected. The model's implication is that higher expected future income should cause parents to invest more in daughters. Perhaps parents are unable to spend more in the short-term if their capital investments have a long-term payoff, and thus their only adjustments can be to reduce investment in children. Or perhaps girl investment is a corner solution, so that parents do not invest more in daughters than their current minimum regardless of increased income from access to banks.

With the rapid global expansion of banking services, and in particular the expansion of microfinance targeted at poor women, there is hope that access to credit will translate into higher incomes and a better life for women. The evidence of the effects of microcredit on female empowerment or child outcomes is limited. For example, Banerjee et al. (2013) find no effect of women's access to microcredit in India on child outcomes or women's empowerment. However, some types of finance, for example commitment savings products targeted at women (Ashraf, Karlan, and Yin, 2010), have been shown to improve female bargaining power. The findings of the negative consequences of bank expansion for sons should caution policy makers about the effects of access to credit. In particular, if credit cannot easily be used to invest in children or the returns to investing in children are small (perhaps because of low quality schools or weak labor markets), then credit expansion may help the current generation at the expense of the next.

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