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Women's economic status and son preference

Empirical evidence from private school enrolment in India

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Social protection

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About this working paper

This paper, *Women's economic status and son preference: empirical evidence from private school enrolment in India,* provides evidence on the effect of improvements in women's access to financial resources on gender inequalities in private schooling investments in rural India. This paper has been copyedited and formatted for publication by 3ie.

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Women's economic status and son preference: empirical evidence from private school enrolment in India

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Executive summary

Gender inequalities in socio-economic outcomes amongst children remain pervasive in many economies, despite significant improvements in household incomes. Due to associated economic and social costs, redressing these imbalances constitutes a priority for most governments. Efforts to ensure greater gender parity in child investment generally take the form of programmes that target women, due to the belief that they exhibit less of a son preference than men. If this is the case, enhancing women's economic positions should reduce gender disparities in future generations.

The prediction that improvements in women's access to economic resources shift expenditure patterns within the household – as indicated by models in which husbands and wives differ in their preferences for different goods – has strong empirical support. However, evidence that these differences include reduced son preference among mothers is derived from a smaller set of influential papers that document reduced gender disparities in schooling and other outcomes for children alongside improvements in women's economic status.

Whether these results also apply to economies with strong patriarchal norms remains an open question. We provide empirical evidence on the effect of improvements in women's access to financial resources on gender inequalities in private schooling investments in one such economy – that of rural India.

In India, as in other economies characterised by strong patriarchal norms, there are good reasons to suspect that mothers – as compared to fathers – display a greater preference for investment in their sons relative to their daughters, even while they may demonstrate a preference for greater gender equality in their own standing vis-à-vis their husbands. One reason for this relates to social norms in patriarchal societies, which place the responsibility for the current and future welfare of both parents on adult sons. Several factors, including mothers' greater life expectancy and consequently longer period of co-residence with sons, suggest that mothers benefit more from this norm than fathers.

Our empirical analysis uses a large household survey covering approximately 15,000 self-help group (SHG) member households in eight of India's poorest states, collected for an evaluation of an intensive version of the National Rural Livelihoods Mission – the National Rural Livelihoods Project. In 2012, the project was phased across India's poorest districts, blocks and villages.[1]

It was first introduced in a set of 'early blocks' in 2012 and extended to 'late blocks' with an average lag of four years.[2] Within early blocks, the programme was also phased over a four-year period across villages in the block. The data we use were collected near the completion of the programme in 2019. They are based on a cross-sectional survey design intended to ensure coverage of SHGs formed during the programme's eight-year duration.

[1] In India, states are divided into districts, with blocks constituting an administrative unit below the district.

[2] Since the project was implemented in two phases, we use the term 'early block' or 'early village' to include those areas where the project started in 2012–2013, and 'late block' or 'late village' for those areas where the project started in 2015–2016.

The variability in children's ages at the time of SHG formation, combined with extensive data on the schooling histories of all resident household members, enables our analysis. We combine administrative data on the date of SHG formation from the National Rural Livelihoods Mission's Management Information System with rich survey data that includes the age (in months) of each child and the history of private school enrolment (for each stage of schooling) for all resident household members.

Using these data, our broad approach is to identify cohorts who were young enough at the time of programme entry to have the choice of private primary school affected by this improvement in women's access to credit. We compare outcomes of 'eligible' and older 'ineligible' cohorts, defined by their age at SHG entry, using two different methods to control for cohort effects.

The first is a difference-in-difference methodology that compares outcomes across cohorts in early- and late-implementing villages, exploiting variation in the timing of the programme across individuals and across space. The second is a fuzzy regression discontinuity design that identifies the local impact of the programme at the cut-off age between eligible and ineligible cohorts. We subject estimates from both methodologies to a series of falsification tests.

Additionally, for our difference-in-difference estimates, we provide strong support for our interpretation of the results as indicative of programme impacts using data on an auxiliary sample of non-SHG households – drawn from the same narrow residential neighbourhoods as the SHG households in our survey.

Both methodologies deliver the same striking result, which is at odds with conventional wisdom and policy practice: improvements in women's access to financial resources under the programme increased private schooling enrolment among sons, but not daughters, thereby widening gender disparity in child investment.

The fact that these different methodologies, based on different samples and exploiting different sources of variation, generate the same result questions the common belief that shifting the intra-household distribution of resources in favour of women will reduce son preference in schooling expenditures.

Our research provides new evidence that it may not do so, helping to explain why improvements in redressing gender inequalities have been so difficult to achieve despite a decade of programmes that have shifted the distribution of welfare benefits in favour of women. It also cautions against casual empiricism that relates greater gender equality in schooling attainment and other child outcomes to improvements in the relative resource position of their mothers.

We caution, however, that our analysis pertains to only one outcome: children's private school enrolment. Evaluating whether this can be generalised to include other child-related outcomes and other settings remains a topic for future research. Additionally, we note the difficulties that arise from addressing this question using retrospective data from a cross-sectional survey. Though our analysis addresses these concerns through careful consideration of regression samples, these shortcomings suggest the importance of validating the results with alternative datasets.

We conclude by emphasising that our results do *not* imply that investing in women constitutes bad policy, or that there is no value to programmes that target women. A body of evidence from other studies provides robust evidence that improvements in women's economic positions significantly enhance their agency, helping to empower women economically, socially and politically, and to improve their standing in society and within their households. These are outcomes that are of significant value.

Instead, our results question assumptions regarding the nature of differences in the preferences of mothers and fathers for investment in sons and daughters, and the related impact of women's empowerment on gender differences in these investments. Contrary to the widespread belief that mothers prefer greater gender equality than fathers, our results suggest that the opposite may hold in patriarchal societies where sons represent a primary source of income for parents in old age, as well as in earlier years. When sons shoulder the responsibility of caring for parents, it is highly likely that mothers benefit more from this relationship than fathers.

In such circumstances, the same set of policies that empower women of one generation may not generate externalities that similarly benefit women in younger generations. This suggests that redressing persistent gender inequalities requires more than improvements in women's access to financial and other economic resources. Programmes that enhance the ability to save for old age, or provide adequate pensions, constitute one such measure. Alternatively, combining women's access to financial resources with training and mentorship that directly address unequal treatment of sons and daughters may have larger returns.

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Abbreviations and acronyms

FRD	Fuzzy regression discontinuity
NRLM	National Rural Livelihoods Mission
NRLP	National Rural Livelihoods Project
SHG	Self-help group

1. Introduction

Gender inequalities in socio-economic outcomes amongst children remain pervasive in many economies, despite significant improvements in household incomes. Due to associated economic and social costs, redressing these imbalances constitutes a priority for most governments. Programmatic efforts to ensure greater gender parity in child investments generally entail targeting women due to the belief that women exhibit less preference for their sons than men. If this is the case, enhancing women's economic positions should reduce gender disparities in future generations.

The prediction that improvements in women's access to economic resources shifts expenditure patterns within the household – as indicated by models in which husbands and wives differ in their preferences for different goods – has strong empirical support. However, evidence that these differences include reduced son preference among mothers is derived from a smaller set of influential papers that document a correlation between reduced gender disparities in schooling and other outcomes for children and improvements in women's economic status (Thomas 1990, 1994; Duflo 2003).

Whether these results also apply to economies with strong patriarchal norms remains an open question. We provide empirical evidence on the effect of improvements in women's access to financial resources on gender inequalities in schooling investments in one such economy – that of rural India.

In India, as in other economies characterised by strong patriarchal norms, there are good reasons to suspect that mothers – as compared to fathers – display a greater preference for investment in their sons relative to their daughters, even while they may demonstrate a preference for greater gender equality in their own standing vis-à-vis their husbands. One reason for this relates to social norms in patriarchal societies, which place responsibility for the current and future welfare of both parents on adult sons. Several factors suggest that mothers benefit more from this norm than fathers.

First, women's greater life expectancy relative to men, combined with significant age differences between husbands and wives, suggests that the period of dependence on sons for old age support is of longer duration for women. Second, as sons enter their primary earning years and parents' contribution to household income falls, the intra-household distribution of expenditure is less likely to reflect the bargaining weights of (elderly) mothers relative to fathers, and more likely to reflect a preference for sons.

Given the importance of mothers in the intergenerational transmission of social norms and in raising and socialising children, this shift may constitute an improvement in mothers' relative status. Put differently, mothers are likely to gain more than fathers from the shift in primary earner status from father to son.

Finally, it is worth noting that women are not just at greater risk than men of income shortfalls in old age but are also more vulnerable to income shocks in any life stage, as recently emphasised by multiple reports of women's greater vulnerability to the economic consequences of the coronavirus pandemic (McKinsey Global Institute 2020; UN Women 2020).

Given the differences in women's and men's income-earning potential, households respond to unexpected reductions in income by meeting the food needs of men over women. In these periods, mothers benefit more than fathers from the availability of income transfers from non-resident sons. Under these conditions, mothers will have a greater preference than fathers for investments in sons and play a larger role in supporting norms that favour sons relative to daughters.[3] Correspondingly, policy changes that further women's empowerment or agency may widen schooling inequalities between boys and girls.

The likelihood of this outcome is strongly borne out by evidence of declining sex ratios at birth in economies such as India, despite decades of strong economic growth that have been accompanied by improvements in women's socio-economic status. Early data from the 2019–2020 National Family Health Survey (the Indian equivalent of the Demographic Health Survey) reveal significant improvements in women's ownership of assets in some of India's states, even those with high levels of poverty, such as the Indian state of Bihar.[4] In this state, ownership of mobile phones used by women increased from 41% in the previous National Family Health Survey (2015–2016) to 51% in 2019–2020, while the percentage of women reporting a bank or savings account that they used jumped from 26–77%.

Mirroring and perhaps reflective of this increase in asset ownership, Bihar has witnessed a steady improvement in women's roles in household decision-making, which is suggestive of increasing empowerment of women. A total of 87 per cent of currently married women reported involvement in three or more household decisions in the 2019–2020 survey, compared to 75 per cent in the 2015–2016 survey.

Despite this, the data reveal that sex ratios at birth continue to fall: in Bihar, an already low sex ratio of 934 in 2015–2016 deteriorated further to 903 in 2019–2020. Data from earlier survey rounds reveal that measures of son preference have, in fact, remained stagnant over the past decade. Surveying these findings, the Government of India's Economic Survey (2017–2018) concludes that 'the area where Indian society … needs to reflect on the most is … "son preference", where development is not proving to be an antidote' (p.105).

Deteriorating sex ratios, despite significant improvements in women's agency, lead to questions about the popular belief that empowering women will reduce gender imbalances in outcomes such as health and schooling in future generations. They also raise the possibility that reductions in gender inequality in related outcomes may reflect other societal changes, occurring not because of women's improved access to financial

[3] Jejeebhoy and colleagues' (2017) in-depth and detailed study of a programme in Bihar that improved women's financial and social assets supports the common observation that mothers play a greater role in the inter-generational transmission of norms relating to gender roles. Their study reports the following quote from a woman describing how she was told at the time of departing her natal home for her husband's to 'live properly, not fight with anyone, do housework and remain quiet in the home'. Clearly illustrating the role of mothers in the inter-generational transmission of gender norms, she said, 'My mother told me all this, no one else' (p. 19).
[4] While schooling levels in the state remain low, the difference in the percentage of (adult) women and men with at least 10 years of schooling has narrowed from 20% in 2015–2016 (43% for men and 23% for women) to 14% in 2019–2020 (43% for men and 29% for women).

and other resources but *despite* such improvements. For example, the near-complete removal of gender inequality in elementary school completion rates may reflect changing social norms around universal elementary education or government policies that have substantially reduced the cost of elementary schooling for girls. Such positive effects could mask the differential preference of parents, including mothers, for investment in sons.

In this context, we evaluate the impact of a programme that improved rural women's access to financial services on gender differences in private school enrolment. Private school enrolment has been growing rapidly, even amongst poor households in rural India. Data from the 2019 Annual Status of Education Report (ASER Centre 2020) reveal that 31 per cent of eight-year-olds in rural India are enrolled in private schools. The same data also document significant gender differences, with 48 per cent of boys but only 39 per cent of girls between the ages of 6–8 enrolled in private schools. The cost of private schooling seems to be the primary factor underlying gender differentials in parental investments in children.

The programme we consider is the National Rural Livelihoods Mission (NRLM), the Indian government's flagship programme to enhance the livelihoods of rural households. The NRLM promotes and supports self-help groups (SHGs) comprising women drawn from below-poverty-line households, with a current coverage of over 70 million women across 600,000 villages. Described in greater detail later in this paper, the NRLM has significantly enhanced women's access to loans.

Our empirical analysis uses a large household survey covering approximately 15,000 SHG member households in eight of India's poorest states, which was conducted during an evaluation of an intensive version of the NRLM, the National Rural Livelihoods Project (NRLP). The pilot programme, initiated in 2012, was rolled out across India's poorest blocks and villages within early blocks.[5]

First introduced in a set of 'early blocks' in 2012, it was extended to 'late blocks' with an average lag of four years. Within early blocks, the programme was also phased over a four-year period across villages in each block. The data we use were collected near the programme's completion in 2019, based on a cross-sectional survey design intended to ensure coverage of SHGs formed during the programme's eight-year duration.

Despite the cross-sectional nature of the data, our analysis is facilitated by the variability in children's ages at the time of SHG formation combined with extensive data on the schooling histories of all resident household members. We combine extensive administrative data on the exact date of formation for the census of all SHGs created under the programme with rich survey data that includes the age (in months) of each child and the history of private school enrolment for each stage of schooling for all resident household members.[6]

[5] In India, states are divided into districts, with blocks constituting an administrative unit below the district.

[6] While the use of retrospective information on outcomes raises justifiable concerns, this is not true of private schooling. Parents are easily able to recall whether children – even much older children – were enrolled in government or private schools.

The data confirm the equality of schooling attainment across boys and girls between the ages of 7–12 (Figure 1). They also confirm the gender differences in private school enrolment that exist in the ASER data referred to earlier (Figure 2).

Figure 1: Highest completed grade by child's age and gender

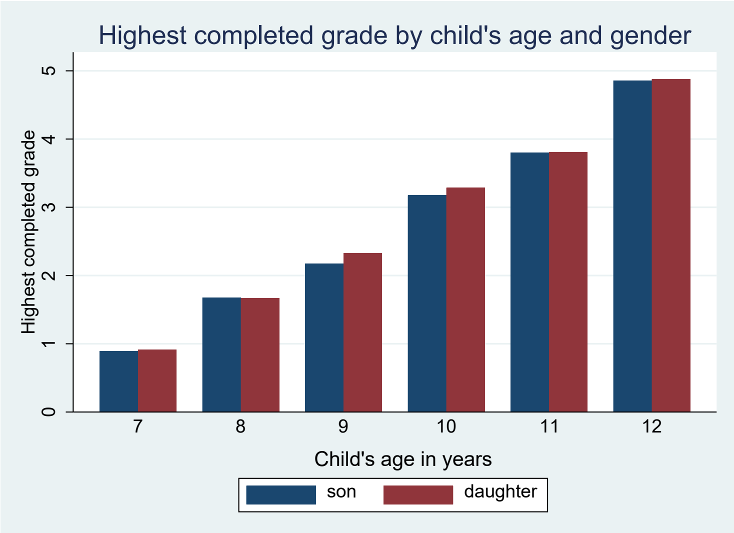
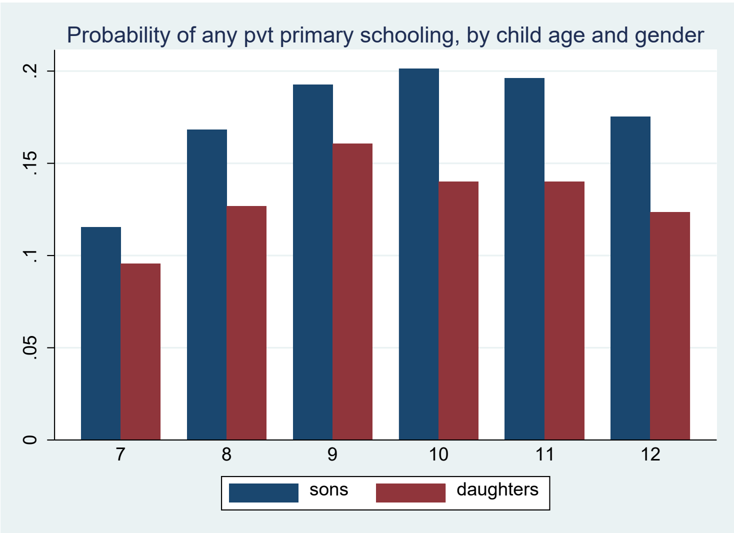


Figure 2: Probability of any private primary schooling by age and gender



Using these data, our broad approach is to identify cohorts who were young enough, at the time of programme entry, to have the choice of private primary school affected by this improvement in women's access to credit. We compare outcomes of 'eligible' and older 'ineligible' cohorts, defined by their age at SHG entry, using two different methods to control for cohort effects.

The first is a difference-in-difference methodology that compares outcomes across cohorts in early- and late-implementing villages, exploiting variation in the timing of the programme across individuals and space. The second is a fuzzy regression discontinuity (FRD) design that identifies the local impact of the programme at the cut-off age between eligible and ineligible cohorts.

Due to the lack of a policy-defined cut-off age, and given the commonness of delayed school entry in rural India, we follow the literature that uses this method in similar contexts lacking a well-defined cut-off by imputting it based on 'goodness-of-fit' measures. We subject estimates from both methodologies to a series of falsification tests. Additionally, for our difference-in-difference estimates, we provide strong support for our interpretation of the results as indicative of programme impacts using data on an auxiliary sample of non-SHG households drawn from the same narrow residential neighbourhoods as the SHG households in our survey.

Both methodologies deliver the same striking result, one at odds with conventional wisdom and policy practice: improvements in women's access to financial resources under the programme increased private schooling enrolment for sons but not daughters, thereby widening gender disparities in child investment. The fact that these different methodologies based on different samples, which exploit different sources of variation, generate the same result questions the common belief that shifting the intra-household distribution of resources in favour of women will reduce son preference in schooling expenditures.

Our research provides new evidence that it may not do so, helping to explain why improvements in redressing gender inequalities have been so difficult to achieve despite a decade of programmes that have shifted the distribution of welfare benefits in favour of women. It also cautions against casual empiricism that relates greater gender equality in schooling attainment and other child outcomes to improvements in the relative resource position of their mothers.

This paper relates to broader literature that establishes gender differences in the investments parents make in their children's health and schooling, including work by Chen and colleagues (1981), Behrman (1988), Subramanian and Deaton (1991) and Das Gupta (1987). More recently (though based on data from the 1992 National Family Health Survey), Barcellos and colleagues (2014) provide evidence of son preference with regard to breastfeeding duration – supporting research by Jayachandran and Kuziemko (2011) – as well as outcomes such as vitamin intake and childcare time devoted to sons relative to daughters.

This literature documents son preference in India and other economies but does not relate it to the intra-household distribution of resources, or to women's bargaining position within the household. Building on work by Chiappori (1988, 1992), Bourguignon and colleagues (1993) and others, a few early and influential papers address this issue, examining whether improvements in women's (unearned) income and other determinants of bargaining power differentially affect investments in daughters relative to sons. Thomas (1990) provides evidence from Brazil that women's unearned income has a greater effect on child health than men's income, and that improvements in women's income favour girls relative to boys. Duflo (2003) reports similar results on the health of granddaughters and grandsons from pensions received by their grandparents.

The primary contribution of our study is the empirical evidence it provides, indicating that these results may not generalise to all economies and all child-related outcomes – particularly its refutation of the widespread belief that programmes targeting financial resources to women will invariably reduce gender inequalities in expenditures on children's schooling. While other studies in similar contexts also report evidence that supports our results (Quisumbing and Maluccio 2003; Filmer et al. 2008), our use of quasi-experimental variation enabled by the phasing of the programme, combined with estimates based on alternative methodologies, provides credible evidence of the causal impact of improvements in women's economic standing on gender differences in investments in children's schooling.

Differences in socio-economic contexts, both in our study and in earlier work on this topic, are important and provide a clue to reconciling these and other results, including results contrary to ours from other studies in India. Supporting the evidence presented in Thomas (1990) and Duflo (2003), Jensen (2012) provides evidence of greater schooling investments in daughters and improvements in health of school-aged girls and women in the 15–21 age group following the introduction of employment opportunities for women in business process outsourcing firms in North India. Similarly, Qian (2008) finds that improvements in the productivity of crops that are intensive in women's employment generated large and immediate improvements in sex ratios.

In both programmes, improvements in women's incomes were substantial. In Jensen's setting, access to recruiting services provided educated women with the opportunity to earn incomes that were approximately twice the average of non-business process outsourcing workers with similar levels of education. Similarly, the effects of improvements in grandparents' pension income on grandchildren studied by Duflo (2003) are from a programme in which the monthly pension amount was twice the median per capita income in rural areas.[7] In contrast, programmes targeted at women who generate smaller gains in their incomes suggest impacts on children that are far more muted.

These include micro-credit programmes. Though some studies report large effects on child health and related outcomes from credit programmes that target women (Pitt et al. 2003), most evaluations report either small or insignificant effects. For example, out of three randomised evaluations of micro-credit programmes that supported groups composed exclusively of women, only one estimated a significant effect on school enrolment, an effect that was small in magnitude (Angelucci et al. 2015). The other two, based in India and Mongolia, found the effect to be insignificant (Banerjee et al. 2015; Attanasio et al. 2015). Of these studies, Banerjee and colleagues (2015) examined differences in outcomes across boys and girls, finding no effect of the programme for either group. The authors also report no effect on private school enrolment or fees.

[7] In contrast, India's scheme for old age pensions (the Indira Gandhi National Old Age Pension Scheme) provides INR300 per month to below-poverty-line individuals over the age of 60. An additional programme provides the same amount to below-poverty-line widows between the ages of 50–59. In comparison, the current daily wage under the Government's Mahatma Gandhi National Rural Employment Guarantee Programme is INR200, while the daily wage for unskilled rural workers ranges between INR347–387 per day. Thus, the amount of the pension is equivalent to approximately 12 days of income.

Our findings regarding the effect of improvements in women's financial access on gender inequalities in schooling may therefore reflect the fact that these improvements have not been large enough to reduce mothers' economic dependence on sons.[8] Correspondingly, our results can alternatively be interpreted as follows: within a context wherein parents rely on sons for old-age support, improvements in women's intra-household bargaining positions may widen gender disparities in schooling expenditures, unless the improvements are large enough to ensure women's economic independence.

When parents lack adequate old-age pensions or other means of transferring income over the life cycle to support old-age expenditure, the 8–12% return conventionally estimated for investment in children's education may provide far greater returns than investments in farm or non-farm business or other savings instruments, particularly given social norms that minimise commitment problems.

These returns will be greater for women relative to men for the reasons previously described, which are supported by our survey data. In these data, among those over the age of 50, 34 per cent of women are widows and only 10 per cent of men, are widowers. Similarly, 19 per cent of women between the ages of 20–40 report that their mothers are still living and their fathers are deceased. In contrast, only 7 per cent of women in this age group report their fathers being alive while their mothers are deceased. These experiences of the differential age-specific mortality rates of parents make young mothers acutely aware of the probability of their economic dependence on their sons in future years.

We caution that our findings are restricted to private school enrolment. The extent to which they can be generalised, even in the same socio-economic and cultural context, to other child-related outcomes remains a topic for future research. Additionally, it is beyond the scope of this study to assess the precise pathways that underlie our findings. Given this, we interpret our results as indicative of the overall effect of an improvement in women's intra-household bargaining positions on son preference in schooling investments, leaving a detailed investigation of the generalisability of our results to other outcomes and of causal pathways to future research.

The remainder of this paper is organised as follows: Section 2 describes the programme and survey data, and Section 3 describes the empirical methodology of the paper. Summary statistics are provided in Section 4. Results from the difference-in-difference specification and the FRD design are discussed in Sections 5 and 6, respectively. Section 7 concludes the report.

[8] In related research, Costa (1997) ascribes the reduction in inter-generational living arrangements in the United States, and hence the value of children as old age support, to rising incomes that reduced the need for such support.

2. Programme and survey data

2.1 The programme

The analysis of this paper is based on India's NRLP, a pilot of the NRLM that extended to cover target households in all blocks of the country. These households were identified as 'deprived' based on data in India's 2011 socio-economic caste census.[9] The primary objective of the programme is to enhance livelihoods through the formation of SHGs comprising approximately 10 women from target households residing in the same neighbourhood or 'hamlet' – a narrow geographic subdivision of the village characterised by significant homogeneity in socio-economic background across households.[10] In its early stages, the programme focused on ensuring women's financial inclusion, promoting savings and 'internal' loans based on these savings.

The formation of SHGs under the programme was not left to village residents but was instead directed and overseen by a project facilitation team comprising approximately five members. Each team was required to reside in the village for 15 days. Upon entry into the village, the team contacted members of the village government and other local functionaries and involved them in a poverty mapping exercise to identify the hamlets that were predominantly composed of targeted poor households and to verify that target households were correctly identified.

Following its formation, the team used the remainder of its time in the village providing basic training to all SHG members on relevant concepts such as the five principles required of all SHGs[11] and how best to ensure the effective functioning and strength of the group. Members were familiarised with the importance of regular savings, regular meeting attendance and internal lending from group savings.

They were also familiarised with the overall programme, its objectives and the additional inputs to be provided as SHGs matured. These included grants from the government to complement internal lending; access to bank loans of larger magnitude; and training in livelihoods, financial literacy and other related topics. The project facilitation team attended the initial meetings of all SHGs and ensured that all procedures were in place prior to exiting the village.

Each SHG had access to two grants from the government to support internal lending. The first, a grant of approximately INR15,000, was provided within the first three months of SHG formation. The second was a larger 'grant-in-perpetuity' to encourage larger loans, with the amount of this second tranche determined by the state's programme administrators and varying from INR30,000–110,000 per SHG.[12] The programme thus differed from earlier precedents in its capacity-building efforts, provision of initial funds to support internal lending, and scale, with a focus on ensuring widespread coverage in any given village in this initial round of village entry.

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[9] The census provided data on a number of measures of deprivation. Households that met at least one of these criteria constituted the target set for the NRLP.

[10] Details of the programme are available in Government of India 2015a, b.

[11] Known as the *Panchsutras,* these entailed the following: regular meetings, regular savings, regular lending, regular repayments and maintenance of books of accounts.

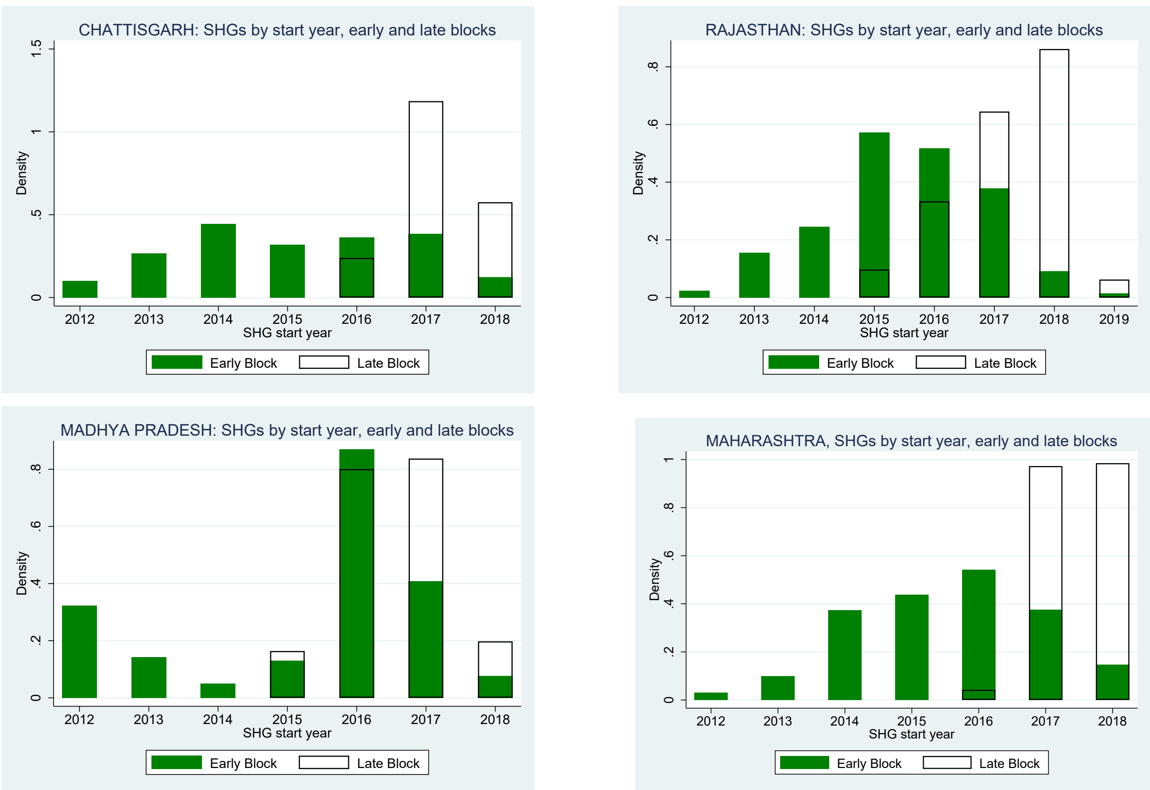
[12] At 2021 exchange rates, this amounts to approximately USD400–1,460. The grant of INR15,000 is approximately equal to USD200.

Early implementation of the pilot NRLP was restricted to blocks that lagged behind others in certain areas. Each state was asked to identify four districts and, within these, a set of four 'early' blocks based on socio-economic indicators such as women's literacy levels and the proportion of households from 'scheduled' castes and tribes, as well as a second set of 'late' blocks to be subsequently included in the pilot. The phasing across early and late blocks within any given district occurred with a considerable time lag. Intervention in early blocks started around 2012, while implementation in late blocks largely commenced in 2016–2017.

In addition to this phasing across blocks, the programme was also phased in across villages within any given block. Implementation required the identification of 'clusters' of villages within each block, with each cluster comprising approximately 25 to 30 villages. Cluster-level plans identified the order of village entry, with larger villages entered first. In early blocks, the process of covering all villages in the cluster was phased in over a period of approximately three to four years. This relatively long time reflected the lack of administrative personnel with sufficient levels of training and experience in the initial phase of the programme to undertake the process of forming SHGs and training its members. Over time, as internal capacity developed, implementation across villages within a block sped up considerably, so that coverage across villages in 'late' blocks occurred over a reduced period of one to two years.

Figure 3 graphs the considerable variation in SHG formation year across early blocks in each state, the lag in phasing between early and late blocks, and the reduced time for implementation in late blocks, using data from the government's management and information system on the date of SHG formation in all programme blocks.

Figure 3: Variation in SHG formation year, across early and late blocks of selected survey states



2.2 Survey data

The data we use for this research is a large cross-sectional survey across eight of India's states, conducted in early 2019 as the NRLP was nearing completion.[13] Since no uniform baseline (pre-programme) study existed, the survey focused on identified SHGs and their members, and built the phasing of the programme into the survey design to ensure wide variation in SHG formation year.[14] As previously described, the NRLP was to be implemented in the poorest blocks of the country, with each state required to identify four districts that contained such blocks, and the exact set of early and late blocks within each such district.

Correspondingly, the survey sample is drawn from NRLP districts with at least two early and two late blocks (as identified by each state's implementing institution, the State Rural Livelihood Mission). Within each of these blocks, two project clusters were selected using management and information system data that identified project clusters and provided information on the year of village entry and SHG formation for the census of all SHGs in the district.

The selection of villages within a cluster was performed by estimating an equation for the age of village entry as a function of the variables from the 2011 census used by state administrators to draw up village entry plans. These were: village population, village population rank within the cluster, remoteness of the village, and the proportion of scheduled caste and tribe households.

By stratifying villages by predicted age, the two oldest villages from the top half of the distribution and the two youngest from the bottom half were selected. The same procedure was used to select 'early' and 'late' villages in early and late blocks. As documented in the summary statistics (Table 1) discussed in Section 4 of this paper, the average time lag between SHG entry in early and late villages of the same block in our sample was three years.

[13] These states are: Bihar, Chattisgarh, Jharkhand, Madhya Pradesh, Maharashtra, Odisha, Rajasthan and Uttar Pradesh.

[14] Though baseline studies were conducted in each state, each was carried out independently. Therefore, it was not possible to construct common baseline measures across surveys, even for basic outcomes such as women's empowerment, expenditure or even household demographics. These studies could therefore not be combined for an overall evaluation study.

Table 1: Summary statistics: sample means

	Difference-in-difference sample			Non-SHG	Regression
	Full sample	**Early villages**	**Late villages**	**member household**	**discontinuity sample**
Proportion private (primary) enrolment	0.15 (0.36)	0.233 (0.423)	0.119 (0.324)	0.16 (0.37)	0.18 (0.38)
Proportion male	0.53 (0.50)	0.546 (0.498)	0.518 (0.500)	0.53 (0.50)	0.51 (0.50)
Child's age (years)	13.27 (2.39)	13.79 (2.160)	13.05 (2.447)	13.08 (2.42)	11.76 (2.34)
Mean SHG year	2015.84 (1.46)	2014.1 (0.262)	2016.6 (1.050)	--	2016.18 (1.59)
Mother's schooling years	2.37 (3.69)	2.486 (3.835)	2.321 (3.619)	2.32 (3.64)	2.54 (3.77)
Mother's age	37.19 (6.13)	37.98 (6.187)	36.85 (6.080)	36.89 (6.28)	35.84 (6.21)
Proportion SC/ST	0.62 (0.48)	0.543 (0.498)	0.659 (0.474)	0.58 (0.49)	0.65 (0.48)
agricultural land (ha)	1.11 (1.70)	0.646 (1.193)	1.304 (1.840)	1.10 (1.89)	1.10 (2.61)
Proportion of villages with population ≤ 1,000 (2011)	0.54 (0.50)	0.143 (0.350)	0.705 (0.456)	0.50 (0.50)	0.41 (0.49)
Proportion of villages with a '*pucca*' road (2011)	0.67 (0.47)	0.891 (0.312)	0.569 (0.495)	0.66 (0.48)	0.70 (0.46)
Proportion of villages with private primary school (2011)	0.16 (0.36)	0.245 (0.430)	0.119 (0.324)	0.19 (0.39)	0.20 (0.40)
N	3,946	1,190	2,756	1,661	9,049

Note: SC/ST = scheduled caste/scheduled tribe. Standard deviations are in parentheses.

Within each village, a set of six SHGs was randomly chosen for a survey module that provided detailed information on SHGs. Of this set, two were selected to serve as the basis for the household and women's questionnaire, with the survey design calling for 10 members of each SHG to be interviewed for these modules. Because SHGs were formed at the hamlet level, members of these SHGs came from the same narrowly defined residential neighbourhoods.

In addition to SHG member households, the survey interviewed 10 households in each village that were not members of the 'index' SHGs associated with these hamlets. These households were drawn from the same residential hamlet as the SHGs whose members were included in the household survey, providing a sample of households from the same narrow residential geography. Because of the extensive residential segregation of households according to caste in villages in India, these households are also very similar to SHG members in socio-economic characteristics. Summary statistics on household characteristics for both samples (SHG members and non-members) are provided in Section 4.

This analysis uses administrative management and information system data as well as survey household data. Data on the exact date of formation of each SHG in a block constitute the basis of our identification of the timing of block entry and the age (in months) of each SHG. They also enable verification of the identification of early and late blocks by each State Rural Livelihood Mission.

The household module – in addition to conventional data on household demographics, highest grade completed and current enrolment status – collected schooling histories for all current resident members of the household. These histories provide information for each resident member on whether they had ever been enrolled in a private school. This question was asked separately for different levels of schooling (pre-primary, primary, secondary, higher secondary, college and beyond), allowing us to identify whether any resident child, regardless of their age and current schooling status, had ever been enrolled in a private primary school.

These data, along with information on each child's year and month of birth and the year and month of SHG formation, allow us to determine the child's age at the time of SHG formation, and to relate this variable to private school enrolment at the primary level.

Although the data allow us to relate private school enrolment to SHG access at the time that this choice was being made, an important restriction on this analysis is that this information was collected in 2019, after these choices were made, and more importantly, after the programme had begun even in late blocks. This has important implications for the construction of control samples.

In a conventional difference-in-difference regression applied to programmes characterised by phased implementation, control samples are conventionally identified by surveys conducted prior to the start of the programme in the phase from which the control sample is drawn. Because our survey was conducted after the introduction of the programme in late blocks, careful attention must be paid to the construction of the control sample to ensure its validity for the analysis.

A similar concern relates to the identification of eligible and ineligible cohorts in each sample, given that data on private school histories are only available for individuals who were residents of the household at the time of the survey. Details of the construction of treatment and control samples and of eligible and ineligible cohorts are discussed in the methodology section of this paper.

3. Empirical methodology

3.1 Broad approach

Our identification strategy derives from the time-bound nature of decisions regarding school choice and the resultant importance of credit availability timing. For credit-constrained households who cannot borrow against potential improvements in future income or credit sources, access to new sources of credit will not affect school choice if it arrives 'too late'; that is, after the age of school entry, when school choices have already been made.

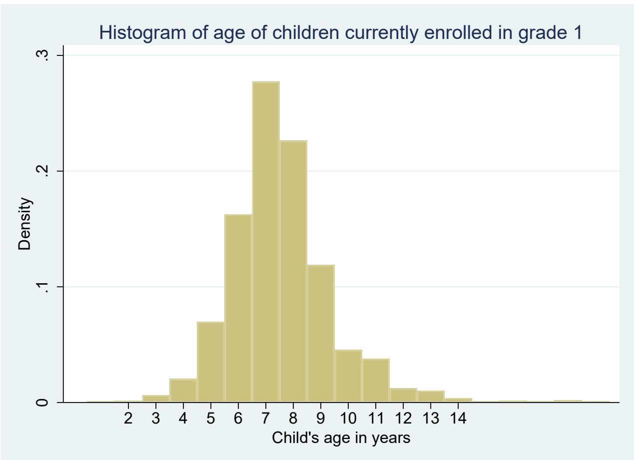
This reflects the fixed one-time entrance or ‘capitation’ fee charged by private schools, which in turn implies that the choice between government and private schools is made at the start of a child’s schooling. Thus, while it is undoubtedly true that the availability of new credit sources in year *t* will not impact primary school enrolment of children who have completed primary school by this year, it is also unlikely to impact the schooling of children who have already completed some years of primary schooling by year *t* – given both the fixed entry fees and the substantial costs involved in transferring from one school to another.[15]

Correspondingly, our approach compares ‘eligible’ and ‘ineligible’ cohorts, defined by their age at the time of SHG formation relative to a cut-off age beyond which access to credit is unlikely to affect school choice. We define and discuss this cut-off age below.

A comparison across adjacent eligible and ineligible cohorts, even when the determination of eligibility status varies by SHG formation year, confounds cohort effects with programme effects. We implement two approaches to isolate programme effects. The first uses a difference-in-difference methodology, exploiting the phasing of the programme across early and late villages within a block. The second is an FRD design that identifies programme effects based on a jump in probability of private school enrolment amongst children at the cut-off age for school choice at the time of SHG formation.

Delayed entry into school, which is common in rural India, complicates the definition of a cut-off age for school entry. We make this choice based on data on the age of survey children currently enrolled in grade 1. These data, graphed in Figure 4, reveal that although a significant number of nine-year-old children are enrolled in grade 1 school entry at older ages is negligible. This in turn implies that the availability of SHGs after this age is unlikely to affect private school enrolment. We accordingly define the cut-off age by which school choice is no longer affected by access to credit as nine years (108 months).

Figure 4: Histogram of age of children currently enrolled in grade 1



[15] Transfers require a student to request and obtain a ‘transfer certificate’. These must be obtained by the parent and counter-signed by officials at the block level, requiring a considerable time and monetary investment by households. Thus, school transfers are most common in school transitions that involve additional costs, such as the transition from primary to middle school.

We validate our choice of cut-off age by comparing it to an estimate that satisfies a goodness-of-fit criterion. In doing so, we build on previous research in contexts similarly characterised by the absence of a well-defined cut-off. For example, Card and colleagues (2008) estimate the point of discontinuity using a goodness-of-fit criterion in testing for a race-based tipping point in residential segregation. Munshi and Rosenzweig (2017) use a similar procedure to estimate the cut-off used in their study of the effect of the population share of ethnic goods on the supply of local public goods.

Following Hansen (2017), we select as a cut-off the age (in months) that minimises the sum of squared residuals in regressions of the probability of private school enrolment on different values of *ageatshg.*[16] Figure 4 plots the sum of squared residuals against the assignment variable, revealing a distinct minimum when the second child is 112 months, validating our cut-off of 108 months (nine years).

In the remainder of this section, we provide an overview of the two methodologies, focusing on the determination of samples, the specification of regression equations and the assumptions required for implementation. The presentation and discussion of regression results that validate these methodologies is combined with our discussion of the main results of this paper in Sections 5 and 6.

3.2 Difference-in-difference regressions

3.2.1 The difference-in-difference estimator in our context

Our difference-in-difference regressions exploit the three-year phasing of the programme across villages in early blocks. In doing so, we follow a literature that uses the geographical phasing of the programme to define treatment and control samples (Imbert and Papp 2015).

We start by defining the difference-in-difference estimator in our context. We define treatment villages as those in which the programme was introduced in the first phase, with the indicator variable *EV* taking the value 1 for this set of villages and 0 for late villages in the same block. Let $elig_c$ be an indicator variable for the cohort of children who, at the time of programme entry into this set of early villages, were of school entry age and therefore in a position to have school choices affected by improvement in women's access to credit. $Elig_c$ thus takes the value of 1 for this school-entry age cohort and 0 for the next-oldest cohort, for whom this expanded access to credit arrives too late to impact school choices.

Identification through a difference-in-difference regression requires us to identify members of these same cohorts (that is, of the same age) in the set of late villages. It also requires that the sample of late villages is such that at the time of programme entry, both these cohorts (identified by their age when the programme commenced in early villages) are too old to have their school choices affected by the programme. For now, we assume that this is the case, discussing the identification of cohorts and samples in the next sub-section.

[16] The regression equation is implemented on our regression sample, detailed later in this section. It allows for 9,165 kink points, after trimming the top and bottom 10 per cent of the sample.

Let the indicator variable EV_b take the value 1 for early villages and 0 for late villages of the block. The outcome in question – private primary school enrolment – takes the value 1 if child i, associated with SHG j in village v and block b, was ever enrolled in primary school. Omitting other determinants for notational simplicity, we assume the following specification for this outcome variable (Y):

$$Y_{ijvb} = \beta_0 + \beta_1 D_{ivb} + \beta_2 EV_b + \beta_3 elig_{ivb} + u_{ijvb}$$

In this equation, D_{ivb} is an indicator variable for individuals who benefit from SHG access at an age that enables enrolment in private school. That is, it takes the value 1 for individuals of eligible cohorts in early villages of the block in question. The error term u_{ijvb} reflects all unobserved determinants of the private school enrolment choice. Differencing across early and late villages of the block, for the same cohort of eligible children, removes the effect of *elig*, while differencing across eligible and ineligible cohorts of early villages removes the effect of *EV.* Thus, the difference between eligible and ineligible cohorts in early villages is:

(1) $$E(Y_i|EV=1, elig=1) - E(Y_i|EV=1, elig=0) = \beta_1 + \beta_3 + E(u_i|EV=1, elig=1) - E(u_i|EV=1, elig=0)$$

while the equivalent difference between eligible and ineligible cohorts in late villages is:

(2) $$E(Y_i|EV=0, elig=1) - E(Y_i|EV=0, elig=0) = \beta_3 + E(u_i|EV=0, elig=1) - E(u_i|EV=0, elig=0)$$

Differencing (3) from (2) thus yields:

(3) $$\hat{\beta}_1 = \beta_1 + \{E(u_i|EV=1, elig=1) - E(u_i|EV=1, elig=0)\} - \{E(u_i|EV=0, elig=1) - E(u_i|EV=0, elig=0)\}$$

Thus, as in a more conventional difference-in-difference methodology applied across time to treated and control individuals, the estimation of equation (1) recovers the average effect of treatment on the treated (β_1) under a 'common trends' assumption. In our context, it requires the assumption that the difference between eligible and ineligible cohorts in early villages in the absence of treatment (the first bracketed term in the right-hand side of equation [4]) equals the difference between the same cohorts in villages that did not have access to the programme when either of these cohorts were making decisions about schools.

3.2.2 Identifying cohorts and ensuring the validity of the control sample

The use of a difference-in-difference regression requires that eligible and ineligible cohorts be identified not just in treatment villages, but also in control villages in which programme entry occurs much later. The fact that both treatment and control villages come from within the same block enables this identification.

Specifically, we determine the date of programme entry as the date of initiation of the programme in the block, and correspondingly define eligible cohorts as those between the ages of 5–9 years (60–108 months) at the time of programme entry, with children between the ages of 9–13 years (109–156 months) classified as 'ineligible' students. Thus, eligible and ineligible cohorts for the control sample are those that would have had access to the programme if the process of SHG formation occurred at the same time as it did in early villages.

We restrict our regression sample by age to account for the fact that data on private school enrolment are only available for children who currently reside in the household. The median age of marriage (18.6 years for women in India[17]) suggests that we lack data on the schooling choices of girls aged 18 and above at the time of the survey. We similarly only observe primary school enrolment for those who were old enough to attend school at the time of our survey. Our regression sample is thus restricted to children between the ages of 5–18 at the time of our survey in 2019.

This necessitates further restrictions on the sample because children aged 13, who constitute the upper bound of the ineligible cohort, would be excluded in blocks where the programme started in 2013 or earlier. This implies that eligible and ineligible cohorts would not be balanced by age, with eligible cohorts being older than ineligible cohorts. This in turn would result in a downward bias in estimates of the impact of the programme on private school choices.

To ensure balance, we therefore further restrict our sample to blocks in which the programme commenced after 2013. Thus, the mean year of block entry is 2014 in early villages and 2017 in late villages.

Correspondingly, 'eligible' cohorts in control (late) villages comprise children who were aged 5–9 when the programme started in early villages of the block, but who were 8–12 years old when SHG formation started in their own village, as a consequence of the difference in year or programme entry in early and late villages of the block. This ensures that both eligible and ineligible cohorts in control village were not impacted by the programme. In these areas, programme entry occurred too late to influence school choices, even for younger eligible cohorts. This enables the use of late villages as a control sample.

3.2.3 Common trends

As discussed in subsection 3.2.1, the identification of programme effects through a difference-in-difference regression requires an assumption that, in the absence of the programme, the difference in private school enrolment between early and late villages for eligible cohorts would have been the same as that for older ineligible cohorts. Examining differences across villages within a block has the distinct advantage of having treatment and control villages located within a relatively homogeneous agroclimatic and socio-economic context, with the additional advantage that the programme is overseen by the same block-level administration in both sets of villages.

There are, however, significant differences in population size and other attributes of early and late villages, such as distance to the block capital. These differences suggest that residents of the sample of late (smaller) villages might have less access to private schools than their counterparts in early (larger) villages.

[17] National Family Health Survey 4 (2015–2016). The corresponding median age at first marriage for males (aged 25–49 years) is 24.5 years.

The difference-in-difference regression allows for such level differences in private school enrolment across early and late villages. However, these regressions would generate biased estimates of programme effects if late villages benefitted from significant infrastructural improvements starting around the same time as the SHG programme, if they increased private school enrolment in late villages at a faster rate than observed in early villages.

For example, government investments in roads in the past decade may have concentrated on smaller and more remote late villages, differentially increasing private school enrolment in this sample and hence violating the common trends assumption. Indeed, though the government's village road construction programme commenced in 2000, well before initiation of the NRLP,[18] it targeted smaller villages on a priority basis. Despite the fact that this specific programme was initiated in the decade prior to the NRLP, the possibility of differential trends in private school enrolment across early and late villages constitutes a valid concern.

As is common, we weaken the common trends assumption, requiring it to hold after conditioning on a set of covariates that generate differences in trends between early and late villages. Focusing on infrastructure developments, we include baseline indicators for whether the village is connected by a *pucca,* or permanent (solid) road; whether it has a private primary school; and whether the village population is under 1,000; as well as interactions of the later variable with *pucca* roads and private school availability. These indicators are taken from the 2011 census and hence reflect conditions at the start of the NRLP.

Mean values of these variables reveal their significant differences across the sample of early and late villages, suggesting their potential for capturing differential trends. Thus, the mean proportion of early villages reporting *pucca* roads and the availability of private schools is 0.78 (standard deviation of 0.41) and 0.23 (standard deviation 0.42) in early villages, but just 0.57 (standard deviation 0.49) and 0.11 (standard deviation 0.31) in late villages. Similarly, 27 per cent of villages in the early village sample, but as many as 70 per cent of those in the late village sample, have a population size of 1,000 or less.

We report regression results that include these variables amongst the set of covariates, as well as results that weight observations by the inverse probability of treatment, based on a logit regression of the indicator for early villages on these same covariates and state fixed effects. The logit specification allows for the choice-based nature of the sample (Heckman and Todd 2009).

We also implement two regression-based tests of the common trends assumption. Our first regression follows a conventional approach of testing trends based on data for additional older cohorts. For this purpose, we add an additional older (ineligible) cohort of children to the regression sample comprising children aged 13–16 at the time of programme entry.

In early programme villages, for example those in which entry occurred in 2014, this is a group of individuals between the ages of 18–21. This suggests that this sample will

[18] Asher and Novosad (2020) evaluate this programme and document its success in connecting small villages.

exclude daughters who may have married early and for whom we have no information on past schooling choices. We therefore restrict our analysis to sons, expanding the sample to include sons between the ages of 5–21 at the time of the survey.[19]

Re-running equation (1) for the two sets of ineligible children (ages 9–13 and 13–16 at the time of programme entry), we test for parallel trends by examining the coefficient on the interaction of indicators for cohort and early village. Results of this test are discussed in conjunction with our discussion of the main regression results in Section 5.

While the regression described above tests for common trends in older cohorts, it does not rule out the possibility of more recent improvements in infrastructure that differentially affected younger cohorts, but not older ones. For example, in any village *v*, infrastructural investments made in the last five years would affect school choices of those between the ages of 6–9, but not those of older cohorts. That is, the test above does not rule out the possibility that any identified difference in trends between eligible and ineligible cohorts, in early relative to late villages, reflect other confounding changes in village-level conditions that occurred at the same time as the introduction of SHGs.

We test for confounding effects caused by other village-level changes using the sample of non-SHG members that were surveyed in the same residential hamlets of the villages that the survey population was drawn from.[20] The fact that this sample comes from the same neighbourhoods (hamlets or residential subdivisions of the village) as our main regression sample provides the basis for a strong test of the common trends assumption.

These households experience the same village-level conditions, and hence are affected by all recent improvements or changes in village infrastructure as are our sample households, differing from our regression sample only in terms of SHG membership. Correspondingly, the difference-in-difference regression implemented on this additional sample provides evidence on the difference in outcomes experienced by treatment and control samples in the absence of treatment. This enables a strong test of the common trends assumption.

3.2.4 Estimating equation

Let *Pvt_prim* be an indicator variable that takes the value 1 for children (ever) enrolled in a private primary school. The equation we estimate for child i in village v of block b, suppressing indexation by households for notation simplicity, is:

$$(4)\quad Pvt\_prim_{ivb} = \tau_0 + \tau_1 EV_b * elig_{ivb} + \tau_2 EV_b + \tau_3 elig_{ivb} + X'_{ivb}\tau_4 + \tau_5 S + v_{ivb}$$

In this equation, EV_b is an indicator variable for early villages in block b, while *elig* is an indicator for the relevant eligible cohort. The set of controls (*X*) includes a minimal set of household covariates, specifically the mother's schooling, an indicator variable for scheduled castes and tribes, the household's ownership of agricultural land, and a set of six age-by-gender indicators for adult members of the households (ages 20–40, 40–60 and over 60). All regressions include fixed effects for the state of residence (*S*), but we

[19] The legal minimum age of marriage for sons in India is 21.

[20] Some women in this sample were members of SHGs located in other hamlets. We restrict the sample for this regression to women who were not members of any SHG.

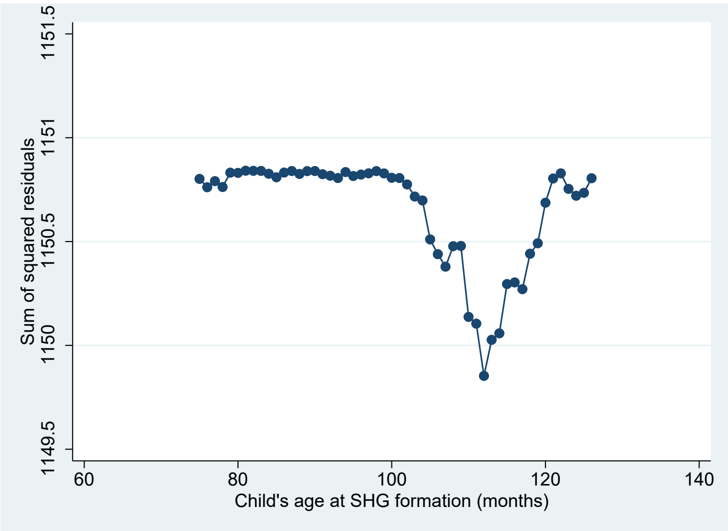
also report results from a specification that replaces state fixed effects with block fixed effects. Robust standard errors are reported for each regression. To test for gender differences in schooling outcomes, we run equation (5) separately for boys and girls.

3.3 Regression discontinuity design

3.3.1 FRD design

The evidence provided in Figure 5 suggesting a discontinuity in the relationship between private primary school enrolment and the age of the child at the time of SHG formation also suggests the potential for identifying the causal effects of SHG access using a regression discontinuity design. Given that treatment compliance is imperfect at the cut-off, affecting some but not all households and hence resulting in a jump in the probability of private school enrolment that is less than 1, identification is based on an FDR. This approach identifies the causal effect of SHG access on private school enrolment only of 'compliers', or households whose schooling choices are affected by the availability of credit at this cut-off (Imbens and Lemieux 2008).

Figure 5: Estimated threshold for private school enrolment



As in a strong regression discontinuity in which the probability of treatment jumps from 0 to 1 at the cut-off score, identification through a fuzzy discontinuity requires standard regularity and continuity conditions to be met. Under these assumptions, the FRD estimand, applied to contexts where the timing of treatment varies, estimates a weighted average of marginal effects (at different treatment times) of the effect of SHG membership on private school enrolment (Cattaneo et al. 2016). We provide graphical and regression support for the identification assumptions in subsequent sections of this paper.

3.3.2 Assignment variable and sample

To implement this approach, we define the continuous running variable, *ageatshg,* as the child's age at the time of formation of the SHG to which the mother belongs. Thus, *ageatshg* is positive for children born before SHG formation and negative for those born after. We compare private school enrolment for children on either side of a cut-off at 108 months (nine years at the time of SHG formation) – the same cut-off used in the difference-in-difference regressions.

Given the local nature of identification, we narrow the age band for the eligible cohort to 7–11 years. This in turn allows us to extend the sample to include SHGs formed after 2012 (rather than after 2013 in the difference-in-difference regressions). For the oldest SHGs (those formed in 2013), the sample is therefore restricted to children between the ages of 7–11 in 2013, or 13–17 in 2019, when the survey was conducted. As before, we restrict the sample to children below the age of 18 at the time of our survey.

3.3.3 Regression equation

Define *Pvt_prim_{ij}*, as before, as an indicator variable that takes the value 1 if a child *i* whose mother is a member of SHG *j* was (ever) enrolled in a private primary school. Suppressing indexation of households, blocks and districts for notational simplicity, the equation we estimate is:

(5) $$Pvt\_prim_{ij} = \delta_0 + \delta_1 I\left[ageatshg_{ij} \leq 108\right] + \delta_2 ageatshg_{ij} + \delta_3 ageatshg_{ij}^2 + I(shgage)_j + \delta_4 X_{ijt} + S + u_{ijt}$$

The basic regression includes an indicator variable for values below the cut-off of 108 months and a quadratic in the running variable. We also report results from regressions that include a cubic in the running variable. The set of control variables (*X*) remains the same as those previously described for the difference-in-difference methodology, with *S* being a set of state-level fixed effects.

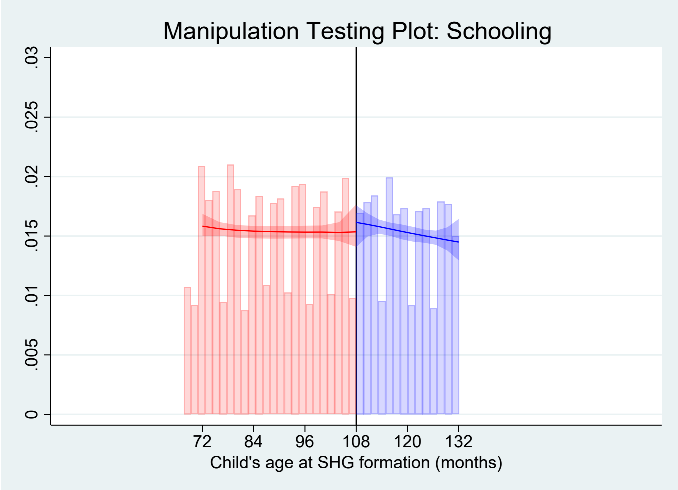
As previously discussed, the sample includes SHGs in early and late blocks and is therefore characterised by significant variation in SHG age. Given this, we also include a set of indicator variables for the year of SHG formation. This ensures that the variation we exploit is across children of similar ages, rather than across SHGs of varying ages.

3.3.4 Density manipulation test

The interpretation of a jump at the cut-off as indicative of a causal effect of treatment on outcomes requires that the cut-off cannot be manipulated by households. We expect this to be the case in our context, given the inability of households to manipulate the age at which the programme entered any given village. Nevertheless, we test for the smoothness of the density function of the assignment variable, *ageatshg,* around the threshold of 108 months using the local polynomial density estimates of Cattaneo and colleagues (2020, 2021).

Plots of the density functions (Figure 6) demonstrate the heaping of age at distinct mass point, a characteristic of data sets in South Asia and many other developing economies. Despite this, the test statistic for the continuity of the density function at the cut-off point T = -0.28 (Pr > |T| = 0.77) rejects the null hypothesis of a discontinuity of the density function at 108 months.

Figure 6: Manipulation testing: private school enrolment



3.4 Robustness and falsification tests for the FRD specification

We test the robustness of our FRD results in a variety of ways. A first set of regressions, which we present graphically, tests for discontinuities in the relationship between the set of covariates used in the regression analysis and the cut-off age of the child at the time of SHG formation. If any discontinuity in the relationship between private school enrolment and the age of the child at SHG formation reflect a discontinuous relationship between unobserved variables that are correlated with private school enrolment and the running variable (*ageatshg*), we would expect to find a similar discontinuity in the relationship between observed covariates and *ageatshg.* We therefore estimate equation (6), replacing the dependent variable, in turn, with the full set of nine covariates used in the estimation of the equation for private schooling.

We also report results from regressions that test the sensitivity of the FRD results to changes in the sample. A first set of regressions reduces the age of children in the sample by six months at the lower and upper bounds, while a second set extends these age limits by six months. Thus, the first set of robustness tests is from a sample that is expanded to include children between the ages of 78–136 months at the time of SHG formation, while the second set is restricted to children between the ages of 66–150 months.

Finally, we consider an additional falsification test similar to tests of difference between older cohorts in treatment and control villages used to validate the results from difference-in-difference regressions. Consider two adjacent but older cohorts defined, as previously, by their age at the time of SHG entry in early villages. As older cohorts, their school choices had been completed by the time of SHG entry.

Correspondingly, under the continuity assumptions that underlie identification through FRD methods, we should find no break in private school enrolment at the cut-off age that separates these cohorts. Testing for such a break provides additional evidence on the validity of the continuity assumptions. That is, unless continuity assumptions do not hold, the school choices of children on both sides of the cut-off between adjacent older cohorts should not be impacted by any improvement in financial access that occurred after their schooling choices had been made.

For this last set of regressions, we therefore consider children between the ages of 120–192 months at the time of SHG formation (12–16 years) and define a cut-off at the midpoint of this range (156 months). While extending the sample, we still need to account for the fact that we only observe outcomes for children who are current residents of the household. This requires restricting the sample to children between the ages of 9–18 at the time of the survey, and to households that are members of SHGs formed after 2016, so that children who were 16 years of age at the time of SHG formation are observed in the sample.

As in the difference-in-difference regressions, we also report regressions only for the sample of sons. This allows us to extend the sample to sons between the ages of 9–21 at the time of the survey, and to SHGs formed after 2013.

3.5 Comparing difference-in-difference and FRD estimates

As is well known, difference-in-difference and FRD estimates differ in interpretation and, therefore, in what they identify. FRD evaluates a local causal effect, identifying the impact of SHGs available at the cut-off age when schooling choices are being made. When the sample is pooled across SHGs formed at different times, FRD estimates represent a weighted average of local average treatment effects for these different formation dates (Cattaneo et al. 2016).

In our context, the FRD estimate is a weighted average of the effect of SHG access on nine-year-old children across SHGs formed between 2013–2018. In contrast, the difference-in-difference estimator pools outcomes for children of different ages within the same cohort, representing a weighted average of treatment effects for the differentially aged children in eligible versus ineligible cohorts.

Because of the restrictions we impose on SHG year to ensure balance, the sample used for the difference-in-difference regressions does not significantly differ from that used to implement the FRD in terms of SHG age. The mean year of SHG formation in the former is 2015.8, while it is 2016.2 in the latter. The difference between the local nature of FRD estimates relative to the difference-in-difference regressions based on a comparison of mean cohort differences is primarily reflected in differences in the mean age of children – 11.8 in the FRD sample and 13.3 in the difference-in-difference sample.

The two samples, however, differ substantially in size, primarily reflecting the fact that the difference-in-difference sample uses observations only from early blocks that represent only half of our full survey sample. In contrast to a sample size of 3,947 used for the difference-in-difference regressions, the FRD sample size is 9,049, including children in both early and late blocks.

Additionally, the programme defined early blocks as the four most disadvantaged blocks, while late blocks came from the next most-disadvantaged set. Thus, there are differences in socio-economic conditions across households in early and late blocks. Villages in late blocks are, on average, larger than those in early blocks (with an average size of 2,150 compared to 1,760) and have a smaller proportion of people from scheduled castes and tribes (0.44 compared to 0.52). Agricultural land ownership is also smaller (1.03 hectares compared to 1.20).

Differences in other covariates are, however, smaller. For example, there is an insignificant difference in the mean schooling attainment of mothers in these two samples (2.6 years for mothers in late blocks relative to 2.5 in early blocks).

The large difference in size between the samples used in the FRD estimation and the difference-in-difference regressions, as well as the difference in socio-economic characteristics of villages in late and early blocks, serve as a test of external validity. Therefore, if both specifications suggest similar effects, despite the extension of the FRD sample to include villages that are better off, this suggests that the results are not only reflective of outcomes amongst the poorest households in the sample.

4. Summary statistics

Summary statistics for the difference-in-difference, non-SHG member and FRD samples are presented in Table 1, with statistics provided separately for early and late villages in the difference-in-difference sample as well as for the full sample. These statistics document the difference in the proportion of children reporting ever having enrolled in private primary school in early villages (0.23) relative to late villages (0.12).

However, as discussed in Section 3, they also reveal differences between these two sets of villages in population, road connectivity and the availability of private schools. Early villages also have a lower proportion of households from scheduled castes and tribes and smaller landholdings relative to late villages. These differences suggest the need for a difference-in-difference regression that controls for these differences in village characteristics across the two samples.

The sample of non SHG members, used to validate the common trends assumption of the difference-in-difference regressions, is significantly smaller than that used for the main difference-in-difference regressions (1,661 as compared to 3,946). However, the statistics in this table suggest that the two samples are similar in village, household and individual child characteristics. Thus, there is little difference in village characteristics between this and the main regression sample. This is expected, given that both samples are drawn from the same residential hamlets. There is, however, also little difference in agricultural land ownership and other household characteristics such as mothers' schooling attainment and age. The comparability of the two samples validates the use of the non-SHG member sample to test the common trends assumption.[21]

Table 2 provides summary statistics by the child's gender for individual and household variables for the difference-in-difference and FRD samples separately. Both samples reveal significant differences in the probability of private school enrolment for boys and girls. In the FRD sample, the proportion of boys ever enrolled in private primary school is 20 per cent and for girls just 15 per cent.

[21] Our survey asked women in these households why they had never joined an SHG. The two most common explanations (18% of reasons cited for each) were that they were not asked to be a member, suggesting their ineligibility according to programme rules, and that they did not understand the concept of SHGs. Sixteen per cent of women stated that they were 'not allowed to become a member' by other members of the household.

Table 2: Summary statistics by gender of child

	Difference-in-difference sample		Regression discontinuity sample	
	Sons	**Daughters**	**Sons**	**Daughters**
Proportion private (primary) enrolment	0.163 (0.370)	0.143 (0.350)	0.197 (0.397)	0.154 (0.361)
Proportion with age ≤ 108 months	--	--	0.515 (0.500)	0.514 (0.500)
Proportion eligible cohort	0.532 (0.499)	0.555 (0.497)	--	--
Child's age (years)	13.33 (2.385)	13.22 (2.390)	11.76 (2.374)	11.73 (2.286)
Mean SHG year	2015.8 (1.473)	2015.9 (1.453)	2016.2 (1.590)	2016.2 (1.582)
Mother's schooling years	2.316 (3.652)	2.432 (3.723)	2.502 (3.746)	2.600 (3.802)
Mother's age (years)	37.25 (6.323)	37.13 (5.918)	35.76 (6.020)	35.65 (5.923)
Proportion SC/ST	0.627 (0.484)	0.621 (0.485)	0.649 (0.477)	0.659 (0.474)
Agricultural land (ha)	1.118 (1.736)	1.092 (1.656)	1.165 (3.158)	1.029 (1.870)

Note: SC/ST = scheduled caste/scheduled tribe. Standard deviations are in parentheses.

5. Results: difference-in-difference specification

5.1 Main results

Table 3 provides results from the estimation of difference-in-difference equation (5) that pools outcomes for sons and daughters, reporting the coefficient on: the interaction of eligible cohorts (*elig*) with the treatment sample of early villages (*EV*); the individual components of this interaction; an indicator variable for sons; and the set of household and village controls previously described. The village controls are chosen to ensure common trends in the absence of the programme.

The last regression in this table uses these same covariates to estimate a logistic equation for the probability of treatment villages, and weights observations by the inverse probability weight estimated from this regression. This specification thus omits village-level controls. All three specifications reported in this table generate statistically insignificant coefficients on *elig * EV* of similar magnitude, suggesting no impact of access to SHGs on private school enrolment.

Though the full regression results reveal that the set of village-level controls are statistically significant (F statistic 5.37 [p-value 0.00]), their inclusion as either covariates or probability weights does not affect the regression results.

Table 3: Difference-in-difference basic regressions

	(1)	(2)	(3)
elig*EV	0.03	0.03	0.03
	(0.03)	(0.03)	(0.03)
Elig	0.04^{***}	0.04^{***}	0.04^{***}
	(0.01)	(0.01)	(0.01)
EV	0.02	0.00	0.02
	(0.02)	(0.02)	(0.02)
Son	0.02^{**}	0.02^{**}	0.02^{**}
	(0.01)	(0.01)	(0.01)
Controls	Household	Household + village	Household
Inverse probability weights	No	No	Yes
F for village covariates	--	5.37	--
		(0.00)	
F	27.64	22.83	27.64
N	3,946.00	3,946.00	,.00

Note: Robust standard errors are in parentheses. All regressions include the same set of household and village controls. Household controls: mother's schooling years, caste indicator, agricultural land owned, number of adult males and females in three age groups (20–40, 40–60 and above 60). Village controls (2011) are: indicator for population ≤1000, connected by 'pucca' road, availability of private primary school and interaction of the indicator for population size with the latter two variables.

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

Table 4 reports results from the same specification, run separately for sons and daughters. These results reveal that the pooled regression masks significant variability in the effects of the programme on sons relative to daughters. Sons' access to SHGs at a time when their school choices can be affected by this improvement in credit access significantly enhances the probability of private school enrolment. In contrast, the coefficient on the interaction term *elig x EV* is statistically indistinguishable from zero for daughters. The result is robust to the inclusion of block fixed effects.

Table 4: Difference-in-difference regressions by gender

	Sons	Daughters	Sons (block FE)	Daughters (block FE)
elig*ev	0.07**	-0.03	0.08**	-0.04
	(0.03)	(0.04)	(0.03)	(0.04)
Elig	0.05***	0.03*	0.05***	0.04**
	(0.02)	(0.02)	(0.02)	(0.02)
Ev	-0.03	0.05	-0.03	0.08**
	(0.03)	(0.03)	(0.03)	(0.03)
Fixed effects	State	State	Block	Block
F	13.63	10.92	7.44	5.81
N	2,077.00	1,869.00	2,077.00	1,869.00

Note: FE = fixed effects. Robust standard errors are in parentheses. All regressions include the same set of household and village controls. Household controls: mother's schooling years, caste indicator, agricultural land owned, number of adult males and females in three age groups (20–40, 40–60 and above 60). Village controls (2011) are: indicator for population ≤1,000, connected by 'pucca' road, availability of private primary school and interaction of the indicator for population size with the latter two variables.

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

These results thus suggest that a relative improvement in *women's* access to financial services exacerbates gender inequality in private school enrolment across sons and daughters – increasing enrolment for sons but with no similar effect on daughters. Calculated at sample means, the proportion of sons in ineligible cohorts of treatment (early) villages who ever attended private primary schools is 0.16, while the corresponding proportion for eligible younger cohorts in the same village is 0.29. Controlling for cohort differences using the control sample of late villages, our estimates suggest that private school enrolment for eligible children increased to 0.23, a 44 per cent increase.

5.2 Testing the common trend assumption

Table 5 presents results from the two regression specifications designed to test the assumption that, in the absence of the programme, trends between eligible and ineligible cohorts would have been the same in treatment and control samples. The first specification tests this assumption by comparing outcomes for the ineligible cohort of previous regressions, aged 9–13 at the time of SHG entry, and the next oldest cohort, aged 13–16 at the time of SHG entry. As previously discussed, this regression is restricted to the sample of sons.

This constitutes a test of trends across the two cohorts older than the eligible cohorts of the main regression specification. If the regression results revealed a difference in trends across early and late villages for these two cohorts, it would call into question the assumption of common trends for younger cohorts. The regression results, however, suggest no difference in trend across older cohorts in treatment and control samples.

Table 5: Difference-in-difference regressions: tests for common trends

	Pre-trends: ineligible and older cohorts	Sample: non-SHG households	
	Sons	Sons	Daughters
elig*ev	-0.02	-0.04	0.04
	(0.04)	(0.04)	(0.04)
Elig	--	0.09***	0.05*
		(0.03)	(0.03)
EV	0.04	0.03	-0.04
	(0.03)	(0.03)	(0.03)
Ineligible	0.00	--	--
	(0.02)		
F	9.29	7.59	4.39
N	1,724.00	1,006.00	930.00

Note: Robust standard errors in parentheses. The first regression is run on sons in ineligible cohorts (ages 9–13) at the time of SHG formation and the next oldest cohort (ages 13–16) at the time of SHG formation. Regressions in the last two columns are based on a sample of non-SHG members drawn from the same residential neighbourhoods as the main regression samples in previous tables. All regressions include the full set of household and village covariates as in previous tables, along with state fixed effects. Household controls: mother's schooling years, caste indicator, agricultural land owned, number of adult males and females in three age groups (20–40, 40–60 and above 60). Village controls (2011) are: indicator for population ≤1,000, connected by 'pucca' road, availability of private primary school and interaction of the indicator for population size with the latter two variables.
*p < 0.10, **p < 0.05, ***p < 0.01

Limited private school enrolment amongst older cohorts, however, restricts what can be learned from this regression. Additionally, rising trends in private school enrolment throughout this period suggest that evidence from older cohorts may not rule out the existence of differential trends across treatment and control villages for younger cohorts.

More convincing evidence comes from regression results in the last two columns of Table 5, based on a sample of non-SHG households drawn from the same residential neighbourhoods as the main regression sample. This regression provides evidence on private school enrolment for equivalent cohorts as in our main regression sample, drawn from the same set of treatment and control villages, but comprising a set of households who are not SHG members. It thus provides a stronger test of trends that would have existed across samples in the absence of the programme.

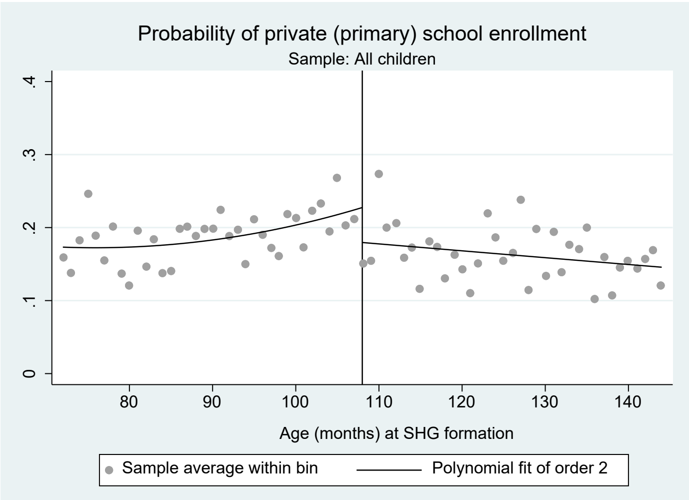
The regression results, reported separately for sons and daughters, suggest that the coefficient on the interaction term *elig * EV* is statistically insignificant for both samples. This supports our interpretation of the previous set of results as indicative of programme effects rather than confounding effects of other changes in the village.

6. Results: FRD specification

6.1 Graphical evidence

This section provides graphical evidence of a causal effect of SHGs on the probability of private school enrolment based on regression discontinuity plots. Following Gelman and Imbens (2019), the plots use local quadratic approximations, with bandwidths that span the full support of the data. Figure 7 plots results from the full sample, while Figures 8a and 8b present regressions based on sons and daughters, respectively. All plots are on the regression discontinuity samples previously described.

Figure 7: Probability of private (primary) school enrolment by age of child at SHG formation



For the full sample, Figure 7 reveals a clear discontinuity at the cut-off age of 108 months, with the probability of private schooling being higher for children younger than this cut-off – that is, for children young enough at the time of SHG formation to have schooling choices affected by the availability of new sources of credit. Figures 8a and 8b, however, reveal that this discontinuity derives from the sample of sons: there is no discontinuity in the relationship between the probability of private school enrolment and the daughter's age at the time of SHG formation. Thus, the graphical evidence suggests that improved access to credit for women affected schooling choices for sons, but not daughters.

We validate the regression discontinuity results by graphing similar plots for the full set of covariates used in the regression analysis (mother's schooling, proportion from scheduled castes and tribes, agricultural land ownership, number of adult males and females in the household, and the set of village covariates).

It is possible that the discontinuity observed in Figures 7, 8a and 8b reflects a non-linear relationship between private school enrolment and unobservable covariates that are correlated with the age of children. If so, we would expect a similar discontinuity in the relationship with observed covariates. Figure 9, which plots these regressions for a full set of nine covariates, suggests that this is not the case, validating the interpretation of the results in Figures 7 and 8 as indicative of the effect of SHG access for some cohorts but not others.

Figure 8: Probability of private (primary) school enrolment by age and gender of child at SHG formation

Figure 8a: Sons

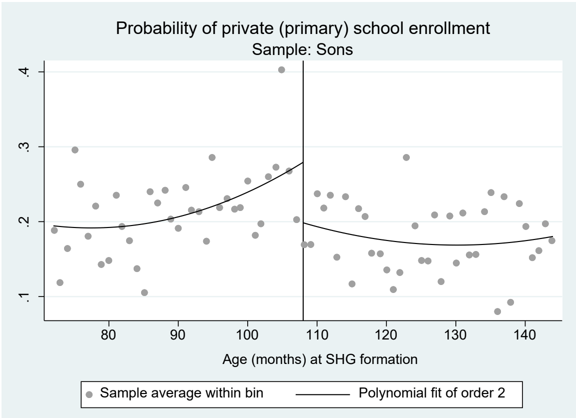


Figure 8b: Daughters

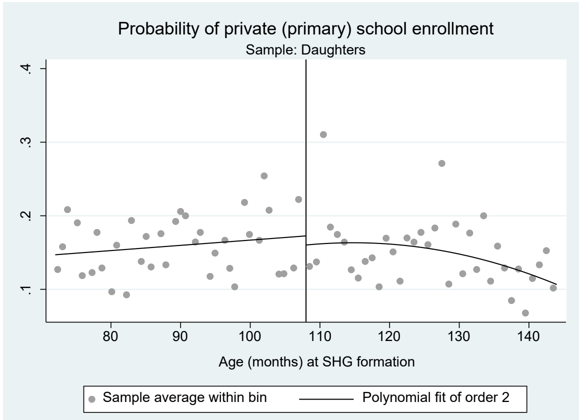
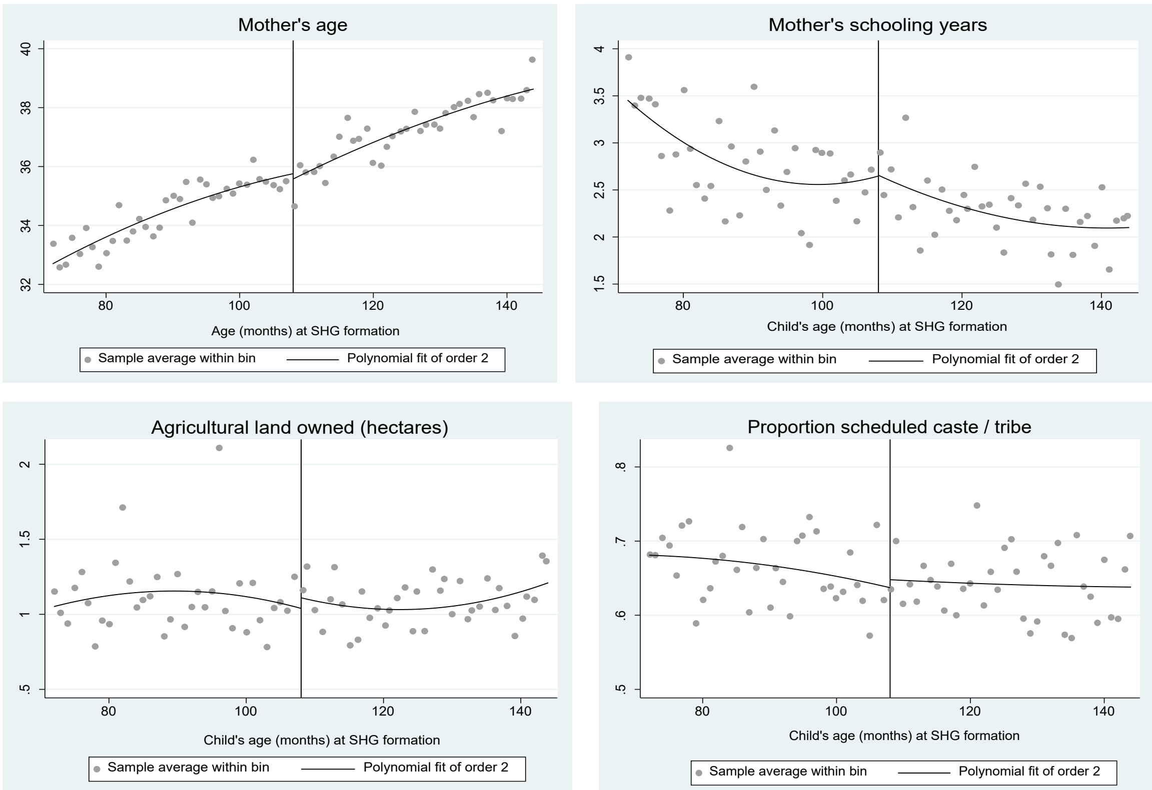
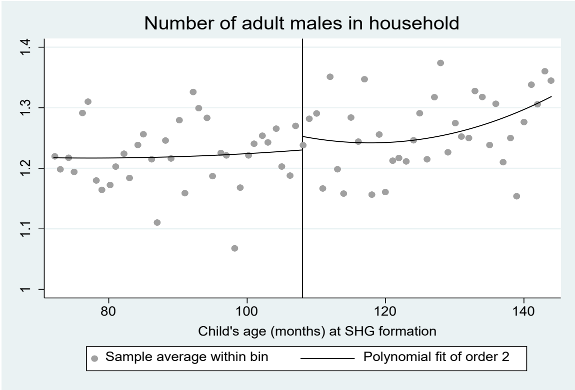
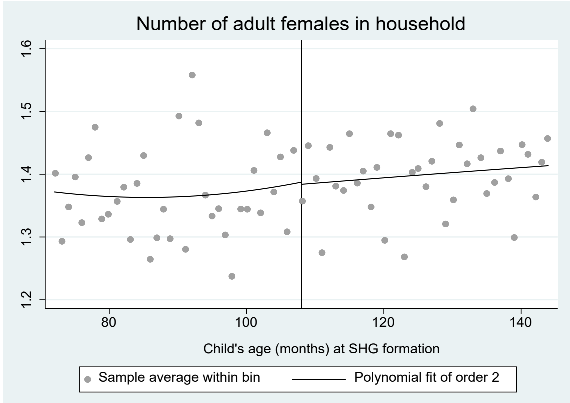
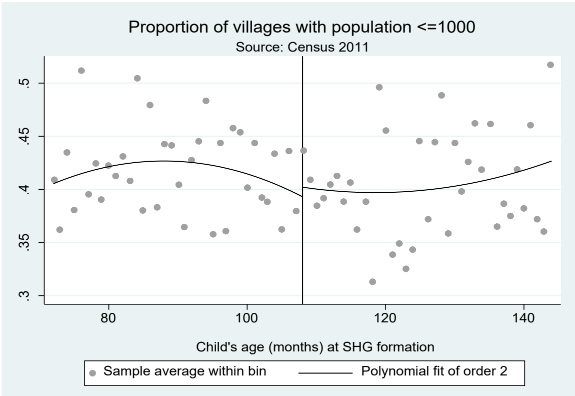


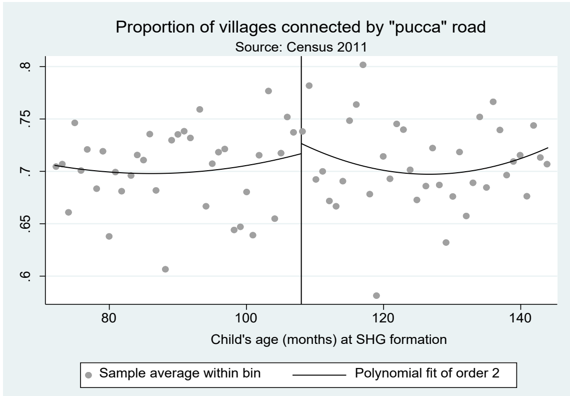
Figure 9: Regression discontinuity plots for covariates used in regression analysis

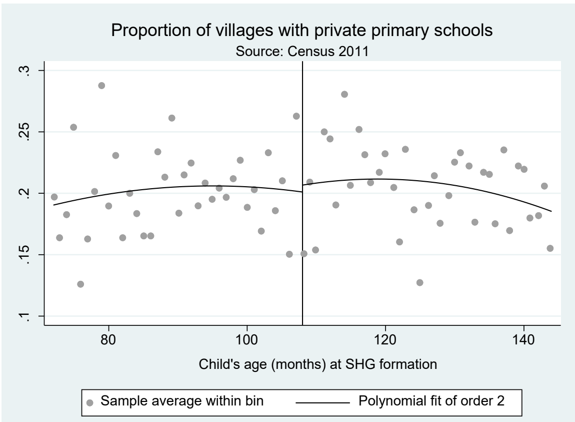












6.2 Regressions results from the FRD design

Results from the regressions that underlie the graphical evidence are presented in Table 6. These regressions are based on the same samples used for the graphical evidence; that is, children between the ages of 78–136 months at the time of SHG formation, further restricted to children who were 18 or younger at the time of the survey. The regressions use the same set of covariates used in difference-in-difference regressions, in addition to the running variable *ageatshg.*

Table 6: Regression discontinuity results

	Full sample	Full sample	Sons	Daughters
son	0.04^{***}	0.04^{***}		
	0.01	0.01		
elig	0.02	0.01	0.05^{**}	-0.01
	0.02	0.02	0.02	0.02
polynomial in ageatshg	Quadratic	Cubic	Quadratic	Quadratic
F	48.38	46.66	26.27	24.26
N	9,110.00	9,110.00	4,679.00	4,431.00

Note: Robust standard errors are in parentheses. All regressions are run on the regression discontinuity sample described in the paper (children between the ages of 72–144 months at the time of SHG formation, and further restricted to children between the ages of 6–18 at the time of the survey and to members of SHGs formed after 2012). The cut-off in this regression is 109 months (at SHG formation). Other covariates included in the regression are: mother's schooling years, indicator for households from scheduled castes and tribes, amount of agricultural land, number of adult males and females in three age groups (20–40, 40–60 and above 60), indicators for SHG formation year and state-level fixed effects.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

The first two regressions report results from the full sample (sons and daughters), with the gender of the child entered as a covariate. These regressions thus correspond to Figure 7, which is similarly based on the full sample. These regressions differ in that specification (1) uses a quadratic in the running variable, while specification (2) is based on a cubic. The results are invariant to this change in specification. Both regressions suggest a statistically insignificant effect for the pooled sample of an improvement in women's access to credit.

The next two columns in this table report results from regressions run separately for sons (regression 3) and daughters (regression 4). They confirm the results of the graphical analysis, which confirm that improvement in women's access to credit significantly enhances the chances of private schooling for sons, but not daughters. For sons, the effect is statistically significant at the 5 per cent level. In contrast, the magnitude of the coefficient at the cut-off is close to zero for daughters.

6.3 Robustness and falsification tests

Table 7 reports results from regressions that test the sensitivity of estimates to changes in sample definitions. As previously discussed, the first set of regressions (the first two result columns) reduce the minimum and maximum ages of children in the sample by six months. This is a significant reduction in sample size – by 926 for sons and 810 for daughters. This in turn reduces precision, so that the effect of mothers' access to SHGs is statistically insignificant for both sons and daughters. However, the magnitude of the coefficient for the sample of sons is unchanged, despite this significant change in sample.

The second set of results expands the regression sample by six months at the lower and upper bounds, so that it now includes children between the ages of 66–150 months at the time of SHG formation. This increases variance, but the magnitude of the coefficients is once again unchanged, attesting to the robustness of the results to different sample definitions.

Table 7: Regression discontinuity results: robustness to sample definitions

	78 ≤ *ageatshg* ≤ 136		**66 ≤ *ageatshg* ≤ 150**	
	Sons	**Daughters**	**Sons**	**Daughters**
elig	0.04	0.00	0.04*	0.00
	0.02	0.02	0.02	0.02
F	22.28	21.42	29.57	26.19
N	3,753.00	3,621.00	5,487.00	5,193.00

Note: Robust standard errors are in parentheses. In addition to covariates listed in Table 6, regressions include a quadratic in ageatshg.
*p < 0.10, **p < 0.05, ***p < 0.01

The results in Table 8 implement the falsification test earlier described, based on children between the ages of 12–16 years at the time of SHG formation, with samples defined to ensure that children of these ages are represented in the survey. We test for a discontinuity in the relationship between private school enrolment and age of the child at SHG formation at the midpoint of this sample (156 months).

The first two regressions report results from the subsamples of sons and daughters, respectively, and reveal no evidence of a discontinuity at this age. The third regression in this table extends the upper age limit of the sample of sons to those 21 years and younger at the time of the survey, revealing that this does not alter the results. There is no evidence of a discontinuity in the relationship between private school enrolment and the age of children at the time of SHG formation for older cohorts.

Table 8: Regression discontinuity results: cut-off at 13 years (156 months)

	Children aged 9–18, SHG year after 2016		**Sons aged 9–21, SHG year after 2013**
	Sons	**Daughters**	**Sons**
elig_13	0.01	-0.03	0.01
	0.03	0.03	0.02
F	13.85	10.85	17.02
N	2,458.00	2,370.00	4,032.00

Note: Robust standard errors are in parentheses. Additional covariates are those listed in Table 6, and a quadratic in ageatshg.
*p < 0.10, **p < 0.05, ***p < 0.01

7. Conclusions

This paper provides empirical evidence that questions the widespread assumption that son preference in patriarchal societies – as manifested in higher expenditure on sons relative to daughters in schooling and other outcomes – reflects the preferences of fathers and their greater bargaining power relative to mothers. This assumption underlies the belief that improvements in women's access to financial and other resources, and their subsequent intra-household bargaining power, will reduce gender inequality with regard to investments in children's human capital. It also forms one justification for the shift in programmes lending to women in the past decade.

Our empirical analysis, based on India's NRLP, compares private school enrolment for children of school-entry age during SHG formation with those of older children, whose schooling choices were completed prior to programme entry in the village and were therefore unaffected by expansion in women's access to credit. Controlling for cohort effects through two different methodologies, we provide robust evidence that improvements in women's access to credit increased private schooling of sons but not daughters. The fact that these different methodologies rely on samples that differ considerably in size and socio-economic conditions supports the credibility of our results.

We caution, however, that our analysis pertains to just one outcome: children's private school enrolment. Evaluating whether it can be generalised to other child-related outcomes and other settings remains a topic for future research. Additionally, we note the difficulties of addressing this question using retrospective data from a cross-sectional survey. Though our analysis addresses these concerns through careful consideration of regression samples, these shortcomings suggest the importance of validating the results with alternative datasets.

We conclude by emphasising that our results do *not* imply that investing in women constitutes bad policy or that there is no value to programmes that target women. A body of evidence from other studies provides robust evidence that improvements in women's economic positions significantly enhance women's agency, helping to empower them economically, socially and politically, and improve their standing in society and within their households. These are outcomes that are of significant value.

Instead, our results question assumptions regarding the nature of differences in the preferences of mothers and fathers for investments in sons and daughters, and the consequent impact of women's empowerment on gender differences in these investments. Contrary to the widespread belief that mothers prefer greater gender equality than fathers, our results suggest that the opposite may hold in patriarchal societies where income levels are such that sons represent a primary source of income for parents in old age and earlier. When sons shoulder the responsibility of caring for parents, it is highly likely that mothers benefit more from this relationship than fathers.

In such circumstances, the same set of policies that empower women of one generation may not generate externalities that similarly benefit women in younger generations. This suggests that redressing persistent gender inequalities requires more than improvements in women's access to financial and other economic resources. Programmes that enhance the ability to save for old age or that provide adequate pensions constitute one such measure. Alternatively, combining women's access to financial resources with training and mentorship that directly address the equal treatment of sons and daughters may have larger returns.

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