Direct and Spillover Effects of a Conditional Cash Transfer Program for the 'Girl Child' in Karnataka, India

Thesis submitted in partial fulfillment of the requirements for the Degree of Master of Science in Economics for Development at the University of Oxford

May 2023

By

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Word count: 9931

Acknowledgements

I would like to thank my dissertation supervisor Professor Christopher Woodruff for his valuable feedback and guidance at every stage. I would also like to express my gratitude to ASER Centre for sharing data for the empirical analysis. Finally, I would like to thank my family and peers for their support, encouragement, and advice throughout the dissertation process.

Abstract

This paper evaluates the causal impact of the *Bhagyalakshmi* scheme, a conditional cash transfer (CCT) program introduced in the Indian state of Karnataka in 2006. The ongoing program aims to reduce sex-selective abortions, child marriage among girls, and underinvestment in educational attainment for female children. The program is offered to girls born in or after 2006, and a lump sum payment is released once they have completed their schooling. I investigate the direct impact on eligible girls, and the spillover effect on siblings of eligible girls, namely the eldest daughters who are not eligible due to their year of birth. Specifically, I analyse effects on school enrolment and learning outcomes for these two groups. To isolate the treatment effect (which is an Intent to Treat or ITT estimate), I employ a modified difference-in-difference (DiD) estimation strategy by comparing the difference in outcomes between untreated and treated groups across time between Karnataka and control states. I investigate heterogeneity in outcomes based on parent education and proximity to local schools. There is no impact on enrolment for eligible girls overall, but there is some evidence of a significant positive impact for girls from disadvantaged households. Furthermore, I find evidence of significantly positive spillover impacts on eldest ineligible daughters, demonstrating that the positive income effects induced by the CCT outweigh potential negative displacement effects on siblings. The program has no significant impact on literacy and numeracy skills for either group. I contribute to the literature on CCTs in India by assessing a program which has not been evaluated empirically before, analysing impacts on learning levels, and investigating the spillover effects on ineligible siblings in the same household.

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I. Introduction

Access to education has long-been a policy priority in India, and governments have invested heavily in developing public education infrastructure since the 1980s (Banerji and Chavan 2016). As a result, the proportion of children in school is very high; since 2007, more than 95% of 6- to 14-year-olds have been enrolled in school every year (ASER 2018). The picture is less rosy when it comes to the quality of education; literacy and numeracy skills among children are worryingly low. In 2018, less than half of Grade 5 children in government schools could read a simple Grade 2-level text (in their mother tongue), while only 22.7% of these children could perform division (*ibid*). Even more concerning is that learning levels in rural India have been declining over the years despite rising expenditures. (*ibid*; Banerji and Chavan 2016). Another dimension is that of gender; the educational cost of poverty falls disproportionately on girls. Indian parents display differential treatment and girls are more likely to be pulled out of school when resources are scarce. This can be explained by lower economic returns of their education relative to boys', beliefs about the gender division of labour, early marriage, and safety concerns (Kingdon 2002; ASER 2017).

In this paper, I analyse the effects of the *'Bhagyalakshmi'* conditional cash transfer (CCT) program on enrolment rates and learning levels. This program, which has not been evaluated empirically before, was introduced in Karnataka (a state in South India) in 2006. The program is available to families below the poverty line and includes conditionalities such as completing the stated level of schooling. The literature on cash transfers finds that CCTs have a positive effect on enrolment, but there is mixed evidence of their impact on learning levels. Moreover, due to displacement effects, CCTs can sometimes adversely impact ineligible siblings (Fiszbein *et al*. 2009). I contribute to this literature by evaluating the direct effect on eligible girls, and the spillover effect on their ineligible eldest sisters. Given the large economic investment made by the Karnataka government annually into this program[1], it is important to understand whether it addresses the issue of low learning levels or has negative consequences on ineligible siblings.

Using data from the Annual Status of Education Reports (ASER), I conduct a difference-in-difference (DiD) analysis to isolate the causal impact by comparing outcomes across time between Karnataka and control states. I use a modified DiD strategy to ensure my results are robust to potential violations of parallel trends by doing a within-state comparison of untreated

[1] Between 2006 to 2019, the government spent INR 3,825 crores or USD 464 million on the *Bhagyalakshmi* program (Mukherjee 2019).

and treated groups in each year. I find that the program has insignificant effects on enrolment for eligible girls. Within this larger group, the effect is positive and significant for those girls from disadvantaged backgrounds, specifically those whose mothers did not attend school. More substantial is the significant positive spillover impact on enrolment for ineligible elder sisters. In accordance with the literature, there is no significant impact on learning levels.

The rest of the paper is organised as follows: Section II outlines the theoretical framework and associated literature on the mechanisms through which CCTs affect schooling outcomes, as well as some background on the *Bhagyalakshmi* program. Section III describes the data. Section IV lays out the empirical strategy, including evidence of parallel trends and the regression specification. Section V discusses the main results and heterogeneity. Section VI reports robustness checks and Section VII concludes.

II. Conditional Cash Transfers

A. Theoretical Framework and Evidence

Conditional cash transfers aim to achieve economic empowerment by transferring cash to poor households conditional on investments in child education and health (Fiszbein *et al.* 2009). The first empirical evaluation of a CCT was of Mexico's *'Progresa'* program in the 1990s, which found positive effects on schooling and was influential in the expansion of CCTs to over 50 countries (Ravallion 2019).

Conditional cash transfers can impact schooling outcomes through several channels. I explain the mechanisms by combining theoretical frameworks developed by Ferreira *et al.* (2009) and Behrman *et al.* (2011). Education has opportunity costs in the form of direct expenditures, or foregone earnings from work. Consequently, multi-child households below a certain income threshold enrol either none or only one of their children. The cash transfer subsidises the costs of schooling (e.g., by reducing the shadow wage), which leads to higher enrolment for the eligible child via the *substitution effect* (they are substituting away from work). In households that can only afford to enrol one child, the CCT causes a *displacement effect*, where eligible children replace their ineligible siblings, to meet the program conditionalities. If a household rises above the income threshold due to the CCT such that they can afford to send more than one sibling, they send the eligible child as well, which is the *income effect*. All three effects improve the educational outcomes for the eligible sibling. On the other hand, the impact on ineligible siblings is ambiguous. The displacement effect impacts them negatively, the income effect impacts them positively, and the substitution effect does not impact them at all, as their opportunity cost of schooling stays the same.

Since the seminal study on *'Progresa'*, a large body of literature has emerged across various developing countries confirming the positive impact of CCTs on school enrolment (Baird *et al*. 2010). Evidence on the impact on siblings is mixed, as predicted by the framework. For instance, Barrera-Osorio *et al.* (2011) and Camilo and Zuluaga (2022) find negative spillovers for ineligible siblings, while Ferreira *et al.* (2009) demonstrate that this group is largely unaffected. There is also a question of whether the channels described above apply to learning levels. Can the income, substitution, and displacement effects impact aspects such as parental investment in learning support, thus improving learning levels? There are fewer studies that focus on this, largely due to the lack of data on cognitive skills or test scores, but the existing empirical studies have not found any significant effect, even after accounting for selection into school (Fiszbein *et al.* 2009; Gaentzsch 2020).

Since the late 1990s, several state governments in India have introduced conditional cash transfer programs targeted at girls with the aim of improving sex ratios, improving education and health outcomes, and preventing child marriage (Sekher 2012). In the literature, CCTs in India have mostly been evaluated using DiD as the empirical strategy. Muralidharan and Prakash (2017) evaluate a "conditional kind program" in Bihar, where instead of cash, girls received bicycles. They find significantly positive impacts on enrolment as the program reduced time, distance, and safety costs of attending school. In contrast, Sinha and Yoong (2009) show that for a CCT in Haryana, girls were not more likely to attend school, but girls already enrolled in school were more likely to complete it. Their findings are complemented by Biswas and Das (2021) who show that this positive effect continues almost two decades after the program started. Jain (2018) finds positive effects of a Madhya Pradesh CCT on both enrolment and learning levels for eligible girls, but no effect on elder ineligible sisters. Thus, studies on CCTs in India have found mixed impacts on enrolment, and there is limited evidence on the effects on siblings or test scores. I aim to fill this gap by examining Karnataka's CCT.

B. The Bhagyalakshmi Program

The objective of the program is to address the issues of low sex ratio at birth, prevalence of child marriage, and the gender gap in access to education (Mukherjee 2019). As per the Census, Karnataka's sex ratio at birth was 960[2] in 1991, which further dropped to 946 in 2001, making it one of the poorest performing among the five southern Indian states (Census of India 1991 - 2011). In 2007-08, 47% of underage girls in the poorest wealth quintile were married (Jha e*t al.* 2019) and the state also witnessed persistently lower enrolment rates for girls in the 2000s (ASER 2018). In response, the *Bhagyalakshmi* scheme was introduced for families who were 'Below the Poverty Line' (BPL), or whose annual income was below INR 11,000[3] (~USD 133)[4] (Mukherjee 2019). The first two daughters born after 31st March 2006 are eligible for the program. The state issues a bond in the name of the girl when she enrols, which can be encashed when she turns 18. The scheme included annual scholarships, but this was removed for those registered under the program from 2008 onwards. In turn, the maturity amount was increased from INR 34,000 (~ 400 USD) to INR 1,00,000 (~1200 USD) (Jha *et al*. 2019). The latter amounts to between 500-900% of the target families' annual income (Mukherjee 2019). The bond is only accessible if the beneficiary meets the conditions, which include completion of schooling until a certain grade, that she does not engage in child labour and that she is not married before the age of 18. The program also restricts the number of total children a family can have[5], and previously required the parents to undergo a terminal family planning treatment. However, evaluations reveal that the fertility restrictions were not enforced strictly until 2016, and most beneficiaries did not meet these requirements (*ibid*).

Take-up of the program was high between 2006 to 2011 when a BPL card was not needed to prove eligibility status. Jha *et al*. (2019) report that 84% of all girls born in 2009-10 in Karnataka were enrolled in the program. After 2011, while overall enrolment dropped, the percentage of BPL households enrolled was between 60-90% annually (*ibid*). This forms a significant portion of the population: 52% of Karnataka's rural population possessed a BPL card in 2013-14, which increased to 72% by 2016-17 (Government of Karnataka 2014; 2017).

III. Data

I utilise data from the eight rounds of the Annual Status of Education Report (ASER) Survey undertaken between 2009 to 2018[6] (ASER Centre 2009-2018). ASER is a national rural household survey in India that collects data on children aged 3 to 16. It is representative at the district level; 30 villages per district are selected, and 20 random households per village are

[2] 960 females for every 1000 males
[3] INR 17,000 in urban areas
[4] At 2023 exchange rates
[5] No more than three children from 2006 to 2014; no more than two children from 2014 onwards
[6] 2009, 2010, 2011, 2012, 2013, 2014, 2016 and 2018

surveyed. This ensures that children who are not in ‘traditional’ school are assessed, and data on household-level characteristics are available. In addition to information on schooling status, assessment data on children’s foundational literacy and numeracy skills are collected. ASER’s internationally recognised testing tool is designed to capture the highest level that each child can comfortably achieve, rather than testing grade-level competencies. Both tests are in the child’s mother tongue, with references contextualised to the local setting. Tests are done one-on-one between the surveyor and child at home and is adaptive to the child’s ability, so they do not have to attempt all levels. The Reading test has four progressive tasks: (i) letters (ii) words (iii) paragraph (Grade 1 level text) and (iv) story (Grade 2 level text). Numeracy skills are assessed using a simple arithmetic tool, which also has four progressive tasks: (i) 1-digit number recognition (1 to 9) (ii) 2-digit number recognition (10 to 99) (iii) subtraction (2-digit) (iii) division (3-digit by 1-digit)[7].

A limitation of this data is that it does not contain indicators of enrolment into the program or BPL card possession. As mentioned in Section II, take-up of the program was high among the entire population, but I restrict the sample to match eligibility by taking those families that have less than 5 children[8], and those households whose homes are not fully made of *‘pakka’* material[9], which indicates low levels of income.

Eligible children born in or after 2006 enter Grade 1 in 2011. Thus, 2009 and 2010 form the pre-treatment years and 2011 to 2018 are post-treatment years. The first outcome of interest is school enrolment, which is a binary variable indicating whether a child is currently enrolled (one) or was never enrolled/has dropped out (zero). Maths and Reading levels are measured from a scale of zero to four (inclusive) indicating the five progressive levels measured through the ASER assessment tools.

IV. Empirical Strategy

A. Identifying Assumptions

To isolate the effect of the program, I use two sources of variation: (i) the eligibility criterion, as the program restricts access to girls born in or after 2006 and (ii) similar states who did not

[7] The testing tools can be viewed on the ASER website: https://www.asercentre.org/

[8] Eligibility was restricted to families with three, and later two children, but as mentioned, this was not strictly enforced. I nonetheless trim extreme values i.e., those families that have more than 4 children and hence are unlikely to meet the criteria.

[9] *‘Pakka’* material refers to bricks, stones, cement, concrete, timber, tiles, iron sheets. Houses that are not *‘pakka’* can be made of bamboos, mud, grass, reeds, thatch, etc.

have a conditional cash transfer program for girls. Specifically, I employ a difference-in-difference (DiD) framework where I form 'treated' and 'untreated' groups based on the eligibility criterion (following Duflo 2011), which are described in Table 1.

Table 1 – Treated and Control Cohorts for each Group of Interest

	Measured Impact	Eligible	Non-Eligible Cohort
Group 1	Direct effect on eligible girls	Girls born in or after 2006 who do not have sisters	Girls born before 2006 who do not have sisters
Group 2	Spillover effect on ineligible elder sisters (born before 2006)	First-born girls (born before 2006) who have younger sisters, all of whom were born in or after 2006	First-born girls (born before 2006) who have younger sisters, all of whom were born before 2006

Note that for Group 2, the "eligible" group members are not themselves eligible, but they have eligible younger siblings, and hence for consistency, are referred to as "eligible". Henceforth, when I use the term "non-eligible" I am referring to the non-eligible (untreated) groups described in the third column of Table 1. The samples were constructed to be comparable and to disentangle spillovers. For instance, if girls who did have sisters were included in Group 1, their outcomes may have been influenced by the eligibility of their sisters. Similarly, outcomes for later daughters may be systematically different from eldest daughters, and hence only eldest daughters are considered in Group 2[10].

In this setup, it is *not* the case that the same population who were not treated in the pre-periods receive the intervention and become treated in the post periods. Rather there is a separate 'untreated' (non-eligible) cohort, and a 'treated' (eligible) cohort, and the latter appears in the data from 2011 onwards. The treatment effect is the *excess* difference between the two cohorts that exists in Karnataka due to the program. The control state helps remove the pre-existing differences between the two groups and construct a valid counterfactual for the eligible group in Karnataka. The difference between observed and counterfactual outcomes gives the causal impact of the program. Specifically, it measures Intent to Treat (ITT), by modelling the potential exposure of the selected population to the program. I use a potential outcomes framework (Rubin 2005) to describe the traditional DiD set up in a two-period model.

[10] An issue with the data is that there is only information on children aged 3 to 16. There may be 'invisible' siblings younger than 3 or older than 16 which are not observed. I control for total family size (which counts all children regardless of their age) but acknowledge that there may be errors in the constructed groups.

The outcomes are displayed graphically in Panel A, Figure 1. For control state a, the outcome y for non-eligible group 0, in time $t-1$ is:

$$y_{0at-1} = \mu_0 + \alpha_a + \epsilon_{0at-1}$$

For control state a, the outcome y for eligible group 1, in time t is:

$$y_{1at} = \mu_1 + \alpha_1 + \gamma_t + \epsilon_{1at}$$ [11]

The difference is: $\Delta y_{at} = y_{1at} - y_{0at-1} = \mu_1 - \mu_0 + \gamma_t + \epsilon_{1at} - \epsilon_{0at-1}$

For Karnataka, which is state b, the potential outcomes for the two groups are:

$$y_{0bt-1} = \mu_0 + \alpha_b + \epsilon_{0bt-1}$$

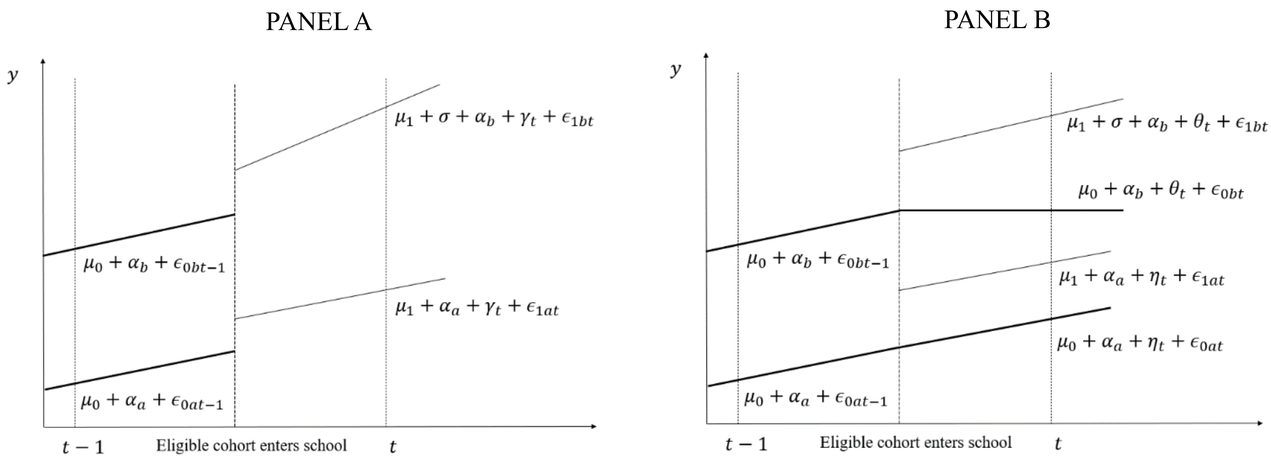
$$y_{1bt} = \mu_1 + \sigma + \alpha_b + \gamma_t + \epsilon_{1bt}$$

$$\Delta y_{bt} = y_{1bt} - y_{0bt-1} = \mu_1 + \sigma + \gamma_t - \mu_0 + \epsilon_{1bt} - \epsilon_{0bt-1}$$

The DiD estimator isolates the treatment effect (σ):

$$E[\Delta y_{bt} - \Delta y_{at}] = (\mu_1 + \sigma + \gamma_t - \mu_0) - (\mu_1 - \mu_0 + \gamma_t) = \sigma$$

Figure 1: Hypothetical Trends of Potential Outcomes



The main identifying assumption here is that because trends are parallel between the two states before the program, trends would have been parallel after the program (they have a common time trend γ_t), and the only difference between the two states in period t is the treatment effect σ. There is no way to verify this assumption as traditionally, the untreated group becomes treated, and hence, there is no data available on untreated individuals in post-periods. However,

[11] Here, μ is the mean of the outcome, and γ_t is the time trend.

this approach may not be sufficient. Roth *et al.* (2023) conclude that testing for differences in pre-trends does not imply that parallel trends hold in post-years, as there may be unobserved confounding factors. In my case, I also have data on the non-eligible (untreated) group in post-years as they are a separate group, and I therefore take the difference between the two groups in post-years. This strategy allows me to control for state-specific shocks that may violate parallel trends. The modified strategy is described below, and the outcomes are displayed graphically in Panel B, Figure 1. For control state a, the outcome y for groups 0 (non-eligible) and 1 (eligible), in time t is:

$$y_{0at} = \mu_0 + \alpha_a + \eta_t + \epsilon_{0at}$$

$$y_{1at} = \mu_1 + \alpha_a + \eta_t + \epsilon_{1at}$$

$$\Delta y_{at} = y_{1at} - y_{0at} = \mu_1 - \mu_0 + \epsilon_{1at} - \epsilon_{0at}$$

For Karnataka, which is state b, the potential outcomes for the two groups are:

$$y_{0bt} = \mu_0 + \alpha_b + \theta_t + \epsilon_{0bt}$$

$$y_{1bt} = \mu_1 + \sigma + \alpha_b + \theta_t + \epsilon_{1bt}$$

$$\Delta y_{bt} = y_{1bt} - y_{0bt} = \mu_1 + \sigma - \mu_0 + \epsilon_{1bt} - \epsilon_{0bt}$$

$$E[\Delta y_{bt} - \Delta y_{at}] = (\mu_1 + \sigma - \mu_0) - (\mu_1 - \mu_0) = \sigma$$ [12]

The DiD estimator isolates the treatment effect (σ). Here, while trends are parallel in pre-years, state-specific shocks cause both states to trend differently in time t (the time trend for state a is η_t, while the time trend for state b is θ_t). The effect of state-specific confounders is removed by using data on the untreated group in post-years. A counterfactual for the eligible group in Karnataka is constructed that takes the trend difference into account and correctly isolates the difference that is caused by the treatment (and not the confounder). The threat to this strategy is if unobserved factors unrelated to the program affect only the eligible group. I discuss potential sources of unobserved confounders, and the direction of bias in Section V.

[12] This is the difference of the difference between states (the DiD estimator)

B. Parallel Trends

While I am using a modified DiD strategy to account for potential state-specific shocks, I select control states that have parallel trends for the non-eligible group in all years (including post-years), to limit the impact of unobserved confounders. Parallel trends are tested by first comparing means across time graphically. The trends are presented in Figures 2 and 3. Note that here, while the program comes into effect in 2011 (as the eligible cohort enters school that year), the outcomes are of the non-eligible group (who are untreated) and for whom it would be preferable for trends to be parallel in all years.

Figure 2: Non-eligible group trends in Karnataka and Control States for Group 1[13]

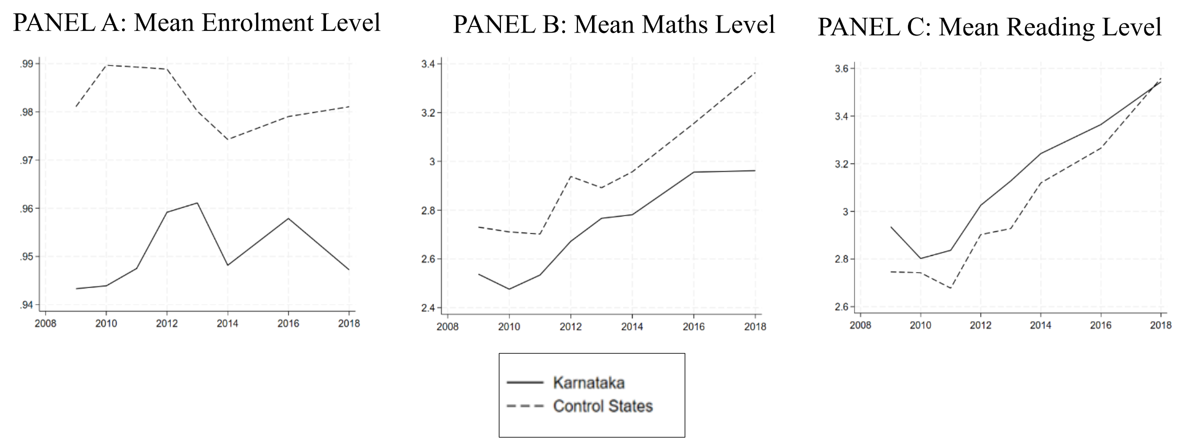
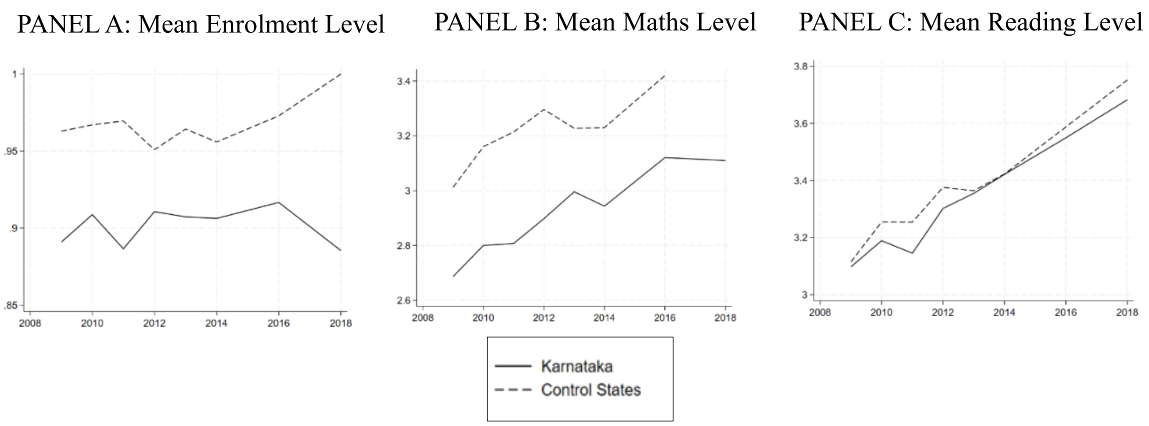


Figure 3: Non-eligible group trends in Karnataka and Control States for Group 2[14]



The trends appear broadly parallel, but at times converge or diverge. As these graphs present a simple comparison of means, I conduct a regression on the selected states by estimating

[13] The control states for school enrolment are Tamil Nadu, Himachal Pradesh, and Kerala, for Maths are Tamil Nadu, and Andhra Pradesh-Telangana, and for Reading are Tamil Nadu, Andhra Pradesh-Telangana, and Jharkhand.
[14] The control states for school enrolment are Tamil Nadu and Himachal Pradesh, for Maths are Tamil Nadu and Andhra Pradesh-Telangana, and for Reading are Tamil Nadu, Himachal Pradesh, Jharkhand, and Andhra Pradesh-Telangana.

differences in outcomes for the non-eligible groups across time. For a non-eligible child i living in state s in year t, I adopt the following DiD regression framework:

$$(1)\quad Y_{ist} = \beta_0 + \beta_1(KN_s \times Treat_t) + \beta_2 Treat_t + \beta_3 KN_s + \theta_t + X_i'\delta + \epsilon_{ist}$$

Y_{ist} is the outcome variable of interest; KN_s is a dummy variable indicating if the observation is in Karnataka; $Treat_t$ is a dummy that takes the value of one if $t > 2009$; θ_t is year fixed effects; X_i' is a vector of basic covariates of child i's age and whether their mother attended school.

Standard Errors: Note that for these regressions and all subsequent ones, I cluster standard errors at the level of treatment assignment (Abadie *et al.* 2017). Consequently, errors are clustered at the state level, and not state-year to avoid any potential issues of serial correlation (Angrist and Pischke 2008; Bertrand *et al.* 2004). However, in this setting with a small number of clusters, cluster-robust standard errors (which have an asymptotic justification) give incorrect inferences (Roth *et al.* 2023). A popular solution to this issue is using the wild cluster bootstrap (Cameron *et al.* 2008) but MacKinnon and Webb (2018) have shown that this method fails in settings with only a few treated clusters, such as DiD. They find that in this case, a subcluster wild bootstrap procedure performs better. The bootstrap data generating process (DGP) clusters at a finer level, such as the individual or district (while the main specification continues to cluster at the state level). In their simulations, they find that a DGP at the individual level (ordinary wild bootstrap) works well even when the number of clusters is very small, and only one cluster is treated. A key condition is that clusters should be equal in size; if not, the ordinary wild bootstrap can over-reject the null hypothesis. In my case, Karnataka has a greater number of observations than some of the control states. I therefore also sub-cluster at an intermediate level (district), which performs well when cluster sizes vary. Overall, I conduct inference based on wild bootstrapped p-values which are sub-clustered at both the individual and district level. I do not report standard errors for the bootstrap procedure, as they cannot be used for inference (which is done using p-values and confidence sets) (Roodman *et al.,* 2018) but rather present standard errors clustered at the state level.

Table 2 depicts the size and significance of β_1 for all 9 outcomes. There are no significant differences between states for the non-eligible cohort, averaged across time. To reinforce these findings by investigating year-level fluctuations, I replace the $KN_s \times Treat_t$ term in Specification (1) with interactions of KN_s and year dummies and conduct a joint F-test for each of the interaction coefficients. The test yields insignificant results for all regressions. I report these in the Addendum (Table A1). I conclude that trends are parallel between Karnataka and the control states for the non-eligible group.

Table 2 - Parallel Trends for the Non-Eligible Group

Dependent Variables	Enrolment	Maths Level	Reading Level
Panel A: Group 1			
KN* × *Treat	**0.003**	**−0.003**	**−0.075**
	(0.002)	(0.014)	(0.071)
	[0.708]	[0.924]	[0.639]
	{0.693}	{0.930}	{0.628}
Observations	40,045	37,443	54,455
R Squared	0.048	0.323	0.338
Panel B: Group 2			
KN* × *Treat	**0.013**	**−0.048**	**−0.059**
	(0.002)	(0.050)	(0.061)
	[0.381]	[0.754]	[0.687]
	{0.378}	{0.704}	{0.693}
Observations	13,263	13,369	23,829
R Squared	0.075	0.194	0.178

Notes: This table reports results from Specification (1). Each cell corresponds to a different regression and displays the coefficient on the state-treat interaction term, with the panels representing the different groups of interest. Robust standard errors clustered at the state level are in parentheses, p-values from the wild bootstrap procedure sub-clustered at the district level are in brackets, and p-values from the ordinary wild bootstrap are in braces. The regression is weighted to be representative at the state level and contains year fixed effects. Covariates include a treatment dummy, Karnataka dummy, child's age, and mother's education.

C. Regression Specification

To estimate the causal impact of the program on child i, belonging to cohort c, state s, and surveyed in year t, I adopt the following DiD framework:

$$(2)\ Y_{icst} = \beta_0 + \beta_1(KN_s \times Elig_c) + \beta_2 Elig_c + \beta_3 KN_s + X_i'\delta + \theta_t + (\phi_s \times t) + (\tau_c \times t) + \epsilon_{icst}$$

Here, Y_{icst} is the outcome variable. The estimator of interest (β_1), is the coefficient to the interaction between KN_s (a dummy indicating if the observation is in Karnataka) and $Elig_c$ (a dummy indicating if the observation is in the eligible or non-eligible cohort). X_i is a vector of covariates that includes child i's demographic, household, and village-level characteristics. The specification includes year fixed effects (θ_t). Testing for parallel trends may not be sufficient due to unobserved confounders or low power (Roth *et al.* 2023). The graphical comparisons of means (Figures 2 and 3) help strengthen the case, but Bilinski and Hatfield (2020) posit that the default DiD estimation equation should always allow for linear trend differences. I therefore control for state-specific linear trends ($\phi_s \times t$), and cohort-specific linear trends ($\tau_c \times t$). This is feasible as there are enough time periods (more than three) to pick up the trend (Angrist and Pischke 2008). With this specification, β_1 isolates the treatment effect[15].

The outcome variables are school enrolment, Maths and Reading levels. I adopt a linear probability model for the DiD estimation of these non-continuous variables, following Muralidharan and Prakash (2017), Anukriti (2018) and von Haaren and Klonner (2021). I also tried to estimate the effects of the program on the brothers of eligible girls, to disentangle the difference in spillovers based on gender. However, at various stages of the analysis, including via robustness checks, it appeared that the states do not follow parallel trends across all years. Thus, I dropped this group from the analysis in the interest of space.

V. Results

A. Direct and Spillover Effects

I first estimate the direct effect of the program on Group 1. As per the theoretical framework, the eligible child should unambiguously experience higher enrolment. Maths and Reading are non-targeted outcomes and represent whether the program also improved learning levels. Table 3 shows results from Specification (2) for Group 1. Each column represents a layer of additional variables. All the results are positive and insignificant. The addition of linear trends does not impact the coefficients by much, but the addition of controls reduces their magnitudes. This implies that some of the variation in the first two columns can be attributed to the covariates.[16] Therefore, I interpret results from column (3).

The point estimate indicates that the proportion of the eligible cohort enrolled in school in Karnataka increases by 2.5 percentage points (δ) relative to if the program had not occurred. In comparative terms, that is an increase of 2.75%. I arrive at this figure by taking the coefficient on the cohort dummy, which is -0.041 ($\mu_1 - \mu_0$), implying that in control states, the eligible group is less likely to be enrolled by 4.1 percentage points. I combine this with the treatment effect ($\mu_1 - \mu_0 + \delta$) and find that the eligible group in Karnataka is 1.6 percentage points less likely to be enrolled than the non-eligible group. The proportion of the non-eligible group in Karnataka enrolled in school is 95.05%. Without the program, the eligible cohort would have had a 90.97% probability of being in school, but now has a 93.47% probability,

[15] Note that for Group 2, the treatment effect is estimated from 2009 itself, as some siblings of eligible girls are already in school by 2009.

[16] I check whether this change is driven by the addition of control variables or the reduction in sample size (as the variables have missing observations) by conducting the regression on the same sub-sample as in column (3), but without the control variables included. In most cases, the coefficient is higher in magnitude indicating that it is the covariates that are accounting for the variation.

which is a 2.75% increase. I tentatively conclude that the program had a positive effect, but this was insignificant. Moreover, the mean enrolment rates are high; changes are occurring in the tails of the distribution, making the outcome more sensitive to small errors in measurement.

Table 3- Program Effects: Group 1

	(1)	(2)	(3)
Panel A: Enrolment			
KN* × *Eligible	**0.034**	**0.019**	**0.025**
	(0.009)	(0.007)	(0.006)
	[0.263]	[0.293]	[0.227]
	{0.181}	{0.248}	{0.194}
Eligible	0.006	0.007	−0.041
KN	−0.036	−0.011	−0.007
Observations	49,165	49,165	27,455
R Squared	0.012	0.015	0.042
Panel B: Maths Level			
KN* × *Eligible	**0.158**	**0.112**	**0.050**
	(0.070)	(0.026)	(0.029)
	[0.535]	[0.180]	[0.479]
	{0.429}	{0.181}	{0.507}
Eligible	−1.462	−0.968	0.150
KN	−0.242	−0.318	−0.246
Observations	46,306	46,306	27,273
R Squared	0.146	0.156	0.418
Panel C: Reading Level			
KN* × *Eligible	**0.119**	**0.073**	**0.001**
	(0.066)	(0.017)	(0.021)
	[0.445]	[0.135]	[0.984]
	{0.463}	{0.109}	{0.992}
Eligible	−1.767	−1.259	0.099
KN	0.018	0.354	0.097
Observations	69,123	69,123	40,321
R Squared	0.167	0.189	0.447
Linear trends	No	Yes	Yes
Socio-economic controls	No	No	Yes

Notes: This table reports results from Specification (2) and each cell reports the coefficient on key variables. Robust standard errors clustered at the state level are reported in parentheses, p-values from the wild bootstrap procedure sub-clustered at the district level are in brackets, and p-values from the ordinary wild bootstrap are in braces. The regressions are weighted to be representative at the state level and contain year fixed effects. Linear trends include state-specific and cohort-specific time trends. Socio-economic controls consist of child age, mother's schooling status, number of children in the family, and whether the household has electricity, a TV, a toilet, and a vehicle. Village level controls include the presence of electricity, a 'pakka' road, a bank, schools, health clinics, and a preschool.

From Panels B and C, the eligible group is 5 percentage points more likely to score one level higher in Maths than without the program, and 0.1 percentage points more likely to in Reading. However, these results are insignificant. I conclude that the program did not impact learning outcomes. Note that the results include children who are out of school and are not impacted by the selection of children into school.

Table 4- Program Effects: Group 2

	(1)	(2)	(3)
Panel A: Enrolment			
KN* × *Eligible	**0.037***	**0.039****	**0.038****
	(0.002)	(0.002)	(0.002)
	[0.049]	[0.018]	[0.016]
	{0.053}	{0.015}	{0.019}
Eligible	0.023	0.016	−0.056
KN	−0.062	−0.112	−0.101
Observations	16,892	16,892	9,280
R Squared	0.018	0.020	0.078
Panel B: Maths Level			
KN* × *Eligible	**0.190***	**0.059**	**-0.000**
	(0.013)	(0.013)	(0.039)
	[0.056]	[0.216]	[0.999]
	{0.048}	{0.178}	{0.999}
Eligible	−0.877	−1.227	−0.495
KN	−0.410	−0.282	−0.216
Observations	17,058	17,058	9,300
R Squared	0.102	0.118	0.250
Panel C: Reading Level			
KN* × *Eligible	**0.198****	**0.039**	**0.035**
	(0.024)	(0.023)	(0.042)
	[0.038]	[0.491]	[0.741]
	{0.036}	{0.490}	{0.726}
Eligible	−0.973	−1.315	−0.555
KN	−0.141	−0.437	−0.322
Observations	31,258	31,258	16,785
R Squared	0.088	0.117	0.243
Linear trends	No	Yes	Yes
Socio-economic controls	No	No	Yes

Notes: This table reports results from Specification (2) and each cell reports the coefficient on key variables. Robust standard errors clustered at the state level are in parentheses, p-values from the wild bootstrap procedure sub-clustered at the district level are in brackets, and p-values from the ordinary wild bootstrap are in braces.

The results for Group 2 are presented in Table 4. The program had a significant positive effect on enrolment for elder sisters. The significance and magnitude are robust to the inclusion of linear trends and covariates and for the sub-cluster bootstrap at both the individual and district levels. Eldest daughters are 3.8 percentage points more likely to be enrolled than if the program had not occurred. The coefficient on the eligible dummy is -0.056 i.e., the treatment group is 5.6 percentage points less likely to be enrolled than the untreated group in the control states. This might seem surprising given that the treatment group is systematically younger, and enrolment rates are higher for younger children (ASER 2018), but this effect only emerges when I control for child's age. This implies that the difference is the 'true' gap between the cohorts. The enrolment rate for the eligible cohort in Karnataka without the program is 84.5% and the program leads to a 4.5% increase in the probability of being in school. Overall, the program results in positive and significant spillover effects on the eldest sister. As per the theoretical framework, the income effect dominates the displacement effect.

The second result is the effect on Maths levels. The addition of linear trends, and then control variables, reduces the point estimate to negligible, and the results become insignificant, implying that the significant difference seen in column (1) was driven by trend differences and socio-economic characteristics[17]. Similarly, there is no effect of the program on literacy levels, as the coefficients are insignificant in all columns except the first. Thus, the program has no effect on numeracy or literacy outcomes for elder sisters.

B. Discussion

Why do eldest ineligible daughters experience a significantly positive impact on enrolment, but eligible girls do not? A potential explanation is that enrolment has become universalised, and hence there is not much of an extensive margin effect on the eligible girls due to their already high enrolment rates. The counterfactual enrolment rate for Group 1 in Karnataka is 90.97%. In contrast, the counterfactual enrolment rate for Group 2 is 84.5%, and there is more of a gap that the CCT can address. Beyond the channel outlined in the theoretical framework (i.e., the income effect) it is also possible that the program's emphasis on protecting 'the girl child', investing in her health and future, and not getting her married early improves perceptions. The change in norms would affect all girls in the household. This cannot be said with certainty as evidence points to there being no impact of 'girl child' CCTs on gender norms

[17] I again check whether the results in this table are driven by the reduction of the sample or the addition of control variables. I conduct the analysis without controls on the same subset of observations as column (3) and find that it is specifically the control variables that are impacting the effect size.

in India in terms of labour force participation, bargaining power, or fertility decisions (Das and Biswas 2021; Anukriti 2018).

A threat to the interpretation of these results is that the gap between the eligible and non-eligible groups in the control states is not indicative of what the gap would have been in Karnataka without the program. The trends between the non-eligible groups are parallel, and hence a confounder that affects only the eligible group would bias the results. The primary difference between the eligible and non-eligible groups is their year of birth (having controlled for many observable characteristics). Even though within a particular year, the eligible cohort is systematically younger than the non-eligible group, it is unlikely that an external factor affects only the schooling outcomes of the eligible group (such as a program targeting primary school students). This is because there is a mix of eligible and non-eligible children in the same level of school, due to the differential age compositions typically seen in the Indian education system. Children follow different pathways through school due to late or early enrolment, and periods of non-participation (Kaul *et al.* 2017; ASER 2018). So, any unobserved factors affecting specific grades impact both groups.

In India, there is evidence that families change their fertility preferences in response to CCTs that target sex-selective abortion (Anukriti 2018). The results would be biased if families in Karnataka that have an inherent son-preference self-select into the program by having another daughter. This could happen if the program most successfully targets families with gender biases. For Group 1, the control states' eligible group would no longer be a valid counterfactual as it contains families that chose to have a daughter without monetary incentives. In the absence of the program, the families in Karnataka either would not have a daughter, or their daughter would have worse outcomes than that modelled by the control states, in which case the result is an underestimate of the true treatment effect. Likewise, for Group 2, the outcomes for the existing daughter might have been worse than that modelled by the control states, as these families prefer sons. The reverse also applies. If the program is only effective for families that have less son preference, and these families self-select into the program, then the outcomes for both groups would be better without the program than modelled by the control states. In this case, the estimated effect would be biased upwards.

I address this issue by looking at the difference in means of family composition across states and groups. The results are reported in the Addendum (Table A2). For Group 1, families with eligible daughters in Karnataka have significantly more sons on average than their counterparts in the south Indian control states, but less sons than families in the north Indian states. This could just represent regional variation in sex-ratios[18]. However, this trend does not uphold for non-eligible families who have more similar or a smaller number of sons relative to the control states. So, there is some indication of son-preference among eligible Group 1 families. For Group 2, the patterns are the same: the average in Karnataka is higher relative to the south Indian states, but lower relative to the north Indian states. More reassuring is that this trend also holds for the non-eligible group and is more likely driven by regional variations. And so, there is less evidence of son-preference for Group 2, which is intuitively appealing- these families already chose to have a daughter. Nonetheless, the evidence of son preference in Group 1 suggests that the results may be an underestimate of the true treatment effect.

C. Heterogeneity

I analyse the heterogeneity of the results across two dimensions. The first is whether the child's village has at least one school and the second is whether the child's mother attended school. To do this, I divide the sample across the stated dimensions and generate the treatment effect as per Specification (2). Table 5 presents the results for Group 1.

Table 5- Heterogeneity Analysis, Group 1

Dependent Variables	Enrolment	Maths Level	Reading Level
Panel A1: School Yes			
KN* × *Eligible	**0.023**	**0.049**	**0.000**
	[0.316]	[0.501]	[0.996]
Observations	28,576	28,623	42,569
Panel A2: School No			
KN* × *Eligible	**0.001**	**−0.155**	**0.110**
	[0.982]	[0.498]	[0.193]
Observations	1,004	571	1,024
Panel B1: Mother School Yes			
KN* × *Eligible	**0.007**	**0.066**	**0.028**
	[0.487]	[0.232]	[0.728]
Observations	18,043	15,450	19,830
Panel B2: Mother School No			
KN* × *Eligible	**0.028****	**−0.007**	**−0.017**
	[0.035]	[0.896]	[0.854]
Observations	9,547	12,051	20,901

Notes: This table reports results from Specification (2) for Group 1. Each cell reports the coefficient on the Karnataka-Eligible interaction term. Each column represents a specific outcome variable. The panels depict the level of heterogeneity. Panel A varies whether a village has a school. Panel B varies whether the child's mother went to school. p-values from the wild bootstrap procedure sub-clustered at the district level are in brackets.

[18] South India has better sex ratios than north India and Karnataka historically performs poorly among south Indian states (Census of India 1991 - 2011)

The presence of a school in the village increases the magnitude of the enrolment impact (Panels A1 and A2). Even more pronounced is the change in Maths levels, which goes from highly negative (−15.5 percentage points) to positive (4.9 percentage points). A potential explanation is that having a school nearby makes it easier for girls to enrol and attend school. On the contrary, the presence of a school in the village diminishes the positive impact on Reading levels. This could be because the counterfactual outcomes for girls in poor communities that do not have a school in the same village are markedly worse without the program, and hence the treatment effect is larger. The main findings in Table 3 resemble Panel A1 as villages with schools form most of the sample. Overall, all the results are insignificant, and I infer a general direction of trends, but conclude that there is no significant impact. In Panel B, the impact on enrolment is higher and significant for those girls whose mothers did not attend school, demonstrating that the program had an impact on girls that come from more disadvantaged backgrounds. Conversely, the impact on learning levels is not only lower, but negative in those households where the mother did not go to school. This could be due to a lack of resources at home or lower enrolment rates among girls whose mothers are uneducated[19].

The results for Group 2 are presented in Table 6. There were too few or zero observations in the case of there being no school in the village, so I do not present those results. As opposed to Group 1, the positive (and significant) spillover effects of the program on enrolment of the eldest daughter only manifest if the mother is educated. However, mother's education is not sufficient to improve the daughter's learning levels, as the coefficients are negative. As the coefficients for learning outcomes are insignificant, these interpretations are tentative.

Table 6- Heterogeneity Analysis, Group 2

Dependent Variables	Enrolment	Maths Level	Reading Level
Panel A1: Mother School Yes			
KN* × *Eligible	**0.032***	**−0.109**	**−0.034**
	[0.051]	[0.380]	[0.754]
Observations	5,520	5,060	7,887
Panel A2: Mother School No			
KN* × *Eligible	**0.008**	**0.115**	**0.103**
	[0.500]	[0.158]	[0.414]
Observations	3,815	4,336	9,086

Notes: This table reports results from Specification (2) for Group 2, with each cell reporting the coefficient on the Karnataka-Eligible interaction term. Each column represents a specific outcome variable, and each panel depicts the level of heterogeneity. Panel A varies whether the girl's mother has gone to school. p-values from the wild bootstrap procedure sub-clustered at the district level are in brackets.

[19] The enrolment rate for girls in Group 1 is 98.8% for those whose mothers are educated, and 93.9% for those whose mothers are not.

VI. Robustness Checks

Placebo Test: To investigate the robustness of my results, I check if Specification (2) yields any significant effects for placebo groups, which did not experience the program. If my results are truly capturing the causal effect of the CCT then I should not see any significant effect for these groups. The results for the placebo tests are reported in the Addendum (Table A3).

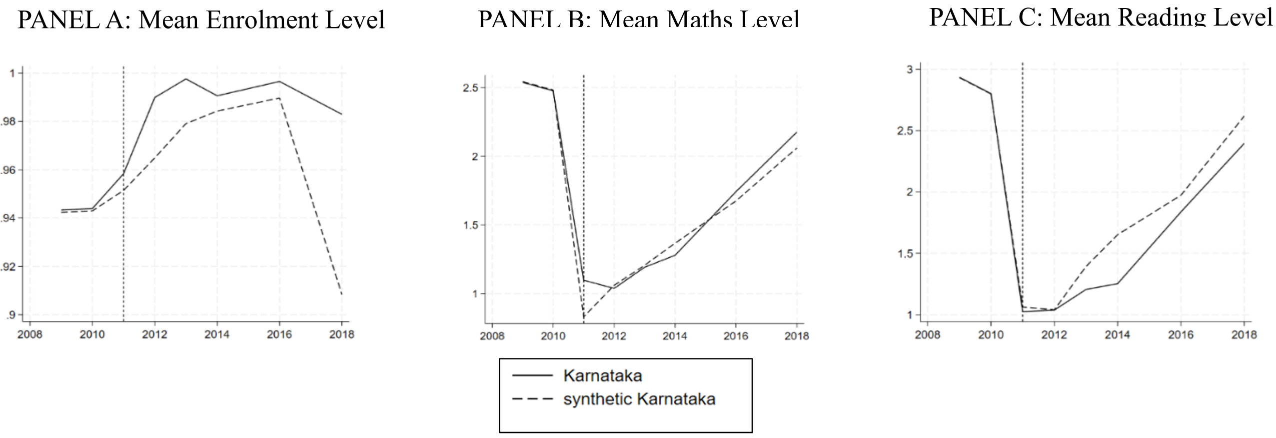
The first test consists of households that only have boys, with boys born in or after 2006 forming the pseudo 'eligible' group. This also sheds light on whether there were any external factors that only affected those born after 2006 which was driving the results. The control states are the same as those for Group 1, as we are comparing the 'direct' results. Only one result is significant- the point estimate for Maths. Since this is a 'fictitious' program, running several tests should incorrectly produce a significant effect around 10 percent of the time, at a 10 percent significant level, and this result may be a statistical artefact (Anukriti 2018). However, if that is not the case, then the result is concerning. It implies that there could be an external factor that is affecting those born after 2006 in Karnataka relative to the control states. The second placebo test consists of reassigning the intervention to an alternate control state, while Karnataka is dropped from the analysis. All the results are insignificant for this test. Thus, the validity of the significant effect on enrolment found for Group 2 is strengthened. As the placebo test is insignificant, state-level confounders that impact this group are less likely.

Changing the Intervention Year: The program is restricted to those daughters born after 31st March 2006. While the data have information on the child's age, the specific date and month of birth is unknown. It is possible that some non-eligible observations (those born before 31st March) are included in the treatment group, as I set the criterion to include those born in or after 2006 (Table 1). This could lead to the underestimation of the program impact as some observations that did not receive treatment are biasing the results downward. As a robustness check, I remove those born in 2006, and their siblings, from the treatment group for all groups of interest and redo the analysis. The results are presented in the Addendum (Table A4). For Group 1, the results remain consistent with Table 3. All the coefficients are insignificant here as well, and the magnitude of the point estimate is the same for enrolment, and marginally higher for Maths and Reading levels. For Group 2, when comparing with outcomes with column (3) in Table 4, the estimated treatment effect on enrolment has increased in magnitude and significance (it is now significant at 1%). Similarly, the point estimates for Maths and Reading levels have also increased substantially (though they remain insignificant). I infer that the inclusion of non-eligible observations may have dampened the estimated program impact.

Synthetic Control: I compute a synthetic control state (Abadie and Gardeazabal 2003) and conduct a graphical analysis of the treatment effect by observing the difference in outcomes between the synthetic counterfactual Karnataka and Karnataka, across years. The advantage of this method is that a combination of units across potential control states may provide a more appropriate comparison group (Abadie 2021). The states available from which observations can be weighted are those which did not have any education or fertility CCT[20]. This method involves assigning weights to observations to match Karnataka as closely as possible in the pre-treatment period, based on the vector of covariates. In my case, the pre-treatment outcomes are the outcomes of the non-eligible group before 2011. I remove the observations for the non-eligible group post 2010 to resemble a canonical DiD structure (as described in Section III)[21]. To construct the synthetic control, I collapse the cross-sectional data into a state-year panel by taking the average of all variables for each state, in each year.

Figure 4 plots the trends of Karnataka and synthetic Karnataka for Group 1. The synthetic state matches Karnataka before 2011 and diverges after. The eligible group experiences a positive effect on enrolment. The impact of the program on Maths levels fluctuates across years, but ultimately the effect size is positive after 2014. This matches the main findings in Table 3. The effect size on Reading is persistently negative, whereas the main results found that the point estimate is insignificant and close to null. The treatment effect for Reading is heterogeneous, as girls whose mothers are not educated experience a negative effect of the program (Table 5). Hence, the direction of the effect on literacy is ambiguous, and the synthetic control result confirms this. Overall, as the results are insignificant, I do not conclude any effect.

Figure 4: Trends for Group 1, Karnataka versus synthetic Karnataka

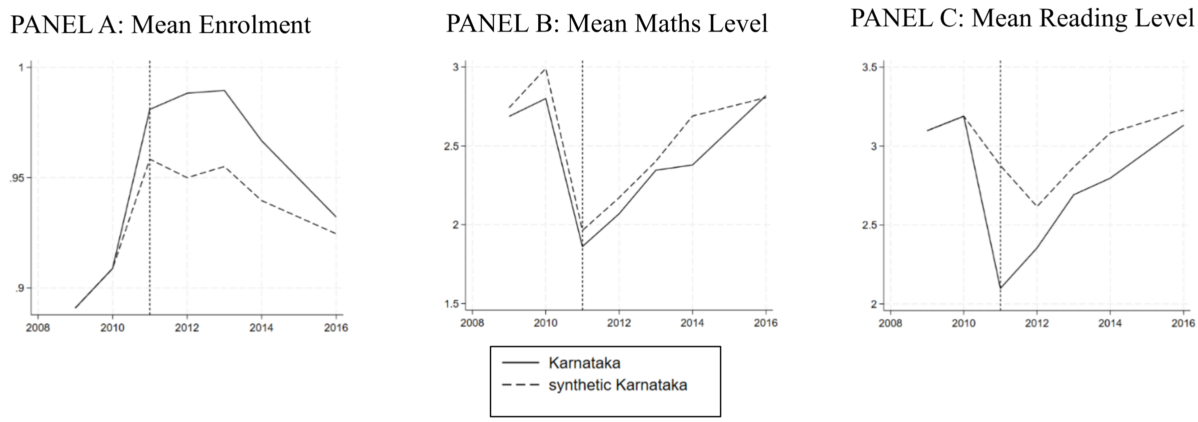


[20] These are: Himachal Pradesh, Uttarakhand, Sikkim, Nagaland, Manipur, Mizoram, Tripura, Meghalaya, Jharkhand, Odisha, Chhattisgarh, Maharashtra, Andhra Pradesh-Telangana, Kerala, Tamil Nadu, and Pondicherry.

[21] The purpose of including the outcomes of the untreated group in the post treatment years in the main analysis was to control for state-level deviations in trends. This is no longer needed, as I now select observations from a pool of states.

From Figure 5, for Group 2, there is a persistent positive effect on enrolment across all years, which supports the main results, the heterogeneity analysis, and the other robustness checks. The effects on Maths and Reading are negative across all years, which does not match the main results in Table 4. However, the synthetic Karnataka does not align with Karnataka perfectly; the trends for these outcomes are persistently lower in pre-treatment years as well. Moreover, it was seen that the direction of the effect of the program on learning levels is heterogeneous.

Figure 5: Trends for Group 2, Karnataka versus synthetic Karnataka



In sum, the robustness checks cast doubt on the validity of some of the Group 1 results. The placebo test showed that there may be some external factor impacting test scores of those born after 2006. The synthetic control analysis further confirmed that the (insignificant) effect size for Reading levels is ambiguous. The main finding for Group 1 was that the program had an insignificant impact on learning levels and is therefore conservative, but I still refrain from making conclusions on the general direction of trends. The positive and significant impact on enrolment for the eldest sister was robust to all the checks, while the impact on learning levels is more ambiguous (as revealed by the synthetic control and heterogeneity analysis).

VII. Conclusion

Due to poor socio-economic outcomes for female children, several state governments in India have invested heavily in conditional cash transfer programs aimed at reducing sex-selective abortion and improving health and education outcomes for these girls. While previous studies have found positive impacts of CCTs on enrolment for the targeted beneficiaries, the evidence on learning outcomes, and of spillover effects on siblings is mixed. My paper investigates the causal impacts of the Bhagyalakshmi program on these outcomes.

The direct effect of the program on enrolment outcomes aligns with theoretical predictions and prevailing evidence: the effect size is positive for eligible girls. The point estimate is insignificant but becomes significant for the sub-set of girls whose mothers are not educated. The magnitude of the estimator is robust when conducting placebo tests, changing the program year, and constructing a synthetic Karnataka. Nevertheless, given that the result is insignificant except for a specific sub-population, and is occurring in the margins of the distribution, I conclude that the program had a generally positive effect, but this was not substantial. Literacy and numeracy levels are non-targeted outcomes of the program, and hence would only be impacted if the displacement, income, and substitution effects also applied to parental investment in learning support. My findings support the literature – there is no significant effect on assessment scores. The direction of the effect size is ambiguous.

In terms of spillovers, the CCT has a significant and positive effect on enrolment for eldest daughters whose younger sisters are eligible for the program, by 3.8 percentage points (4.5%). In contrast to the previous group, this effect is driven mainly by households where the mother is educated. As per the theoretical framework, this implies that the income effect outweighs the displacement effect in Karnataka- the CCT provides enough excess income that families can afford to send several children to school, instead of replacing the eldest daughter with the younger one in school preferences. It is robust to placebo tests and when constructing a synthetic Karnataka. Furthermore, tightening the eligibility restriction increases the magnitude and significance of the result. There are no spillovers of the program on learning levels, for which the coefficients are all insignificant, and the direction of the effect size is heterogeneous.

There are limitations to these findings. While I have tried several methods to verify that trends between Karnataka and control states are parallel, and attempted to control for any violations, it is possible that my test was underpowered to detect significant deviations. There is also always a risk of unobserved confounders. It is possible that unobserved factors (like preference for sons) bias the results. I find some suggestive evidence of son-preference among Group 1 families, but not among Group 2 families. If it is the case that families with a son-preference select into the program, the results are underestimates of the true treatment effect. Another limitation is that the data has no information on take-up of the program, and while I try to restrict the sample, the estimates are noisy.

There are three main contributions of this paper. Firstly, I evaluate an ongoing, and costly, cash transfer program (that has not been empirically studied before) and find limited evidence of positive effects on enrolment on eligible girls. This may be due to high baseline enrolment rates, which limit the potential for significant changes on the extensive margin. Secondly, I demonstrate that the program significantly increases enrolment for eldest daughters who are ineligible due to their age. They have lower baseline enrolment rates and hence there is more scope for extensive margin effects. Thirdly, I confirm what has been seen in previous studies: there is no impact on learning outcomes for either group. My findings add to the literature on the effect of CCTs on siblings and learning levels, both of which have mostly been investigated in non-Indian settings.

As mentioned, I attempted to estimate the impact on brothers whose sisters were eligible for the program but came across issues (namely, violation of parallel trends). Future research can focus on the differential impact of spillovers between ineligible sisters and brothers. Another similar question is whether CCTs targeted towards girls crowd out access to schooling for boys. It is important to note that cash transfer programs such as the *Bhagyalakshmi* scheme aim to improve outcomes for girl children across a range of dimensions not exclusive to education. While the effect on enrolment and learning levels are limited in this case, the potential positive influence on perceptions of female children cannot be understated. Future research can thus focus on investigating a wider range of socio-economic indicators, and beliefs and attitudes.

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Addendum

Table A1 - Year-Wise Parallel Trends for the Non-Eligible Group

Dependent Variables	Enrolment	Math Level	Reading Level
Panel A: Group 1			
KN × Year			
2010	−0.007	−0.019	−0.141
2011	−0.003	0.021	−0.079
2012	0.009	−0.105	−0.113
2013	0.015	0.060	−0.024
2014	0.009	0.031	−0.039
2016	0.017	0.065	−0.006
F-test p-value	[0.798]	[0.960]	[0.824]
Observations	40,045	37,443	54,455
Panel B: Group 2			
KN × Year			
2010	0.018	−0.041	−0.091
2011	−0.011	−0.113	−0.135
2012	0.030	−0.124	−0.034
2013	0.025	0.042	0.027
2014	0.012	0.015	−0.028
2016	0.009	0.009	0.037
F-test p-value	[0.807]	[0.829]	[0.796]
Observations	13,263	13,369	23,829

Notes: This table reports results from Specification (1), where 'Treat' has been replaced with year dummies. Each cell displays the coefficient on the state-year interaction term. The columns represent different outcome variables, and panels represent the different groups of interest. The p-values from the F-test of joint significance using the wild bootstrap sub-clustered at the district level are in brackets. The regression is weighted to be representative at the state level and contains year fixed effects. Covariates include a treatment dummy, Karnataka dummy, child's age, and mother's education.

Table A2- Average Number of Sons per Family

State	Group 1: Eligible		Group 1: Non-Eligible		Group 2: Eligible		Group 2: Non-Eligible	
	Avg.	**Diff**	**Avg.**	**Diff**	**Avg.**	**Diff**	**Avg.**	**Diff**
KN	0.744		0.740		0.473		0.545	
TN	0.368	0.376^{***}	0.752	−0.012	0.384	0.089^{***}	0.457	0.088^{***}
KL	0.601	0.143^{***}	0.655	0.085^{***}	0.188	0.285^{***}	0.264	0.281^{***}
AP-TS	0.673	0.071^{***}	0.711	0.029^{***}	0.334	0.139^{***}	0.378	0.167^{***}
HP	0.792	-0.048^{**}	0.843	-0.104^{***}	0.523	-0.050^{*}	0.700	-0.154^{***}
JH	0.962	-0.219^{***}	1.058	-0.318^{***}	0.795	-0.322^{***}	0.817	-0.272^{***}

Notes: This table reports the average number of boys in a family for every sub-group in each state. The first column for each group depicts the mean and the second column depicts the difference in mean between Karnataka and the control state. The difference is tested using a t-test. *p< 0.10, **p< 0.05, *** p<0.01. KN is Karnataka, TN is Tamil Nadu, KL is Kerala, AP-TS is Andhra Pradesh-Telangana, HP is Himachal Pradesh, and JH is Jharkhand.

Table A3- Placebo Tests

Dependent Variables	Enrolment	Maths Level	Reading Level
Panel A: Boys			
KN* × *Eligible	**0.024**	**0.060***	**0.014**
	[0.225]	[0.098]	[0.698]
Observations	77,315	68,492	87,113
Panel B: Group 1			
Control State* × *Eligible	**0.018**	**−0.046**	**0.078**
	[0.438]	[0.860]	[0.400]
Observations	12,553	13,353	26,382
Panel C: Group 2			
Control State* × *Eligible	**0.022**	**0.113**	**−0.014**
	[0.208]	[0.535]	[0.895]
Observations	3,861	4,367	11,835

Notes: This table reports results from Specification (2), with each cell reporting the coefficient on the State-Eligible interaction term. Each column represents a specific outcome variable, and each panel depicts the placebo group. Panel A contains results for the 'boy' placebo group, and Panels B and C contain outcomes where the intervention has been reassigned to a control state. p-values from the wild bootstrap procedure sub-clustered at the district level are in brackets.

Table A4- Changing the Intervention Year

Dependent Variables	Enrolment	Math Level	Reading Level
Panel A: Group 1			
KN* × *Eligible	**0.025**	**0.063**	**0.005**
	[0.191]	[0.376]	[0.898]
Observations	25,695	25,443	37,427
Panel B: Group 2			
KN* × *Eligible	**0.041*****	**0.030**	**0.063**
	[0.003]	[0.725]	[0.290]
Observations	8,341	8,466	15,002

Notes: This table reports results from Specification (2) where the program intervention year has been changed to 2007. Each cell reports the coefficient on the Karnataka-Eligible interaction term for the specified group. Each column represents a specific outcome variable, and each panel represents the group of interest. p-values from the wild bootstrap procedure sub-clustered at the district level are in brackets.