Journal of Population Economics (2024) 37:66
https://doi.org/10.1007/s00148-024-01045-4

ORIGINAL PAPER

Persistent effects of a conditional cash transfer: a case of empowering women through *Kanyashree* in India

Subarna Banerjee[1] · **Gitanjali Sen**[1]

Received: 9 April 2023 / Accepted: 9 September 2024

Abstract

Launched in 2013 in West Bengal, India, Kanyashree Prakalpa is a conditional cash transfer program incentivizing girls to stay in school and delay marriage. The program provides annual scholarships to 13–18-year-old girls for remaining enrolled in school, and a lump-sum transfer upon attaining adulthood, conditional on remaining unmarried and pursuing education. Using pre- and post-program data and cohort-specific eligibility rules for 15–35-year-old women and by employing double-differences and triple-difference frameworks, we find that exposed women experience 7 to 8 percentage points higher likelihood of independent movement outside the home and have a lower tolerance for domestic violence. We find the affected cohort to have a 4 to 5 percentage points lower likelihood of justifying wife-beating by husbands. We find suggestive evidence of these results being mediated by access to bank accounts and increased schooling. This underscores the importance of education and financial independence as a pathway to women's empowerment.

Keywords Conditional cash transfer · Education · Women · Empowerment · Domestic violence · Perception · Mobility · Kanyashree · India

JEL Classification I20 · I24 · I25 · I28 · J10 · J16 · O53

1 Introduction

Many cash transfer programs are designed to place resources in the hands of women instead of men (Armand et al. 2020; Bastagli et al. 2016). The basis of this is the premise that such resources will not only promote gender equity and empower

Responsible editor: Kompal Sinha

✉ Gitanjali Sen
gitanjali.sen@snu.edu.in

1 Shiv Nadar Institution of Eminence Deemed to Be University, Dadri , Gautam Buddh Nagar 201314, Uttar Pradesh, India

Published online: 26 September 2024

women but will also be utilized to cater to child-specific interests, resulting in human capital gains (Mookherjee & Napel 2021; Thomas 1990; Yoong et al. 2012). Interventions that use gender-based targeting can enhance women's bargaining power within the household by increasing women's relative income versus men's (Adato et al. 2000). By reducing their financial dependence or poverty-related stress, such transfers can lessen the probability of abuse a woman may face from her partner. On the other hand, consistent with theories of status inconsistency or male backlash (Zhang & Breunig 2023), such an increase in the woman's relative income may challenge her husband's status, prompting him to use violence to gain control of resources and restore his position in the household (Bobonis et al. 2013; Eswaran & Malhotra 2011; Hidrobo & Fernald 2013). However, most of this literature looks for program effects on women who were provided with cash transfers when they were already married.

Our paper attempts to contribute to this literature by studying the persistent impact of a conditional cash transfer (CCT) program on women's empowerment measured by mobility outside the home and perceptions toward domestic violence, as the program we examine explicitly targets unmarried women to alter their educational participation. Launched in the year 2013 in the state of West Bengal, India, Kanyashree Prakalpa (henceforth, KP) is a conditional cash transfer program aimed at incentivizing girls to stay in school for longer and defer their age of marriage. The program provides annual scholarships to eligible girls aged 13 to 18 for every year they remain in school, and a lump-sum transfer upon reaching the age of 18, conditional on remaining unmarried and pursuing education. The program requires that the beneficiary has a bank account in her name, and all transfers are made directly to this account. As a result, KP girls are ensured financial inclusion and access to the financial market from a very young age.

Using three rounds of India's National Family Health Survey (NFHS) and cohort-specific eligibility rules in a sample of 15–35-year-old women, we estimate the impact of the program on several empowerment indicators. As the data on women's outcomes is collected only for women above 15 years of age, the lower bound of our sample is 15-year-old women. In 2013 (the policy year), a woman above 18 years of age was not eligible for the program, creating our control cohort.

By using the pre- and post-program (pooled cross-section) data and employing difference-in-differences and triple-difference, this paper finds that women exposed to KP experience greater mobility outside the home and have less tolerance of domestic violence. The affected cohort seems to have a 7 to 8 percentage points higher likelihood of going alone outside their home to visit health facilities, marketplaces, or traveling out of their immediate village or community, and a 4 to 5 percentage points lower likelihood of justifying wife-beating by husbands. These estimates are robust to the alternative methods of kernel-based propensity score matching with difference-in-differences, inverse probability weighted matching with difference-in-differences, and survive several sensitivity checks and falsification tests.

While exploring the mechanisms, we find that an increase in the years of education, coupled with the receipt of transfers to one's bank account, may be the potential mechanisms behind our results. The ITT effect of a higher probability of having one's bank account points to program effects since program beneficiaries were necessitated to have a bank account in their name. An exploratory analysis of administrative data by the authors on program funds displacement suggests that funds allocation to districts was based on eligible population and per capita income.

The closest literature estimating the immediate and direct effects of KP has documented that it successfully increased secondary school enrollment among girls (Sen & Thamarapani 2023), and seems to have reduced underage marriages and school dropouts (Dey & Ghosal 2021; Dutta & Sen 2020). KP does not seem to improve learning outcomes (Das & Sarkhel 2023), pointing toward the concerns regarding quality of teaching in public schools.

Since KP is specifically aimed at improving girls' well-being and status, little is known about its medium- to long-term influences on women's empowerment indicators of autonomy, mobility, and perceptions of domestic violence. While the primary objective of this paper is to capture the average effect of a particular CCT program from the immediate to long run, we find evidence of effects persisting long term, that is, even after the transfers are discontinued. Being a beneficiary of KP can improve the woman's status in the household, which can happen broadly through the following channels.

The first pathway could be through an increase in education, which can increase expected returns to human capital, expand labor market opportunities, and subsequently alter women's preferences toward fertility, contraception, and tolerance for violence (Grönqvist & Hall 2013). The second pathway could be through the conditionalities of having a bank account that receives regular transfers. This is expected to instill a sense of financial empowerment, which may transform how other household members perceive the woman's worth. Such consequences of increased years of education coupled with the other program features can translate to a lasting rise in self-worth. As a result, beneficiaries may exhibit a greater sense of empowerment.

The third pathway, which is beyond the scope of this paper, is an improvement in women's status through the possibility of marital sorting, which can lead to program beneficiaries marrying better (Bobonis 2011). Postponing marriage until a girl is physically and emotionally equipped can improve the pool of prospective spouses in the marriage market. "Better" spouses and in-laws can imply better outcomes for the bride. Also, if one's education can influence her social networks, then with an increase in education, beneficiaries might be more likely to interact with potential partners with similar education levels. On the other hand, since the program exclusively targets girls' education, this may increase the search costs of finding a partner with equivalent education. Simultaneously, the program may influence beneficiaries' decision to marry or the timing of marriage. This means that KP can endogenously change the marital choices of its beneficiaries. While disentangling the interlinkage between marital sorting and women's empowerment due to KP is not straightforward, we focus on the first two pathways for investigating the impact on women's empowerment on average.

In recent years, literature across the world has closely studied the implications of providing both conditional and unconditional cash transfers to young, unmarried female beneficiaries (Baird et al. 2019; Duflo et al. 2015; Angrist et al. 2006; Özler et al. 2020; Sandøy et al. 2016). A significant share of this literature is concentrated in Africa and Latin America, where cultural norms surrounding marriage are very different compared to countries in South Asia. In such settings, a girl does not have much say in whom she marries, and the prevalence of cohabitation and pregnancy before marriage is extremely low. For countries like Bangladesh and Pakistan, there exists scant literature in this area (Alam et al. 2011; Hahn et al. 2018). Moreover, due to unaddressed selection issues in these studies, the causal evidence surrounding the longer-term empowering effects of cash transfers in the South Asian context (Baird et al. 2011; Buchmann et al. 2023) is limited. At the same time, unlike Bangladesh and Pakistan, India has been witnessing a puzzling and consistent decline in female labor force participation rates despite decreasing fertility and rising educational participation of women (Deshpande & Singh 2021; Fatima & Sultana 2009; Jayachandran 2021; Neff et al. 2012; Rahman & Islam 2013). These have implications for women's economic participation and empowerment, which makes India a pertinent set-up to explore this research question.

Our paper contributes to several strands of literature. First, it relates to the broader literature on the indirect effects of CCTs on sustained improvements in human capital (Baez & Camacho 2011; Millán et al. 2020; Peruffo & Ferreira 2017). We investigate how a persistent improvement in the human capital of the CCT beneficiaries through increased years of schooling may generate positive externalities for their status in the long run. Second, it contributes to extensive literature on targeting CCTs to specific groups of individuals and the empowerment repercussions of the same. The existing studies (Almås et al. 2018; Bandiera et al. 2020; Bonilla et al. 2017; Ellis 2012; Ladhani & Sitter 2020) estimate the empowerment outcomes such as women's decision-making power within the household, willingness to pay to have control over the transfers, the likelihood of teen pregnancy, early entry into marriage/cohabitation, use of contraception, and having multiple sexual partners. Outcomes such as cohabitation before marriage, number of sexual partners, practicing safe sex, and prevalence of teen pregnancies before marriage might not be relevant indicators of women's empowerment for countries in South Asia which operate on different cultural norms.[1] Our paper instead focuses on two separate outcomes associated with women's empowerment—mobility outside the home and perceptions toward domestic violence. To the best of our knowledge, these outcomes have not been studied before in the context of evaluating the impact of CCTs.

Our study also relates to the literature on child marriage and interventions to reduce that in low-income countries (Lee-Rife et al. 2012). This literature has established that by empowering girls through economic incentives and support to their families, providing information, and improving skillsets through training and access to support networks can successfully reduce child marriages in countries like Ethiopia, Bangladesh, Malawi, Nepal, and India. By exploring how benefits targeted at the right age and time could have a long-run impact on several dimensions of development, our paper also adds to the ongoing discussions on the benefit–cost tradeoffs associated with implementing large-scale CCTs. Costs involved in continuing

[1] For the meta-analysis of this literature, see Baranov et al. (2021); Simon (2019); van den Bold et al. (2013).

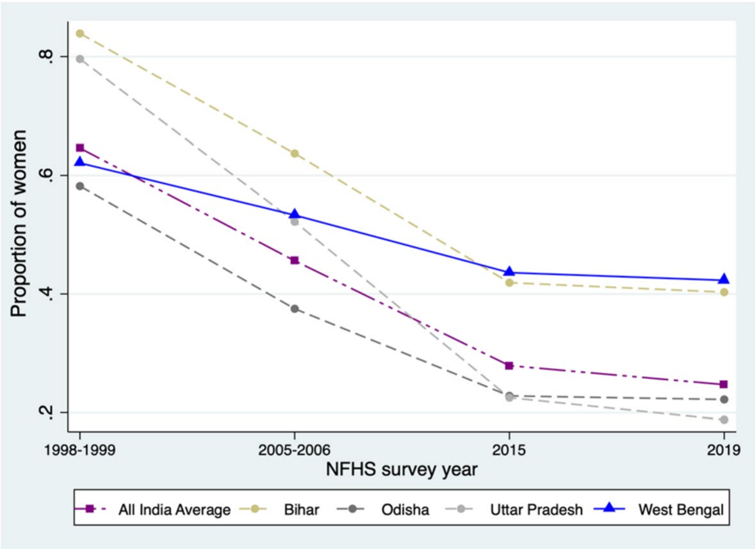
such CCTs in a developing country might be well justified when one can measure and incorporate their impacts over a long horizon.

The rest of the paper proceeds as follows: Sect. 2 describes the context and the program; Sect. 3 discusses the data, identification challenges, and empirical strategies; Sect. 4 contains the main results; Sect. 5 includes several robustness checks; Sect. 6 discusses potential mechanisms driving the results; Sect. 7 consists of a discussion on potential concerns regarding our strategy and a few extensions; and Sect. 8 concludes.

2 Context and program description

West Bengal is India's fourth most populous state, with a population projection of almost 100 million in 2021 (Government of India, 2011). The state has historically been plagued by high proportions of underage marriages (NFHS-4 and NFHS-5) and, as of 2019, had the highest percentage of women aged 18–29 who were married before 18 years (see Fig. 1).

Underage marriages increase the prevalence of teen pregnancies which can have grievous consequences for maternal and child health (Perez-Alvarez & Favara, 2023). To address these concerns, in recent years, the state government has introduced measures in the form of various social safety nets. One such measure is KP,



This figure uses data from four rounds of NFHS conducted between 1998 to 2021. It plots the proportion of women aged 18 – 29 in the survey year who were first married before the legal age of 18 years. We plot this statistic for the whole of India, West Bengal, and other states that have historically had high rates of underage marriages. We see that as of 2019, West Bengal has the highest rate of underage marriages for women who were between the ages 18 – 29 during the time of survey.

Fig. 1 Proportion of women aged 18–29 who were married for the first time before age 18 which has transpired to be the state's flagship program garnering international interest and acclaim.[2]

Launched on October 1, 2013, KP is a conditional cash transfer program funded by the state government of West Bengal. It aims to improve the status of young girls from economically disadvantaged families[3] by keeping them in school for longer and delaying their early marriage. The scheme is two-tiered. The first component is the K1 grant—an annual scholarship of 1000 INR (approximately 12.55 USD) for unmarried girls aged 13 to 18 for every year they remain in education.[4] The second component is the K2 grant—a lump sum payment of 25,000 INR (approximately 314 USD) to a girl when she turns 18, conditional on remaining unmarried and pursuing either mainstream education or vocational, technical, sports training, and such. The K1 and K2 grants amount to about 3.75 and 18.8% of West Bengal's per capita GDP (for 2021–2022), respectively.

The program requires opening a zero-balance savings account in the beneficiary's name, ensuring that the intended beneficiary directly receives the transfer and thereby enables financial inclusion of girls from a young age. The nominee for the bank account can only be a woman—either the beneficiary's mother or another female guardian. The program's technical partner is UNICEF, which supports several aspects of the scheme, especially in developing communication and capacity-building strategy, program monitoring, and evaluation. Kanyashree Online (wbkanyashree.gov.in) is the multi-user, Government-to-Citizen (G2C) online portal that provides comprehensive e-governance of KP. Educational institutions play a pivotal role in delivering a single-window service delivery mechanism for the scheme by informing students about the scheme, facilitating bank account opening, uploading all applications to the e-portal of the program, and addressing and escalating all grievances related to the program. If educational institutions cannot process applications because of a lack of computer facilities, then application forms are sent for data entry to the appropriate block or sub-divisional office, or the nearest Circle Level Resource Centre. There is no physical movement of paper forms, and no checks are done manually. Thus, unlike other conditional cash transfers, which demand a significant portion of the beneficiary's time, KP aims to minimize the time commitment from applicants and the system warrants very few leakages. As of August 1, 2022, the program has 7,814,445 unique beneficiaries, and as of August 12, 2022, there are 18,183 educational institutions in the state registered under the program.

Table 1 depicts the cohort-based variation in KP exposure for every survey year and years of exposure to the program. For example, a respondent aged 15 years in West Bengal, when interviewed in 2019 (first column), was 9 at the time of the launch of KP in 2013. Since the program targets only girls aged 13–18, starting from

[2] Kanyashree Prakalpa received the United Nations Population Award in 2017.

[3] Program eligibility required the beneficiary's parents to have an annual income of less than 120,000 INR (1507 USD), but this condition was dropped on October 1, 2018.

[4] The annual scholarship amount was 500 INR (around 6.28 USD) from 2013 to March 2015 and was revised to 750 INR (about 9.42 USD). From June 23, 2018, the amount has been re-revised to 1000 INR (about 12.55 USD).

Table 1 Tracking Cohort-wise Exposure to Kanyashree Prakalpa

Age in survey year	15	16	17	18	19	20	21	22	23	24	25–35
Interviewed in 2019:											
Age in policy year	9	10	11	12	13	14	15	16	17	18	19–29
Years of exposure	3	4	5	6	6	5	4	3	2	1	0
Dose of treatment (INR)	1,953	2,529	3,129	20,583	21,170	21,117	21,274	21,466	21,875	22,767	0
Interviewed in 2015:											
Age in policy year	13	14	15	16	17	18	19	20	21	22	23–33
Years of exposure	3	3	3	3	2	1	0	0	0	0	0
Dose of treatment	1,466	1,466	1,466	21,466	21,875	22,767	0	0	0	0	0
Interviewed in 2006:											
Age in policy year	22	23	24	25	26	27	28	29	30	31	32–42
Years of exposure	0	0	0	0	0	0	0	0	0	0	0
Interviewed in 2005:											
Age in policy year	23	24	25	26	27	28	29	30	31	32	33–43
Years of exposure	0	0	0	0	0	0	0	0	0	0	0

The table shows exposure to the program for different cohorts across various NFHS survey years. *Kanyashree Prakalpa* was launched in 2013. The annual scholarship amount was 500 INR (around 6.28 USD) from 2013 to March 2015, and was revised to 750 INR (about 9.42 USD). From 23 June 2018, the amount has been re-revised to 1000 INR (about 12.55 USD). The dose of Treatment, or the amount of potential transfer to be received by each cohort is calculated by adding up the respective annual scholarship amounts (K1 grants) over the years based on exposure duration along with the lump sum amount (K2 grant) if the individual is 18 and above. All the amounts are normalized by deflating with the price index based on 2012 prices

2017, this respondent has about 3 years of exposure to KP. Similarly, a respondent interviewed in the survey years before 2013 had zero exposure.

In addition, we also calculate the amount of potential transfer to be received by each cohort based on their eligibility. The annual scholarship amount was 500 INR (around 6.28 USD) from 2013 to March 2015, when it was revised to 750 INR (about 9.42 USD). From June 23, 2018, the amount has been revised again to 1000 INR (about 12.55 USD). Therefore, for every individual of a given age, the potential transfer is calculated by adding up the respective annual scholarship amounts (K1 grants) over the years based on her potential duration of exposure, along with the lump sum amount (K2 grant) if the individual is 18 and above. All the amounts are normalized by deflating it with the price index based on 2012 prices. This table clearly indicates that for each age in the survey year, there is a strong association between the years of exposure and the amount received.

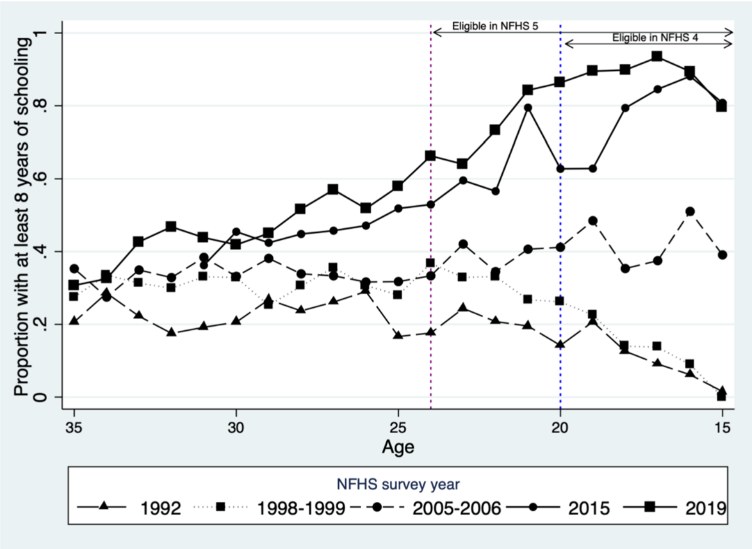
KP is not the only conditional cash transfer program in the country dedicated to improving and empowering the lives of young girls. States like Andhra Pradesh, Bihar, Delhi, Gujarat, Haryana, Madhya Pradesh, and Maharashtra have their versions of CCTs, which target increasing years of education and delaying early marriages of girls.[5] However, there are subtle nuances in KP's design, which make it an exciting program to study. First, the program does not specify grade completion eligibility requirements, which could exclude weak students, first-generation learners, or students in schools with poor-quality teaching staff[6] from reaping the program's benefits. Second, instead of only requiring the girls to be unmarried until 18 years of age, the program also necessitates them being in school until at least the minimum legal age of marriage to be eligible for the K2 grant, thereby internalizing the role of education in delaying early marriages. Third, the program creates much more pronounced aspirational effects by starting in the formative years, making girls the direct beneficiaries, and reducing the role of parents or guardians in the application process. Bihar and Jharkhand have recently launched similarly designed CCT programs in 2018 and 2020, respectively, but not enough time has passed to expect any long-run effect on the empowerment of the beneficiaries.[7]

Figures 2 and 3 plot the raw means of the proportion of women with secondary education and the average age of marriage for different age groups, respectively. This provides us with suggestive evidence of the direct effects of KP. Since the program is explicitly aimed at improving the lives of young women from economically disadvantaged backgrounds, we see two stylized trends after the launch of KP in 2013. First, secondary school enrollment of young women exposed to the program appears to be rising sharply for nationally representative survey rounds conducted after the program (Fig. 2). In contrast, there is no visual evidence of such a rise

[5] See Dutta and Sen (2020) for a discussion on CCTs in India, and Sen and Thamarapani (2023) for positive effects of KP on educational participation using the NFHS data in a difference-in-difference framework.

[6] Das and Sarkhel (2023) observe no positive effect of the program on students' learning outcomes which point toward a lack of state capacity in providing better quality teaching.

[7] Mukhya Mantri Kanya Utthan Yojana launched in 2018 and Mukhya Mantri Sukanya Yojana launched in 2020.

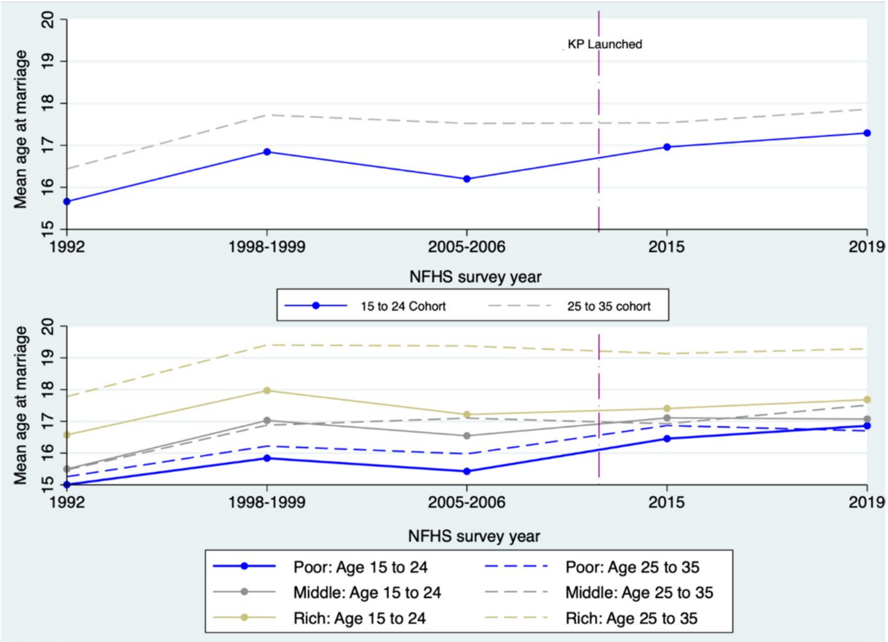


This figure uses data from four rounds of NFHS conducted between 1998 to 2019 for West Bengal. For different survey rounds and ages, the figure plots the proportion of women aged 15 and above who have at least 8 years of education. For young women exposed to the program, this variable, appears to be rising sharply for nationally representative survey rounds conducted after the program. In contrast, there is no visual evidence of such a rise before its launch.

Fig. 2 Proportion with 8 or more years of schooling by cohort for each survey year

before its launch. Second, the average age of marriage of young women exposed to the program appears to have risen (Fig. 3, top panel) since KP was implemented. Furthermore, this rise visually is the steepest for younger women from economically disadvantaged backgrounds (Fig. 3, bottom panel). This is consistent with the intended targeting for KP and the studies that causally estimate KP's direct impact on increasing secondary schooling and reducing child marriage prevalence (Das & Sarkhel 2023; Dey & Ghosal 2021; Sen & Thamarapani 2023). This lays out an apt canvas to explore the indirect effects of the program and if those persist even when the potential beneficiaries cease to receive the transfers.

We acknowledge that a woman's empowerment or lack thereof is highly contextual and primarily dictated by social and cultural norms (Almås et al. 2018). What may be considered an active choice by an "empowered" woman may only be a reflection of the same structures of subordination of women that she has internalized. At the same time, the many amorphous definitions of empowerment are all associated with some form of autonomy, freedom, expansion of agency, and ownership and control over assets. Although difficult to quantify, large household surveys attempt to capture women's status through direct empowerment measures like involvement in household decision-making, access to control over resources, mobility, and perceptions toward abuse and intimate partner violence. Some of these measures that are used in our paper are explained in the next section.



This figure uses data from five rounds of NFHS conducted between 1992 to 2019 for West Bengal. For different survey rounds and cohorts, the figure plots the raw mean age at marriage. The average age of marriage of young women exposed to the program appears to have risen (top panel) since KP was implemented. Furthermore, this rise visually is the steepest for younger women from economically disadvantaged backgrounds (bottom panel).

Fig. 3 Average age of marriage of women by survey round (top panel is for all, bottom panel separates sample by economic conditions)

3 Data, identification, and empirical strategy

3.1 Data

The paper uses three rounds of the NFHS conducted in 2005–2006, 2015–2016, and 2019–2020 (rounds 3, 4, and 5, respectively). The NFHS is a large-scale, nationally representative survey of households in India. Conducted by the International Institute for Population Sciences (IIPS) in Mumbai and administered under the Government of India's Ministry of Health and Family Welfare (MoHFW), NFHS is a part of the global Demographic Health Survey (DHS) program. It collects data on fertility, infant and child mortality, the practice of family planning, maternal and child health, reproductive health, nutrition, anemia, utilization, and quality of health and family planning services (International Institute of Population Sciences (IIPS) and ICF, 2017; International Institute of Population Sciences (IIPS) and ICF, 2021).

The NFHS administers a separate woman's questionnaire to collect information from women aged 15–49 in the surveyed household. The "district module" questionnaire covers various topics such as background characteristics, marital history, fertility history, family planning methods, maternal and child health, social networks, nutrition, sexual activity, and fertility preferences and is part of the NFHS district module. All eligible women in the household are asked the questions in the district module. The NFHS has another component—the state module, which captures information on women's work, husband's background, women's empowerment indicators, incidence and perception of domestic violence, HIV/AIDS, and other health issues. However, the state module questions are only asked to a subsample of 15% of households. The survey design ensured that this subsample was representative with no scope of selection bias.[8]

Our primary outcomes of interest are binary variables that capture information on women's mobility and their perceptions of domestic violence (Table 1).

The NFHS asks a respondent in the state module whether she can go outside her home to a health facility, marketplace, or beyond her community/village alone. We categorize this as three types of mobility—Health, Market, and Outside (community) mobility. These binary variables are coded to take the value 1 if the respondent responds with a yes and 0 otherwise. Respondents are presented with hypothetical scenarios and asked whether it is justified for a man to beat his wife in that situation. These outcomes take the value 1 if the response to the statement is yes and 0 otherwise. As a robustness check, we also combine the different measures into aggregate indices for overall mobility and perceptions following (Duflo et al. 2007; Erten & Keskin 2018; Kling et al. 2007) and see that the results hold.

Although NFHS also captures information on other empowerment indicators, such as involvement and level of say in household decision-making, we do not consider these outcomes because those are asked to married women only. By limiting the sample to only married women, we would face selection concerns as the program endogenously altered marital decisions. This is discussed in detail in the section on identification. So, instead, our analytical sample consists of all women between the age of 15 to 35 who were selected for the NFHS state module, and our choice of outcomes is based on what questions were asked to all women in the module. Similarly, we avoid using other empowerment outcomes like fertility and modern contraception because these are again conditional on marriage in a setting like India, where pre-marital sex is considered taboo. We use NFHS rounds 3, 4, and 5 because these were the closest rounds conducted around the implementation of KP (2013). NFHS 4 (2015–2016) and NFHS 5 (2019–2020) comprise data from the post-program. For our pre-program data, we use NFHS 3, completed in 2005–2006. We refrain from using the round before that (NFHS 2) as pre-period data for two reasons. First, NFHS 2 was conducted in 1998–1999, and many factors could have changed in this period, which could confound our results. Second, and more importantly, NFHS 2 surveyed married women only, and using it would bring inherent inconsistency in our cross-sectional samples. However, we use previous rounds of NFHS while testing for pre-trends in outcomes. An important caveat to this choice is that district-level identifiers are missing for NFHS 3. As a result, we cannot include district-fixed effects or district-level time trends in our specifications.

[8] Information on the survey design can be found in the documentation provided by the NFHS.

3.2 Identification strategies

Our research question seeks to tease out the effects of conditional cash transfers given to unmarried girls in improving their status and empowerment later in life. Given this question, an ideal experiment would involve randomly assigning some girls to treatment, where they receive cash transfers, and having a control group of young girls who get no such transfers but with other similar characteristics as the treated girls. Tracking these women on their empowerment indicators and comparing the treated and untreated women would uncover the causal effect of such transfers.

Since KP was launched across the entire state of West Bengal in October 2013, in the absence of a randomized experiment, the primary challenge in the identification of the program's impact is that increases in women's mobility, autonomy, and empowerment could be attributed to the common trend of society getting progressive with time—a product of gender gaps narrowing due to broader trends, which may be unrelated to the program in any way. There is no spatial variation in program exposure or intensity within West Bengal to exploit. We cannot use the variation across genders like Sen and Thamarapani (2023) because outcomes of autonomy, mobility, and empowerment, as reported in NFHS, are not symmetric across genders.

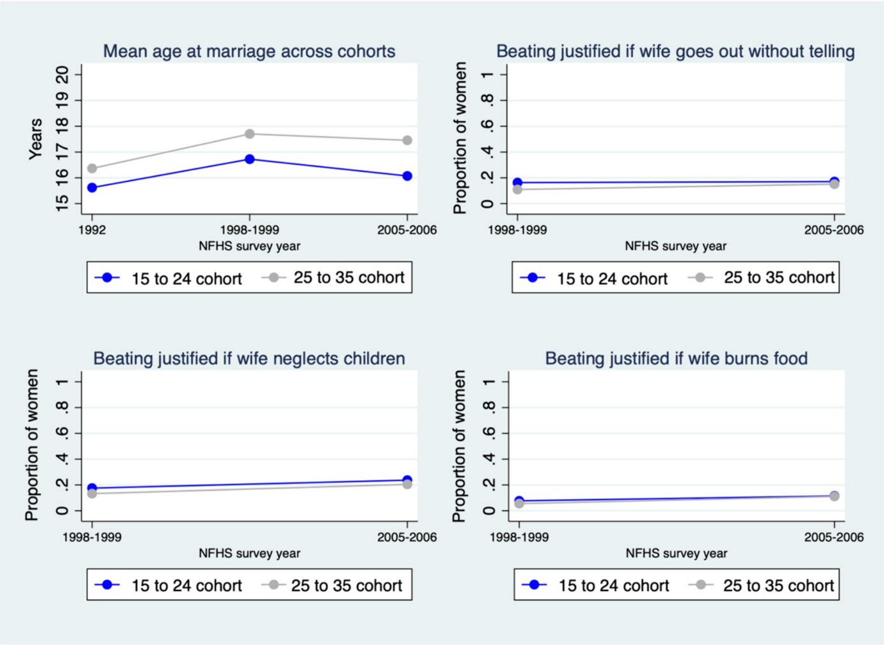
3.2.1 Difference-in-difference estimates

Our first identification strategy uses the fact that the program (launched in 2013) targets girls between the ages of 13 and 18, so women in the age group 15–20 years in NFHS 4 (2015) and women aged 15–24 years in NFHS 5 (2019) had exposure to the program. Our first strategy uses this cohort-based variation generated due to KP's age eligibility conditions to estimate the intention-to-treat (ITT) effects. We compare the outcomes of this younger cohort with an older cohort not exposed to KP by employing a time-based difference-in-differences design. In other words, our first specification compares the differences in the outcomes of the *Young* (eligible) and *Old* (ineligible) women in the years after KP got implemented to differences in outcomes of the *Young* and *Old* women before the launch of KP. To keep the age bandwidth of eligible and ineligible women symmetric, we use women ages 25 to 35 as our untreated (*Old*) group. As we are using data from more than one survey year, for any survey year T, $(18 + |T - 2013|)$ years becomes the upper bound of the cut-off age for eligibility. Therefore, for survey year T, the *Young* (that is, eligible or treated) cohort can be defined as someone in the age range: 15 to $(18 + |T - 2013|)$ years. *Old* (or control) cohort is in the age range of $(19 + |T - 2013|)$ to 35 years.

This identification strategy is similar in spirit to that of Duflo (2001) and Muralidharan and Prakash (2017), which also use a cohort-based comparison. However, the age bandwidth of our younger and older cohorts is much broader, which may raise concerns about the choice of our counterfactual. We argue in favor of our identification strategy as follows. First, as a robustness check, we vary the bandwidth of our age cohorts to check the sensitivity of our results and observe that compared to other bandwidth definitions, our baseline results remain reasonably robust and similar in magnitude. Using this broader definition helps us with statistical power as our analysis looks within a state, and our analytical sample is shrunken even more because only a random subsample of 15% of NFHS is asked the state module questionnaire. The second concern could be that younger and older cohorts may have a differential trend in outcomes before the introduction of KP due to some unobservable factors. Therefore, following Muralidharan and Prakash (2017), we test for conditional parallel pre-trends between the two cohorts for outcomes that have remained consistent throughout the different rounds of NFHS (Table A1) by regressing the outcomes of interest on the interaction between the younger cohort dummy and time (years in the pre-period).

Our pre-period sample satisfies our assumption, as we cannot reject the null hypothesis of parallel trends for all outcomes except one (Table A1, column 6). We only reject the null for a secondary education attainment dummy, but the magnitude of the effect size is minimal to introduce substantial bias. Third, another concern could be that the outcomes of our interest may have evolved faster for the younger cohort than the older cohort (Muralidharan & Prakash 2017). This becomes more apparent from Fig. 4, which shows a difference in average levels between the cohorts.

Over time, older and younger cohorts may experience differential returns to both education and marriage. To address this, we employ a second identification strategy that uses spatial variation in program exposure using a triple difference framework.



This figure uses NFHS data prior to the launch of KP for West Bengal. For each of the younger and older cohort, the figure plots the raw means of the outcomes of our interest that have remained consistent over the survey rounds conducted before KP was launched.

Fig. 4 Difference in outcomes between cohorts over time (survey rounds)

3.2.2 Triple-difference estimates

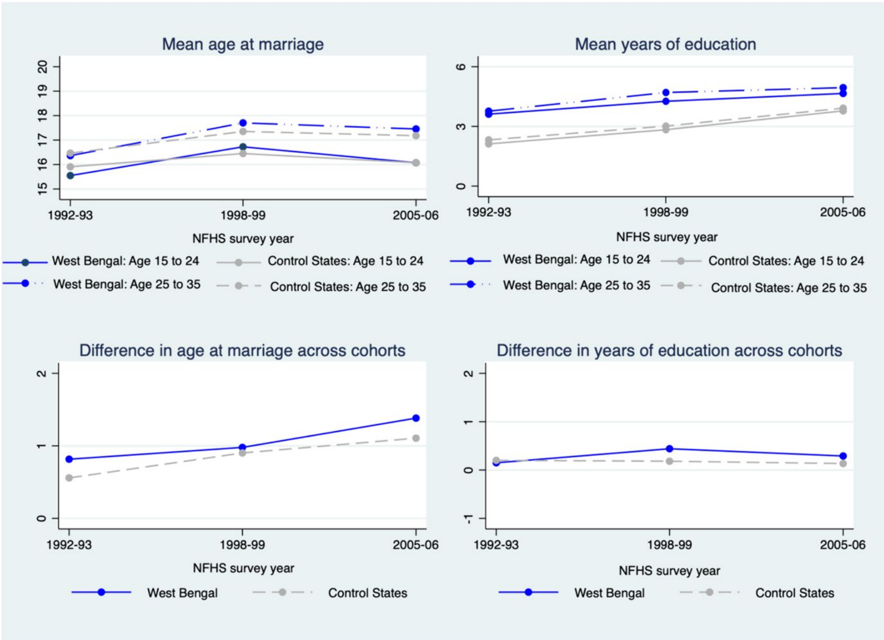
Our second identification strategy uses neighboring states of Bihar, Jharkhand, Orissa, and Assam as control states in a triple difference framework such that the problem of differential rates of empowerment between the two cohorts can be canceled out. For estimating unbiased coefficients using this identification strategy, the underlying assumption is that there is no systematic difference between the younger and older cohort in West Bengal versus the control states before the launch of the program. Figure 5 plots the mean outcomes for West Bengal and control states for years before KP was launched.

We formally test for this by regressing our outcomes of interest on the triple interaction of the younger cohort dummy, a dummy for West Bengal, and time using data before KP's launch (Table A2). We find no evidence of such a systemic difference.

We run the following two regression specifications. Equation (1) is our difference-in-differences (DID) regression specification, and Eq. (2) is our triple difference (DDD) specification.

$$Y_{iht} = \delta_0 + \delta_1 Young_i + \delta_2 (Young_i \times Post_t) + \gamma_t + X_{it}\prime\theta_1 + X_{ht}\prime\theta_2 + u_{iht} \tag{1}$$

$$\begin{aligned} Y_{ihst} = {} & \beta_0 + \beta_1 Young_i + \beta_2 WB_s + \beta_3 (Young_i \times \gamma_t) + \beta_4 (WB_s \times \gamma_t) + \beta_5 (WB_s \times Young_i) \\ & + \beta_6 (Young_i \times Post_t \times WB_s) + \gamma_t + X_{it}\prime\pi_1 + X_{ht}\prime\pi_2 + \epsilon_{ihst} \end{aligned} \tag{2}$$



This figure uses NFHS data prior to the launch of KP for West Bengal and control states. For each of the younger and older cohort, the figure plots the raw means of the outcomes of our interest that have remained consistent over the survey rounds conducted before KP was launched.

Fig. 5 Difference in average outcomes between West Bengal and control states

In specification 1, Y_{iht} is the outcome of interest for individual i, belonging to household h, for survey year t. $Young_i$ is a dummy variable that takes the value 1 for the younger cohort and 0 for a person belonging to the older cohort. $Post_t$ is a dummy variable taking the value 1 for years post-2013 (that is, for NFHS 4 and 5) and 0 otherwise (for NFHS 3). The coefficient of interest is δ_2, associated with the double interaction between the cohort and period dummy, and captures the effect of KP. Individual controls like age, height-for-age z-score, and religion are included in vector $X_{it}\prime$. Household level controls like age and sex of household head, household size, and household asset dummies are included in $X_{ht}\prime$. We control for survey year fixed effects denoted by γ_t. Standard errors are clustered using wild bootstrapping at the birth-year level.

In specification 2, all other variables remaining same, WB_s is a dummy variable taking the value 1 if the respondent belongs to West Bengal and 0 for the control states. In addition, in this specification, the subscript s denotes the state where the individual i belongs to. The coefficient of interest is β_6, which corresponds to the triple interaction between the cohort, period, and West Bengal dummy. Standard errors are clustered using wild bootstrapping at the state-birth year level.

To lend additional support to our difference-in-differences and triple-difference specifications, we also use two types of matching with differences-in-differences estimators. Even though matching on observables is likely to introduce bias due to differences in unobservables between the younger and older cohorts, combining it with difference-in-differences cancels out biases brought by time-invariant confounders (Heckman et al. 1997).

3.2.3 DID and DDD with kernel-based propensity score matching or inverse probability weighting

First, we follow Blundell and Dias (2009) and use a kernel-based propensity score matching with a difference-in-differences treatment effects estimator in a repeated cross-section setting. It should be noted that since the data is not longitudinal, the treated and control units cannot be followed over the pre- and post-periods. Each of the three control groups—the younger cohort in the pre-period and the older cohort in both periods—is matched to the younger units in the post-period separately. In other words, this ensures that for every eligible woman in the post-period, a counterfactual is identified in each of the three non-treated sample before and after the treatment, constituting the overlapping support region. Three sets of weights are then used to estimate the ATT using difference-in-differences. To get the correct standard errors and confidence intervals, we use bootstrapping to take into account the variation arising from the estimation of propensity scores.

Second, we also use an inverse probability weighting (IPW) difference-in-differences estimator with stabilized weights (Abadie 2005). The inverse probability weights are normalized such that the sum of weights within a group equals 1. Intuitively, this method reweights the data to accommodate variations in the response rates. The untreated units are re-weighted in a manner that accounts for the fact that individuals in the younger cohort with low propensity scores are underrepresented in the treatment group and overrepresented in the control group. Like the previous method, bootstrapping is implemented to arrive at unbiased standard errors.

We use ethnicity, religion, household size, age, and sex of household head as observables to match on. These covariates are likely to be correlated with the chances of being a program beneficiary and our outcomes of interest. These are also unlikely to be affected because of KP. We do not match the respondent's age as it determines non-participation in KP with certainty and would violate the overlapping support requirement.

To summarize, our matching-with-difference-in-differences methodology, in essence, would reweight observations such that, conditional on the covariates, younger and older cohorts have identical distributions of the probability of treatment. In this way, this exercise would identify correct counterfactuals from the untreated units for every treated unit. An important caveat is that treatment probability is unknown because of the lack of administrative data. In the program's initial years, only students studying in government schools were eligible to be beneficiaries. NFHS does not capture information on whether the respondent went to a public or private school. We also cannot use the fact that eligibility required some level of secondary school enrollment because that was directly affected by the program and would raise selection concerns. As a result, instead of using any endogenous eligibility criteria for being a program beneficiary, we only stick to using the exogenous age eligibility condition to estimate the ITT effects. In doing so, through the matching exercise, we are matching the probability of receiving the treatment for someone in the younger cohort with that of an older cohort unit conditional on some household-level covariates. Since the likelihood of treatment in our set-up is only determined by age, this may raise concerns regarding the matching methodology. We argue that despite this, our motivation to use matching combined with difference-in-differences and the triple-difference is only to demonstrate the robustness of our baseline results.

The weighted sample means for some of the relevant individual and household-level characteristics from our analytical sample are reported in Appendix Table A3. Fifty-two percent of women in the total sample have at least some secondary education level. The average age of the respondents in our full sample is close to 24.5 years, and the average age at first marriage is around 17 years. Nearly 71% of respondents reside in rural areas, and a vast majority (67%) belong to the Hindu religion. Only a small proportion of households (about 13%) have a female head, with the average age of a household head standing at 44.5 years and the average household size at about 5.1 members (Table 2).

Table 3 analyzes the sample further by reporting the difference in means of the covariates between older and younger cohorts before the KP was launched. Although the caste compositions do not seem to be different between the older and younger cohorts, the share of Muslims seems to have increased over the years. This seems to be a general trend due to the difference in fertility rates over the years. Since we control for the religious affiliations of women at the individual level, which is mostly time invariant, our estimates of the program effect should not be confounded by such a differential growth of population. The distribution of assets may have been different between older and younger cohort, but our estimates are conditional on the asset

Table 2 Description of Outcome Variables

Outcome variable	Description
Mobility: Health	If the respondent is allowed to go to a health facility alone
Mobility: Market	If the respondent is allowed to go to the market alone
Mobility: Outside community	If the respondent is allowed to go outside her village/community alone
Beating is justified if the wife goes out without telling husband	If the respondent agrees with the given statement
Beating is justified if wife argues with her husband	If the respondent agrees with the given statement
Beating is justified if wife disrespects in-laws	If the respondent agrees with the given statement
Beating is justified if wife refuses sex with husband	If the respondent agrees with the given statement
Beating is justified if wife burns food	If the respondent agrees with the given statement
Beating is justified if wife is unfaithful	If the respondent agrees with the given statement
Beating is justified if wife neglects children	If the respondent agrees with the given statement

Note: All variables are generated from pooled cross-section of NFHS data and equal 1 if yes and 0 otherwise

category that a woman belongs to. Unless there is a major change in one women's asset category post-KP, we should not expect our estimates to be confounded by this difference. Since we use assets and not income, we may safely assume that a change in the distribution of assets may need a longer period than the one considered here. We do not seem to find a significant difference in any other covariates between the cohorts before the KP program.[9]

Table 4 provides summary statistics for the outcome variables of interest: mobility measures and perceptions toward domestic violence. More than 55% of women in the entire sample have some form of mobility. Around 18% of women feel that it is justified for a husband to beat his wife if she goes out without informing him, 26–27% think that beating is justified if a wife neglects her children or if she argues with her husband, 12% believe that beating is justified if the woman burns food or refuses to have sexual intercourse. About 33% of women believe it is warranted for a husband to beat his wife if she disrespects her in-laws, and 20% feel that beating is justified when a wife is unfaithful to her husband. On average, the mobility-related outcomes seem to have significantly higher values for younger cohorts after KP (column 3) and compared to before KP (column 2). The perceptions-related outcomes too show some improvement for younger cohort after KP as compared to before, but the difference is not as high as the mobility outcomes. Sixty-seven percent of the younger cohort owns bank account after KP as compared to only 8% before KP.

[9] The sample means and standard deviations of covariates for each type of separate sample are presented in Appendix Table A3.

Table 3 Summary Statistics of Covariates with t-test of Difference in Means - Younger and Older Cohorts before KP

	Younger cohort before KP	Older cohort before KP	Difference in means (2) – (1)
Variables	(1)	(2)	(3)
At least 8 years of schooling	0.32	0.26	-0.065***
	(0.468)	(0.437)	
Age at marriage	15.83	16.83	1.006***
	(2.215)	(3.427)	
Age	19.91	29.79	9.876***
	(2.772)	(3.143)	
Rural	0.73	0.71	-0.021
	(0.442)	(0.453)	
SC	0.27	0.26	-0.009
	(0.444)	(0.439)	
ST	0.06	0.06	-0.004
	(0.239)	(0.229)	
OBC	0.04	0.04	0.002
	(0.186)	(0.191)	
Hindu	0.66	0.74	0.073***
	(0.473)	(0.440)	
Muslim	0.33	0.25	-0.072***
	(0.469)	(0.435)	
Asset index = Poorest	0.31	0.30	-0.011
	(0.462)	(0.458)	
Asset index = Poorer	0.30	0.25	-0.053***
	(0.458)	(0.431)	
Asset index = Middle	0.20	0.24	0.040**
	(0.398)	(0.426)	
Asset index = Richer	0.10	0.09	-0.007
	(0.300)	(0.289)	
Asset index = Richest	0.09	0.13	0.031***
	(0.292)	(0.331)	
Female household head	0.14	0.12	-0.013
	(0.344)	(0.329)	
Age of household head	42.13	41.66	-0.471
	(15.93)	(12.60)	
Household size	5.10	4.99	-0.113
	(2.442)	(2.034)	
N	1357	1807	3164

Sample is NFHS 3—surveyed before KP was launched. It reports summary statistics of the analytical sample of West Bengal. Columns 1 and 2 report the means and standard deviations (in parentheses) of covariates for the younger and older cohorts respectively, before KP was launched. Column 3 reports the difference in means between columns 2 and 1, indicated by their significance levels using t-test for the difference in means

$^{*}\ p<0.1$, $^{**}\ p<0.05$, $^{***}\ p<0.01$

Table 4 Summary Statistics of Outcomes of Interest

	Full sample	Younger cohort after KP	Younger cohort before KP	Older cohort after KP	Older cohort before KP
Dependent variable	(1)	(2)	(3)	(4)	(5)
Mobility: Health facility	0.57	0.52	0.32	0.69	0.58
	(0.496)	(0.500)	(0.468)	(0.462)	(0.493)
Mobility: Market	0.55	0.56	0.29	0.67	0.50
	(0.498)	(0.497)	(0.453)	(0.469)	(0.500)
Mobility: Outside community	0.56	0.59	0.22	0.71	0.43
	(0.499)	(0.491)	(0.413)	(0.453)	(0.495)
Beating is justified if wife goes out without telling	0.18	0.16	0.21	0.19	0.19
	(0.376)	(0.366)	(0.409)	(0.393)	(0.392)
Beating is justified if wife neglects children	0.26	0.26	0.29	0.26	0.25
	(0.429)	(0.437)	(0.453)	(0.440)	(0.432)
Beating is justified if wife argues with her husband	0.27	0.27	0.24	0.30	0.21
	(0.426)	(0.444)	(0.430)	(0.457)	(0.410)
Beating is justified if wife refuses sex	0.12	0.10	0.11	0.13	0.11
	(0.310)	(0.303)	(0.316)	(0.335)	(0.316)
Beating is justified if wife burns food	0.12	0.10	0.14	0.11	0.14
	(0.313)	(0.298)	(0.351)	(0.309)	(0.345)
Beating is justified if wife in unfaithful	0.20	0.21	0.15	0.23	0.15
	(0.381)	(0.409)	(0.361)	(0.422)	(0.358)
Beating is justified if wife disrespects in-laws	0.33	0.31	0.39	0.32	0.34
	(0.463)	(0.461)	(0.488)	(0.467)	(0.473)
Has own bank account	0.45	0.67	0.08	0.57	0.16
	(0.490)	(0.472)	(0.277)	(0.496)	(0.369)
N	6716	1561	1357	1991	1807

The table uses data from NFHS 3, 4, and 5 and reports the weighted sample means of all variables from the analytical sample of West Bengal. Standard deviations are reported in parentheses

4 Results

Table 5 presents the main results from the difference-in-differences (DID) and triple-difference (DDD) designs, respectively. It reports the estimated effects of KP from all six types of estimations. The DID (or DDD) with covariates results are reported in column 1 (or column 4). Columns 2 and 3 report the results from matching with DID—using propensity score matching and inverse probability weighting, respectively. These two types of matching estimates using DDD methods are reported in columns 5 and 6, respectively.

In Table 5, the first row shows the impact of KP on health mobility—that is, if the respondent is allowed to go to a health facility alone. The estimated coefficients are positive and significant across different specifications, and the estimate sizes remain reasonably consistent. The DID estimates in columns 1–3 imply that women exposed to KP are about 7.5 to 8.7 percentage points more likely to travel alone to a health facility. This is equivalent to nearly a 13–15% increase from the baseline of 58%. The second row estimates the impact of KP on a respondent's marketplace mobility—that is, if the respondent is allowed to go to a market facility alone. The DID estimates (in columns 1–3) imply that women exposed to KP are about 7.7 to 8.6 percentage points more likely to travel alone to a health facility. This is equivalent to nearly a 13–15% increase from the baseline of 57%. When looking at the effects on outside-community autonomy, we see that the program increases the likelihood of a respondent going outside her immediate community alone by about 7.1 to 7.9 percentage points (columns 1–3 of the third row). This roughly translates to a 16–18% increase from the baseline of 43%.

We find a significant impact of KP in revising perceptions about domestic violence in some of the components. In our DID estimates, women exposed to KP are about 4.3 percentage points less likely to justify wife-beating if a woman went out without informing her husband. This translates to approximately a 23% decrease from the baseline of 19%. The program decreases the likelihood of a respondent justifying wife-beating if a woman argues with her husband by about 4.6 percentage points, which is a 22% decline from a baseline of 21% (column 1). Women exposed to KP are about 5 percentage points less likely to justify wife-beating if a woman disrespects her in-laws. This translates to approximately 15% decrease from the baseline of 34%. We get indications of some negative effects of the program on justifying domestic violence in instances where a woman is unfaithful, neglects children, refuses sex, or does not cook properly. However, the point estimates are imprecise across all specifications of the DID (columns 1–3).

We see a very similar picture when comparing West Bengal with control states in the last three columns of Table 5. The point estimates from triple-difference and matching estimators are very similar in magnitude to their double-difference counterparts for the Health and Market-related mobility outcomes. We see that the program significantly improves mobility measures and some perceptions of domestic violence. More specifically, women exposed to KP are about 9.2 to 10.2 percentage points more likely to travel alone to a health facility, 10.6 to 12.5 percentage points more likely to visit a marketplace alone, and 7.8 to 23 percentage points more likely to go outside their

Table 5 Coefficient estimates from Difference in Difference (DID) and Triple Difference (DDD) – Combined sample of those currently in and out of the program

Dependent variable	DID (1)	Kernel-based PSM DID (2)	Stabilised IPW DID (3)	Triple Difference (DDD) (4)	Kernel-based PSM DDD (5)	Stabilised IPW DDD (6)
Mobility: Health	0.075***	0.085***	0.087***	0.092***	0.102**	0.092***
	(0.033)	(0.025)	(0.024)	(0.019)	(0.042)	(0.034)
Mobility: Market	0.077***	0.086***	0.085***	0.106***	0.125***	0.122***
	(0.021)	(0.026)	(0.024)	(0.024)	(0.024)	(0.023)
Mobility: Outside community	0.071***	0.078***	0.079***	0.078***	0.237***	0.235***
	(0.020)	(0.021)	(0.023)	(0.028)	(0.027)	(0.029)
Beating justified if wife goes out without telling	-0.043***	-0.042**	-0.046***	-0.045***	0.005	0.002
	(0.015)	(0.018)	(0.017)	(0.017)	(0.020)	(0.017)
Beating justified if wife argues with husband	-0.046***	-0.041***	-0.052***	-0.031	-0.065***	-0.064***
	(0.017)	(0.016)	(0.021)	(0.021)	(0.020)	(0.017)
Beating justified if wife disrespects in-laws	-0.050***	-0.046*	-0.055***	-0.036*	0.016	0.018
	(0.015)	(0.026)	(0.022)	(0.020)	(0.022)	(0.021)
Beating justified if wife refuses sex	-0.021**	-0.015	-0.016	-0.021	0.003	0.002
	(0.009)	(0.015)	(0.014)	(0.016)	(0.010)	(0.010)
Beating justified if wife burns food	-0.009	-0.007	-0.009	-0.012	-0.028**	-0.026**
	(0.013)	(0.016)	(0.016)	(0.019)	(0.013)	(0.012)
Beating justified if wife is unfaithful	-0.012	-0.007	-0.013	-0.009	-0.071***	-0.070***
	(0.016)	(0.019)	(0.018)	(0.019)	(0.018)	(0.016)
Beating justified if wife neglects children	-0.027*	-0.023	-0.029	-0.009	-0.039**	-0.038*
	(0.014)	(0.023)	(0.02)	(0.020)	(0.019)	(0.020)
Observations	6716	6716	6716	42355	42355	42355

Data: NFHS 3, 4, and 5. Sample of women between 15 – 35 years from West Bengal (for DID estimates in first 3 columns) along with control states (for DDD estimates in last 3 columns). Estimates with covariates (specification 1 in column 1, specification 2 in column 4), kernel-based propensity score matching with DID (column 2) and DDD (column 5), and normalised inverse probability weights with DID (column 3) and with DDD (column 6). All outcomes are binary, taking value 1 if the respondent responds with yes, and 0 otherwise. Covariates in Columns 1 and 4 include age, HAZ scores, dummies for scheduled castes, scheduled tribes, other backward classes, Hindu, Muslim and asset index quantiles. For other columns, we match on ethnicity, religion, household size, age and sex of household head. Bootstrapped standard errors clustered at the birth-cohort level are in parentheses for all the specifications. $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$

community alone. Overall, the main findings indicate that KP successfully increases women's mobility outside. It is important to note that point estimates from the simple difference-in-differences (or triple-difference) specifications with covariates are similar in magnitudes to estimates from other specifications in mobility outcomes.

For perception related to physical violence, the DDD estimates are higher in the PSM-DID and IPW-DDD estimates where we see a 6.5 percentage point lower chances of women justifying the incidence of beating due to arguments with husbands; about 2.6–2.8 percentage point less, 7 percentage point less, and 4 percentage point less, if wife burns food, unfaithful, or neglects children, respectively. However, we do not have strong evidence of treatment effects for perception in physical violence outcomes because the DDD estimates have much smaller magnitudes in most of the perception-related outcomes in our preferred specification (column 4).

5 Robustness checks

Apart from using matching methods to complement our double- and triple-difference designs, we also perform several robustness checks to justify our baseline results. These are explained in following three subsections.

5.1 Sensitivity analysis

First, we check the sensitivity of our estimates by varying the bandwidth of age cohorts. Our primary analysis compares a younger cohort aged 15 to 24 with an older cohort aged 25 to 35. However, our estimates will be biased if a particular bandwidth selection drives our results. To eliminate such concerns, we use different bandwidths to check if our results remain robust to such alterations. Tables 6 and 7 show the results from this robustness check for our double- and triple-difference specifications, respectively.

For both tables, columns 1 to 5 report results from re-running our specifications for different subsamples by redefining our cohort bandwidths, whereas column 6 reports the estimates from our initial analysis. Compared with other age-cohort bandwidths, our original estimates remain reasonably robust and close to the estimates from different bandwidth definitions. This analysis helps us conclude that no particular bandwidth selection drives our initial results.

5.2 Alternative measures

Second, our baseline measures of mobility and perceptions are only binary indicators. This may be too restrictive a parameter to capture nuanced outcomes of true empowerment. Furthermore, instead of looking at different types of mobility disjointedly, it would make more intuitive sense to combine the related outcomes into indices to get a holistic picture. We construct two alternative indices for mobility and perceptions (Duflo et al. 2007; Erten & Keskin 2018; Kling

Table 6 Robustness Checks - Sensitivity Analysis (Difference in Difference)

	20–29	19–30	18–31	17–32	16–33	15–35
Age band	(1)	(2)	(3)	(4)	(5)	(6)
Mobility: Health	0.112***	0.126***	0.098***	0.106***	0.100***	0.075***
	(0.025)	(0.022)	(0.03)	(0.030)	(0.029)	(0.033)
Mobility: Market	0.065**	0.094***	0.081***	0.087***	0.085***	0.077***
	(0.027)	(0.019)	(0.026)	(0.026)	(0.021)	(0.019)
Mobility: Outside community	0.088***	0.095***	0.073***	0.076***	0.069***	0.071***
	(0.027)	(0.022)	(0.027)	(0.024)	(0.024)	(0.020)
Beating is justified if wife:						
- goes out without telling	-0.054**	-0.046***	-0.056***	-0.051***	-0.046***	-0.043***
	(0.021)	(0.017)	(0.013)	(0.014)	(0.015)	(0.015)
- argues with her husband	-0.068**	-0.059**	-0.073***	-0.058***	-0.058***	-0.046***
	(0.032)	(0.029)	(0.025)	(0.021)	(0.017)	(0.017)
- disrespects in-laws	-0.056**	-0.055**	-0.054***	-0.053***	-0.057***	-0.050***
	(0.028)	(0.022)	(0.019)	(0.019)	(0.016)	(0.015)
- refuses sex	-0.030	-0.034	-0.034*	-0.029**	-0.023	-0.021**
	(0.030)	(0.021)	(0.018)	(0.013)	(0.017)	(0.009)
- burns food	-0.021	-0.023	-0.011	-0.009	-0.005	-0.009
	(0.021)	(0.015)	(0.013)	(0.015)	(0.016)	(0.013)
- is unfaithful	0.000	-0.011	-0.03	-0.028	-0.019	-0.012
	(0.038)	(0.016)	(0.02)	(0.022)	(0.019)	(0.016)
- neglects children	-0.069**	-0.048**	-0.05*	-0.035*	-0.037**	-0.027*
	(0.031)	(0.024)	(0.026)	(0.019)	(0.016)	(0.014)
Observations	3419	4170	4824	5373	5889	6716

The table uses data from NFHS 3, 4, and 5 on eligible women in a household between ages 15 – 35 in West Bengal. It reports the sensitivity of our estimates to altering the age-cohort bandwidth definitions. Column 6 reports our preferred estimates from Difference-in-differences specification. All outcomes are binary variables taking the value 1 if the respondent responds with yes, and 0 otherwise. Covariates include age, HAZ scores, dummies for scheduled castes, scheduled tribes, other backward classes, Hindu, Muslim and asset index quantiles. Bootstrapped standard errors clustered at the birth-cohort level are in parentheses for all the specifications. $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$

et al. 2007). The first alternative measure of mobility takes a simple average of all three mobility outcomes. A higher index value means greater mobility outside the home. Similarly, our first measure of perceptions takes a simple average of all related scenarios asked about perceptions toward wife-beating. A lower index value implies lesser tolerance for domestic violence. The second alternative measure entails constructing z-scores for each mobility category and then taking a simple average of the z-scores to get an index of mobility. A higher index value means greater mobility outside the home. The same approach is repeated to construct an index of perceptions. A lower index value implies lesser tolerance for domestic violence. Table 8 reports the results from this section.

Panel A displays the difference-in-differences results, and panel B shows the triple-difference results. We see that KP positively and significantly impacted the mobility

Table 7 Robustness Checks - Sensitivity Analysis (DDD)

	20–29	19–30	18–31	17–32	16–33	15–35
Age band	(1)	(2)	(3)	(4)	(5)	(6)
Mobility: Health	0.124***	0.139**	0.109**	0.114***	0.112***	0.092***
	(0.035)	(0.041)	(0.037)	(0.031)	(0.023)	(0.035)
Mobility: Market	0.091***	0.125***	0.107***	0.108***	0.111***	0.122***
	(0.024)	(0.034)	(0.039)	(0.040)	(0.035)	(0.023)
Mobility: Outside community	0.095***	0.104***	0.077	0.076***	0.069***	0.235***
	(0.029)	(0.029)	(0.049)	(0.026)	(0.025)	(0.029)
Beating is justified if wife:						
- goes out without telling	-0.048*	-0.056*	-0.061**	-0.054***	-0.049**	0.002
	(0.029)	(0.030)	(0.025)	(0.020)	(0.021)	(0.019)
- argues with her husband	-0.041	-0.041	-0.056*	-0.044**	-0.044*	-0.064***
	(0.059)	(0.030)	(0.034)	(0.022)	(0.024)	(0.023)
- disrespects in-laws	-0.014	-0.036	-0.034*	-0.036	-0.04*	0.018
	(0.043)	(0.033)	(0.020)	(0.030)	(0.022)	(0.017)
- refuses sex	-0.026	-0.031	-0.027	-0.029**	-0.022	0.002
	(0.032)	(0.027)	(0.018)	(0.013)	(0.015)	(0.010)
- burns food	-0.008	-0.02	-0.006	-0.0097	-0.003	-0.026**
	(0.037)	(0.024)	(0.017)	(0.019)	(0.017)	(0.013)
- is unfaithful	-0.003	-0.012	-0.026	-0.021	-0.008	-0.070***
	(0.02)	(0.018)	(0.023)	(0.023)	(0.017)	(0.016)
- neglects children	-0.039	-0.028	-0.022	-0.012	-0.016	-0.038*
	(0.033)	(0.028)	(0.032)	(0.021)	(0.020)	(0.020)
Observations	20,441	25,030	28,823	32,577	36,207	42,355

The table uses data from NFHS 3, 4, and 5 on eligible women in a household between ages 15 – 35 in West Bengal and Control States. It reports the sensitivity of our estimates to altering the age-cohort bandwidth definitions. Column 6 reports our preferred estimates from triple difference specification. All outcomes are binary variables taking the value 1 if the respondent responds with yes, and 0 otherwise. Covariates are same as that of Table 5. Bootstrapped standard errors clustered at the state-birth cohort level are in parentheses for all the specifications. $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$

indices and negatively and significantly impacted the indices for perceptions of wife-beating. Reassuringly, all these results are consistent with our baseline results.

We also present the sensitivity of the outcomes measured by above indices by varying the bandwidth of age cohorts. The estimates presented in Table A4 remain reasonably robust even when the age-bandwidths are varied at the margins.

5.3 Tests of exact randomization

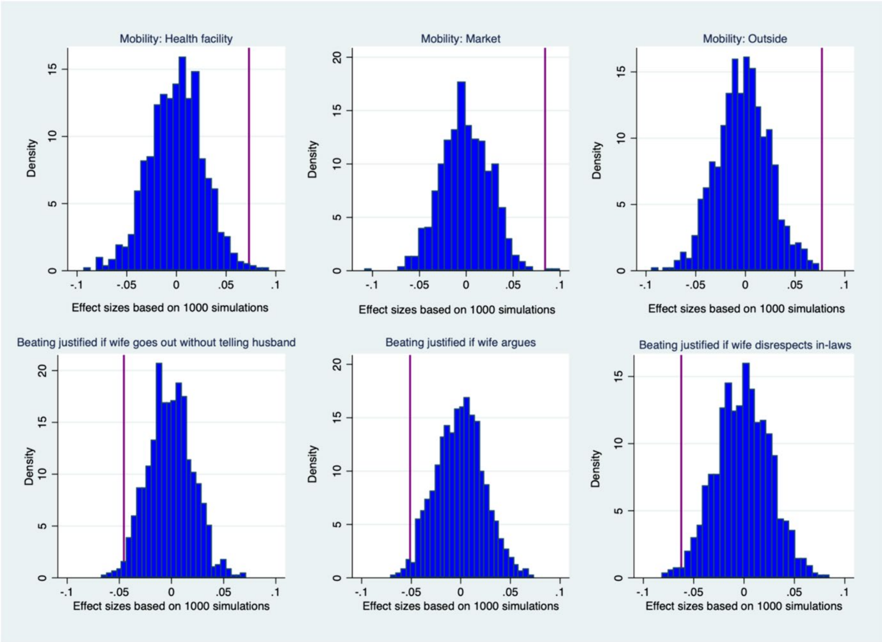
Third, to establish that the program effects of KP drive our reduced form results, we perform two tests of exact randomization, which serve as placebo experiments (Chatterjee and Poddar, 2024).

Table 8 Alternative Measures of Mobility and Perception

Dependent Variable	Simple DID/DDD estimate (1)	PSM DID/DDD estimate (2)	IPW DID/DDD estimate (3)
Panel A: Difference-in-Differences:			
Mobility: Average for all binary indicators	0.074***	0.102***	0.086***
	(0.015)	(0.023)	(0.025)
Mobility: Average for z-scored of all binary indicators	0.149***	0.205***	0.174***
	(0.045)	(0.045)	(0.054)
Perceptions: Average for all binary indicators	-0.03***	-0.056***	-0.038**
	(0.010)	(0.017)	(0.018)
Perceptions: Average for z-scored of all binary indicators	-0.073**	-0.138**	-0.095*
	(0.033)	(0.048)	(0.0)
Panel B: West Bengal and Control States:			
Mobility: Average for all binary indicators	0.092***	0.152***	0.146***
	(0.029)	(0.025)	(0.025)
Mobility: Average for z-scored of all binary indicators	0.185***	0.306***	0.294***
	(0.053)	(0.051)	(0.055)
Perceptions: Average for all binary indicators	-0.023*	-0.024*	-0.024*
	(0.013)	(0.014)	(0.013)
Perceptions: Average for z-scored of all binary indicators	-0.057*	-0.056	-0.054*
	(0.034)	(0.035)	(0.032)

The table uses data from NFHS 3, 4, and 5 on eligible women in a household between ages 15 – 35 in West Bengal and Control States. The robustness of our initial results to alternate indices is reported here. Outcome variables are constructed by either taking a simple average of all binary variables associated with mobility and perceptions, respectively or by taking an average of the z-scores of each of the mobility and perceptions variables. Panel A reports the results across all specifications for our difference-in-difference analysis within West Bengal. Panel B reports the results across all specifications for the triple difference analysis of West Bengal and the Control States. Covariates include age, HAZ scores, dummies for scheduled castes, scheduled tribes, other backward classes, Hindu, Muslim and asset index quantiles. Bootstrapped standard errors clustered at the birth-cohort level. $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$

Instead of using age eligibility–based treatment status for the first test, we generate a simulation of random treatment status assignments. As a result, some women who are eligible for KP, are assigned out of treatment, and some ineligible women are given a treated status. We run our specifications repeatedly 1000 times for each outcome variable, recording the results each time. We hypothesize that if our empirical strategy is correct, results from this randomization of treatment status should yield insignificant results, and their magnitude should be much smaller compared to the estimated impact of KP on the outcomes. For all the variables of interest, Fig. 6 shows the distribution of coefficients from the regressions based on 1000 simulations randomly assigning treatment status to individuals in our sample. We see that most coefficients are centered around zero, whereas our estimated actual program effect on outcomes is much larger. For example, when looking at the placebo experiment for Health Mobility, we observe that only 4.1% and 10.3% of the results are significant at 95% and 90% confidence intervals, respectively. When we run the

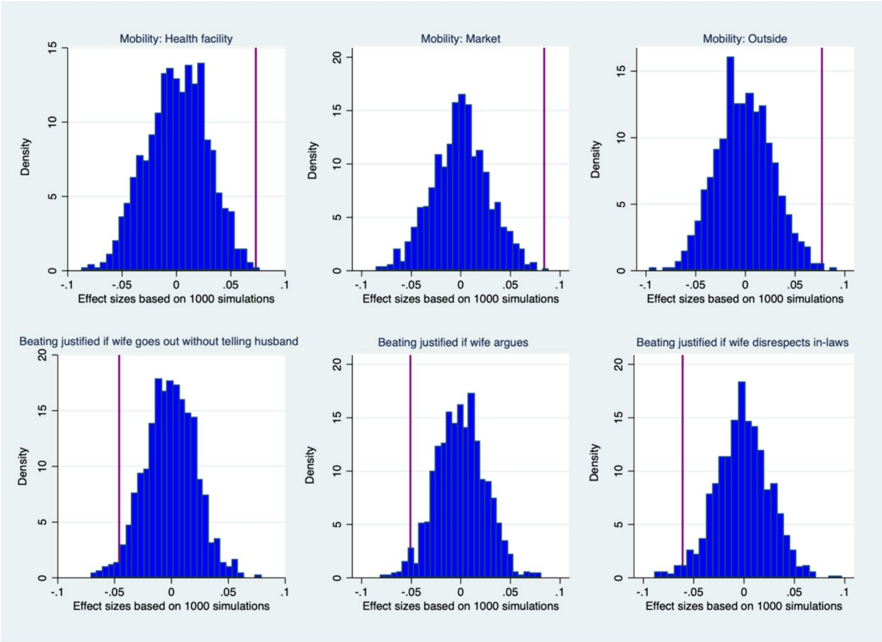


The figure shows the distribution of coefficients from regressions for both mobility and perceptions outcomes based on 1000 simulations randomly assigning treatment status to individuals in our sample. We see that most coefficients are centered around zero, whereas our estimated actual program effect on outcomes is much larger.

Fig. 6 Distribution of coefficients from 1000 simulations based on random assignment of status to individuals

randomization test for perceptions of domestic violence, for example, if the respondent thinks that beating is justified when a wife goes out without telling her husband, we observe that only 5.2% and 10.1% of the results are significant at 95% and 90% confidence intervals, respectively.

For the second test, we randomize the treatment period status of the women in the sample. Our identification strategy rests on the assumption that the difference between ineligible and eligible cohorts should only be different in the pre- and post-treatment periods because of the impact of KP. If results from the treatment period randomization give us insignificant results most of the time, then this would lend further support to the identifying assumption. We again run our specifications repeatedly 1000 times for each of our outcome variables, recording the results. We see that most coefficients are centered around zero, whereas our estimated actual program effect on outcomes is much larger (Fig. 7). For example, when looking at the placebo experiment for Health Mobility, we observe that only 5.7% and 10.5% of the results are significant at 95% and 90% confidence intervals, respectively. When we run the randomization test for perceptions of domestic violence, for example, if the respondent thinks that beating is justified if a wife goes out without telling her husband, we observe that only 5.3% and 9.8% of the results are significant at 95% and 90% confidence intervals, respectively.



The figure shows the distribution of coefficients from regressions for both mobility and perceptions outcomes based on 1000 simulations randomly assigning treatment period status to individuals in our sample. We see that most coefficients are centered around zero, whereas our estimated actual program effect on outcomes is much larger.

Fig. 7 Distribution of coefficients from 1000 simulations based on randomization of treatment period status

6 Mechanisms

This section explores potential channels that drive our main results. An increase in the years of schooling, and more specifically, an increase in secondary education, is a part of the conditionalities of KP. This increment in education can increase expected returns to human capital, expand labor market opportunities, and subsequently alter women's preferences toward marriage, fertility, contraception, and tolerance for violence (Grönqvist & Hall 2013). The extant literature studies the association between women's education and their autonomy, exposure, and attitudes toward domestic violence (al Riyami et al. 2004; Boyle et al. 2009; Moursund & Kravdal 2003; Stickley et al. 2008; Torche 2019; Wang 2016). There is also suggestive evidence that greater accessibility to secondary and higher education improves perceptions of intimate partner violence in a way that access to primary education cannot (Bhuwania & Heymann 2022).

The program also makes it mandatory for beneficiaries to have their bank accounts, ensuring that the targeted beneficiaries directly receive the transfers. As a result, from a very young age, beneficiaries of KP, through these bank account openings in their name, are given access to financial markets. Having a bank account that directly receives the transfer may alter how family members perceive a girl child's worth in a context like India, where the prevailing cultural norm is to treat a girl child like an economic burden. It may also instill a sense of financial empowerment within the beneficiaries. A vast body of literature looks at how women's status in households may improve when given control of resources and access to financial services (Ashraf et al. 2006; Duflo 2012; Dupas & Robinson 2013; Hendriks 2019). The NFHS does not capture information on whether a respondent receives any form of transfer. We proxy receipt of the direct transfer with information on whether the respondent has a bank account in her name, a prerequisite of the program.

We thus hypothesize that an increase in the years of education, coupled with the receipt of transfers leading to financial empowerment, proxied by access to one's bank account, are potential mechanisms behind our results. We investigate these possible channels by directly including them as independent covariates in our difference-in-differences specification. First, we show how our coefficients attenuate upon the inclusion of our mediating variables in our baseline specifications (Table 9). Second, we show that the program had a statistically significant impact on these mediating variables (panel A, Table 10), and third, these mediating variables are significantly associated with our outcomes of interest even after controlling for other variables (panel B of Table 10).

Table 9 shows that the coefficients attenuate with the inclusion of information on total years of education and access to bank accounts, which is true for the indexed outcomes as well. It should be noted that even though the point estimates drop in magnitude when the mechanism variables are included in the baseline specification, the coefficients for perception outcomes are not statistically different in most of the columns of Table 9.

The KP seems to be associated with an increase of 1.37 years of schooling on average, with about 0.15 percentage points higher chance of completing secondary education or having a bank account (panel A of Table 10). Panel B of Table 10 shows that the mediating variables seem to be significantly associated with the outcomes and the indexes.

7 Potential concerns on ITT and a few extensions

7.1 Validating cohort-based eligibility

In the absence of micro-level administrative data on targeting, our analysis is able to estimate the intention-to-treat effects only. However, in this section, we investigate the district-level heterogeneity in actual fund disbursal to check if we could validate our identification strategy that uses cohort-based eligibility. In order to support our strategy that the eligible cohorts are indeed treated, we expect the districts with more eligible cohorts of women to have higher amounts of fund disbursals.

To investigate this, we use three additional databases. First, we extract the information on the total number of allocations for each of the K1 and K2 grants for every district between 2013 and 2019 from the publicly available administrative data in the program's online archive (available at: https://wbkanyashree.gov.in/kp_dashboard_report.php). This information gives us the total number of beneficiaries for

Table 9 Investigating Mechanism by including Years of Education, Completion of Secondary Education, or Access to Bank Accounts as Additional Covariates

Panel A: Dependent variables:	(1)	(2)	(3)	(4)
Mobility: Health	0.075***	0.062**	0.070**	0.055*
	(0.033)	(0.024)	(0.027)	(0.032)
Mobility: Market	0.077***	0.066***	0.074***	0.055***
	(0.021)	(0.024)	(0.025)	(0.021)
Mobility: Outside community	0.071***	0.060***	0.066***	0.051***
	(0.020)	(0.019)	(0.022)	(0.013)
Beating is justified if wife-				
- goes out without telling	-0.043***	-0.029*	-0.033*	-0.041*
	(0.015)	(0.015)	(0.017)	(0.013)
- argues with her husband	-0.046***	-0.029*	-0.034	-0.044**
	(0.017)	(0.016)	(0.022)	(0.022)
- disrespects in-laws	-0.050***	-0.036**	-0.041**	-0.049***
	(0.015)	(0.014)	(0.016)	(0.013)
- refuses sex	-0.021**	-0.009	-0.012	-0.018
	(0.009)	(0.010)	(0.011)	(0.012)
- burns food	-0.009	0.002	-0.0007	-0.007
	(0.012)	(0.014)	(0.014)	(0.012)
- is unfaithful	-0.012	-0.0019	-0.005	-0.008
	(0.019)	(0.018)	(0.021)	(0.020)
- neglects children	-0.027*	-0.013	-0.018	-0.027
	(0.019)	(0.022)	(0.018)	(0.019)
Panel B: Dependent variables are various indices of the above outcomes:				
Mobility:				
Average of z-scores of binary indicators	0.149***	0.127***	0.141***	0.109**
	(0.045)	(0.045)	(0.046)	(0.048)
Average of binary indicators	0.074***	0.063***	0.070***	0.054**
	(0.027)	(0.015)	(0.022)	(0.021)
Perception:				
Average of z-scores of binary indicators	-0.073**	-0.039*	-0.049*	-0.068**
	(0.033)	(0.022)	(0.026)	(0.028)
Average of binary indicators	-0.03***	-0.016	-0.020*	-0.028***
	(0.010)	(0.013)	(0.012)	(0.010)
Potential channel in each column	Baseline	Yrs of educ	Secon educ	Bank acc
Observations	6716	6716	6716	6716

The table uses data from NFHS 3, 4, and 5 on eligible women in a household between ages 15 – 35 in West Bengal. Column 1 reports results from the baseline specification. Column 2 includes years of education as a covariate in our main specification. Column 3 includes a dummy on whether the respondent has some level of secondary education. Column 4 includes information on whether the respondent has her own bank account. Bootstrapped standard errors clustered at the birth-cohort level are in parentheses for all the specifications. $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$

Table 10 KP Effects on the Mediating Variables

Panel A		Mediating Variables	
	Years of education	Secondary education	Bank account
KP effect	1.366***	0.153***	0.149***
	(0.303)	(0.026)	(0.031)
Observations	6716	6716	6716
Panel B			
Effects of Mediating Variables on Outcomes of Interest			
	Years of education	Secondary education	Bank account
Mobility: Health	0.013***	0.074***	0.167***
	(0.001)	(0.013)	(0.012)
Mobility: Market	0.014***	0.078***	0.205***
	(0.001)	(0.013)	(0.011)
Mobility: Outside community	0.018***	0.115***	0.237***
	(0.001)	(0.013)	(0.012)
Beating is justified if wife goes out without telling	-0.009***	-0.062***	-0.016*
	(0.001)	(0.010)	(0.009)
Beating is justified if wife argues with her husband	-0.009***	-0.055***	0.014
	(0.001)	(0.011)	(0.010)
Beating is justified if wife disrespects in-laws	-0.010***	-0.065***	-0.031***
	(0.001)	(0.012)	(0.011)
Beating is justified if wife refuses sex	-0.006***	-0.046***	-0.003
	(0.0009)	(0.008)	(0.007)
Beating is justified if wife burns food	-0.008***	-0.060***	-0.021***
	(0.0009)	(0.008)	(0.007)
Beating is justified if wife is unfaithful	-0.004***	-0.022***	0.012
	(0.001)	(0.010)	(0.009)
Beating is justified if wife neglects children	-0.008***	-0.048***	0.005
	(0.001)	(0.011)	(0.010)
Observations	6716	6716	6716

The table uses data from NFHS 3, 4, and 5 on eligible women in a household between ages 15 – 35 in West Bengal. It gives the effect of KP on the potential mechanisms. Covariates age, HAZ scores, dummies for scheduled castes, scheduled tribes, other backward classes, Hindu, Muslim and asset index quantiles. Bootstrapped standard errors clustered at the birth-cohort level are in parentheses for all the specifications. $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$

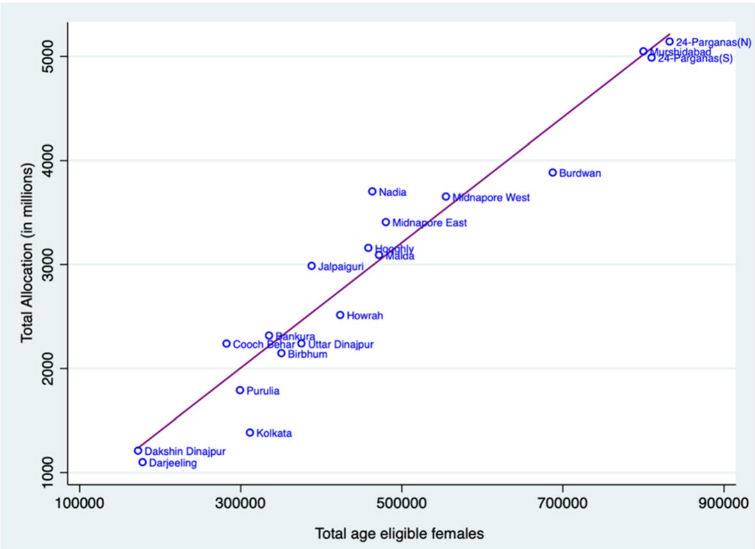
each grant between the years 2013 and 2019, for every district. By using the disbursal amount of the corresponding type of grant, we calculate the total district-level funds disbursement during this 7-year period (that is, by multiplying the number of grants with the corresponding grant amount). Second, using the Census of India, 2011 (https://censusindia.gov.in/census.website/data/census-tables), we get the data on the total population of age-eligible women in a district who are the potential target groups for KP during the same period. Third, we also extract per capita district

GDP data from publicly available sources (Bureau of Applied Economics and Statistics, 2014). We merge these three databases at the district level.

In order to depict a naïve association, if any, between the district-level fund disbursals and district-level number of eligible cohorts, we do a scatter plot with line fit in Fig. 8, where *y*-axis captures the total fund disbursals at the district level and *x*-axis captures the total number of district-level eligible women in the given period. This unconditional plot indicates a positive association between the fund disbursals (according to the state administrative data) and the number of eligible women (according to the census data).

Moreover, the program being demand driven and the small amount of annual disbursement (13.55 USD) having higher marginal utilities among the poor children, the children of richer households or richer areas may not be interested in these transfers. So, we wanted to check if the total fund disbursal at the district level is higher for districts having a higher number of eligible women, conditional on district-level per capita GDP. Our naïve regression estimate of district-level total fund disbursals on the district-level number of eligible women, conditional on district-level per capita GDP, provides us a coefficient of 6022.5 with a standard error close to zero. This indicates that even for a given district-level per capita GDP, having one additional eligible woman in a district is associated with additional disbursal of 6022 INR.

We are unable to use this variation in program intensity to uncover differential program impacts in our difference-in-differences setting because our pre-period data (NFHS



The figure uses census data along with state administrative data for West Bengal. It shows the naïve association between the district-level fund disbursals (y-axis) and district-level number of eligible cohorts (x-axis).

Fig. 8 Scatter plot with line fit between District-level total disbursal (from administrative data) and total eligible (from census data)

3) does not have district-level identifiers. We cannot use NFHS 2 as it only surveyed married women and could lead to selection concerns, as discussed earlier. However, the above exercise helps us to strengthen the justification of our estimated ITT effects.

7.2 Dose–response analysis

It would also be interesting to conduct a dose–response analysis to examine if the treatment effects change as a function of years of exposure to the program. In general, one would expect the treatment effects to be higher with the increase in years of exposure. However, in reality, 1 year of exposure may mean a girl currently studying in the ninth grade has been exposed from the beginning of the program at her eighth grade, or a girl currently studying in twelfth grade might have been exposed in the last year at her higher secondary school only. Since the girl in the latter case would receive a lumpsum transfer just after a year into program, it is difficult to hypothesize who among these two would have a larger impact on mobility or perceptions-related outcomes. Since husband's behavior or the wife's perceptions may take some time to change, years of exposure seems to be important influencing factor too. On the other hand, one cannot ignore the fact that higher transfers may have more impact.

Ideally, if we could estimate the treatment effects of the amount received, conditional on years of exposure, or the treatment effects of years of exposure conditional on the amount received, we could disentangle these two effects. It is also important to note that in this specification, we need to control for the age of the women at the time of the survey because responding to perceptions or behavior-related questions should be sensitive to the age of the respondents. However, Table 1 indicates that we do not have much variation left in the transfer amount if we control for age in survey years and years of exposure. Therefore, we estimate the following two specifications separately.

In the first specification, we regress the outcomes on the years of exposure, conditional on the treatment status of women (eligible versus ineligible is used as a dummy in the regression) and the age of the respondents in the survey year. In the second specification, we regress the outcomes on the amount of potential transfer, conditional on the age in the survey year. In the second specification, we do not control for treatment status because that leaves barely any variability in the amount received (see Table 11).

The OLS estimates as reported in Table 11 indicate the desired effects in a few outcomes; that is, more years of exposure conditional on treatment status (column 1) seems to be positively associated with mobility outcomes and negatively associated with perception outcomes, although the estimations may not be precise in a few cases. The unconditional association between the amount of transfer and the outcomes, too, indicates effects in the desired direction (column 2).

7.3 Threats from differential evolution of outcomes

One threat to identification is that the outcomes of interests may naturally evolve differently over time between young cohorts and older cohorts. One of the reasons could be if improvements in transportation infrastructures would disproportionately

Table 11 Dose–response – Using Years of Exposure or the Amount of Potential Transfer

Dependent variable	Years of exposure to KP (1)	Amount eligible to be received (2)
Mobility: Health	0.028***	0.005***
	(0.006)	(0.001)
Mobility: Market	0.039***	0.006***
	(0.008)	(0.001)
Mobility: Outside community	0.051***	0.007***
	(0.009)	(0.001)
Beating is justified if wife goes out without telling	-0.010***	-0.002***
	(0.002)	(0.000)
Beating is justified if wife argues with her husband	0.001	-0.000
	(0.002)	(0.001)
Beating is justified if wife disrespects in-laws	-0.016***	-0.001***
	(0.004)	(0.000)
Beating is justified if wife refuses sex	-0.002	-0.000
	(0.002)	(0.000)
Beating is justified if wife burns food	-0.005	-0.001***
	(0.003)	(0.000)
Beating is justified if wife is unfaithful	0.005	0.000
	(0.003)	(0.000)
Beating is justified if wife neglects children	-0.016***	-0.001**
	(0.005)	(0.000)
Observations	6716	6716

The table uses data from NFHS 3, 4, and 5 on eligible women in a household between ages 15 – 35 in West Bengal. Column 1 looks at the effect of years of exposure to KP on outcomes of interest unconditional on treatment status. Column 2 looks at the effect of the dose response given by the scholarship amount to be received on outcomes of interest unconditional on treatment status. Both the specifications control for age in survey year. For every individual of a given age, the amount to be received is calculated by adding up the respective annual scholarship amounts (K1 grants) over the years based on exposure duration along with the lumpsum amount (K2 grant) if the individual is 18 and above. All the amounts are normalised by deflating it with the price index based on 2012 prices. Other covariates include HAZ scores, dummies for scheduled castes, scheduled tribes, other backward classes, Hindu, Muslim and asset index quantiles. Bootstrapped standard errors clustered at the birth-cohort level are in parentheses for all the specifications. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

benefit adolescents and younger women. However, due to the absence of district identifiers in NFHS 3, we are unable to merge pre-period data to other datasets that could capture information on local levels of connectivity to schools and other institutions. To check if our estimates are confounded by such effects, we explore two additional information. First, using data from three rounds of DLHS conducted in 2003, 2007–2008, and 2013 for West Bengal, we attempt to check if the average distance for traveling to the nearest railway station or the bus stop has changed differently over a period of time until the policy period (that is, pre-trend in those). Appendix Fig. A1 plots the average distance to the nearest railway station and bus stops for villages in West Bengal over the years and a 95% confidence interval of the same. We see that this mean distance has remained reasonably stable over a decade.

Second, we check if household-level means of access to transportation might have evolved differently over time and how this might affect the assumption of parallel trends. Appendix Table A5 reports tests for pre-trends in household-level access to means of transportation (car, bike, and bicycle) for younger versus older cohorts in West Bengal before the launch of KP. Using the pre-period data from NFHS 1 (1992), 2 (1998–1999), and 3 (2005–2006), we estimate the full model of the above outcomes on the interaction between NHFS year and Younger dummy. We do not find any systematic differences between younger and older women for these outcomes in the period before the KP was launched. The above two findings reassure us that the differential access to infrastructure or transportation facilities may not confound our estimates.

7.4 Isolating immediate and medium-term effects from persistent effects

Throughout our main analyses, we capture the average of effects ranging from immediate to long term—extending up to 6 years after the program ends. One may be interested to see the effects separately for the cohort during the program and the persistent effects after the program ends. Documenting persistent effects after the program ends would have important policy implications as well. Therefore, keeping all other variables, model, and column specifications same as in Table 5, we present the estimates separately for women who are potential beneficiaries at the time of the survey (Table A6) and the women who are not program eligible at the time of the survey but have been eligible (hence, potential beneficiaries) at some point earlier (Table A7). The latter is a sample of women aged 19–35 at the time of the survey, who have been potential beneficiaries in the past and are not currently eligible. The point estimates from Table A7 are able to capture long-term program effects and show that the effects are likely to be persistent even after the cash transfers stop.[10]

8 Conclusion

This paper studies the indirect effects of a conditional cash transfer program targeted to alter the educational participation and marital age of women. We estimate the average impact on women's status in households and perceptions toward domestic violence. Little is known about the impact of cash transfers on young girls in improving their later-life empowerment outcomes once the transfer of payments is

[10] One would also be interested to know if the CCT program affected the direct incidences of physical violence faced by the women. It appears that accessing KP is positively associated with the incidences of emotional and severe violence among married women (Table A6). However, we are unsure how to interpret these estimates as these might be severely biased because of the selection concerns. For example, the KP may have affected marriage decisions, and the women choosing to get married by a certain age are only those who had higher chances of facing domestic violence even without the KP. It could also be a male backlash story as an effect of KP. It is difficult to associate any of these outcomes with KP causally, so we avoid further discussion on these results.

over. Our paper also explores how a persistent improvement in the human capital of CCT beneficiaries due to increased years of schooling combined with other program features may generate positive outcomes beyond the direct program objectives in the long run. We focus on two outcomes associated with women's empowerment, which are mobility outside home and perceptions toward domestic violence.

The purest intention-to-treat effects translate into the beneficiaries having higher mobility and revisions in their perceptions regarding domestic violence. Our estimates are robust to different methodological specifications and survive several other robustness and falsification tests. We present suggestive evidence of these results being mediated by access to bank accounts and increased years of schooling, more specifically, by a greater likelihood of having secondary education. In other words, greater years of education coupled with financial autonomy seem to be the potential mechanisms behind these changes. The estimates of higher probability of having one's bank account points to desired program outcomes because the program beneficiaries are necessitated to have a bank account in their names. An exploratory analysis of administrative data on KP funds disbursement strengthens our strategy of targeting eligible girls as treated because the funds allocation to districts seems to be consistent with the number of eligible women of the district.

Our dose–response analysis suggests that higher years of exposure or higher amount of total transfers is associated with better outcomes. At the time of this study, it has been about 9 years since the implementation of KP. It has only been around 3 years since eligible girls who had complete exposure to the program received the lumpsum transfer. However, changes in social and cultural norms surrounding the age at which a girl is married, or changes in the perceptions about rights, are much more gradual.[11] With consistent efforts, we expect that the effects on women's empowerment, as observed through the indicators in this paper, will be amplified over time.

Supplementary Information The online version contains supplementary material available at https://doi.org/10.1007/s00148-024-01045-4.

Acknowledgements The authors thank the seminar participants for their helpful comments at the Asia Meeting of the Econometric Society-2023 held at IIT-Bombay, 17th Annual Conference on Economic Growth and Development at the Indian Statistical Institute-New Delhi 2022, Annual Conference on Contemporary Issues in Development Economics 2022 at the Jadavpur University, Delhi Winter School 2022, and the PACDEV 2023 held at the University of Washington, Seattle. The authors also thank Dan Maggio, editor Kompal Sinha, and two anonymous reviewers for their helpful and encouraging comments on an earlier version of this paper.

Funding The authors have no relevant financial or non-financial interests to disclose.

Declarations

Conflict of interest The authors declare no competing interests.

[11] If CCTs are able to change perception or behavior among women, a longer run effect could be a positive change in men's behavior or perception too.

References

Abadie A (2005) Semiparametric difference-in-differences estimators. Rev Econ Stud 72(1):1–19

Adato M, De la Briere B, Mindek D, Quisumbing AR (2000) Final Report: The impact of PROGRESA on women's status and intrahousehold relations. Int Food Policy Res Inst (No. 600–2016–40133)

Al Riyami A, Afifi M, Mabry RM (2004) Women's autonomy, education and employment in Oman and their influence on contraceptive use. Reprod Health Matters 12(23):144–154

Alam A, Baez JE, Del Carpio XV (2011) Does cash for school influence young women's behavior in the longer term? Evidence from Pakistan. Policy Research Paper No. 5669, Washington DC, World Bank

Almås I, Armand A, Attanasio O, Carneiro P (2018) Measuring and changing control: women's empowerment and targeted transfers. Econ J 128(612):F609–F639

Angrist J, Bettinger E, Kremer M (2006) Long-term educational consequences of secondary school vouchers: evidence from administrative records in Colombia. Am Econ Rev 96(3):847–862

Armand A, Attanasio O, Carneiro P, Lechene V (2020) The effect of gender-targeted conditional cash transfers on household expenditures: evidence from a randomized experiment. Econ J 130(631):1875–1897

Ashraf N, Karlan D, Yin W (2006) Tying Odysseus to the mast: evidence from a commitment savings product in the Philippines. Q J Econ 121(2):635–672

Baez JE, & Camacho A (2011) Assessing the long-term effects of conditional cash transfers on human capital: evidence from Colombia. IZA Discussion Paper No. 5751, Institute of Labor Economics, Bonn, Germany

Baird S, McIntosh C, Özler B (2011) Cash or condition? Evidence from a cash transfer experiment. Q J Econ 126(4):1709–1753

Baird S, McIntosh C, Özler B (2019) When the money runs out: do cash transfers have sustained effects on human capital accumulation? J Dev Econ 140:169–185

Bandiera O, Buehren N, Burgess R, Goldstein M, Gulesci S, Rasul I, Sulaiman M (2020) Women's empowerment in action: evidence from a randomized control trial in Africa. Am Econ J Appl Econ 12(1):210–259

Baranov V, Cameron L, Contreras Suarez D, Thibout C (2021) Theoretical underpinnings and meta-analysis of the effects of cash transfers on intimate partner violence in low-and middle-income countries. The Journal of Development Studies 57(1):1–25

Bastagli F, Hagen-Zanker J, Harman L, Barca V, Sturge G, Schmidt T, Pellerano L (2016) Cash transfers: what does the evidence say. A rigorous review of programme impact and the role of design and implementation features. London: Overseas Dev Inst ODI 1(7):1

Bhuwania P, Heymann J (2022) Tuition-free secondary education and women's attitudes toward intimate partner violence: evidence from Sub-Saharan Africa. SSM-Population Health 17:101046

Blundell R, Dias MC (2009) Alternative approaches to evaluation in empirical microeconomics. J Hum Resour 44(3):565–640

Bobonis GJ (2011) The impact of conditional cash transfers on marriage and divorce. Econ Dev Cult Change 59(2):281–312

Bobonis GJ, González-Brenes M, Castro R (2013) Public transfers and domestic violence: the roles of private information and spousal control. Am Econ J Econ Pol 5(1):179–205

Bonilla J, Zarzur RC, Handa S, Nowlin C, Peterman A, Ring H, Seidenfeld D (2017) Cash for women's empowerment? A mixed-methods evaluation of the government of Zambia's child grant program. World Dev 95:55–72

Boyle MH, Georgiades K, Cullen J, Racine Y (2009) Community influences on intimate partner violence in India: women's education, attitudes towards mistreatment and standards of living. Soc Sci Med 69(5):691–697

Buchmann N, Field E, Glennerster R, Nazneen S, Wang XY (2023) A signal to end child marriage: theory and experimental evidence from Bangladesh. American Economic Review 113(10):2645–2688

Bureau of Applied Economics and Statistics (2014) State Domestic Product and District Domestic Product of West Bengal, 2013-14. Department of Statistics and Program Implementation, Government of West Bengal

Chatterjee S, Poddar P (2024) Women's empowerment and intimate partner violence: evidence from a multidimensional policy in India. Econ Dev Cult Change 72(2):801–832

Das U, Sarkhel P (2023) Does more schooling imply improved learning? Evidence from the Kanyashree Prakalpa in India. Econ Educ Rev 94:102406
Deshpande A, Singh J (2021) Dropping out, being pushed out or can't get in? Decoding declining labour force participation of Indian women. IZA Discuss Paper 14639
Dey S, Ghosal T (2021) Can conditional cash transfer defer child marriage? Impact of Kanyashree Prakalpa in West Bengal. Warwick Economics Research Papers, India, p 1333
Duflo E (2001) Schooling and labor market consequences of school construction in Indonesia: evidence from an unusual policy experiment. American Economic Review 91(4):795–813
Duflo E (2012) Women empowerment and economic development. J Econ Lit 50(4):1051–1079
Duflo E, Dupas P, Kremer M (2015) Education, HIV, and early fertility: experimental evidence from Kenya. American Economic Review 105(9):2757–2797
Duflo E, Glennerster R, Kremer M (2007) Using randomization in development economics research: a toolkit. Handbook of Development Economics, 4, 3895–3962, edited by Schultz T. PaulStrauss John A. (Amsterdam: Elsevier, 2007)
Dupas P, Robinson J (2013) Savings constraints and microenterprise development: evidence from a field experiment in Kenya. Am Econ J Appl Econ 5(1):163–192
Dutta A, Sen A (2020) Kanyashree Prakalpa in West Bengal, India: justification and evaluation. International Growth Centre. Reference No. S-35321-INC, 1
Ellis F (2012) 'We Are All Poor Here': economic difference, social divisiveness and targeting cash transfers in Sub-Saharan Africa. J Dev Stud 48(2):201–214
Erten B, Keskin P (2018) For better or for worse?: Education and the prevalence of domestic violence in turkey. Am Econ J Appl Econ 10(1):64–105
Eswaran M, Malhotra N (2011) Domestic violence and women's autonomy in developing countries: theory and evidence. Canadian J Econ/Rev Canadienne D'économique 44(4):1222–1263
Fatima A, Sultana H (2009) Tracing out the U-shape relationship between female labor force participation rate and economic development for Pakistan. Int J Soc Econ 36(1/2):182–198
Government of India (2011) Population projections, Report of the Technical Group on Population Projections for India and States, 2011-2036. Census of India, Ministry of Health and Family Welfare
Grönqvist H, Hall C (2013) Education policy and early fertility: lessons from an expansion of upper secondary schooling. Econ Educ Rev 37:13–33
Hahn Y, Nuzhat K, Yang HS (2018) The effect of female education on marital matches and child health in Bangladesh. J Popul Econ 31:915–936
Heckman JJ, Ichimura H, Todd PE (1997) Matching as an econometric evaluation estimator: Evidence from evaluating a job training programme. Rev Econ Stud 64(4):605–654
Hendriks S (2019) The role of financial inclusion in driving women's economic empowerment. Dev Pract 29(8):1029–1038
Hidrobo M, Fernald L (2013) Cash transfers and domestic violence. J Health Econ 32(1):304–319
International Institute for Population Sciences (IIPS) and ICF. (2017). National Family Health Survey (NFHS-4), 2015–16: India. Mumbai: IIPS
International Institute for Population Sciences (IIPS) and ICF. (2021). National Family Health Survey (NFHS-5), 2019–21: India: 1. Mumbai: IIPS
Jayachandran S (2021) Social norms as a barrier to women's employment in developing countries. IMF Econ Rev 69(3):576–595
Kling JR, Liebman JB, Katz LF (2007) Experimental analysis of neighborhood effects. Econometrica 75(1):83–119
Ladhani S, Sitter KC (2020) Conditional cash transfers: A critical Review. Dev Pol Rev 38(1):28–41
Lee-Rife S, Malhotra A, Warner A, Glinski AM (2012) What works to prevent child marriage: a review of the evidence. Stud Fam Plann 43(4):287–303
Millán TM, Macours K, Maluccio JA, Tejerina L (2020) Experimental long-term effects of early-childhood and school-age exposure to a conditional cash transfer program. J Dev Econ 143:102385
Mookherjee D, Napel S (2021) Welfare rationales for conditionality of cash transfers. J Dev Econ 151:102657
Moursund A, Kravdal Ø (2003) Individual and community effects of women's education and autonomy on contraceptive use in India. Popul Stud 57(3):285–301
Muralidharan K, Prakash N (2017) Cycling to school: Increasing secondary school enrollment for girls in India. Am Econ J Appl Econ 9(3):321–350
Neff DF, Sen K, Kling V (2012) The puzzling decline in rural women's labor force participation in India: A reexamination, GIGA working paper no. 196, GIGA Research Unit, Institute of Asian Studies

Özler B, Hallman K, Guimond MF, Kelvin EA, Rogers M, Karnley E (2020) Girl Empower–A gender transformative mentoring and cash transfer intervention to promote adolescent wellbeing: Impact findings from a cluster-randomized controlled trial in Liberia. SSM-Population Health 10:100527

Perez-Alvarez M, Favara M (2023) Children having children: early motherhood and offspring human capital in India. J Popul Econ 36:1573–1606. https://doi.org/10.1007/s00148-023-00946-0

Peruffo M, Ferreira PC (2017) The long-term effects of conditional cash transfers on child labor and school enrollment. Econ Inq 55(4):2008–2030

Rahman RI, Islam R (2013) Female labour force participation in Bangladesh: trends, drivers and barriers. ILO Asia Pacific Working Paper Series, International Labour Organization, Geneva, Switzerland

Sandøy IF, Mudenda M, Zulu J, Munsaka E, Blystad A, Makasa MC, Musonda P (2016) Effectiveness of a girls' empowerment programme on early childbearing, marriage and school dropout among adolescent girls in rural Zambia: study protocol for a cluster randomized trial. Trials 17:1–15

Sen G, Thamarapani D (2023) Keeping Girls in Schools Longer: The Kanyashree Approach in India. Fem Econ 29(4):36–64

Simon CA (2019) The Effect of Cash-based Interventions on Gender Outcomes in Development and Humanitarian Settings, Discussion Paper. UN Women

Stickley A, Kislitsyna O, Timofeeva I, Vågerö D (2008) Attitudes toward intimate partner violence against women in Moscow, Russia. J Fam Viol 23:447–456

Thomas D (1990) Intra-household resource allocation: An inferential approach. J Hum Res 635–664

Torche F (2019) Educational mobility in developing countries, WIDER Working Paper Series, (wp-2019–88), World Institute for Development Economic Research (UNU-WIDER)

Van den Bold M, Quisumbing AR, Gillespie S (2013) Women's empowerment and nutrition: An evidence review. International Food Policy Research Institute, Washington, DC

Wang L (2016) Factors influencing attitude toward intimate partner violence. Aggress Violent Beh 29:72–78

Yoong J, Rabinovich L, Diepeveen S (2012) The impact of economic resource transfers to women versus men: a systematic review, Technical Report, University of London

Zhang Y, Breunig R (2023) Female breadwinning and domestic abuse: evidence from Australia. J Popul Econ 36:2925–2965. https://doi.org/10.1007/s00148-023-00975-9