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Restricting mothers' international migration and human capital investment

Takuya Hasebe [a,*], Yuma Noritomo [b], Bilesha Weeraratne [c]

[a] Sophia Institute for Human Security and Faculty of Liberal Arts, Sophia University, Tokyo, Japan
[b] Charles H. Dyson School of Applied Economics and Management, Cornell University, Ithaca, NY, USA
[c] Institute of Policy Studies of Sri Lanka Colombo Sri Lanka

HIGHLIGHTS

- We analyze the effect of a unique policy in Sri Lanka that restricts mothers with young children from migrating internationally for employment on human capital investment.
- The policy reduced international migration and increased mothers' presence at home.
- It led to improved child health (fewer inpatient stays) and positive spillovers on older siblings' education (reduced grade retention).
- The drop in foreign remittances was offset by an increase in domestic remittances.

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ABSTRACT

International migration offers significant economic opportunities for developing countries, but it can also separate parents from their children, potentially harming child development. This paper examines the effects of restricting mothers' international migration on left-behind children, leveraging a unique Sri Lankan policy that restricted mothers with children under age five from migrating abroad for employment. Using a difference-in-differences approach, the results reveal the following: First, the policy reduces international migration, increasing mothers' presence at home. Second, policy exposure leads to better healthcare outcomes, including a significant reduction in inpatient stays, particularly treatment for illnesses. This improvement appears to result from increased child-care and monitoring by mothers. Although the policy decreases remittances from abroad, this reduction is offset by an increase in domestic remittances. Furthermore, we find evidence of positive spillovers on non-targeted children with younger, policy-targeted siblings, as indicated by reduced grade retention. These findings highlight the trade-offs between a mother's presence and the economic opportunities associated with international migration in shaping human capital development.

1. Introduction

International migration plays an important role in developing countries. Remittances remain a crucial source of external finance for low- and middle-income countries. Officially recorded remittance flows to these countries reached an estimated $656 billion in 2023 (World Bank, 2024). Remittances provide people in low-income countries with higher incomes and greater economic opportunities, and they are linked to improved child outcomes, including better educational outcomes and reduced child labor in sending communities (Alcaraz et al., 2012; Edwards and Ureta, 2003). However, such migration opportunities, especially for mothers, may also adversely affect the children left behind due to reduced interaction or monitoring by their mothers (Cortes, 2015; Meng and Yamauchi, 2017). The overall effect of parental migration—particularly mothers'—on children remains unclear, as it entails a trade-off between the income gains from international migration and the loss of maternal presence at home.

This paper examines the effects of restricting mothers' international migration by leveraging a unique policy in Sri Lanka. Historically, Sri Lanka has sent a large number of female migrants internationally, mostly as domestic workers in the Middle East. This migration has been an important income source for both households and the country. Recently, however, there is a growing concern around well-being of children left behind, where a mother's absence leads to child neglect and various adverse consequences (Abeyasekera and Jayasundere, 2015). This concern led to the policy of the Family Background Report (FBR, hereafter)

* Corresponding author.
Email addresses: thasebe@sophia.ac.jp (T. Hasebe), yn266@cornell.edu (Y. Noritomo), Bilesha@ips.lk (B. Weeraratne).

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in 2013, such that mothers who have children aged below 5 are not allowed to migrate internationally for employment as domestic workers. This provides an ideal setting to test the trade-offs associated with maternal migration on child development.

Our empirical analysis relies on a difference-in-differences (DID) approach to identify and estimate the impacts of restricting mothers' international migration on child development and other household-level outcomes which potentially mediate the impacts, using repeated cross-sectional data from the nationally representative Sri Lanka Household Income and Expenditure Survey. The DID approach employs two dimensions of comparison. First, we compare households with the youngest children above and below age 5, as the restriction applies to mothers with children under 5. Second, we compare outcomes from the years before (the 2009/2010 and 2012/2013 waves) and after the policy (the 2016 wave). We restrict our analytical sample to households whose youngest child is aged 2–10, to mitigate concerns related to fertility responses and differences associated with secondary school enrollment. The DID approach effectively isolates the effects of interest, assuming the parallel trends assumption holds—that households with the youngest child under 5 and those with the youngest child over 5 would have followed similar trends absent the policy.

The results indicate that the policy improved child health by successfully discouraging mothers from migrating internationally and encouraging them to stay at home. First, we find that policy exposure leads to a 1.5 percentage point decrease in the likelihood of any household member migrating abroad, relative to the control mean of 7.6 %. This decrease seems to be driven by the reduction in mothers' migration abroad. Furthermore, this is accompanied by a 1.2 percentage point increase in the likelihood of a mother's presence at home, relative to the control group of 97 %.

Second, the policy induces better child health outcomes. We examine each child's healthcare utilization, intending to capture underlying health conditions, alongside an analysis of chronic diseases. While we do not find statistically significant effects on outpatient visits for treatment for illness or for check-ups, we find that the policy significantly decreases the likelihood of any inpatient stay by 1.1 percentage points, relative to the control mean of 7.2 %, representing a 15 % decrease. Inpatient stays specifically for treatment for illness decrease significantly by 0.8 percentage points from the control mean of 5.7 %, a 14 % reduction. We do not observe significant effects on chronic disease, which may be too early to diagnose in the children included in our study as it may take time for any potential effects to manifest. These findings hold up in a battery of robustness checks, such as prior treatment exposure, different subsamples, and a falsification test using a pseudo cutoff age.

The effects on mothers' presence and improved child health are closely linked, with the increase in child health primarily driven by the mother's greater presence at home. A mother's presence may reduce the likelihood of illness by enabling greater investment in her child's health capital. The policy resulted in mothers staying at home to care for their children, which, in turn, contributed to improved child health outcomes.

We then examine another potential channel: income effect. It is unlikely to be the main driver of the observed effects. Specifically, a negative income effect could arise if restricting mothers' international migration reduces household income due to decreased international remittances. Our results show that while policy exposure significantly reduces remittances from abroad, domestic remittances increase correspondingly. Therefore, there is no statistically significant impact on total remittances (international and domestic combined) or overall household income. Given the absence of changes in household income and Sri Lanka's free universal healthcare system, the observed decrease in inpatient stays is more plausibly attributed to improved health status rather than financial constraints limiting healthcare access.

We next extend our analysis to examine whether the observed effects of improved child health are primarily driven by the policy target—children under 5 years old. It is important to note that the children who were treated consist of not only these target children but also their older siblings. In other words, our estimated effects capture both the direct impact on the target population and the indirect effects on their siblings. Our analysis confirms that the main results are driven by the policy-targeted children, showing significant direct but no indirect effects.

We also examine spillover effects on non-targeted children's educational outcomes, defined only for school-aged children.[1] The results suggest positive spillover effects. While policy exposure does not significantly increase school attendance (control mean: 98.4 %), it is associated with a statistically significant reduction in grade retention. This finding also highlights the importance of current mother–child interactions.

The contribution of this paper is to evaluate the net effects of restricting mothers' international migration and examine the potentially conflicting mechanisms driving these effects. Two features of the Sri Lankan policy make it especially unique and interesting. First, it is restrictive rather than expansionary, allowing us to test for asymmetries relative to the migration-promoting interventions that dominate existing evidence.[2] Second, it is gender-targeted, applying only to mothers. Because much of the literature comes from male-dominant migration contexts,[3] examining gender differences in impacts is essential. Our results complement this work by showing that the restriction-induced increase in maternal presence positively affects human-capital investment in children.

In the remainder of the paper, Section 2 lays out the conceptual framework and reviews key related literature. Section 3 describes the Sri Lankan background and the policy. Section 4 and Section 5 explain the data and econometric strategy, respectively. Section 6 presents the results, and Section 7 concludes.

2. Related literature and conceptual framework

The policy restricting mothers' international migration may affect children through two pathways: (i) a positive effect via increased maternal presence, interaction, and monitoring, and (ii) a negative effect via reduced international remittances.

Mothers' presence may be particularly important in the context of migration. First, existing evidence raises concerns that migration-induced parental separation harms left-behind children. Second, the human capital literature emphasizes that maternal time input is critical for children's human capital development. For example, in the context of China's rural-urban migration, being left behind by both parents significantly impairs children's cognitive development (Zhang et al., 2014) and health (Li et al., 2015), whereas the effects are much smaller and insignificant when only one parent is absent. Meng and Yamauchi (2017) show that maternal urban migration adversely affects the health and educational outcomes of rural children in China. Similarly, Bai et al. (2022) find negative effects of maternal migration on the critical cognitive development of left-behind children in China. Outside China, Botezat and Pfeiffer (2020) link parental migration to serious health problems and depression in Romania. In a comparative study, Viet Nguyen (2016) finds that parental migration negatively affects child health outcomes in India, Peru, and Vietnam, but has no significant effect in Ethiopia. Cortes (2015) examines the gendered impacts of parental migration on education in the Philippines—a country with a high share of female migrants, similar to Sri Lanka—and shows that a mother's absence has a more pronounced detrimental effect than a father's. Unlike previous studies,

[1] In Sri Lanka, the school starting age is 5 years, coinciding with the migration policy cutoff.

[2] For example, several studies have used randomly assigned visa lotteries or cash transfers to identify the causal effects of migration (Bryan et al., 2014; Gibson et al., 2011, 2018; McKenzie and Yang, 2012; Mobarak et al., 2023).

[3] For example, empirical studies have shown that male migration reduces the labor supply for market work among left-behind females in countries such as Mexico (Amuedo-Dorantes and Pozo, 2006), Egypt (Binzel and Assaad, 2011), and Albania (Mendola and Carletto, 2012).

we assess mothers' presence—rather than separation—by leveraging a policy that restricted their international migration.

In the human capital literature, the importance of maternal time investment for children's human capital accumulation has been well recognized (Francesconi and Heckman, 2016). Early childhood—particularly the preschool period—is a critical window for development with lasting consequences (Almond et al., 2018; Luby et al., 2016). For example, Brooks–Gunn et al. (2002) and Waldfogel et al. (2002) show that early maternal employment, which reduces maternal care time, has been associated with lower cognitive development. In the migration context, Bai et al. (2022) find that maternal migration is detrimental to preschool-aged children's cognitive development. The FBR policy primarily targets children under age 5 by restricting their mothers from migrating, with the explicit aim of ensuring maternal presence during this critical stage.

On the other hand, a complementary strand of research documents the importance of remittances for left-behind families—particularly children. Mobarak et al. (2023) examine migration from Bangladesh to Malaysia and find that increased remittances raise the living standards of migrants' families. Carletto et al. (2011) report higher height-for-age z-scores and lower stunting among children in Guatemalan households with a migrant in the United States. Several studies find positive effects of remittances on schooling outcomes (e.g., Antman, 2012; Edwards and Ureta, 2003). In the Philippines, Yang (2008) shows that positive remittance shocks increase human-capital investment, including school attendance and education expenditures, whereas in Mexico, Alcaraz et al. (2012) show that negative remittance shocks raise child labor and reduce school attendance.

Because the two policy channels—greater maternal presence and reduced international remittances—operate in opposite directions, the net effect on children is ultimately an empirical question.

In addition, the policy impacts on maternal presence and international remittances may be attenuated if households cope with the restriction by reallocating labor or adjusting remittances. Migration decisions are made jointly within the family (Stark and Bloom, 1985). In response to a ban on mothers' international migration, households may reallocate labor to offset lost earnings–fathers or other members might migrate abroad or domestically to send remittances back instead of mothers, or increase labor supply and income in local labor markets. Mothers themselves may pursue domestic migration to increase domestic remittances, since the policy restricts only international moves. Households may also draw on existing networks for support–for example, receiving remittances from relatives in migration hubs beyond the immediate family (Stark and Lucas, 1988). Recent evidence suggests that domestic migration networks and intra-household labor diversification can buffer income shocks through remittances (Batista and Vicente, 2023; Bettin et al., 2024). Understanding both mothers' responses and broader household adjustments is therefore essential to assess the overall policy's impact on children. We explore this channel in the mechanism analysis.

3. Background

Sri Lanka has sent more than 200,000 migrants every year since 2002, and the number reached a peak of 300,000 in 2014 (SLBFE, 2018). This scale of international labor migration is notable, considering the total labor force was approximately 8 million in 2014. This migrant labor contributes to the economic development of the nation by sending remittances, which are one of the nation's main sources of foreign revenue earnings. Remittances amounted to US$ 6.4 billion and accounted for 8.3 % of GDP in 2013 (World Bank, 2015). In the same year 2013, approximately 40 % of the migrants were female, and more than 80 % of them worked as domestic workers, with the Middle Eastern countries as popular destinations. Most labor migrants from Sri Lanka are recruited through registered foreign employment agencies under typically two-year contracts, making migration temporary in nature. Re-migration often occurs when migrants fail to meet their intended goals—such as savings or investment targets—or when returnees encounter difficulties in reintegrating into society.

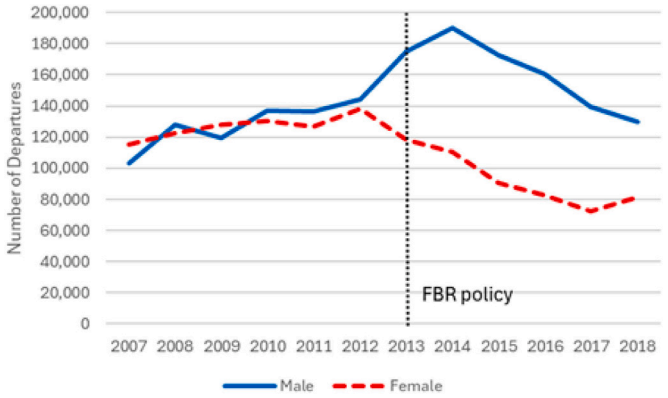

Fig. 1. Trends in departures for foreign employment from 2007 to 2018 in Sri Lanka. Notes: Source: Sri Lanka Bureau of Foreign Employment (2018). "FBR policy" refers to the introduction of the FBR, which began in 2013. The y-axis represents the number of departures, defined as the number of individuals departing for foreign employment who are registered with the Sri Lanka Bureau of Foreign Employment.

Although the migrant labor has brought the benefits to the country, it has also imposed costs on household members left, particularly children. To protect the welfare of children separated from their mothers, the Sri Lankan government took a policy action by issuing Circular 13/2013 in June 2013. The Circular requires female domestic workers to fill out a Family Background Report (FBR) as a pre-departure requirement, which came into effect on 15th July 2013. It, in principle, restricts female domestic workers with children under the age of 5 from migrating internationally for employment. Initially, the Circular covered females who sought employment in domestic worker jobs abroad, but in August 2015, its coverage was expanded to all female employment abroad. There is no FBR requirement for male migrant workers. We use the age of 5 as a policy cut-off to define treatment and control statuses. However, it should be noted that although females with children above 5 years old are able to migrate, they are also required to arrange a substitute caregiver to protect children.[4] This requirement may lead to an underestimation of the policy effects, as compared to a scenario where a clear comparison could be made between those with and without policy exposure. This is because our control households may also benefit from the policy, particularly through its effects on children, thereby diluting the measured impact of the intervention.

As intended, the policy, which was introduced in 2013, led to a sharp decline in female international migration departures, as shown in Fig. 1. In contrast, male migration continued to increase until 2014.

The FBR policy has been the subject of policy and academic discourse. Abeyasekera and Jayasundere (2015) critically analyze the FBR policy from a feminist perspectives, and the gender-specific nature of the policy has made it controversial (UN, 2015). By targeting female workers, the policy constrains their economic opportunities and may infringe on their rights.[5] Some existing literature has also examined the effects of the FBR policy quantitatively. Weeraratne (2016) finds that the policy negatively affected female foreign employment based on official departure statistics. In a subsequent study, Weeraratne (2021) further shows that the decline was mainly concentrated among low-skilled groups, which include domestic workers. By exploiting exogenous variation in

[4] The Circular also establishes minimum age requirements for migrants themselves, which vary by destination regions.

[5] The policy is reported to make female migrants vulnerable at the destination and induce some corruption in the process (Weeraratne, 2016, 2022).

the FBR policy, similar to the approach taken in this study, Peru (2023) finds that the policy's impact on fertility decisions varies by age and wealth of females. However, little is known about its causal effects on children. While not only Sri Lanka but also several countries in South and Southeast Asia have implemented or previously adopted similar restrictive migration policies with comparable objectives (Lenard, 2022), there has been limited causal analysis of their impacts. By providing the causal evidence on how such migration restrictions affect children left behind, this paper speaks to ongoing policy debates on the restriction of international migration, particularly in developing countries.

Child outcomes examined in this study include health and education. A relevant institutional background is Sri Lanka's provision of free universal healthcare and education to its citizens. The public healthcare system, funded by the government, ensures free access to hospitals, clinics, medications, and preventive programs. While public hospitals sometimes experience overcrowding, they remain the primary healthcare providers for the majority. Those who can afford it also have access to private hospitals for quicker service and specialized treatments. Similarly, primary education is free and compulsory from the age of 5 through 5th grade, followed by an additional four years of free and compulsory secondary education, as well as free public higher education. In addition to free tuition, the government supplies free textbooks and uniforms, ensuring widespread access to learning. As with healthcare, private schools also exist and charge tuition for those seeking alternative options. Together, these free public services play a crucial role in the country's social and economic development.

4. Data

We use repeated cross-sectional data from the Household Income and Expenditure Survey (HIES), conducted by the Department of Census and Statistics in Sri Lanka. The survey collects household-level expenditure data and individual-level income information, along with some demographic characteristics.

To evaluate the FBR policy, we use three survey waves: HIES 2009/10 and HIES 2012/13, which were conducted before the FBR policy,[6] and HIES 2016, conducted after the policy. Our main sample includes households with the youngest child aged 10 or younger. The age of 10 is the final year of primary education. We also restrict the analysis to households with the youngest child aged 2 or older, as the policy may influence fertility decisions as discussed by Peru (2023), which we will elaborate on further later. We also use a sample of individual children within this age range (i.e., 2 to 10 years old) to evaluate the policy impact on child outcomes.

Although the data do not provide information about migrants themselves (e.g., age and sex), we can identify whether a household sends a migrant and whether they migrate domestically or internationally. Additionally, for a subsample of households with a clear parent–child link (hereafter, the parent–child subsample), we can infer whether the mother is present at home, i.e., living together with the child.[7] Note that while this subsample is informative about mothers' responses to the policy, it may be subject to self-selection—and that selection could itself be influenced by the policy. To assess overall impacts, we therefore rely primarily on the full sample and present subsample results as corroborating evidence.

Table 1
Summary statistics of migration and household characteristics.

	Mean	[SD]
All sample: $N = 22419$		
Migration outcomes		
Any migrant abroad	0.08	[0.27]
Any remittance abroad	0.09	[0.29]
Amount of remittance abroad	14,200.79	[61882.21]
Any remittance domestic	0.08	[0.28]
Amount of remittance domestic	9340.43	[44393.19]
Family composition		
# of hh members incl. migrants	4.84	[1.41]
# of children 0–4 years old	0.47	[0.56]
# of children 5–9 years old	0.81	[0.64]
# of children 10–14 years old	0.56	[0.69]
Parent-child sumsample: $N = 17213$		
Mother present	0.97	[0.16]

This table summarizes household characteristics, including migration outcomes and family composition. The sample size is 22,419, except for "Mother present," which is available only for the "parent–child" subsample of 17,213 observations. Remittances are defined for the past 12 months.

Table 1 presents summary statistics for the migration and family composition variables. The sample pooling households with at least one child aged 2–10 from the 2009/10, 2012/13, and 2016 waves contains 22,419 observations. Note that the presence of the mother is only known for the parent–child subsample ($N = 17,213$, which corresponds to 77 % of the all sample households).

Eight percent of households in our sample have a migrant abroad. Nine percent of the sample households reported receiving remittances from abroad, with an annual average amount of 14,200 LKR. Remittances are relatively common: 9 % of households reported receiving remittances from abroad within the last 12 months, while 8 % received domestic remittances within the same period. The annual average amount is 9340 LKR, which is about two-thirds of the amount received from abroad.[8] The average household consists of 4.84 members, including migrants. On average, households have 0.47 children aged 0–4 and 0.81 children aged 5–9. In the parent–child subsample, 97 % of them have mothers at home.[9]

Table 2 presents the summary statistics of child-level outcomes of human capital investment. In our analysis, child health is measured by healthcare utilization by children, such as outpatient visits within the last month and inpatient stays within the past year. We also categorize outpatient visits and inpatient stays based on their underlying reasons such as check-ups and treatment for illness. We also analyze the presence of any chronic diseases. There is notable variation across different outcomes. On average, 33.6 % of children experienced any outpatient visit, primarily due to illness, while reported check-ups are relatively rare, with an average of only 0.8 %. Additionally, 6.4 % of children experienced inpatient care, with the majority of cases being illness-related. The

[6] HIES 2012/13 was conducted from July 2012 to June 2013. The FBR policy was announced in June 2013 and took effect in July 2013.

[7] The survey records the relationship between the household head and each member, except for migrating members. When a young household member (aged 10 or below in our analysis) is recorded as a child of the head, the mother is either the head or the spouse of the head, allowing us to infer whether she is at home. However, if the young household member is a grandchild of the head or a nephew/niece, their relationship is recorded as "other relative," making it impossible to identify their parent and, consequently, whether the mother is present at home.

[8] Remittance amounts are collected via the questionnaire item "current remittances and transfers," referring to the past 12 calendar months. Respondents report amounts separately for "outside the country" and "within the country," which we define as international and domestic remittances, respectively. The questionnaire does not identify the sender, so remittances cannot be linked to a specific household migrant; the sender may be someone other than the household's migrant.

[9] Appendix Fig. A.1 shows the relationship between the age of the youngest child and two outcomes: migration and the mother's presence, before and after the FBR policy. The share of households with any migrant abroad was lower for children aged 2–4 before the policy but increased afterward, while the probability of the mother being at home rose for households with children under 4 after the policy. At the age cutoff of 5, both effects diminish, indicating a neutralizing impact of the policy for older children.

Table 2
Summary statistics of child development outcomes.

	Mean	[SD]	N
Health			
Any outpatient	0.336	[0.472]	32,621
Outpatient for illness	0.323	[0.467]	32,621
Outpatient for check-up	0.008	[0.091]	32,621
Any inpatient	0.064	[0.245]	32,621
Inpatient for illness	0.050	[0.218]	32,621
Any chronic disease	0.035	[0.184]	32,621
Education			
School attendance	0.986	[0.117]	20,221
Grade retention	0.003	[0.054]	18,479
Current Grade	3.428	[1.576]	19,892
Age appropriate grade	0.975	[0.156]	19,892

Notes: This table presents the summary statistics of child outcomes. The sample is restricted based on age criteria: for health outcomes, the analysis includes children aged 2 to 10 years. For education-related outcomes, the sample is further restricted to children aged 5 to 10 years, as they are expected to be in school. Educational outcomes are well-defined only within this age range, with school retention specifically considered for children aged 6 to 10 years. The last three questions are only asked conditionally on her being attending school.

prevalence of chronic diseases is 3.5 %, which aligns with expectations given the population of young children.[10]

Apart from chronic disease, we use healthcare utilization to measure child health, whereas previous studies (e.g., Gosselin-Pali, 2025; Meng and Yamauchi, 2017) have commonly relied on anthropometric measurements such as height-for-age z-scores, which are not collected by HIES. While healthcare utilization primarily captures short-term and acute health conditions, anthropometric measures tend to reflect long-term nutritional status. Our study complements previous findings by examining child health from a different perspective. However, healthcare utilization requires a more nuanced interpretation, as it depends not only on a child's underlying health status but also on access to healthcare services. We will further discuss this when presenting and interpreting our results in Section 6.

While health outcomes are available for all the children in our analysis, education outcomes are only defined for children above 5 years as primary education starts at the age of 5 in Sri Lanka. We examine the following educational outcomes: school attendance, grade retention, current grade, and age appropriate grade.[11] Table 2 shows that primary education, which is both mandatory and free in Sri Lanka, appears to be highly effective—99 % of children attend school, the rates of grade retention are minimal, and 97 % of children are in an age-appropriate grade without any cumulative grade repetition. Consequently, our empirical analysis focuses on the relatively small margins of these outcomes.

5. Econometric strategy

We now turn to the empirical setup. Our central question is whether restricting mothers' migration ultimately affects human capital investment in children's health and education. We test this using a difference-in-differences design that compares households whose youngest child is below age 5 (treated) to those whose youngest child is age 5 or older (control), before and after the introduction of the FBR policy. We also examine potential mechanisms to interpret the overall effects, guided by Section 2.

Policy exposure is defined by the age of the youngest child at the survey date, since the FBR restricts migration for households with a youngest child under 5.[12] This definition captures current policy restrictions rather than the duration of exposure. Our DID specification uses this cross-sectional treatment contrast and the pre- vs. post-policy timing as the temporal dimension.[13]

We use the household as the unit of observation for analyzing migration and maternal presence, as well as for examining remittances, income, and labor substitution. The regression specification for the difference-in-differences analysis is as follows:

$$y_{ht} = \gamma_h + \lambda_t + \alpha(\text{Treated}_h \times \text{After}_t) + \mathbf{X}'_{ht}\beta + \varepsilon_{ht} \quad (1)$$

for household h at the time of survey $t \in \{2009/10, 2012/13, 2016\}$. Treated_h is a dummy variable equal to 1 for households with the youngest child aged below 5, and 0 otherwise. After_t is an indicator variable equal to 1 for the period after the introduction of the FBR policy ($t = 2016$). λ_t captures survey wave fixed effects and γ_h captures fixed effects for age of youngest child in household. We control for household characteristics $\mathbf{X}_{ht}$ (a school dummy, and family composition including the numbers of children aged 0–4 years, 5–9 years, and 10–14 years, and ethnicity, religion, and education of household head) and district fixed effects, sector (urban, rural, or estate) fixed effects, and survey month fixed effects, and ε_{ht} is the error term. We cluster standard errors at the district sector level. The coefficient of interest is α.

We also conduct the child-level analysis to estimate the effects of the policy on human capital investment. The regression specification is almost the same as Eq. (1), but the sample consists of children whose ages range from 2 to 10. The treatment variable Treated_h is still defined at the household level, meaning that a child is treated if they belong to a household where the youngest child is below the age of 5. This definition is motivated by our proposed main mechanisms, which suggest that the mother's presence and income effects are crucial for child outcomes and operate at the household level. In other words, policy exposure may benefit older siblings if they have younger siblings below the age of 5. That is, a child aged above 5 will have a value of 1 for this variable if they have a younger sibling under the age of 5. We include child characteristics such as sex, ethnicity and own age fixed effects in addition to the household characteristics. At the child analysis, we cluster standard errors at the household level.

The empirical approach leverages a natural experiment comparing households with the youngest child in different cohorts. Our identification relies on the parallel trends assumption—that households with the youngest child under 5 and those with the youngest child over 5 would have followed similar trends in the absence of the policy. With only two waves before and one after the policy, we cannot formally test

[10] In Appendix Fig. A.2, we present child-level health outcomes across different ages. Some of these outcomes are age-sensitive: we observe a clear pattern of monotonic decline with age for outpatient visits (both general and illness-related) and inpatient visits. Check-ups are more frequent at younger ages, while the prevalence of chronic diseases appears constant across all ages at very low rates.

[11] School attendance is a binary indicator of whether a child is currently attending school. Grade retention is also a binary indicator, defined as 1 if a child's grade in the current year is the same as in the previous year, and 0 otherwise. Current grade refers to the grade the child is currently enrolled in. Age-appropriate grade is a binary indicator of whether a child is enrolled in the grade typically expected for their age (e.g., Grade 1 for age 5, Grade 2 for age 6, and so on). Note that the last three outcomes are only defined for children who are attending school, and grade retention is well-defined by construction only for those above age 6.

[12] For instance, if a household has two children, aged 3 and 8, the mother is restricted from migrating under the policy because the youngest child is under 5 years old. This also effectively allows us to estimate the spillover effects on non-policy-targeted children—those who are above age 5—by comparing households with and without younger siblings.

[13] Note that as discussed in Section 3, households in our comparison group are partially affected by the policy though less restrictive. Thus, the estimated effects below are likely to underestimate the impact of restricting international migration compared to pure control households.

for pre-trends. However, we offer a contextual discussion supporting the plausibility of this assumption.

First, Appendix Table A.1 presents summary statistics for outcomes and family composition variables by treatment status (i.e., whether the household's youngest child is under 5), focusing on the pre-policy period (2009 and 2012 waves). We find no significant pairwise differences by treatment status. Household composition differs by design—control households have no children aged 0–4 but tend to have more older children.

Second, the policy may affect fertility decisions (Peru, 2023). With the new policy, having a child decreases economic opportunities abroad for a certain number of years. If fertility decisions are influenced by unobservable characteristics, the parallel trends assumption may not hold. For example, if those who are more passionate about child-rearing are less likely to be affected by the policy, the estimated DID coefficients will overestimate the true impact of the policy. To address this concern, the analysis focuses on cohorts that should not be affected by the policy in terms of fertility decisions. Specifically, children born after June 2014 (i.e., under the age of 2 in the 2016 survey) are likely to have been affected by the policy, whereas the decision to have a child born before that date (i.e., over the age of 2 in the 2016 survey) had already been made, and households could not have altered it in response to the policy. Therefore, we restrict the sample to households whose youngest child is over 2 years old.

Third, the policy's age cutoff closely aligns with the timing of primary school entry. Outcome trends may differ between preschool and school-aged children if school attendance influences migration decisions by reducing childcare burdens at home, though the direction of this effect is unclear. Therefore, our regression analysis explicitly controls for an education cohort dummy.[14]

In addition, there is a concern regarding the timing and exposure to the policy. For instance, a mother with a 5-, 6-, and 7-year-old child in 2016 is not currently restricted from migrating under the policy but were restricted when the policy was in effect three years earlier. This could alter their migration decision due to the earlier policy enforcement. Additionally, the policy may impact child health gradually rather than immediately. Children aged 5 to 7 in 2016 may have been influenced by the policy implemented in 2013. Including these children and their households in the control group may bias the estimated effects of the policy. We refer to this issue as "previously treated". We will test the impact of this issue on our findings as a robustness check later.

6. Estimation results

Following the conceptual framework in Section 2, we first show that the policy increases mother's presence at home and is associated with improvements in child health. We then examine an alternative channel—the policy's effects on remittances, household income, and intra-household labor reallocation. Finally, we assess impacts on children's educational outcomes.

6.1. Main results: effects on migration, mother's presence, and child health

Table 3 shows the DID estimates of mother's international migration restriction on mother's presence. The dependent variable of Column 1 is any household member migrating abroad while Columns 2 and 3 represent whether the mother migrates abroad and whether the household has the mother present at home. While Column 1 uses all the entire sample of households, Columns 2 and 3 use the parent–child subsample.

The results show an economically and statistically significant impact of restricting mothers' international migration on both the migration decision and mothers' presence at home. The estimated effect is a 1.5 percentage point decrease in the likelihood of any household member migrating abroad, statistically significant at the 5 percent level, compared to the control group of 7.6 %, which represents a 19.7 % decrease. Column 2 shows that the FBR policy significantly decreases mothers' international migration by 0.7 percentage points, which is substantial compared to the control group of 1.5 %. Column 3 reports a significant increase in mothers' presence at home. The result indicates that exposure to the policy increases mothers' presence by 1.2 percentage points from the control group of 97.4 %.[15,16]

Table 3
Impact of mother's international migration restriction on mother's presence.

	Any household member migrating abroad	Mother migrating abroad	Mother present at home
	(1)	(2)	(3)
Treated × After	−0.015**	−0.007*	0.011**
	(0.006)	(0.004)	(0.005)
Control mean	0.076	0.015	0.974
Sample	All	Parent-child	Parent-child
Observations	22,419	17,213	17,213

Note: The table presents DID estimates of the impact of restricting mothers' international migration on mothers' presence outcomes. The dependent variable in column 1 is whether any household member migrates abroad; column 2 is whether the mother migrates abroad, constructed by two conditions: whether the mother is not present and any household member migrates abroad; and column 3 is whether the mother is present at home. "Treated" is a dummy variable indicating that the household's youngest child is below age 5, and "After" is a dummy indicating that the survey wave occurred in 2016. All columns include fixed effects for the age of the youngest child and survey wave. Other control variables include a school dummy, and family composition (including the number of children aged 0–4 years, 5–9 years, and 10–14 years), ethnicity of the household head, religion of the household head, education of the household head, district fixed effects, sector fixed effects, and survey month fixed effects. Standard errors are clustered at the district-sector level. The row labeled "Control mean" indicates the average outcome for households whose youngest children were over age 5 before the 2013 survey. The row labeled "Sample" indicates the sample of households, where column 1 is restricted to households with the youngest children aged 2 to 10 years, while columns 2 and 3 further restrict the sample to 'Parent–child subsample' for whom a detailed parent– child relationship can be identified (See Section 4 for the definition). * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

Appendix Fig. A.3 shows the event-study plots on any migrant abroad and mothers' presence displaying wave-specific treatment coefficients. The DID estimates shown above appear to be driven by the change between 2012 and 2016, which coincides with the timing of the introduction of the FBR policy in 2013.6, rather than capturing general trends or unusual events before the policy. We cannot reject the null hypothesis that the treatment coefficient in 2009 is equal to zero; indeed, the point estimates are very close to zero. After the introduction of the policy, there are significant point estimates in 2016, where we observe a significant drop in any migrant abroad and a significant jump in mothers' presence, as expected.

Table 4 presents the effects of restricting mothers' international migration on child health outcomes. We do not observe any significant effects on outpatient visits, although the sign of the point estimates aligns with the expectation of improvement in child health. There are negative but insignificant effects on outpatient visits for treatment for illness, and positive but insignificant effects on checkups. However, there

[14] Policy exposure remains in effect until a child turns 5, while primary education begins in January of the year after the child turns 5. This creates a gap that varies depending on the timing of the survey and the child's birth date.

[15] As outlined in the policy, control group households are also required to arrange a caregiver, which likely attenuates our results. Consequently, our estimates provide a lower bound of the true effect of restricting mothers' international migration.

[16] As shown in Column (1) of Appendix Table A.2, we also confirm that mothers' domestic migration does not increase.

Table 4
Impact of mother's international migration restriction on child health.

	Outpatient			Inpatient		Chronic disease
	Any	Illness	Check-up	Any	Illness	Any
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × After	−0.009	−0.012	0.002	−0.011**	−0.008*	−0.000
	(0.012)	(0.012)	(0.002)	(0.006)	(0.005)	(0.005)
Control mean	0.345	0.333	0.007	0.072	0.057	0.035
Observations	32,621	32,621	32,621	32,621	32,621	32,621

Note: The table presents DID estimates of the impact of restricting mothers' international migration on child health. The dependent variables are dummy variables indicating outpatient visits for any reason, illness, and check-ups; inpatient visits for any reason and illness; and the presence of any chronic disease. "Treated" is a dummy variable indicating that the household's youngest child is below age 5, while "After" is a dummy variable indicating that the survey wave was conducted in 2016. Standard errors are clustered at the household level. All columns include fixed effects for the age of youngest child and survey wave. Other control variables include a school dummy, and family composition (including the number of children aged 0–4 years, 5–9 years, and 10–14 years), ethnicity of the household head, religion of the household head, education of the household head, own age fixed effects, district fixed effects, sector fixed effects, and survey month fixed effects. The row labeled "Control mean" indicates the average outcome for households where the youngest child was above age 5 before the 2013 survey. The sample is restricted to children aged 2 to 10 years, with at least one sibling aged 2 to 10 years. Clustered standard errors are at the household level. * denotes significance at the 0.10 level; ** at the 0.05 level; and *** at the 0.01 level.

is a significant decrease in inpatient stays for any reason, particularly for inpatient stays for treatment for illness, and these decreases are economically significant compared to the control mean. Any inpatient stay decreases by 1.1 percentage points relative to the control group of 7.2 %, representing a 15.2 % decrease. Inpatient stays for treatment for illness decreased by 0.8 percentage points compared to the control group of 5.7 %, representing a 14 % decrease. Finally, the introduction of the FBR policy does not appear to affect chronic diseases, likely because these conditions are too early to be diagnosed in the young children included in our analysis.[17]

These results appear to be driven by the increased presence of mothers at home. The policy leads to mothers remaining at home to care for their children, which, in turn, contributes to improved child health outcomes. This suggests that the policy as intended improves the child human capital development. The results are in line with the results by Meng and Yamauchi (2017), which demonstrate the adverse effects of parental, particularly maternal, migration on child nutritional outcomes.

However, we should interpret healthcare utilization carefully, as it is related to not only health conditions but also access to healthcare services. In contrast to our preferred interpretation, there is an alternative interpretation of the results. As discussed in Section 2, the restrictive migration policy leads to income decreases by reducing earning opportunities abroad and remittances from abroad. Due to these income reductions, healthcare services may become unaffordable. If this is the case, significant decreases in inpatient stays would not indicate improvements in child health; instead, they may merely suggest less access to healthcare services, without implying any actual change in children's underlying health conditions. However, we argue this interpretation is not plausible in the context of this study. First, as shown later, while the policy reduces remittances from abroad, this decrease is offset by an equivalent increase in domestic remittances, resulting in no significant change in household incomes. Additionally, Sri Lanka's free universal healthcare system, as explained in Section 3, minimizes the relevance of financial constraints in accessing healthcare. Therefore, our results suggest that the income channel is neutralized, and the observed decrease in impatient stays reflects an improvement in child health, which can be attributed to the increased presence of mothers at home.

Before discussing other mechanisms including such income channels, we check the robustness of our main results. Here, we provide four pieces of evidence to support the main findings.

First, we provide evidence that the issue of previously treated households, discussed in Section 5, may not affect our results in a substantial way. Appendix Fig. A.5 shows the youngest child's age-specific treatment coefficients for any migrant abroad and mothers' presence. The results for any migrant abroad seem to be driven by a decrease in the outcome for households with the youngest child aged 2–4, where the magnitude decreases as age increases, compared to the reference age of 5. However, we observe an increase or zero coefficients for children aged 6 or older. A similar (opposite sign) pattern is observed for mothers' presence. However, we also note that the effects for the age of the youngest child at 6 and 7 seem to move in the opposite direction, which may suggest some influence of the previous treatment.

Appendix Table A.3 also presents the results of an additional robustness check addressing the issue of the previous treatment. We exclude children from households with the youngest children aged 5–7, as they were exposed to the policy at its introduction but are currently classified in the control group. The results are consistent with those in Table 4: both the reductions in any inpatient stays and inpatient stays for treatment for illness are statistically significant at the 5 % level, with estimated decreases of 1.5 percentage points.

Next, we examine the effects on the parent–child subsample to assess sensitivity and the potential impact of sample selection bias.[18] Appendix Table A.4 presents the effects of restricting the samples to the parent–child subsample on child health outcomes. We observe similar coefficients for any inpatient stays and inpatient stays for treatment for illness, although the latter becomes slightly less precise.

Finally, we conduct falsification tests by redefining policy exposure to a different timing: treatment is defined for at the cutoff of ages 6 to 10, instead of the actual treatment age of 5. Appendix Fig. A.6 illustrates the placebo effects. Although we observe some significant effects at age 6 (and at age 7 for mothers' presence), likely due to the previously treated issue discussed above, we confirm that the coefficients are not statistically significant at the pseudo cutoff ages of 8 to 10.

[17] In Appendix Fig. A.4, we estimate the heterogeneous treatment effects by age and find that the effect on outpatient visits is significant and positive at age 4, while the effect on inpatient care appears to be driven by younger ages.

[18] The DID coefficient for being in the parent–child sample is negative and insignificant.

Table 5
Impact of mother's international migration restriction on remittance and income.

	Amount of remittance abroad (i.h.s)	Amount of remittance domestic (i.h.s)	Amount of total remittance (i.h.s)	Any remittance abroad	Any remittance domestic	Total household income (i.h.s)
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × After	−0.189*	0.196*	0.008	−0.014*	0.016	0.030
	(0.097)	(0.113)	(0.126)	(0.008)	(0.010)	(0.031)
Control mean	1.038	0.822	1.818	0.088	0.073	13.364
Observations	22,419	22,419	22,419	22,419	22,419	22,419

Note: The table presents DID estimates of the impact of restricting mothers' international migration on household remittance and income. The dependent variables are remittances and total household income. Columns (1), (2), (3), and (6) show the amounts of remittances from abroad, domestic, and total remittances, and total household income, respectively, all transformed using the inverse hyperbolic sine. Columns (4) and (5) indicate whether there are any remittances from abroad and domestic remittances, respectively. "Treated" is a dummy indicating that the household's youngest child is below age 5, and "After" is a dummy indicating that the survey wave was in 2016. All columns include fixed effects for the age of youngest child and survey wave. Other control variables include a school dummy, and family composition (including the number of children aged 0–4 years, 5–9 years, and 10–14 years), ethnicity of the household head, religion of the household head, education of the household head, district fixed effects, sector fixed effects, and survey month fixed effects. Standard errors are clustered at the district-sector level. The row labeled "Control mean" indicates the average outcome for households whose youngest children were over age 5 before the 2013 survey. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

6.2. Other possible mechanisms: income effects and intra-household labor substitution

Our main results above show that policy exposure increases mothers' presence by discouraging international migration, which appears to enhance child health. However, there is the possibility of other potential mechanisms that the policy could affect children as discussed in Section 2. Below, we examine whether the policy had a negative impact on income and how households adjusted their intra-household labor supply in response.

Table 5 presents the effects of restricting mothers' international migration on remittances and income (inverse hyperbolic sine transformed). We find that the inverse hyperbolic transformed amount of remittances from abroad decreases by 0.19 due to policy exposure at the 10 % significance level. On the other hand, policy exposure increases the inverse hyperbolic transformed amount of domestic remittances by 0.20. This likely reflects household coping behavior: since the FBR policy restricts only mothers' international migration and thereby caused a significant drop in remittances from abroad, households may have sought domestic remittances to compensate the loss in international remittance. Column (3) shows the effect on total remittances, and we do not find statistically significant effect, indicating the decreased remittance abroad appears to be offset by the increased domestic remittances.

Consistent with the policy's intent and the resulting decline in maternal migration, we observe a significant decline in the likelihood of receiving any remittances from abroad at the extensive margin. The FBR policy reduces this probability by 1.4 percentage points, as shown in Column (4). However, Column (5) shows no significant increase in the likelihood of receiving domestic remittances, despite its positive sign and a magnitude comparable in absolute terms to that in Column (4). This suggests that the observed increase in remittance amounts reported in Column (2) is driven by the intensive margin—higher remittances among existing recipients—rather than a broader expansion at the extensive margin. One possible interpretation is that domestic remittances increase in response to shocks (in this case, the restrictive migration policy), facilitated by pre-existing migration networks, as suggested by Batista and Vicente (2023) and Bettin et al. (2024).

Column (6) shows the effects on total household income. Interestingly, despite policy exposure decreasing the amount of remittances from abroad significantly, there are no significant impacts on total household income. The decrease in remittances from abroad appears to be offset by household coping responses, mainly through an increase in domestic remittances.[19]

Table 6 presents the impact of restricting mothers' international migration on intra-household labor reallocation and household composition. We find no evidence that policy restrictions targeting mothers leads to a substitution toward fathers' international migration nor by other household members. Column (1) shows that the policy did not affect fathers' presence at home, as they are not directly targeted by the policy. Column (2) shows no significant effect on the likelihood of fathers migrating abroad.[20] Column (3) shows no significant effect on the likelihood of domestic migration by any household member although the estimate is positive and its magnitude is comparable in absolute terms to the decrease in international migration shown in Column (1) of Table 3. Appendix Table A.2 further examines effects on domestic migration. While there is no impact on mothers' migration—consistent with the earlier finding of increased maternal presence—we observe an increase in fathers' domestic migration in the parent–child subsample. Columns (3) and (4) examine changes in household composition by gender. The number of female adults increases slightly, though the estimate is imprecise, which is consistent with mothers remaining at home.[21,22]

However, Column (5) suggests a significant increase in maternal involvement within the household. The FBR policy results in a 2.6 percentage point rise in the likelihood of females reporting housework as their main activity. Although the data do not provide specific details on the nature of housework, it likely includes childcare. This supports the interpretation that improved child health outcomes are driven by increased maternal presence and enhanced interaction between mothers and their children.

In summary, although policy exposure led to a decrease in remittances from abroad, this reduction appears to have been offset by an increase in domestic remittances, resulting in a neutral overall income effect. Taken together with the findings from the previous section, the policy has a positive impact on human capital investment, as reflected by the reduction in healthcare utilization. This improvement in child health is primarily driven by the increased presence of mothers at home, consistent with the policy's intended goal. However, these positive outcomes seem to be made possible through household compensation for lost income via increased domestic remittances.

[19] See Appendix Table A.5 for the effects on the detailed disaggregated composition of income.

[20] We also find no significant effect on the probability of fathers being at home.

[21] This potential underestimation is likely due to the policy design—specifically, households with the youngest child aged above 5 still requiring to arrange a caregiver when migrating. As a result, the policy's indirect effects may extend to these households, thereby attenuating the measured impact.

[22] We also find no significant effects on the number of working female or male adults.

Table 6
Impact of mother's international migration restriction on labor reallocation.

	Father present at home	Father migrating abroad	Any household member migrating domestic	Any female housework
	(1)	(2)	(3)	(4)
Treated × After	−0.014	−0.006	0.015	0.026**
	(0.013)	(0.006)	(0.012)	(0.012)
Controls	✓	✓	✓	✓
Control mean	0.867	0.035	0.106	0.674
Sample	Parent-child	Parent-child	All	All
Observations	17,213	17,213	22,419	22,419

Note: The table presents DID estimates of the impact of restricting mothers' international migration on household labor reallocation. The dependent variables are household labor allocation outcomes including whether the father is present at home, whether the father is migrating abroad, any household member migrating domestically, and any female doing housework. "Treated" is a dummy indicating that the household's youngest child is below age 5, and "After" is a dummy indicating that the survey wave was in 2016. Standard errors are clustered at the district-sector level. Control variables include a school dummy, and family composition (including the number of children aged 0–4 years, 5–9 years, and 10–14 years), ethnicity of the household head, religion of the household head, and education of the household head. All columns include district fixed effects, sector fixed effects, and survey month fixed effects. The row labeled "Control mean" indicates the average outcome for those whose youngest children were aged above 5 before the 2013 survey. The row labeled "Sample" indicates: 'All' includes households whose youngest child is aged 2–10, while the 'Parent–child subsample' includes households for which detailed parent–child relationships can be identified (see Section 4 for the definition). * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

Table 7
Sibling spillover effects on child health.

	Outpatient			Inpatient		Chronic disease
	Any	Illness	Check-up	Any	Illness	Any
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Direct (children who are the youngest or are above age five)						
Treated × After	−0.020	−0.021	0.003	−0.017**	−0.013**	0.000
	(0.013)	(0.013)	(0.003)	(0.007)	(0.006)	(0.005)
Control mean	0.371	0.358	0.007	0.079	0.063	0.035
Observations	23,254	23,254	23,254	23,254	23,254	23,254
Panel B: Indirect (children who are not the youngest and are above age five)						
	Outpatient			Inpatient		Chronic disease
	Any	Illness	Check-up	Any	Illness	Any
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × After	0.017	0.009	0.003	0.002	0.002	−0.002
	(0.021)	(0.020)	(0.004)	(0.010)	(0.008)	(0.009)
Control mean	0.282	0.273	0.005	0.056	0.043	0.034
Observations	9367	9367	9367	9367	9367	9367

Note: The table presents sibling spillover effects of restricting mothers' international migration on child health. Panel A shows the effects on the subsample of children who is the youngest in the household or under 5, while Panel B shows the effects on the subsample of children whose age is not the youngest the household or is 5 years or older. The dependent variables are dummy variables indicating outpatient visits for any reason, illness, and check-ups; inpatient visits for any reason and illness; and the presence of any chronic disease. "Treated" is a dummy variable indicating that the household's youngest child is below age 5, while "After" is a dummy variable indicating that the survey wave was conducted in 2016. Standard errors are clustered at the household level. Control variables include a school dummy, and family composition (including the number of children aged 0–4 years, 5–9 years, and 10–14 years), ethnicity of the household head, religion of the household head, and education of the household head. All columns include age fixed effects, district fixed effects, sector fixed effects, and survey month fixed effects. The row labeled "Control mean" indicates the average outcome for households whose youngest children were aged above 5 before the 2013 survey. The sample is restricted to children aged 2 to 10 years, with at least one sibling aged 2 to 10 years. * denotes significance at the 0.10 level; ** at the 0.05 level; and *** at the 0.01 level.

6.3. Extension: policy target and sibling spillover effects

Next, we distinguish between the direct effects of the FBR policy on target children and potential spillover effects on their non-targeted siblings. Although the policy directly targets children under the age of 5, the increased presence of mothers at home may also benefit older siblings in the same household, potentially improving their outcomes as well. It is important to note that the estimates presented above capture both direct and indirect effects by design. To separate these effects, we split the sample into two groups: (1) children who are the youngest in the household or are above age 5, representing the direct effect, and (2) children who are not the youngest and are above age 5, representing the indirect or spillover effect.

Table 7 presents this subsample analysis.[23] The findings indicate that our main results are primarily driven by the direct effects on children targeted by the policy target. Panel A shows that the direct effects on inpatient stays are statistically significant at the 5 % level, while Panel B shows no statistically significant spillover effects. These results suggest that maternal presence is especially important during the targeted ages (i.e., under 5) for child health, consistent with evidence that maternal

[23] We also conduct a subsample analysis using an alternative definition of spillover effects, where direct effects are defined for children who are the youngest in the household, and indirect effects for those who are not. The results remain very similar.

Table 8
Impact of mother's international migration restriction on child education.

		Conditional on attending school		
	School attendance	Grade retention	Current grade	Age appropriate grade
	(1)	(2)	(3)	(4)
Treated × After	0.000	−0.003*	−0.025	0.000
	(0.004)	(0.001)	(0.019)	(0.005)
Control mean	0.984	0.005	3.404	0.971
Observations	20,221	18,479	19,892	19,892

Note: The table presents DID estimates of the impact of restricting mothers' international migration on child education. The dependent variables are current grade and dummy for retention. "Treated" is a dummy variable indicating that the household's youngest child is below age 5, while "After" is a dummy variable indicating that the survey wave was conducted in 2016. Standard errors are clustered at the household level. Control variables include a school dummy, and family composition (including the number of children aged 0–4 years, 5–9 years, and 10–14 years), ethnicity of the household head, religion of the household head, and education of the household head. All columns include age fixed effects, district fixed effects, sector fixed effects, and survey month fixed effects. The row labeled "Control mean" indicates the average outcome for households where the youngest child was above age 5 before the 2013 survey. The sample is restricted to children aged 5 to 10 years, as school age begins at 5. Educational outcomes are well-defined only within this age range, with grade retention specifically considered for children aged 6 to 10 by definition. In the survey, grade retention and current grade are asked only for children who are attending school, and thus age-appropriate grade is also defined only for this group. Clustered standard errors are at the household level. * denotes significance at the 0.10 level; ** at the 0.05 level; and *** at the 0.01 level.

migration during the preschool period delays cognitive development (Bai et al., 2022) and with findings from other settings (Currie and Almond, 2011).

We extend the analysis to examine the effects on children's educational outcomes. Since these outcomes are only measured for children above age 5, we focus on estimating spillover effects on non-policy-targeted children by comparing those over age 5 with and without younger siblings under age 5. While school attendance is observed for all school-aged children, grade retention and current grade are only available for those attending school. By definition, grade retention is well-defined only for children above age 6.

Table 8 presents the results. Column (1) indicates that the FBR policy does not improve school attendance among non-policy-targeted children who have younger siblings under the age of 5.[24] Similarly, we do not find evidence that policy exposure affects the current grade of enrollment or the probability of being in an age-appropriate grade. It is worth noting that the mean values of these outcomes are already very high, reflecting the strong compliance with Sri Lanka's mandatory schooling, which may limit the scope for large observable effects. However, we do find a statistically significant reduction in grade retention in the current year. Policy exposure reduces grade retention by 0.3 percentage points, compared to a control group mean of 0.5 %—a 60 % reduction. Given the very low control mean, this improvement in educational status is likely concentrated among children facing more challenging educational environments.

This result aligns with our research design, which focuses on the current mothers' presence. The null results for current grade progression are consistent with the fact that our treatment only addresses relatively immediate effects. However, the observed reduction in grade retention is likely driven by the current presence of mothers. The interpretation of these findings, however, requires caution. Our findings of spillover effect on grade retention but not on healthcare utilization may reflect different channels affecting these outcomes.[25] While we emphasize the importance of mothers' presence and income effects, these may influence only specific educational achievements, particularly in settings where nearly all children attend school.

7. Conclusion and policy implications

International migration is an important economic opportunity in developing countries, but it can separate mothers from their children, potentially harming child development. This paper studies a unique policy in Sri Lanka that restricts mothers from international migration in order to protect children.

Our results suggest that the introduction of the FBR policy is effective in improving human capital investment. The policy successfully discourages mothers from migrating internationally, increasing their presence at home. We show that the policy leads to a decrease in any inpatient stays of child, particularly for treatment for illness, indicating improvements in child health. Despite the reduction in remittances from abroad, the overall income effect is neutral, as households compensate through increased domestic remittances. We also find a suggestive evidence of positive spillover effects on non-policy-targeted children's education, as reflected in reduced grade retention.

We believe our findings have broader relevance beyond the Sri Lankan context. Restricting mothers' international migration increases their presence at home, with evidence indicating positive effects on human capital investment, particularly in children's health and education. Given that migration restrictions are a realistic policy tool considered by other developing countries, providing causal evidence on their impacts is valuable.

However, caution is needed in generalizing these findings. In our context, domestic remittances helped offset the loss of income from abroad. But in settings with limited labor market access or weak remittance systems, the impact on child development may be ambiguous—or even negative—if income losses outweigh the benefits of increased maternal presence. These results highlight the trade-offs between the economic opportunities provided by international migration and the benefits of a mother's presence for child development. A key policy implication at the household level is that ensuring sufficient domestic labor opportunities is crucial to compensating for the loss of international remittances at the household level.

It is also important to note that there are additional concerns surrounding the policy. First, the loss of international remittances at the

[24] Among the children not attending school in the sample, the stated reasons of non-attendance were: disability or illness (22.3 %), unwillingness to attend or poor academic progress (17.2 %), financial problems (6.3 %), and other reasons, each accounting for less than 1 %.

[25] Appendix Table A.6 repeats the spillover analysis for healthcare utilization outcomes, restricting the sample to match that used in the education analysis. We find results similar to Panel B of Table 7: there is no evidence of spillover effects on health outcomes. This also reconfirms that our main results of health are driven by the policy-targeted children.

household level is estimated to be substantial decrease. This poses a concern for governments in developing countries, as remittances are a critical source of foreign currency acquisition and may have significant macroeconomic implications. Second, there is reported unintended negative consequences. Weeraratne (2016) documented that although the FBR was successful in restricting female migration for domestic work, it also promoted migration outside Sri Lanka's legal framework, often through visitor visas, thereby increasing workers' vulnerability at their destination. This vulnerability was further exacerbated as women resorted to corrupt practices to circumvent the FBR requirement by forging documents. Often, the costs of falsifying FBR documents were covered by sub-agents or licensed recruitment agents, which led to exploitation and abuse of potential female migrants during the recruitment process. Additionally, the FBR has been associated with delays in the recruitment process, creating further barriers for women seeking legal migration opportunities (Weeraratne, 2022).

While protecting children is an important policy goal, it is equally essential to safeguard the rights of female workers. This study focuses on one side of this trade-off. Ongoing policy evaluation and discussion are crucial to fully understand and address these competing goals.

CRediT authorship contribution statement

Takuya Hasebe: Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Yuma Noritomo:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Bilesha Weeraratne:** Writing – review & editing, Visualization, Validation, Supervision, Resources, Methodology, Investigation, Formal analysis, Data curation, Conceptualization.

Declaration of generative AI and AI-assisted technologies in the manuscript preparation process

During the preparation of this work the authors used ChatGPT 5 in order to enhance the readability of the manuscript. After using this tool/service, the authors reviewed and edited the content as needed and take full responsibility for the content of the published article.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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Appendix A. Appendix figures and tables

See Table A.1–A.6 and Fig. A.1–A.6

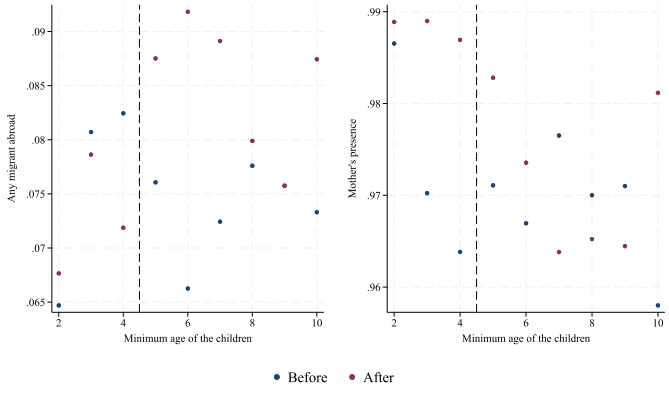

Fig. A.1. Migration and the age of the youngest child. Notes: these figures depict the relationship between the age of the youngest child in households and migration outcomes: any migrant abroad (left panel) and mother's presence (right panel). "After" refers to data from 2016, while "Before" refers to data from 2009/10 and 2012/13, indicating whether the data was collected before or after the introduction of the FBR policy.

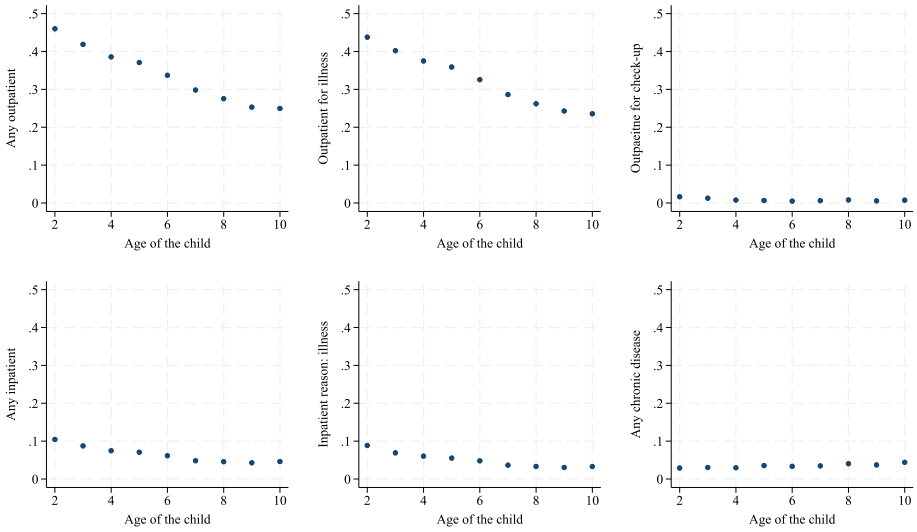

Fig. A.2. Child health behavior over age. Notes: $N = 32621$ children. These figures show the distribution of the child health outcomes over age of the child.

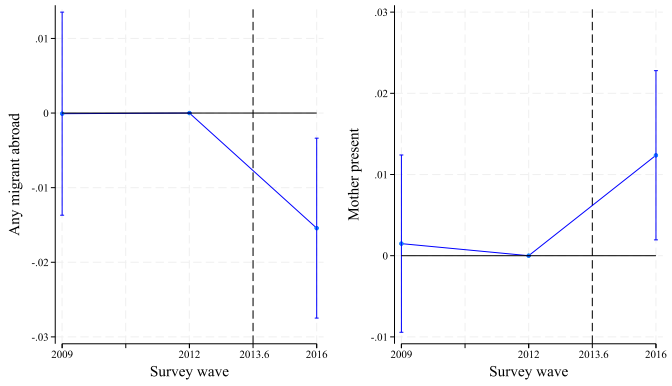

Fig. A.3. Event study – Wave-specific coefficients on mother's presence. Notes: the figure estimates the effects of restricting mothers' international migration on the likelihood of having any migrant abroad and on mothers' presence. The coefficients are estimated for three survey waves in the data: 2009, 2012, and 2016. The introduction of the FBR policy restricting mothers' international migration was announced in June 2013 and became effective in July 2013.

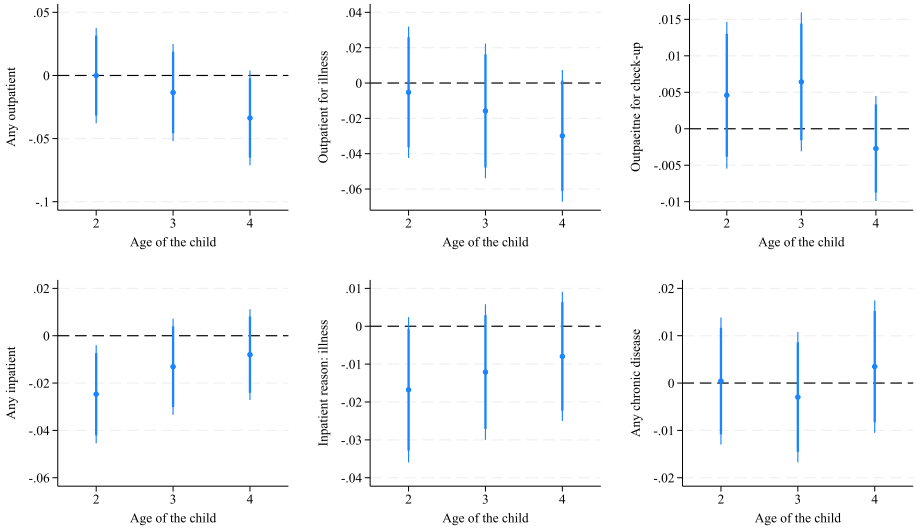

Fig. A.4. Own age-specific coefficients of child health behavior. Notes: the figure shows the heterogeneity by the age of the child for DID coefficients on child health outcomes.

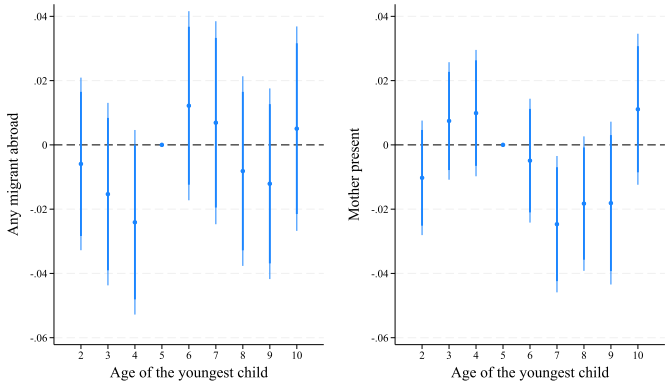

Fig. A.5. Age-specific coefficients of mother's presence. Notes: these figures show the effects of restricting mothers' international migration on the likelihood of having any migrant abroad and on mothers' presence, estimated by the age of youngest children. The reference category is the age of youngest child at 5.

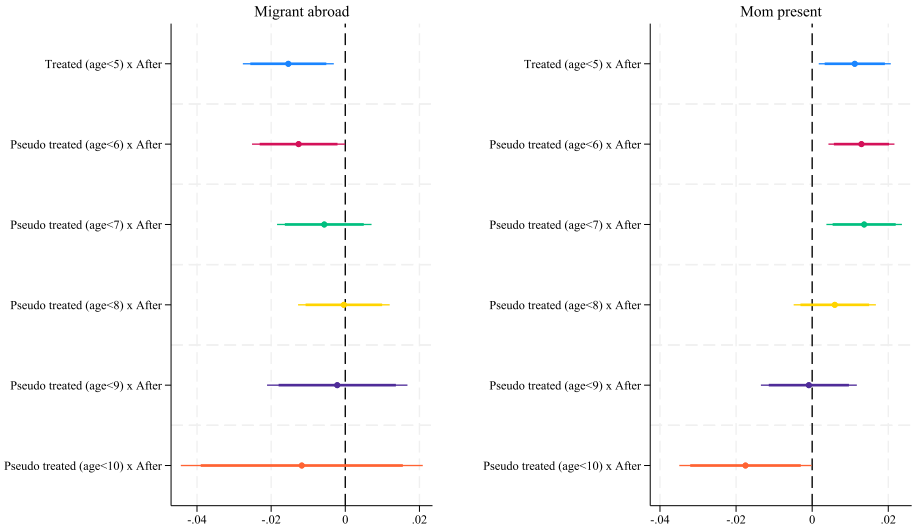

Fig. A.6. Placebo test using different age to define psuedo treatment. Notes: the figure shows the DID coefficients using different ages as treatment definitions for the likelihood of having any migrant abroad and for mothers' presence.

Table A.1
Summary statistics by treatment (pre-policy periods).

	(1) All Mean/SD	(2) Treat Mean/SD	(3) Control Mean/SD	(4) Pairwise *t*-test Mean difference
Outcome variables				
Mother present	0.97	0.97	0.97	−0.00
	[0.17]	[0.16]	[0.17]	
Any migrant abroad	0.07	0.08	0.07	−0.00
	[0.26]	[0.26]	[0.26]	
Any remittance abroad	0.09	0.09	0.09	0.00
	[0.28]	[0.28]	[0.29]	
Amount of remittance abroad	10,133.39	10,046.52	10,206.45	159.93
	[44085.94]	[43857.83]	[44279.49]	
Any remittance domestic	0.07	0.07	0.07	−0.00
	[0.26]	[0.26]	[0.26]	
Amount of remittance domestic	5870.00	6127.76	5653.22	−474.54
	[29517.81]	[30261.03]	[28878.10]	
Household composition				
# of hh members incl. migrants	4.86	4.96	4.78	−0.18***
	[1.45]	[1.57]	[1.35]	
# of children 0–4 years old	0.49	1.07	0.00	−1.07***
	[0.56]	[0.26]	[0.00]	
# of children 5–9 years old	0.81	0.54	1.04	0.50***
	[0.65]	[0.63]	[0.57]	
# of children 10–14 years old	0.56	0.35	0.74	0.39***
	[0.70]	[0.60]	[0.72]	
Observations	14,658	6696	7962	14,658

Notes: This table shows the summary statistics of household characteristics by "treatment" (i.e., whether the households have a child aged younger than 5). "Mother present" is defined based on the restricted samples where we are able to identify detailed relationships of household members. The sample is restricted to pre-policy periods, i.e., the 2009 and 2012 waves. Annual household income and remittance are evaluated by LKR.

Table A.2
Impact of mother's international migration restriction on other labor reallocation.

	Mother migrating domestic	Father migrating domestic	Number of working female adult	Number of female housework
	(1)	(2)	(3)	(4)
Treated × After	0.001	0.018**	0.031	0.019
	(0.002)	(0.007)	(0.019)	(0.021)
Controls	✓	✓	✓	✓
Control mean	0.003	0.058	1.421	1.168
Sample	Parent-child	Parent-child	All	All
Observations	17,213	17,213	22,419	22,419

Note: The table presents DID estimates of the impact of restricting mothers' international migration on other household labor reallocation. The dependent variables are household labor allocation outcomes including whether the mother migrating domestically, whether the father migrating domestically, number of female adult (without migrant), and number of male adult (without migrant). "Treated" is a dummy indicating that the household's youngest child is below age 5, and "After" is a dummy indicating that the survey wave was in 2016. Standard errors are clustered at the district-sector level. Control variables include a school dummy, and family composition (including the number of children aged 0–4 years, 5–9 years, and 10–14 years), ethnicity of the household head, religion of the household head, and education of the household head. All columns include district fixed effects, sector fixed effects, and survey month fixed effects. The row labeled "Control mean" indicates the average outcome for those whose youngest children were aged above 5 before the 2013 survey. The row labeled "Sample" indicates: 'All' includes households whose youngest child is aged 2–10, while the 'Parent–child subsample' includes households for which detailed parent–child relationships can be identified (see Section 4 for the definition). * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

Table A.3
Impact of mother's international migration restriction on child health care excluding the partially treated.

	Outpatient			Inpatient		Chronic disease
	Any	Illness	Check-up	Any	Illness	Any
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × After	0.002	−0.000	0.004	−0.015**	−0.016**	0.003
	(0.015)	(0.015)	(0.003)	(0.007)	(0.006)	(0.006)
Control mean	0.354	0.341	0.008	0.076	0.060	0.035
Observations	22,832	22,832	22,832	22,832	22,832	22,832

Note: The table presents DID estimates of the impact of restricting mothers' international migration on child health care. The dependent variables are dummy variables indicating outpatient visits for any reason, illness, and check-ups; inpatient visits for any reason and illness; and the presence of any chronic disease. "Treated" is a dummy indicating that the household's youngest child is below age 5, and "After" is a dummy indicating that the survey wave was in 2016. Standard errors are clustered at the household level. Control variables include a school dummy, and family composition (including the number of children aged 0–4 years, 5–9 years, and 10–14 years), ethnicity of the household head, religion of the household head, and education of the household head. All columns include age fixed effects, district fixed effects, sector fixed effects, and survey month fixed effects. The row labeled "Control mean" indicates the average outcome for those whose youngest children were aged above 5 before the 2013 survey. The sample is restricted to children aged 2 to 10 years old excluding children from households whose youngest child was aged 5–7. Clustered standard errors are at the household level. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

Table A.4
Impact of mother's international migration restriction on child health care (Parent–child sample).

	Outpatient			Inpatient		Chronic disease
	Any	Illness	Check-up	Any	Illness	Any
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × After	−0.003	−0.007	0.003	−0.011*	−0.009	0.000
	(0.014)	(0.014)	(0.003)	(0.007)	(0.006)	(0.005)
Control mean	0.350	0.337	0.007	0.075	0.059	0.038
Observations	25,267	25,267	25,267	25,267	25,267	25,267

Note: The table presents DID estimates of the impact of restricting mothers' international migration on child health care. The dependent variables are dummy variables indicating outpatient visits for any reason, illness, and check-ups; inpatient visits for any reason and illness; and the presence of any chronic disease. "Treated" is a dummy indicating that the household's youngest child is below age 5, and "After" is a dummy indicating that the survey wave was in 2016. Standard errors are clustered at the household level. Control variables include a school dummy, and family composition (including the number of children aged 0–4 years, 5–9 years, and 10–14 years), ethnicity of the household head, religion of the household head, and education of the household head. All columns include age fixed effects, district fixed effects, sector fixed effects, and survey month fixed effects. The row labeled "Control mean" indicates the average outcome for those whose youngest children were aged above 5 before the 2013 survey. The sample is restricted to children aged 2 to 10 years old from households in the 'Parent–child subsample.' Clustered standard errors are at the household level. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

Table A.5
Impact of mother's international migration restriction on income sources.

	Total income (i.h.s)	Labor income (i.h.s)	Seasonal agriculture income (i.h.s)	Other agriculture income (i.h.s)	Non-agriculture income (i.h.s)	Winfall income (i.h.s)	Other income (i.h.s)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated × After	0.030	0.050	0.044	−0.147	−0.239	−0.041	−0.056
	(0.031)	(0.185)	(0.130)	(0.168)	(0.180)	(0.189)	(0.159)
Control mean	13.364	8.582	1.873	1.787	3.466	5.011	5.169
Observations	22,419	22,419	22,419	22,419	22,419	22,419	22,419

Note: The table presents DID estimates of the impact of restricting mothers' international migration on household income sources. The dependent variables are detailed income sources. Columns (3) and (4) indicate whether there are any remittances from abroad and domestic remittances, respectively. "Treated" is a dummy indicating that the household's youngest child is below age 5, and "After" is a dummy indicating that the survey wave was in 2016. Standard errors are clustered at the district-sector level. Control variables include a school dummy, and family composition (including the number of children aged 0–4 years, 5–9 years, and 10–14 years), ethnicity of the household head, religion of the household head, and education of the household head. All columns include district fixed effects, sector fixed effects, and survey month fixed effects. The row labeled "Control mean" indicates the average outcome for those whose youngest children were aged above 5 before the 2013 survey. The sample is restricted to households with the youngest children aged 2 to 10 years old. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

Table A.6
Spillover effects of mother's international migration restriction on child health care.

	Outpatient			Inpatient		Chronic disease
	Any	Illness	Check-up	Any	Illness	Any
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × After	0.002	−0.005	0.003	−0.005	−0.002	−0.002
	(0.015)	(0.015)	(0.003)	(0.007)	(0.006)	(0.006)
Control mean	0.288	0.276	0.007	0.051	0.038	0.038
Observations	20,219	20,219	20,219	20,219	20,219	20,219

Note: The table presents spillover effects of restricting mothers' international migration on child health care. The dependent variables are dummy variables indicating outpatient visits for any reason, illness, and check-ups; inpatient visits for any reason and illness; and the presence of any chronic disease. "Treated" is a dummy variable indicating that the household's youngest child is below age 5, while "After" is a dummy variable indicating that the survey wave was conducted in 2016. Standard errors are clustered at the household level. Control variables include a school dummy, and family composition (including the number of children aged 0–4 years, 5–9 years, and 10–14 years), ethnicity of the household head, religion of the household head, and education of the household head. All columns include age fixed effects, district fixed effects, sector fixed effects, and survey month fixed effects. The row labeled "Control mean" indicates the average outcome for households whose youngest children were aged above 5 before the 2013 survey. The sample is restricted to children aged 5 to 10 years, who are expected to be in school, corresponding to the education analysis. * denotes significance at the 0.10 level; ** at the 0.05 level; and *** at the 0.01 level.

Data availability

The authors do not have permission to share data.

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