SOPHIA INSTITUTE FOR HUMAN SECURITY

Discussion Paper Series

SIHS Discussion Paper No. 5

Restricting Mothers' International Migration and Human Capital Investment

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April 2025

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Tokyo, Japan

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ABSTRACT

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Keywords: human capital, health, education, remittance, Sri Lanka
JEL code: F22, F24, I12, O15.

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*We thank Nancy Chau, Chris Barrett, Brian Dillon, Johannes Haushofer, Shuhei Kainuma, and Kazushi Takahashi as well as seminar and conference participants at Australasian Development Economics Association Conference, GRIPS, Tohoku University, Nagoya City University, and Kobe University for their constructive comments. This work is financially supported by Sophia University Special Grant for Academic Research, Grant-in-Aid for Research at Sophia Institute of Human Security, and the Joint Usage / Research Center, Institute of Economic Research, Hitotsubashi University. (Grant ID: IERPK2511).

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1 Introduction

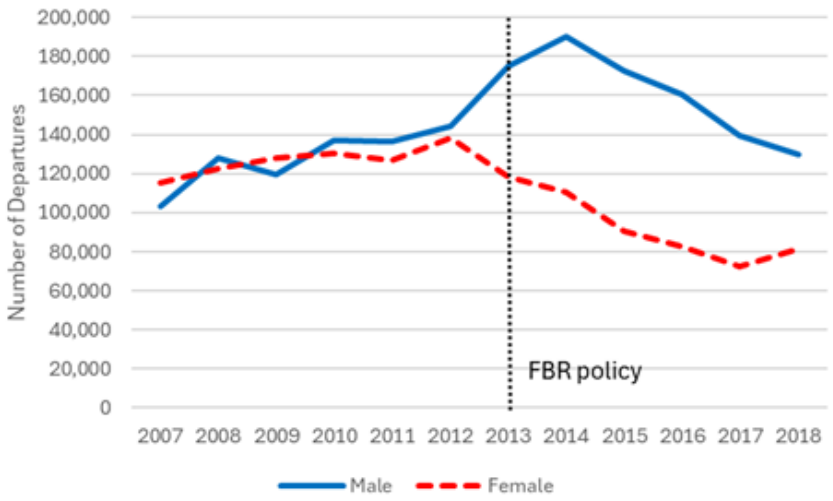
International migration plays an important role in developing countries. Remittances remain a crucial source of external finance for low- and middle-income countries. Officially recorded remittance flows to these countries reached an estimated $656 billion in 2023 (World Bank, 2024). Remittances provide people in low-income countries with higher incomes and greater economic opportunities, and they are linked to improved child outcomes, including better education outcomes and reduced child labor in sending communities (Edwards and Ureta, 2003; Alcaraz, Chiquiar, and Salcedo, 2012). 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 restricting parental migration, particularly that of mothers, on children remains unclear, as it involves trade-offs between the income gains from international migration and the loss of a mother's 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, arguing that 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) 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. As intended, the policy, which was introduced in 2013, led to a sharp decline in female international migration departures, as shown in Figure 1. In contrast, male migration continued to increase until 2014.[1] In particular, the policy indeed decreased departures among lower-skilled female groups including domestic workers (Weeraratne, 2021).

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 mediates 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

[1] In the analysis below, we do not find strong evidence of substitution toward migration by males or fathers.

Figure 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.

child is aged 2 to 10, to avoid 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. This finding supports the idea that a mother's presence is crucial, mirroring the findings of Meng and Yamauchi (2017), which demonstrate the negative impact of parental migration (that is, parental absence) on 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. However, 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.

The increase in domestic remittances is not statistically attributable to intra-household labor reallocation, although the point estimates suggest a possible association. There is no strong evidence that household members are more likely to migrate domestically or that the number of working adults increases in response to policy exposure. This is probably because the margin affected by the FBR policy applies to a relatively small proportion of the population. Another interpretation of the results is that households compensate for income loss due to migration restrictions by increasing domestic remittances, rather than reallocating labor within the household, reflecting the idea that existing domestic migration networks or household labor diversification can buffer income shocks (Stark and Lucas, 1988; Batista and Vicente, 2023; Bettin, Jallow, and Zazzaro, 2024). Despite no significant impacts in labor market activities, policy exposure raises the likelihood of a household having a female engaged in housework activity by 2.6 percentage points, suggesting that remaining mothers contribute more to childcare.

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 this 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 target population rather than their siblings, showing significant direct effects but no significant indirect effects.

We also examine spillover effects on non-targeted children's educational outcomes, defined only for school-aged children.[2] 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.

This paper contributes to three strands of literature. First, it adds to the growing body of work on the impacts of migration on left-behind household members. There are two main perspectives on how international migration opportunities affect left-behind children. The first suggests migration can improve child human capital development in origin households, primarily through increased remittances or foreign income (Yang, 2008; Gibson and McKenzie, 2014; Carletto, Covarrubias, and Maluccio, 2011; De Brauw and Mu, 2011; Alcaraz, Chiquiar, and Salcedo, 2012).[3] For instance, Mobarak, Sharif, and Shrestha (2023) examines the effects of international migration from Bangladesh to Malaysia on remittances, finding positive impacts on the living standards of migrants' families.[4] The second perspective highlights the negative effects of parental separation due to migration, often studied in the context of Chinese rural-urban migration (Cameron, Meng, and Zhang, 2022; Zhang et al., 2014).[5] For example, Zhang et al. (2014) found that being left behind by both parents significantly impairs children's cognitive development and reduces test scores, whereas the effects are much smaller and insignificant when only one parent is absent.

There are at least two distinct features in this paper. First, our findings underscore the potential

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

[3]Gibson and McKenzie (2014) studied a seasonal worker program in New Zealand, finding significant effects on household outcomes, including child education. Carletto, Covarrubias, and Maluccio (2011) found higher height-for-age z-scores and lower stunting prevalence among children in Guatemalan households with a migrant to the U.S. De Brauw and Mu (2011) linked migration in China to underweight outcomes for older children, but found no such effect for younger children, especially if cared for by grandparents. Alcaraz, Chiquiar, and Salcedo (2012) showed that negative shocks in remittance receipts led to higher child labor and reduced school attendance among Mexican migrant families.

[4]Similarly, Bryan, Chowdhury, and Mobarak (2014) shows that migration induced by cash transfers increased food and non-food expenditures for migrants' families by 30-35% and improved caloric intake by 550-700 calories per person per day.

[5]Cameron, Meng, and Zhang (2022) found that parental absence during childhood, due to migration, is associated with increased criminality in adulthood in rural China. Meng and Yamauchi (2017) showed parental urban migration adversely affects health and educational outcomes of rural children in China. Huang, Jiang, and Sun (2024) demonstrated that mother-child separation negatively impacts child development.

asymmetric effects of migration restriction, in contrast to migration promoting policies such as visa lotteries and cash transfers, which have been more extensively documented in the literature (Gibson, McKenzie, and Stillman, 2011; McKenzie and Yang, 2012; Bryan, Chowdhury, and Mobarak, 2014; Gibson et al., 2018; Mobarak, Sharif, and Shrestha, 2023). Examining this aspect is crucial, given that the absolute impacts of expanding versus restricting opportunities to international migration may differ. Households appear to cope with the income loss due to reduced remittance ex post, as evidenced by the increased domestic remittance. Second, this policy is unique in that it is gender-specific, by exclusively restricting mothers' migration. Existing evidence largely come from countries with male-dominant migration,[6] making it important to investigate gender differences in migration impacts. In this respect, Cortes (2015) examines the gendered impacts of parental migration on child education in the Philippines, a country with a high share of female migrants similar to Sri Lanka, demonstrating that a mother's absence has a more pronounced detrimental effect than a father's. Our results complement the literature by showing that an increased mother's presence, encouraged by the mother-targeted migration restriction, indeed positively affects human capital investment in children.

Second, this paper speaks to the literature on the importance of parental investment in children (Francesconi and Heckman, 2016). Maternal care is one of the most important factors for child development in early childhood (Luby et al., 2016). Early maternal employment, which reduces maternal care, has been shown to lower cognitive development (Brooks–Gunn, Han, and Waldfogel, 2002; Waldfogel, Han, and Brooks-Gunn, 2002). A vast literature also explores the relationship between maternal employment and child health (Anderson, Butcher, and Levine, 2003; Morrill, 2011). The link between maternal care and child development is a critical issue, particularly in developing countries where access to supplementary formal childcare is limited and patriarchal norms tend to place heavy expectations on women for child-rearing. We contribute to this literature by demonstrating that increased maternal presence—likely driven by migration restrictions that encourage maternal care—improves child health outcomes. While previous studies primarily examine the effects of maternal care along the intensive margin (e.g., the number of hours spent together, full-time vs part-time employment), our study focuses on the sharp extensive margin by comparing maternal presence versus absence.

Third, this paper contributes to ongoing policy debates on the restriction of international migration, particularly in developing countries.[7] While not only Sri Lanka but also several countries

[6]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).

[7]For example, on June 27, 2022, Sri Lanka's Cabinet of Ministers partially eased the requirements under the FBR, allowing women with children over two years old to migrate for employment abroad (Weeraratne, 2022; Arambepola,

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. This study provides causal evidence on how such migration restrictions affect children left behind. Our findings suggest that the policy benefits children, aligning with its intended goal of child protection. However, we also highlight the other side of the coin: restricting international migration significantly reduces remittance inflows, thereby limiting income-generating opportunities at both household and national levels. The availability of alternative sources, such as domestic remittances, may play an important compensatory role. Moreover, 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.[8]

2 Study Design

2.1 Background

Sri Lanka has sent more than 200,000 migrants every year since 2002, and the number reached the 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 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 of 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.

Although the migrant labor has brought the benefits to the country, it has also imposed costs on household members left, particularly children. Given the concerns on the welfare of children left behind due to mother's absence, the Sri Lankan government took a policy action by issuing Circular 13/2013 in June 2013. The Circular requires female domestic workers to fill in a Family Background Report (FBR) as a pre-departure requirement, which came into effect on 15th July

2022).

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

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 seek 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.[9] 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.

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 its gender-related aspect has also been discussed and explored by UN (2015). 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 declined was mainly concentrated among low-skilled groups, which include domestic workers. By exploiting exogenous variation in 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.

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 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.

[9] The Circular also establishes minimum age requirements for migrants themselves, which are different by destination regions.

2.2 Conceptual Framework

There are two potential pathways through which the policy on restricting mother's international migration affects human capital investment. First, the policy may positively impact children through the increased presence of mothers. Maternal time investment is crucial for child human capital accumulation, as highlighted by a vast body of literature (Francesconi and Heckman, 2016). Early childhood, in particular, is a critical period for development and subsequent life outcomes (Luby et al., 2016; Almond, Currie, and Duque, 2018). The aim of the restrictive migration policy is to ensure that mothers are present during this crucial stage of a child's life. Previous studies have found adverse impacts of maternal migration of child outcomes such as health, education, and cognitive development (Cortes, 2015; Meng and Yamauchi, 2017; Bai et al., 2022).

Second, the policy may have a negative impact due to the reduction in international remittances. The loss of economic opportunities caused by migration restrictions, and the resulting decline in household income, can adversely affect children. The importance of remittances for children has also been documented (Edwards and Ureta, 2003; Antman, 2012; Alcaraz, Chiquiar, and Salcedo, 2012).

While these pathways may have conflicting effects, the negative impact of reduced remittances may be less severe than expected, as households cope with the restriction. First, they may rely on pre-existing networks for financial support—for instance, receiving remittances from relatives in migration hubs. Second, households may reallocate labor to offset lost economic opportunities abroad. While mothers stay home to care for children as intended by the policy, fathers or other members may compensate by migrating abroad, migrating domestically, or increasing their local labor supply. Alternatively, mothers themselves may pursue domestic migration, as the policy restricts only international migration but not domestic migration. How households respond to the policy is an empirical question.

The primary target of the FBR policy is children under the age of 5, as it restricts their mothers from migrating. However, both the potential benefits of increased maternal presence and the potential costs of reduced international remittances extend beyond these targeted children to their older siblings as well. This policy design enables us to examine not only direct and overall effects but also potential spillover effects on older siblings. This spillover channel is particularly relevant to our analysis of educational outcomes, such as school attendance. Since primary education begins at age 5 in Sri Lanka, the targeted children are preschool-aged, meaning the policy's effects on educational outcomes are likely to operate through this spillover channel.

2.3 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,[10] 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 last 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 discuss 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 migrating or present at home.[11]

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 to receive remittances from abroad, with an annual average amount of 14,200 LKR. Remittances are relatively common: 9% of households reported to receive remittances from abroad within the last 12 months, while 8% received domestic remittances within the same period. The annual average amount is 9,340 LKR, which is about two-thirds of the amount received from abroad. 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.[12]

[10] 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.

[11] 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 listed as a grandchild of the head or as a nephew/niece, their relationship is recorded as "other relative," making it impossible to identify their parent and, consequently, whether the mother is present.

[12] Appendix Figure A1 shows the relationship between the age of the youngest child and two outcomes: migration

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	14200.79	[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. Migration outcomes are defined for the past 12 months.

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 child 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-up and treatment for illness. We also analyze the presence of any chronic disease. There is notable variation across different outcomes. On average, 33.6% of children experienced any outpatient visit, primarily due to illness, while reported checkups 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 prevalence of chronic diseases is 3.5%, which aligns with expectations given the population of young children.[13]

Apart from chronic disease, we use healthcare utilization to measure child health, whereas pre-

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.

[13]In Appendix Figure A2, 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 disease appears constant across all ages at very low rates.

Table 2: Summary statistics of child development outcomes

	Mean	[SD]	N
Health			
Any outpatient	0.336	[0.472]	32621
Outpatient for illness	0.323	[0.467]	32621
Outpatient for check-up	0.008	[0.091]	32621
Any inpatient	0.064	[0.245]	32621
Inpatient for illness	0.050	[0.218]	32621
Any chronic disease	0.035	[0.184]	32621
Education			
School attendance	0.986	[0.117]	20221
Grade retention	0.003	[0.054]	18479
Grade (current year)	3.428	[1.576]	19892
Age appropriate grade	0.975	[0.156]	19892

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 conditional on her being attending school.

vious studies (e.g., Meng and Yamauchi, 2017; Gosselin-Pali, 2025) 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 3.

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.[14] Table 2 shows that primary education, which is both mandatory and

[14]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 6, Grade 2 for age 7, and

free in Sri Lanka, appears to be highly effective—99% of children attend school, the rate of grade retention are minimal, and 97% of children are in age-appropriate grade without any cumulative grade repetition. Consequently, our empirical analysis focuses on the relatively small margins of these outcomes.

2.4 Econometric Strategy

We now turn to the empirical set-up. The main research question of the paper is whether the migration restriction of mothers affects human capital investment in heath and education ultimately. We test this question by comparing households with the youngest child above or below age 5 to capture the policy exposure, before and after the the introduction of the FBR policy. To interpret the overall effects on children, we explore potential mechanisms guided by Section 2.2.

Policy exposure is defined based on the age of the youngest child in the household at the time of the survey, as the FBR policy restricts migration for households with a youngest child under the age of 5.[15] This definition captures the current policy restriction rather than the duration of exposure. Our estimation is a difference-in-differences specification using this policy exposure as the cross-sectional dimension and the pre- vs. post-policy comparison as the temporal dimension.[16]

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:

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

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,

so on). Note that the last three outcomes can only be defined for children who are attending school.

[15] 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.

[16] Note that as discussed in Section 2.1, households in our comparison group is previously affected by the policy though less restrictive. Thus, the estimated effects below is likely to underestimate the impact of restricting international migration than comparing with pure control households.

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 equation (1), but the sample consists of children whose ages 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 five. 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 5. That is, a child aged above five will have a value of 1 for this variable if they have a younger sibling under the age of five. 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.

2.5 Identifying Assumptions

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 for pre-trends. However, we provide a discussion supporting the plausibility of this assumption in this context.

First, Appendix Table A1 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, there is a concern regarding the timing and exposure to the policy. For instance, a mother with a 5- and 6-year-old child in 2016 is not currently restricted from migrating under the policy but were restricted when the policy was in effect two 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 and 6 in 2016 may have been influenced by the policy implemented in 2013. Including these children and their households into the control group may bias the estimated effects of the policy. We refer to this issue as "previously treated". We will test whether this issue affects our findings later.

Fourth, 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.[17]

3 Estimation Results

3.1 Main Results

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 is whether the mother migrates abroad and whether the households have mother present at home. While Column 1 uses all the 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 decreased

[17] 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 birthdate.

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	22419	17213	17213

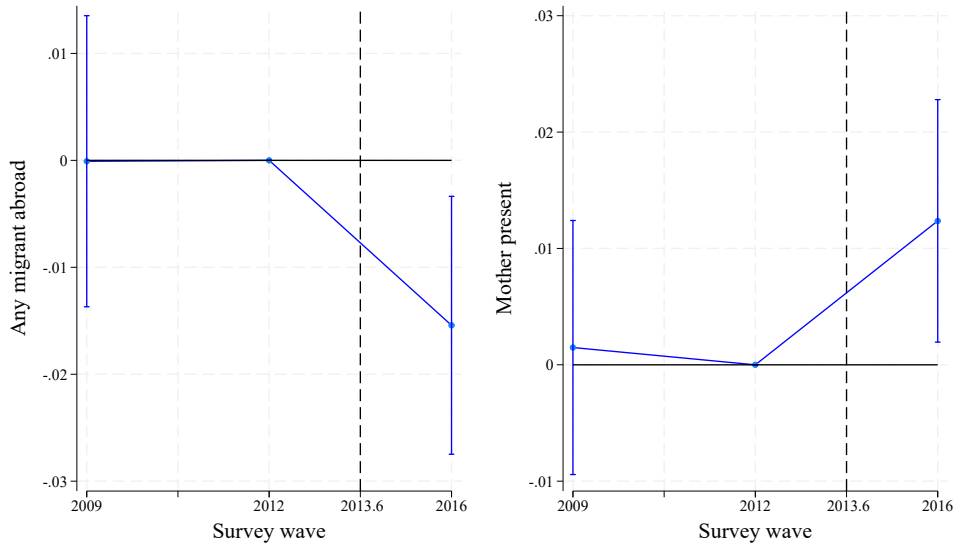
Note: The table presents DID estimates of the impact of restricting mothers' international migration on mothers' presence outcomes. The dependent variable in column 1 is whether any household member migrates abroad; column 2 is whether mother migrating abroad, constructed by two conditions: whether mother not present and any household member migrating 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 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 household head, religion of household head, education of 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 sample' those for whom detailed household composition can be identified (See 2.3 for the definition). * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

mothers' international migration by 0.7 percentage points, which is substantial 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 point from the control group of 97.4%.[18]

Figure 2 shows the event-study plots on any migrant abroad and mothers' presence showing wave-specific treatment coefficients. The DID estimates shown above seem 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.

[18] 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.

Figure 2: 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.

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 align 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 checkup. However, there 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.[19]

These results appear to be driven by the increased presence of mothers at home. The policy led to mothers remaining at home to care for their children, which, in turn, contributed to improved

[19]In Appendix Figure A3, 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.

child health outcomes. This suggests that the policy as intended improved 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 nutritious outcomes.

However, we should interpret healthcare utilization carefully, as it is related to not only health conditions but also access to healthcare service. In contrast to our preferred interpretation, there is an alternative interpretation of the results. As discussed in Section 2.2, 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 service, without implying any actual change in children's underlying heath 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 2.1, 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.

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	32621	32621	32621	32621	32621	32621

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 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 household head, religion of household head, education of 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. Cluster standard errors at the household level. * denotes significance at the 0.10 level; ** at the 0.05 level; and *** at the 0.01 level.

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

First, we provide evidence that the issue of previously treated households, discussed in Section 2.5, may not affect our results in a substantial way. Appendix Figure A4 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 A2 also presents the result of an additional robustness check addressing the issue of the previously treatment. We exclude children from households with youngest children aged 5–6, as they were exposed to the policy at its introduction but 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 1.3 percentage points.

Next, we examine the effects on the parent-child subsample to assess sensitivity and the potential impact of sample selection bias.[20] Appendix Table A3 presents the effects of restricting the samples to 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 the 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 Figure A5 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.

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

3.2 Other possible mechanisms: Income effects and intra-household labor substitution

Our main results above show that the 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 2.2. 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 the policy exposure at the 10% significance level. On the other hand, the policy exposure increases the inverse hyperbolic transformed amount of domestic remittances by 0.20. We interpret this as households adjusting their decision-making in response to the policy. 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 remittance.

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), Bettin, Jallow, and Zazzaro (2024), and Stark and Lucas (1988).

Column (6) shows the effects on total household income. Interestingly, despite the 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.[21]

[21] See Appendix table A4 for the effects on the detailed disaggregated composition of income.

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	22419	22419	22419	22419	22419	22419

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 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 household head, religion of household head, education of 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 sample' those for whom detailed household composition can be identified (See 2.3 for the definition). * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

Table 6 presents the impact of restricting mothers' international migration on intra-household labor reallocation and household composition. We find no evidence that policy restriction targeting mothers leads to a substitution toward fathers' international migration or domestic migration by other household members. Column (1) shows no significant effect on the likelihood of fathers migrating abroad.[22] Column (2) 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. 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.[23,24]

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 the 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.

3.3 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 five, 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

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

[23] This potential underestimation is likely due to the policy design—specifically, households with the youngest child aged above five 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.

[24] 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 migrant abroad	Any household member migrating domestic	Number of female adult	Number of male adult	Any female housework
	(1)	(2)	(3)	(4)	(5)
Treated × After	-0.006	0.015	0.031	0.019	0.026**
	(0.006)	(0.012)	(0.019)	(0.021)	(0.012)
Control mean	0.035	0.106	1.421	1.168	0.674
Sample	Parent-child	All	All	All	All
Observations	17213	22419	22419	22419	22419

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 migrating abroad, any household member migrating domestic, number of female adult (without migrant), number of male adult (without migrant), 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 household head, religion of household head, and education of 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.

effects, we split the sample into two groups: (1) children who are the youngest in the household or are above age five, representing the direct effect, and (2) children who are not the youngest and are above age five, representing the indirect or spillover effect.

Table 7 presents this subsample analysis.[25] 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.

[25] 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 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	23254	23254	23254	23254	23254	23254
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 household head, religion of household head, and education of 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.

3.4 Effects on (non-targeted) sibling's education

This section extends 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.

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.[26] 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 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.[27] 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.

4 Conclusion and policy implication

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

[26] 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%.

[27] Appendix Table A5 repeats the spillover analysis for health care 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.

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	20221	18479	19892	19892

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 household head, religion of household head, and education of 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 5–10, who are expected to be in school, as educational outcomes are well-defined only within this age range (with retention specifically defined for children aged 6–10). Cluster standard errors at the household level. * denotes significance at the 0.10 level; ** at the 0.05 level; and *** at the 0.01 level.

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.

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 study context, domestic re-

mittances helped offset the loss of income from abroad. But in settings with limited access to labor markets or weak remittance transfer mechanisms, the overall impact on child development may be ambiguous—income losses could 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 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.

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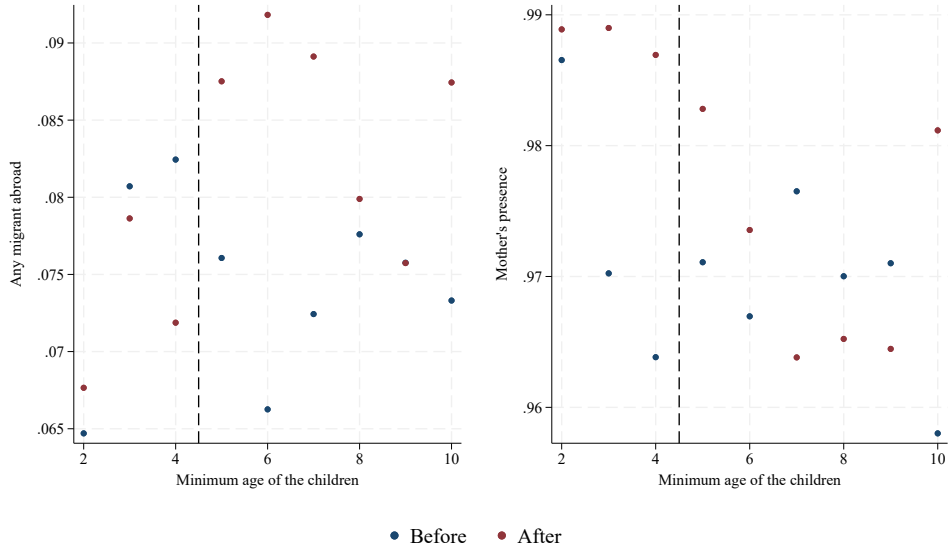
Appendix

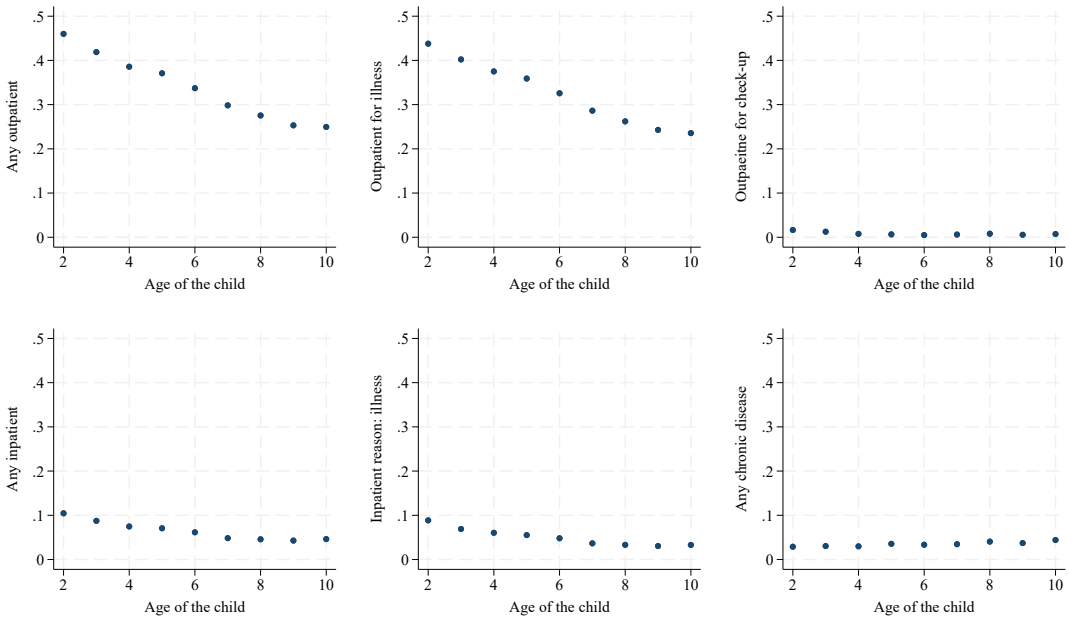
A Appendix Figures and Tables

Figure A1: Migration and the age of the youngest child



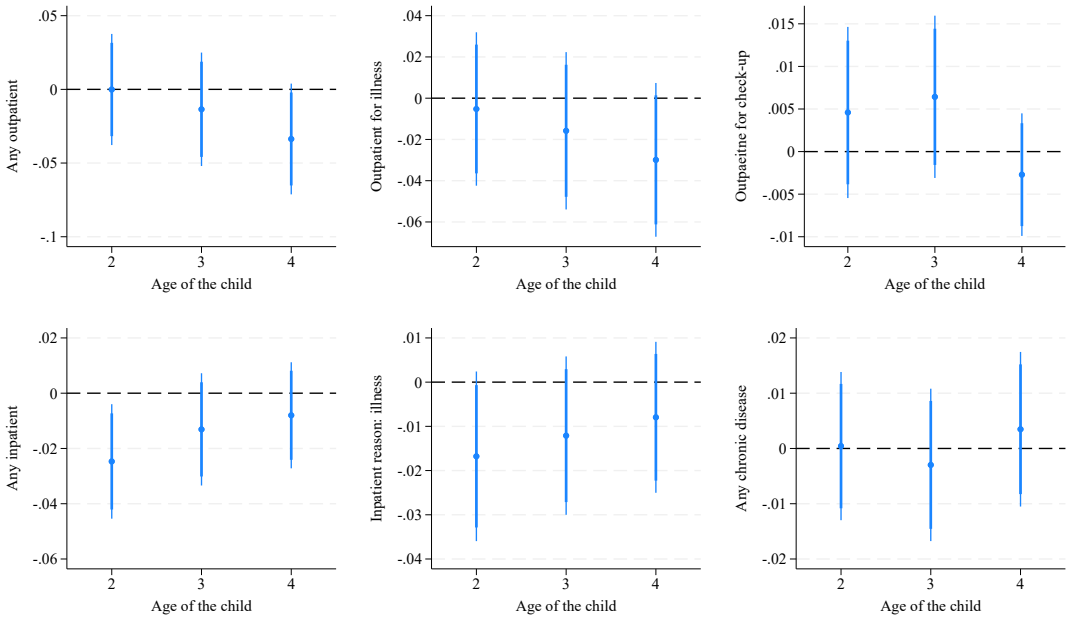
Notes: These figures depict the relationship between the age of the youngest child in households and migration outcomes: any migrant abroad (left panel) and mother's presence (right panel). "After" refers to data from 2016, while "Before" refers to data from 2009/10 and 2012/13, indicating whether the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

Figure A2: Child health behavior over age



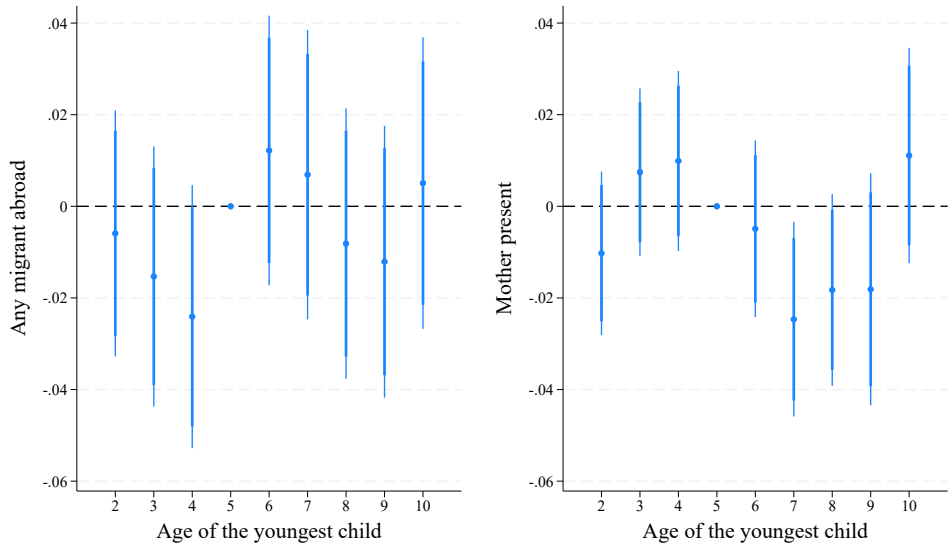
Notes: $N = 32621$ children. These figures show the distribution of the child health behavior outcomes over age of the child.

Figure A3: Own age-specific coefficients of child health behavior



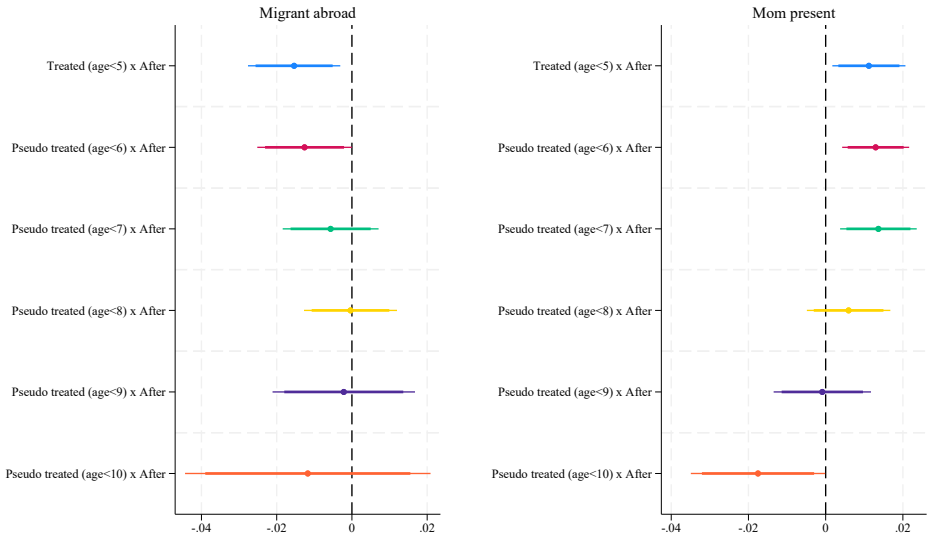
Notes: The figure shows the heterogeneity by the age of the child for DID coefficients on child health behaviors.

Figure A4: 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.

Figure A5: 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 A1: Summary statistics by treatment (pre-policy periods)

	(1) All Mean/SD	(2) Treat Mean/SD	(3) Control Mean/SD	(4) Pairwise t-test Mean/SD
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	10133.39	10046.52	10206.45	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	14658	6696	7962	14658

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 on the restricted samples that we are able to identify detailed relationship of household members. The sample is restricted to pre-policy periods, i.e., 2009 and 2012 wave. Annual household income and remittance are evaluated by LKR.

Table A2: 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	-0.008	-0.009	0.002	-0.013**	-0.013**	0.001
	(0.014)	(0.013)	(0.003)	(0.006)	(0.006)	(0.005)
Control mean	0.347	0.335	0.007	0.073	0.058	0.035
Observations	25543	25543	25543	25543	25543	25543

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 household head, religion of household head, and education of 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 with minimum brothers/sisters aged 2 to 10 years old among siblings and exclude those whose ages are 5 and 6. Cluster standard errors at the household level. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

Table A3: 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	25267	25267	25267	25267	25267	25267

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 household head, religion of household head, and education of 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 with minimum brothers/sisters aged 2 to 10 years old among siblings and restricted to child-parent samples. Cluster standard errors at the household level. * denotes significance at 0.10; ** at 0.05; and *** at 0.01.

Table A4: 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	22419	22419	22419	22419	22419	22419	22419

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 household head, religion of household head, and education of 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 A5: 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	20219	20219	20219	20219	20219	20219

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 household head, religion of household head, and education of 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.