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Moving towards a better future? Migration and children's health and education

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Moving towards a better future?

Migration and children's health and education

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Abstract

Do the returns to migration extend beyond migrants themselves and accrue to the children of migrants? Drawing upon data from a unique 19-year longitudinal survey from Tanzania, this paper empirically investigates this question by exploiting the variation in the outcomes of the children of migrants and the children of the migrants' siblings who stayed behind conditional upon a range of individual characteristics of their parents. I show that parental migration has important implications for child development. This relation depends on the destination and the timing of the move. More specifically, children whose parents migrated from rural areas to cities are heavier, taller and more educated for their age. The effects on height and schooling are strongest for children who were exposed to the city environment during their early childhood. In contrast, children whose parents moved to a different rural village do not appear to experience any health advantage and those moving alongside their parents even start schooling at a later age. In addition to conferring a broader view of the returns to physical mobility, this analysis contributes to the debate on the origin of spatial inequalities in developing countries.

JEL: R23, O15, I15, I25

Keywords: Internal migration, health, education

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1. Introduction

Human mobility is recognized as a salient part of livelihood strategies in developing countries and widely acknowledged as integral to the process of development. Although data on internal migration is scarce, available estimates suggest that movements within country borders are pervasive. Bell and Charles-Edwards (2013), for example, show that 763 million individuals in the world can be characterized as internal migrants. These migrants bring their children with them or start families at their destinations. As such, the long-run implications of these movements are likely to be determined by the impact they have on the human capital formation of the next generation. Moreover, ample evidence of substantial disparities in child health and education outcomes across space raise the question whether moving and therefore exposing children to a different environment can contribute to their well-being. In fact, the hope for a better life for their (future) children is a commonly cited motivation for migration. Yet, we know surprisingly little about the consequences of migration for children of migrants.

Research on the relationship between migration and the health and education of migrants' children in developing countries is relatively sparse and yields diverging results. Most importantly, while the available studies provide some interesting insights, they fail to address the issue of selection that is inherent in the decision to move.¹ That is, a simple comparison of children of migrants and children of non-migrants is uninformative about the causal impact of moving, as the propensity to migrate may be related to some of the same attributes that determine children's health and education outcomes. The observed differences thus combine the causal effect of moving with a selection effect. As such, the crucially important question of whether and how parental migration causally affects the human capital formation of the next generation remains largely unanswered.

Drawing upon data from a unique 19-year longitudinal survey, which tracked migrants to their destinations, this paper presents an empirical study of the relation between parents' migration and children's growth, immunization, reported health and schooling in Tanzania. I address the aforementioned identification problem arising from the endogeneity of migration by exploiting the variation in the outcomes of the children of migrants and the children of the migrants' siblings who stayed behind. The resulting estimates are thus purged of the between-household component of migrant selection that can, for example, arise from differing propensities to migrate among households that are financially constrained. In addition, this removes the component of individual unobserved characteristics that is shared by siblings including, for example, certain genetic health or ability predispositions. Furthermore, I rule out confounds due to individual characteristics observed before migration that could influence the propensity to move and the health and education of the children. As such, this paper goes much further in addressing selection bias than previous studies.

The results reveal that parental migration to urban areas and especially to large cities is signi-

¹Some studies aim to address selection by controlling for or matching on parent or household characteristics observed after migration (e.g. Smith-Greenaway and Thomas, 2014; Stephenson et al., 2003; Escobal and Flores, 2009). Yet, this will reflect adaptation after relocating and thus mechanisms through which migration causally affects children. In addition, many factors likely to influence selection into migration are unmeasured.

ificantly associated with improved child growth. Specifically, I show that moving from a rural area to a city leads to an increase in weight-for-age and reduces the likelihood of being underweight by 8.7 percentage points. I also find evidence of a large gain in height-for-age for children whose parents moved to cities. Assuming that this difference persists through adulthood, these children are predicted to be 3.5 cm taller. For the offspring of relatively poorer families, migration to cities also halves the likelihood of being stunted. In addition, the results demonstrate that children of migrants born in cities obtain significantly more schooling for their age. The effects on height and schooling are strongest for children who were exposed to the city environment during their early childhood.

Taken together, these findings support the view that migration to urban areas and especially to cities can play an important positive role in further development and poverty reduction through human capital accumulation. In particular in the context of a country where 3.3 million children under five risk not reaching their full physical and cognitive potential due to inadequate nutrition, the establishment of a large and robust positive association between parental migration to cities and child growth has important implications. A large body of evidence demonstrates the pertinence of child anthropometrics as predictors of long-term health, cognitive development, and labor market outcomes (e.g. Currie and Vogl, 2013). Notably, one extra centimeter of height has been associated with wage increases ranging from two to ten per cent in a developing country context (Haddad and Bouis, 1991; Vogl, 2014; Strauss and Thomas, 1998; Schultz, 2002). In combination with the demonstrated positive association between growing up in a city and schooling, this highlights the crucial role that migration from rural to urban areas can play in the structural transformation of developing countries (e.g. De Brauw et al., 2014).

Intra-rural migration – still the dominant migration stream in Tanzania and the rest of sub-Saharan Africa – does not appear to be significantly associated with child growth. What is more, children who moved alongside their parents to a different rural village experience a delay in the start of their schooling. Given the substantial internal migration flows in the country, any potential educational disadvantage for children of rural-rural migrants could have detrimental consequences for future human capital development in Tanzania. At the very least, this suggest that there is a compelling case for further work.

These results pass a large set of robustness tests using alternate definitions of migration. In addition, I show that exposure to an urban destination is positively associated with height-for-age even after accounting for all parent characteristics that are fixed. That is, the difference in height-for-age between siblings born to rural-urban migrants is proportional to the difference in the share of their life they have spent in the urban environment. It is reassuring to see that this alternative strategy leads to the same conclusion for the impact of migration to urban areas on child growth. This suggests that unobserved heterogeneity between migrant and non-migrant parents is not driving the results derived from the main regression model. The validity of the approach proposed in this paper is further supported by the fact that the findings are robust to restricting the analysis to children born several years after their parents' relocation, thus avoiding concerns of reverse causality.

Beyond providing a more accurate estimate of the intergenerational impact of migration, I am

able to infer the nature and extent of migrant selection across households. Specifically, a comparison of the “within-family” estimates and the “naïve” estimates reveals that rural-urban migrants are drawn from families that have certain attributes contributing to improved child growth and education outcomes. Depending on the outcome of interest, this positive selection leads the naïve estimates to overstate the child growth returns to migration to cities by 9 to 82 per cent. Similarly, although I find a sizeable effect on schooling-for-age even when controlling for factors that are fixed within the family, it amounts to less than one third of what a simple comparison of children of migrants and non-migrants would predict. While the differences between the naïve and within-family estimates are attenuated by controlling for observable family characteristics, they remain economically and statistically significant. This highlights the importance of addressing observed and unobserved household selection into migration for estimating the impact of moving on children’s health and education.

Furthermore, in addition to describing the relationship between parental migration and child health and education outcomes in Tanzania, I seek to understand how these effects come to be. This exploration of the underlying mechanisms indicates that an important part of the growth benefit is related to higher incomes and improved housing conditions for those who moved to cities. Yet, even after accounting for these differences, the estimated returns to parental migration on child height and weight remain sizeable and statistically significant. This shows that there are other mechanisms at play and again reveals a need for continued research efforts.

This paper contributes to different strands of research. First, it speaks to literature on physical mobility in developing countries (e.g. Beegle et al., 2011; Christiaensen et al., 2013; de Brauw et al., 2013; Garlick et al., 2016; Nguyen et al., 2015). In particular, this paper makes an important contribution by going much further in addressing selection bias than previous studies on the relationship between parents’ migration and children’s health and education. Specifically, it is the first to use the variation in the outcomes of the children of siblings to identify the effect of parental migration. Moreover, this research confers a broader view of the benefits and costs of internal migration by showing that the returns to migration extend beyond migrants themselves and that migration can contribute to intergenerational mobility. In addition to improving their own welfare, migrant parents in Tanzania are able to enhance their children’s health and education and as such their current well-being as well as their future earning potential.

More broadly, this analysis contributes to debate on the origin of spatial inequalities in developing countries (e.g. Ravallion and Wodon, 1999) and sheds new light on the research on rural-urban labor productivity gaps (e.g. Hicks et al., 2017; Gollin et al., 2002). While the findings regarding selection into migration across households support the hypothesis that the population sorts itself geographically (e.g. Lagakos and Waugh, 2013; Young, 2013), these results indicate that the observed variation in child outcomes across space is not merely an intergenerational extension of these patterns of selection. That is, the evidence of large returns to moving that increase with the duration of exposure to the city environment even when accounting for selection across households and on observable pre-migration characteristics strongly suggest that growing up in a city causally affects child development. These findings therefore confirm the importance of childhood environments for

development (e.g. Chetty et al., 2016; Chetty and Hendren, 2018; Chyn, 2018). Most importantly, this implies that further identification and understanding of the characteristics of the city environments that are conducive to human capital accumulation can provide important insights for designing rural development strategies as well as health and education policies.

The remainder of the paper is organized as follows. Section 2 briefly introduces the conceptual framework for the analysis and reviews potential channels through which migration may affect children’s health and education. Section 3 describes the context and the data. The empirical approach is outlined in Section 4. Section 5 presents the results. Potential threats to the validity of the research design are evaluated in Section 6. Section 7 explores the underlying mechanisms through which migration affects the human capital of the next generation and Section 8 concludes.

2. Conceptual framework

In general, the impact of migration on the well-being of the next generation can be assumed as a function of where parents move. If growing up in a certain environment has a causal effect on child health or education, moving there can be expected to have a similar impact on the children of migrants. Migration may, for example, directly promote or harm child health by exposing them to a different epidemiological environment (Brockerhoff, 1990). In addition, given the considerable heterogeneity in the provision of public goods across space in most developing countries (e.g. Gollin et al., 2017), moving will affect families’ physical proximity to several inputs that have been shown to be important contributors to child health and education differentials. This includes clinics, clean drinking water, electricity, and schools (e.g. Van de Poel et al., 2007; Handa, 2002; Filmer, 2007).

In addition, the structure of the labor market at the destination is also likely to determine how migration will affect child well-being. First, this will shape parents’ employment opportunities and, therefore, their budget available for health and education inputs. More income may also relax liquidity constraints that prevent parents from investing in children in the current period in order to reap the returns in the future (Hildebrandt and McKenzie, 2005). At the same time, however, increased employment may reduce parental time investments, as it raises the opportunity cost of time spent on child rearing (e.g. Glick and Sahn, 1998; Cawley and Liu, 2012). Secondly, female labor market participation may influence fertility behavior (e.g. Van den Broeck and Maertens, 2015). Reduced family size has been found to be associated with greater human capital investments when these are severely constrained (e.g. Hatton et al., 2018; Li et al., 2008; Rosenzweig and Zhang, 2009). Greater birth spacing has also been found to promote child health (Conde-Agudelo et al., 2006). Thirdly, parents will allocate their time and budget based on their expectations regarding returns to human capital. Higher rates of return to education in particular are assumed to encourage migrant parents to increase their investments in children’s schooling (e.g. Kong et al., 2010). Yet, when demand for low-skilled labor is more abundant, the lower return on investment combined with the increased opportunity cost of time spent in school may reduce school attendance.

Through exposure to and interaction with the local population and their norms, values, and

practices, moving can also lead to alternative patterns of knowledge and skill acquisition and the adoption of different child-raising practices (Brockhoff, 1990; Macours and Vakis, 2010; Smith-Greenaway and Madhavan, 2015; Ssengonzi et al., 2002). Interactions with groups that have lower fertility norms are, for example, expected to lower fertility among migrants (e.g. Brockhoff and Yang, 1994; Kulu, 2005; Lindstrom, 2003).

In particular, urban areas are assumed to offer conditions that would contribute positively to child well-being including improved infrastructural conditions, more (diverse) income earning opportunities, higher returns to education, better access to services and greater exposure to more desirable child-care practices and fertility behavior (e.g. Smith et al., 2005; Van de Poel et al., 2007). Hicks et al. (2017) further hypothesize that the nature and density of social interactions in urban areas may provide a more intellectually stimulating environment for developing children. It is, however, worth noting that there is great diversity among urban environments (Dorosh and Thurlow, 2013). An important dimension of this heterogeneity is across city sizes. Specifically, the distinction between large “cities” and smaller “secondary towns” is deemed central (e.g. Christiaensen and Kanbur, 2017; Roberts, 2014). Cities and secondary towns, for example, differ in the sectoral composition of their economy, potentially influencing income levels and opportunity costs of time. Several studies point to a positive association between various measures of productivity and urban scale (e.g. Duranton, 2008,0). In addition, an urban gradient with higher average incomes and lower poverty levels in cities compared to secondary towns has been documented (e.g. Christiaensen and Kanbur, 2017). Furthermore, partly because of a lower per capita cost in more densely populated locations, the provision of public goods tends to be greater in larger cities. Gollin et al. (2017) demonstrate that denser areas in sub-Saharan Africa are, for example, more likely to have health facilities, electricity grids, piped water and sewerage systems. A similar pattern holds for the ownership of consumer durables and housing quality.

Yet, there may be important trade-offs between different aspects of urban living. As mentioned above, increased income could come at the expense of reduced parental time investment. Growing up in urban environments can also increase children’s exposure to pollution, their risk of traffic-related injuries, and is often associated with more sedentary lifestyles and unhealthy dietary patterns (Van de Poel et al., 2007; WHO, 2016; Popkin et al., 2012). In addition, the increasingly rapid pace of urbanization and the growing pressure on infrastructure and services in cities in developing countries have conjured up less favorable views of life in urban areas. That is, there are rising concerns that migrants from rural areas are unable to fully integrate in and enjoy the benefits of urban society as they end up living in crowded dwellings in slums with poor infrastructure and services (e.g. Mberu and Mutua, 2015; Omariba and Boyle, 2010).

The impact of migration is also likely to differ depending on when parents move. That is, given the growing evidence on the formative role of the earliest years of life for further childhood development (e.g. Black et al., 2017; Grantham-McGregor et al., 2007), the effect of moving at a later age may be more limited. To the extent that the act of moving is disruptive, migration could also have harmful effects on children who move during these formative years of their lives regardless of the destination.

The physical process of moving can, for example, be associated with hardship for young children and pregnant women (Brockhoff, 1990). Moreover, at least in the short term, the cost of moving and loss of earnings may put families under financial strain (Omariba and Boyle, 2010; Smith-Greenaway and Madhavan, 2015). Moving away from home can also result in a break in networks and support groups. Migrant parents are likely to be physically separated from additional caregivers and stand to lose contact with people who would give (financial) support and advice on childcare (Brockhoff, 1994; Ssengonzi et al., 2002).

More generally, it is important to acknowledge that physical proximity does not guarantee immediate utilization of services. That is, parents will have to familiarize themselves with, locate and gain access to health, education and other services at the destination (Brockhoff, 1994; Smith-Greenaway and Thomas, 2014). This process is likely to be more challenging when the origin and destination are very different and can be strained by legal, language or cultural barriers (Smith-Greenaway and Madhavan, 2015). In consequence, migration is often assumed to (temporarily) interfere with health seeking behavior, particularly in the case of immunization, which tends to be performed by campaigns at specific moments of which parents may not (yet) be aware. Similarly, children moving alongside their parents will be unable to attend school during relocation and enrollment in a new school may not be immediate. Coffey (2013) argues that migration may therefore break the habit of going to school for this group of children. In addition, leaving home and coping with conflicting norms at the destination may cause psychological stress (Brockhoff, 1994).

Despite the magnitude and importance of the issue, there remains a dearth of rigorous research documenting the relation between parental migration and the human capital of their children in a developing country setting. Previous studies have mainly concentrated on child mortality with several studies pointing to a survival advantage for children of rural-urban migrant mothers (Bocquier et al., 2011; Brockhoff, 1990,9; Islam and Azad, 2008; Omariba and Boyle, 2010; Stephenson et al., 2003; Tam, 1993; Thomas, 2007).² Research on other measures of health, including immunization (Antai, 2010; Kiros and White, 2004; Smith-Greenaway and Madhavan, 2015) and child growth (Deb and Seck, 2009; Escobal and Flores, 2009; Xu and Xie, 2015), is more sparse and yields diverging results. Similarly, while a number of papers assess the relation between parental migration and education outcomes, especially in the context of China (Coffey, 2013; Ferrone and Giannelli, 2015; Kusadokoro and Hasegawa, 2017; Liang and Chen, 2010; Lai et al., 2014; Resosudarmo and Suryadarma, 2014; Wang et al., 2017; Xu et al., 2018), they reach different conclusions.

Given its association with urbanization and structural transformation, available research has mainly concentrated on flows between rural areas and urban centers, leaving the question of whether and how other migration streams, including intra-rural movements, the dominant migration flows in much of the developing world (Lucas et al., 2015), affect the next generation largely unanswered.³ More importantly, as mentioned above, these studies largely fail to address the issue of selection that

²Smith-Greenaway and Thomas (2014) and Ssengonzi et al. (2002), however, conclude that there is no evidence of a significant difference in child mortality among children of rural-urban mothers and their rural native counterparts.

³Exceptions are Kiros and White (2004), Smith-Greenaway and Thomas (2014) and Ssengonzi et al. (2002).

is inherent in the decision to move. The fundamental question is therefore whether the differences in health and education outcomes between children of migrants and non-migrants are the result of migration or whether they simply reflect differences in the observable and unobservable characteristics of their parents. It is important to note that this distinction between selection and the causal impact of moving is not solely of scholarly interest. If parental migration causally affects child health and education, the ongoing population movements are likely to have far-reaching consequences for human capital formation, productivity, economic growth and welfare. In addition, the existence of a causal effect of migration suggests that the observed geographic disparities in child outcomes in developing countries do not merely reflect selection or systematic differences in the types of people living in each area but rather causal effects of growing up in a certain environment. This can provide important insights into the underlying determinants of human capital accumulation

3. Context and survey data

3.1. Tanzania and Kagera

Tanzania, a low income, low human development country in East Africa, provides an interesting case study to investigate the relationship between migration and the health and education of migrants' children.⁴ Migration features prominently in the country's demographic landscape. According to the 2002 census, 5.3 million Tanzanians were living outside their region of birth and over 1.1 million people had moved to a different region in the year preceding the survey (NBS, 2006). In 2012, the corresponding estimates amounted to 7.8 million and 1.5 million (NBS, 2015). Dar es Salaam, the former capital, is by far the main destination, to the extent that the number of in-migrants exceeds that of its' native population (NBS, 2015).

In addition, the country is characterized by stark differences in living conditions and indicators of child health and education, giving even more prominence to the question whether relocating is a viable strategy for parents to improve their children's well-being and future earning potential. Poverty in Tanzania remains overwhelmingly rural with minimal progress over time (NBS, 2014). This is reflected in large heterogeneity in housing conditions including access to clean drinking water, electricity, and toilet facilities (NBS, 2011,0). The availability and quality of education and health services similarly differs across rural and urban zones (Mtahabwa and Rao, 2010; NBS, 2011,0). Unsurprisingly, children living in rural areas and less-developed regions fare worse on a wide range of health and education outcomes. For instance, whereas an estimated 37.8 per cent of children under the age of five living in rural Tanzania in 2015/16 suffered from stunting, the corresponding rates were 24.7 and 14.6 per cent in urban areas and Dar es Salaam respectively. Similarly, net secondary school attendance was estimated at 36.3 per cent in urban areas compared to 16.2 per cent in rural zones (NBS, 2016).

⁴In 2017, GDP per capita was estimated at 2,683 international dollar at purchasing power parity (World Bank, 2018) and the country was ranked 154th out of 189 countries in terms of the Human Development Index (UN, 2018).

Kagera, the predominantly rural region of origin of the migrant parents in this study, is one of the least developed regions in Tanzania. An estimated 42 per cent of the population was living below the national poverty line in 2010 (UNDP, 2014). More than three quarters of households lived in houses with an earth or sand floor in 2011/12 and only 37 per cent of households had access to water from a protected source (NBS, 2016). Child malnutrition remains endemic in the region. The prevalence of stunting has been largely stagnant at around 40 per cent (NBS, 2011,0). An estimated 75.9 and 18.9 per cent of children were attending primary and secondary school in 2015/16 (NBS, 2016). As in the rest of Tanzania, there has been substantial migration within and outside Kagera over the past decades (Beegle et al., 2011). According to the 2002 Census, 176,312 individuals or 9.1 per cent of the population born in the region, were residing elsewhere in the country and 52,694 had moved in the preceding year (NBS, 2006). The out-migration rate remained at 9.1 per cent in 2012, with 222,404 individuals born in the region residing elsewhere (NBS, 2015).

3.2. Kagera Health and Development Survey (KHDS)

The Kagera Health and Development Survey (KHDS) was conducted in three rounds and six waves (see Figure 1). During the first survey round (four waves), 915 households from 51 villages in all five districts of the Kagera region (see Figure 2) were interviewed up to four times from fall 1991 to spring 1994. While initially designed to assess the impact of the spread of HIV/AIDS in the area, later rounds of the KHDS aimed at providing data on economic mobility and changes in living standards within North-West Tanzania. In the second and third round in 2004 and 2010 respectively, the survey team attempted to re-interview all baseline household members including those who were residing in new households and/or had moved away from their original village, region, or country (Beegle et al., 2006). This effectively transformed the original household survey into an individual longitudinal survey.

While the baseline sample consisted of 6,353 individuals living in Kagera, efforts to track them and the subsequent inclusion of members from the newly formed split-off households in the following rounds ultimately resulted in a sample of 15,094 individuals living in 3,324 households in 18 different regions by 2010 (see Figure 1). The KHDS has maintained remarkably low attrition rates. Excluding those who had passed away, 82 and 85 per cent of original respondents were re-contacted and re-surveyed in 2004 and 2010 respectively (De Weerdt et al., 2012). In summary, the KHDS records migration moves of around five thousand individuals over a 20-year period. Approximately 97 percent of the moves in the sample took place within the borders of Tanzania.⁵

This extraordinarily long panel survey is particularly suitable for studying the impact of parental migration on child health and education since it allows us to observe the parent before migration and his or her children after migration. This offers interesting opportunities to address the issue of selection (e.g. Beegle et al., 2011). The long time-span covered is additionally helpful because, even

⁵Only those who moved to Uganda were re-interviewed. Another 29 and 58 respondents were reported to have moved to other countries by 2004 and 2010 respectively by their initial household members, other relatives or neighbors residing in the baseline communities (Beegle et al., 2006; De Weerdt et al., 2012).

in countries with high migration rates, migration is a rare event. Consequently, household surveys covering relatively short periods may fail to identify enough occurrences of migration within the sample to make statistically meaningful conclusions. This is particularly true when focusing on the intergenerational impact of migration as this requires us to concentrate on the subsample of migrants who have children. To the best of my knowledge, this paper will be the first to use longitudinal data covering the period before and after migration to study the impact of parental migration on children’s health and education in a developing country setting.

3.3. Defining “children” and “parents”

As the explanatory variable of interest, migration, is determined at the level of the child’s parent, correctly linking parents to their offspring is essential. The household questionnaires of the KHDS include a module collecting information on the parents of all household members. More specifically, respondents were asked to report for each household member whether their natural mother and father were still alive and whether they were residing in their household. In addition, the respondents were required to indicate the parents’ names, which were then linked to a personal identification number if they were part of any of the six survey waves. In combination with information on reported sex and relationship to the household head, this information is used to identify parental relationships (see Appendix A1). The unit of analysis for this paper are individuals below the age of 20 observed in 2004 and/or 2010 residing in the same household as at least one of their identified parents. They are from here on referred to as “children”.⁶

3.4. Defining “migration”

When studying migration, particular difficulty lies in defining a “migrant”. Rather than focusing on self-reported information of migration as an event, which is subject to considerable measurement error, my preferred measure of migration is instead based on changes of residence between the survey rounds as reported by the survey team. More specifically, the tracking forms indicate whether an individual was found living in a (split-off) household in (i) the baseline village, (ii) a neighboring village, (iii) elsewhere in Kagera, (iv) elsewhere in Tanzania, or (v) in a neighboring country. Moves are thus reported in reference to the individual’s location in 1991/94. Consequently, migration status can only be determined for those individuals who were interviewed during the baseline survey round and at least one of the subsequent rounds. In the main analysis, only those who are found to no longer reside in their baseline or a neighboring village are considered migrants. The use of this measure also implies that even though migration in this particular context has been shown to be an iterative process (Ingelaere et al., 2018), only the move to the current location is taken into account. As there is some ambiguity around migrant status, various robustness checks are conducted to examine how the results vary with alternate definitions (see Appendix B).

⁶While the legal age of majority in Tanzania is 18, the choice of the age cut-off was based on the WHO adolescent growth reference data which are available up to the age of 19.

As mentioned in section Section 2, the characteristics of the destination are expected to shape the impact of parental migration. In particular, the vast differences in living conditions between rural areas and urban zones in Tanzania raise the question whether relocating to towns and cities as opposed to a different rural village may have a distinct impact on children. Table 1 therefore describes parents' migration patterns according to the rural or urban nature of their destination.⁷ To take into account the aforementioned diversity among urban environments, parents who moved to cities with a population exceeding the common threshold of 500,000 inhabitants are distinguished from those who relocated to other urban areas, referred to as secondary towns.⁸

Although traditionally the focus of much of the literature is on movements from rural to urban areas, migration in this sample is dominated by intra-rural relocation, accounting for more than half of the moves observed between 1991/94 and 2010 in this sample. Moreover, secondary towns are by far the most common urban destination for migrant parents originating from rural Kagera. Only one out of three rural-urban migrant parents moved to a larger city.⁹ Given the predominantly rural nature of the Kagera region, the sample of migrants moving out of an urban area (and their offspring) is relatively limited. As such, the analysis reported in this paper will focus on migration flows originating from rural Kagera. More specifically, the explanatory variable of interest is a dummy variable at the level of the child equal to one when at least one of his or her parents can be identified as a rural migrant based on the definition discussed above.¹⁰

Baseline data for these future migrants and their respective households are shown in Table 2. On average, they were living in relatively large farming households with total annual consumption per capita corresponding to approximately 606 dollar (PPP). While the difference in income between migrant in non-migrant households is not statistically significant, a significantly higher share of migrant households were living in dwellings with a non-earth floor. The highest level of education in these households corresponds roughly to finishing primary school. A higher level of education was reported for only 20 per cent and 11 per cent of the migrant and non-migrant sample respectively. Interestingly, these differences appear to derive mostly from the households of future rural-urban migrants. While there are no clear observable differences in the household characteristics of those who stayed and those who moved to different rural areas, on average future rural-urban migrants are part of significantly wealthier, more educated households with improved living conditions. This

⁷This classification follows from the 2002 Census for which all localities identified as urban by the district authority were classified as urban (NBS, 2006).

⁸This cut-off of 500,000 inhabitants was proposed, for example, by OECD(2012), Christiaensen and Todo (2014) and Christiaensen et al.(2017). According to this threshold, Mwanza city, which consists of the Illemela and Nyamanga district (343,001 and 363,452 inhabitants), Dar es Salaam (4,364,541 inhabitants) and Kampala (1,507,080) are the only cities (NBS, 2015, UBS, 2016). The results are robust to changing this threshold to 1,000,000. These results are available upon request.

⁹Of the 400 rural-urban migrant parents observed 76 moved to Dar es Salaam, 57 to Mwanza, and 7 to Kampala.

¹⁰For the majority of children, the migration status of either the mother or the father is observed. Most often, the (future) parents of children below the age of 20 in 2004 and/or 2010 were not yet residing in the same household during the first survey round in 1991/94, which implies that the likelihood of having observed them both at baseline is very small. When referring to migrant "parents" this should therefore not necessarily be equated to couples who moved together.

positive selection into migration to urban areas in terms of initial household wealth is not surprising. Narratives of the life histories of migrants from the KHDS reveal the importance of liquidity or cash constraints in determining the choice of destination (Ingelaere et al., 2018). Interestingly, these differences are most pronounced when focusing upon those who will move to towns. Future migrants are on average younger and more likely to be unmarried. Moreover, those who will relocate to cities obtained significantly more schooling for their age. This pattern is often assumed to be related to higher demand for skilled labor in cities (e.g. Hicks et al., 2017; Young, 2013).

Women in Kagera tend to migrate more than men, though they often travel differently and for different reasons. That is, women tend to move between rural areas and their mobility is likely to be related to the marriage system, which is characterized by patrilocal residence, where a woman leaves her kin to live with her husband (Kudo, 2015). As shown in Figure 3, marriage is indeed the most-cited reason (31%) for moving out of the baseline villages. This share amounts to 44 per cent for migrant women and is particularly common for women who moved to different rural areas (56%). It is worth noting that economic conditions at the destination have historically been embedded in women’s decision-making pertaining to marriage-related relocation. Using the KHDS data, Kudo (2015), for example, demonstrates that the probability of males marrying increases in villages where land inheritance rules changed in favor of widows.

3.5. Outcome variables

3.5.1. Measuring health

The analysis reported in this paper will focus on six measures of child health, four of which pertain to child growth or nutrition status.

More specifically, for children up to the age of 19, standardized height-for-age z-scores were calculated as per WHO guidelines. This measure shows how many standard deviations the child is away from the age- and sex-specific median height in a reference population of well-nourished children (Onis et al., 2007; WHO, 2009). A child is considered stunted when his or her height-for-age z-score is more than two standard deviations below the reference median. Stunting indicates cumulative growth deficits in children largely caused by inadequate diets and frequent infections in the first few years of life. Linear growth failure in children is strongly associated with increased child mortality as well as shorter stature, impaired cognitive development, and reduced school achievement and economic productivity in adulthood (e.g. Black et al., 2008; Dewey and Begum, 2011; Victora et al., 2008). More generally, height has been demonstrated to be an important predictor of (future) labor market outcomes in developing countries (e.g. Hoddinott et al., 2008; Schultz, 2002; Thomas and Strauss, 1997).

Similarly, weight-for-age scores were derived for children below the age of 10.¹¹ Underweight is

¹¹Weight-for-age reference data are not available beyond age 10 because this indicator does not distinguish between height and body mass in an age period where many children are experiencing a pubertal growth spurt and may appear as having excess weight (by weight-for-age).

defined as having a weight-for-age z-score more than two standard deviations below the reference median. Whereas height-for-age and stunting reflect cumulative growth deficits (especially during the first thousand days after conception), weight-for-age and underweight pertain more to the child’s current nutrition status and are more responsive to short-term changes. Though stunting is argued to represent a more important overall indicator of undernutrition (Victora et al., 2008), evidence shows that the mortality risk of children who are even mildly underweight is increased (Black et al., 2008).

In addition, respondents were asked how many times a child under the age of five was vaccinated to protect them against polio, tuberculosis, tetanus and measles. Children are considered fully immunized when they are reported to have been administered at least one dose of a vaccine that protects against tuberculosis; three doses of a vaccine that protects against tetanus; three doses of a polio vaccine and one dose of a measles vaccine by the age of one. The variable of interest is therefore a dummy equal to one when a child above the age of one is reported to have received all eight required vaccinations.

Finally, the reported health of the children is examined. Respondents were asked whether they perceived their health as (i) bad, (ii) fair, (iii) good, (iv) very good, or (v) excellent. For young children this question was answered by their parents or other responsible adults. While offering the advantage of providing an indication of the overall health status rather than some specific sub-components, it is important to acknowledge the limitations of this type of measure. First, different people may have different ideas about what it means to be healthy. In addition, the difference between these ranks does not represent a fixed or known interval. That is, we know that “bad” is lower than “fair” and “fair” is lower than “good”, but it is not clear whether the distance between “bad” and “fair” is greater or smaller than the difference between “fair” and “good”. Schroeder and Yitzhaki (2017) discuss the associated perils of the cardinal treatment of ordinal variables. To overcome the latter problem, the analysis is focused on the likelihood of being in good health as captured by a dummy variable equal to one when a child is reported to be in good, very good or excellent health and zero otherwise.¹²

3.5.2. Measuring education

In addition to these indicators of child health, this paper aims to investigate how parental migration influences three distinct measures of child education: schooling-for-age, school attendance and age at school entry.

In particular, to facilitate comparison across different ages, the information on the highest grade obtained by the child is transformed into a schooling-for-age z-score (e.g. Joshi and Schultz, 2005; Orazem and King, 2008).¹³ That is, each grade is assigned to the associated number of years of

¹²The results are unaltered when the analysis is repeated for a dummy variable equal to one when a child is reported to be in i) very good or excellent health, or ii) fair or bad health. These results are available upon request.

¹³The use of different measures for comparing schooling among children of different ages does not alter my findings.

schooling (for details see Appendix A2). By subtracting from this the median years of schooling for children of that age and sex and dividing this difference by the age- and sex-specific standard deviation of years of schooling in this sample, a z-score is derived that indicates the number of standard deviation the child is removed from the age- and sex-specific median years of schooling in this sample.

I also assess whether migration affects school attendance for children of primary or secondary school age (i.e. between seven and 19 years old) in the 12 months preceding the survey. Another education outcome of interest is the age at which the child started school. While the relationship between school starting age and education and labor market outcomes is not straightforward and evidence mostly derives from developed countries (e.g. Angrist and Krueger, 1992; Black et al., 2011), advanced age at school entry has been associated with primary school dropout in Uganda and Mozambique (e.g. Grogan, 2008; Wils, 2004). Moreover, partaking in pre-primary education has also been associated with improved education outcomes in later life in Uruguay and Mozambique (Berlinski et al., 2009; Martinez et al., 2012).

Table 3 summarizes the individual and household characteristics of children of migrant parents observed in 2004 and/or 2010. It is important to note that the average age of migrant children in this sample is 5.7 years, which is below the starting age for (compulsory) primary school. The subsample of migrant children whose education outcomes are observed is therefore somewhat limited. From Table 3, we can further derive that children of migrants live in smaller, wealthier households with more educated members and improved living conditions. In line with Gollin et al. (2017), I find that concerns that rural-urban migrants end up in slums with worse than rural living conditions thus appear to be largely unwarranted. Instead, on average, children of rural-urban migrants and especially those who moved to larger cities have far better living conditions. The data further confirm the existence of an urban gradient, with higher incomes and improved housing conditions in cities as compared to secondary towns. Less than five per cent of children of migrants who relocated to large cities, for example, live in houses with an earth floor and 96 and 61 per cent have access to clean drinking water and electricity.

4. Empirical approach

Identifying the causal impact of parental migration on child well-being requires comparing the current health and education of children of migrant parents to their health and education outcomes had their parents stayed in their community of origin. This counterfactual is of course not observed, so the measurement of the returns to migration is most often obtained from comparing migrants to non-migrants. A naïve approach to estimating the intergenerational impact of migration is to simply compare the outcomes of children of migrants to children of non-migrants. That is, the following

More specifically, the results are robust to using a “grade-for-age” measure that is obtained by dividing the years of education a child has obtained by the number of years a child is expected to have obtained in the Tanzanian schooling system at this age. These results are available upon request.

equation is estimated from the sample of children surveyed in 2004 ($t + 1$) and/or 2010 ($t + 2$):¹⁴

$$Y_{i,t+r} = \alpha M_{i,t+r}^{Parent} + \beta X_{i,t+r} + \gamma Z_{i,t+r}^{Parent} + \epsilon_{i,t+r} \quad r = 1, 2 \quad (1)$$

Where $Y_{i,t+r}$ denotes the health or education outcome of child i observed in period $(t+r)$, where r takes on the value of one or two to indicate the 2004 or 2010 survey round. $M_{i,t+r}^{Parent}$ is a dummy variable that equals one when child i is born to at least one parent who is observed to have moved out of his or her baseline rural community in Kagera between 1991/94 (t) and 2004 ($t + 1$) or 2010 ($t + 2$). The term $X_{i,t+r}$ represents a vector of individual level characteristics of the child including sex and birth order, and age cohort fixed effects to account for potential time trends. In addition, a dummy variable indicating the survey round in which the child's outcomes were measured is added.¹⁵ $Z_{i,t+r}^{Parent}$ denotes a vector of parents' characteristics observed in period $(t+r)$, i.e. sex and age group fixed effects. Following Abadie et al. (2017) all results reported in this paper are presented with standard errors clustered at the level of the primary sampling unit, which corresponds, to the parents' village of origin.

The results summarized in Table 4 show that children of migrants are significantly taller and heavier for their age than their non-migrant counterparts. The latter is further reflected in a 3.3 percentage point decrease in the likelihood of being underweight. On average, children of migrants also start school approximately 1.5 months earlier.

These estimates would represent the true intergenerational return to migration if migrants were selected randomly from the population in Kagera, but not if the parents of these children self-selected into migration. In this case, there is likely to be heterogeneity across migrant and non-migrant households and migrating and non-migrating household members (see Table 2). To the extent that this heterogeneity is correlated to the health and education of their children, the estimated coefficients will be biased since the observed differences between migrant and non-migrant children combine the causal effect of moving with a selection effect. That is, those who move may have characteristics contributing to improved or impaired parental inputs regardless of their location. The mere fact that migrant parents observed in these data tend to stem from wealthier and more educated families, for example, could easily imply that their children would have been taller and heavier even if their parents had stayed in their village of origin.

I address the issue of heterogeneity across migrant and non-migrant families by including "parent sibling fixed effects" (S_i^{Parent}). In essence, this implies that the effect of parental migration is

¹⁴Whether a child is stunted, underweight, perceived as in poor health, fully vaccinated or attending school is captured by binomial variables. For these outcomes this equation is estimated using a linear probability model (LPM) following Angrist and Pischke (2008). The main findings reported in this paper based on estimations according to the LPM are not altered when using the method suggested by Beck (2018) for estimating marginal effects with grouped data and a binary outcome variable. These results are available upon request.

¹⁵An obvious concern when pooling the data from the 2004 and 2010 survey rounds is that child outcomes differ depending on the year of observation. The main specification includes survey round fixed effects that will capture any general shift in the distribution of child outcomes between the two survey rounds. The results are, however, robust to including round-specific parent sibling fixed effects. This effectively implies that the outcomes of a child of a migrant can only be compared to his or her non-migrant cousin observed in the same round.

identified using the variation in the outcomes of the children of migrants and the children of their parents' siblings who remained in their village of origin or a nearby village in Kagera.¹⁶ The resulting estimates are thus purged of the between-households component of migrant selection. This approach therefore removes bias due to aspects of family background that are correlated with the probability of migration and child health and education outcomes such as household wealth or attitudes. In addition, it eliminates the component of unobserved characteristics that is shared by siblings including, for example, certain genetic health or ability predispositions.

It is worth noting that the collective dimension of migration decisions and the role of migration as a household welfare-maximizing strategy in developing countries have long been recognized in the literature (e.g. Stark and Bloom, 1985). Using the first two rounds of the KHDS data, Beegle et al. (2011), for example, confirm the importance of household selection into migration empirically. Yet, even within families, migration is unlikely to be random and siblings may differ in personal attributes that are correlated with the propensity to migrate as well as with their children's health and education outcomes. To address this concern of heterogeneity across migrant and non-migrant siblings, the regression specification is further augmented with a vector of parent's baseline characteristics ($Z_{i,t}^{Parent}$) that includes sex, age group fixed effects, birth order, relation to the household head, schooling-for-age, marital status, and height.¹⁷ In sum, the regression model looks as follows:

$$Y_{i,t+r} = \alpha M_{i,t+r}^{Parent} + \beta X_{i,t+r} + \gamma Z_{i,t}^{Parent} + S_i^{Parent} + u_{i,t+r} \quad r = 1, 2 \quad (2)$$

The main interest of this study lies in establishing the sign, size and statistical significance of α , which reflects the difference in the health and education outcomes between children of migrant parents and the children of their siblings who stayed behind in their baseline or neighboring villages in rural Kagera conditional on their parents' observable (pre-migration) characteristics. In order to explore the heterogeneity of the intergenerational effects of migration, several augmented versions of equation (2) will be estimated. The migration dummy will, for example, be interacted with the rural or urban nature of the destination or the proportion of the child's life that has been spent at the destination.

5. Results and discussion

5.1. Parental migration and health

This section examines the impact of parental migration on children's (i) height-for-age, (ii) being stunted, (iii) weight-for-age, (iv) being underweight, (v) having received all required vaccinations,

¹⁶Fifty-eight per cent of the migrant parents in this sample have at least one sibling whose children were also observed in 2004 and/or 2010. The identification of siblings is discussed in detail in Appendix A.

¹⁷Individuals were often not measured in all four waves of the first Survey Round in 1991/94. Data from the latest survey wave in which the parent was measured are therefore used. The regressions also includes survey wave fixed effects that will capture any general shift in the distribution of child outcomes depending on the survey wave in which their parents were last measured.

and (vi) reported health.

The estimations following equation 2 are reported in Table 5. These results reveal that only for weight-for-age, is there a statistically significant difference between children of migrants and their non-migrant counterparts when restricting the comparison to cousins and controlling for parents' observable pre-migration characteristics.

As mentioned above, it seems plausible that the characteristics of the destination play a crucial role in shaping the intergenerational impact of migration. To assess whether relocating to a town or city as opposed to a different rural village has a distinct impact, Table 6 presents two sets of results for each health outcome. The first column reports the results of the regressions where the dummy variable indicating whether a child is born to a migrant parent is interacted with the rural or urban nature of the destination, generating a dummy variable equal to one for a child of a rural-rural migrant (M_{Rural}^{Parent}) or a rural-urban migrant (M_{Urban}^{Parent}). In the second column, children born to parents who moved to cities (M_{City}^{Parent}) are further distinguished from those who relocated to secondary towns (M_{Town}^{Parent}).

The results firmly indicate that destination matters. Whereas there is no evidence of any significant difference between the children of migrants who moved to a different rural village and children of stayers, rural-urban migration appears to be associated with a significant gain in weight and height. That is, even when compared to their cousins in their mother's or father's baseline or neighboring village, children of rural-urban migrant parents are significantly heavier for their age. The estimated difference amounts to 0.24 standard deviations. This implies that 5.5 year old children (the average age of the children of migrants in this sample) born to rural-urban parents are on average approximately 0.5 kg heavier than their cousins growing up in rural Kagera. Similarly, although the difference of 0.21 standard deviations is only marginally significant, children of rural-urban migrants are taller for their age. For a 5.5-year-old child this corresponds to a height difference of approximately one cm.¹⁸

The results of the regressions distinguishing secondary towns from cities (see Table 6, column 2) show that children of parents who moved to larger cities mainly drive this relation between rural-urban migration and child growth. The estimated coefficient in the regression of weight-for-age indicates that 5.5-year-old children of migrants to cities are more than one kg heavier than the children of their parents' siblings who stayed behind.¹⁹ These children are also nearly nine percentage points less likely to be underweight. Moreover, there is a highly significant and positive relation between parental migration to cities and height-for-age. The point estimate of the difference

¹⁸This can be derived by taking the average of the product of the point estimates (corresponding to the difference in z-scores) and the sex-specific standard deviation of height and weight for a child aged five years and six months.

¹⁹Even though the weight of 83 per cent of the children in this sample is below reference median, it is important to acknowledge the concerns about the rising prevalence of overweight and obesity in developing countries and the role of cities as obesogenic environments (e.g. Popkin et al., 2012; Ruel et al., 2017). However, there is no evidence of a significant, positive association between parental migration to cities and the likelihood of being overweight, defined as having a BMI-for-age z-score more than one standard deviation above the reference median (WHO, 2009), for five to 19 year old children. Results are available upon request.

in z-scores represents a height gain of approximately 2.5 cm for a child aged 5.5. Assuming that this difference persists through adulthood, the offspring of migrants who moved to cities is predicted to be approximately 3.5 cm taller than the children of their parents' siblings who stayed behind in rural Kagera.²⁰ The economic importance of these growth benefits should not be underestimated. One extra centimeter of height has, for example, been associated with wage increases ranging from two to ten per cent in a developing country context (e.g. Haddad and Bouis, 1991; Vogl, 2014; Strauss and Thomas, 1998; Schultz, 2002). This height premium in wages is most often attributed to the greater physical strength and improved cognitive development of taller individuals, which increases their productivity (Currie and Vogl, 2013).

Despite this sizeable gain in height, the reduction in the likelihood of being stunted is not statistically different from zero. Interestingly, there is evidence of a large and highly significant difference in the prevalence of stunting when allowing for heterogeneous effects of parental migration on human capital formation along the initial distribution of wealth. In particular, children of parents from the lower half of the distribution of initial consumption per capita who relocated to cities are 20 percentage points less likely to be stunted than the children of their parents' siblings who stayed behind in their baseline villages in Kagera. Similarly, the difference in the likelihood of being underweight appears to largely derive from children of relatively poorer migrant parents (see Appendix C, Table 1). Finally, it is worth noting that the difference in the growth indicators of children whose parents moved to secondary town and their non-migrant cousins is subject to more variation. The coefficients are much less precisely estimated and smaller in terms of magnitude.

The advantages or disadvantages of the destination may become more pronounced with additional time spent at the destination. Moreover, as outlined in Section 2, it stands to reason that the early phases of in-migration may have some disruptive effects. As such, in addition to the destination, the timing of the move is likely to be important in shaping its effects on children.

From the reported information on the year of the move and the duration of residence at the destination, it can be derived whether children were born before (or in the year of) or after the move to their current location.²¹ The large majority (65 %) of children of migrants in this particular sample was in fact born at the destination. For the children that were born before the move to their current location, the average age at migration was 3.6 years. In Table 7, the results of the regressions where the dummy variable for migration is interacted with the rural or urban (or town or city) nature of the destination as well as the timing of the move are reported.

When distinguishing between children who were born before or in the year of the move ($M_{Destination,before}^{Parent}$) and those born at the destination ($M_{Destination,after}^{Parent}$), it appears that the previously established effect of parental migration to cities on height derives from the latter group of

²⁰While interesting for the purpose of interpreting the magnitude of the coefficients, this assumption may not hold. Using data from the KHDS, Hirvonen (2014) shows that catch-up growth after low growth in infancy is possible and occurs most frequently during puberty.

²¹In the 2010 round respondents were asked to report the year in which they moved to their current location. In 2004, the question was not asked in this manner, but the information could be derived from the length of time respondents reported living at their current location.

children. While there is no significant difference in height-for-age between children of migrants who were born before the move and their non-migrant cousins, those born after their parents had moved to a city are significantly taller. This is in line with expectations, given the demonstrated importance of the first two years of life for linear growth (Shrimpton et al., 2001; Victora et al., 2010; Leroy et al., 2014). As mentioned above, weight is generally more responsive to short-term changes. Unsurprisingly, the results confirm that the positive effect of migration on weight-for-age extends to children born before and after the move. In fact, there appears to be a linear relationship between the share of the child's life spent at the destination and height- and weight-for age (see Appendix C, Table 2). Contrary to the findings of Brockerhoff (1994), there is no support for the existence of a harmful effect of migration on children born in the two years surrounding the move, which can be argued to represent a particularly disruptive phase of migration. There is no statistically distinguishable difference between the health outcomes of these children of migrants and their non-migrant cousins growing in rural Kagera (see Appendix C, Table 3).

Interestingly, none of the estimated regressions show support for the hypothesis that parental migration to cities contributes to an increase in the likelihood of a child's health being perceived as good or very good. This appears to stand in contrast to the established growth advantage for children whose parents moved to a city. Yet, it is likely to reflect changes in the parents' reference points for what good health means upon moving. The results do not support the hypothesis that migration affects vaccination coverage. This is also confirmed when looking at the different types of vaccinations separately (see Appendix C, Table 4). It is worth noting that when focusing upon children whose parents moved to towns outside of Kagera, a region that has consistently had among the highest vaccination coverages in the country (NBS, 2006,0), the results reveal that children are significantly less likely to have received all necessary vaccinations (see Appendix C, Table 5- 6). The effect appears to derive from a reduction in likelihood of having received the three necessary doses of the vaccine against tetanus.

5.2. Parental migration and education

Next, I aim to assess whether migration influences the education of the migrants' offspring. In Table 8, I present the results from estimating the within-family relationship between parental migration and (i) schooling-for-age, (ii) school attendance, and (iii) school-starting-age. The first column present the results from the estimations according to equation (2). In the estimations summarized in the second (third) column, the dummy for being the child of a migrant is again interacted with the rural or urban (town or city) nature of the destination. Somewhat surprisingly, the differences between children of migrants and the children of their parents' siblings who stayed behind in terms of schooling-for-age and school attendance are not significantly different from zero. Children of rural-rural migrants, however, on average start school approximately 2.5 months later.

In Table 9, the estimations according to the specification where children who were born before or in the year of the move are distinguished from those born at the destination are summarized. It

appears that this distinction is important. While there is no significant difference in schooling-for-age between children of migrants who were born before the move and their non-migrant cousins, those born after their parents had moved to a city obtain significantly more schooling for their age. This estimated difference of nearly 0.35 standard deviations, for example, corresponds to almost half a year of schooling for a girl aged 11. In addition, the results show support for the existence of a disruptive effect of parental migration. That is, compared to their non-migrant cousins, on average, children moving alongside their parents to a different rural village start school approximately five months later.

There are some important caveats to this part of the analysis. First, this sample includes only 322 children of migrants of school-going age of whom at least one non-migrant cousin that is also of school going age was surveyed. Secondly, the large majority of children in this sample have not yet finished their schooling. As such, it may be too early to find any significant differences in education outcomes. Similarly, the possibility that children of migrants who experienced a disruption to their education may still catch up cannot be excluded. Finally, in particular in the context of sub-Saharan Africa, the quality of education and learning achievements have come under scrutiny (e.g. Chimombo, 2005; UNESCO, 2014). As such, while moving may not result in a statistically distinguishable difference in the quantity of schooling children obtain, it may still influence what and how much they are learning. Unfortunately, these data do not allow us to derive any conclusions on the impact of parental migration on the quality of education or learning outcomes.

5.3. Selection across families

Comparing the naïve and within-family estimates allows us to infer the direction and magnitude of selection across households. Since the analysis in Section 5.1 confirmed that the characteristics of the destination shape the intergenerational effects of moving, this discussion will focus on the comparison of the estimated return to migration that allows for heterogeneity across the rural or urban (or town or city) nature of the destination. This is particularly relevant, as in addition to the decision to move, migrants' choice of destination is clearly endogenous and, for instance, likely to be influenced by networks and different expected returns to various household and individual characteristics across destinations (e.g. Fafchamps and Shilpi, 2008; McKenzie and Rapoport, 2007). Moreover, the pattern of selection has been argued to depend on how costly migration is (e.g. McKenzie and Rapoport, 2010). Arguably, the latter may well vary across destinations. The direction of these selection effects is a priori unclear.

The estimated returns to migration to rural and urban areas based on the naïve comparison of children of migrants to children of stayers conducted on the subsample of children who contribute to the within-family estimation are therefore summarized in Table 10 (column 1) and contrasted with the estimated coefficients resulting from the regressions with the same set of covariates augmented with parent sibling fixed effects (column 2) that are free from selection across families.²² This reveals

²²To gain some insight into what the findings would have been had we only had access to cross-sectional data, only

an interesting pattern of household selection into migration according to the destination. Whereas the naïve and within-family estimates of the difference in health and education outcomes of the children of rural-rural migrants and their non-migrant counterparts are statistically indistinguishable, the inclusion of parent sibling fixed effects reduces the magnitude of the estimates of the effect of being the child of rural-urban migrant parents dramatically. For instance, the estimated return to parental rural-urban migration decreases by 59 and 42 per cent for height- and weight-for-age respectively with the inclusion of sibling fixed effects. Similarly, the differences between children of rural-urban migrants and their non-migrant counterparts in terms of education appear to be fully explained by selection across families. In this case, even the direction of the effect changes after accounting for selection.

Interestingly, when repeating this comparison distinguishing children of migrants who moved to large cities from those whose parents relocated to smaller secondary towns (see Table 11), selection across families appears to play a particularly important role in explaining the difference in height and weight between children of stayers and the offspring of migrants who migrated to secondary towns. The difference in the estimated coefficients is significant for all four indicators of child growth. Families of migrants who moved to cities are then again clearly positively selected in terms of characteristics influencing child education. After the inclusion of parent sibling fixed effects, the differences between the children of migrants who moved to cities and the children of non-migrants are statistically insignificant.

Overall, these comparisons confirm the importance of accounting for selection across families and reveal that children originating from the types of families in rural Kagera that send migrants to urban areas and secondary towns in particular would have been taller and heavier even if their parents had stayed in their baseline villages. In addition, migrants to cities are drawn from families that have certain attributes contributing to improved education outcomes. While the overestimations of the intergenerational return to migration are attenuated by controlling for observable family characteristics such as consumption per capita, whether the household is headed by a farmer, highest level of education in the household and sex of the household head, they remain economically and statistically significant (see Appendix C, Tables 5-6). Migrant and non-migrant families thus differ both in terms of observable and unobservable characteristics. This could, for example, be related to rural-urban migrant families' positive selection in terms of health knowledge or aspirations for their children.

6. Threats to validity

It is important to acknowledge that while the proposed within-family comparison provides a more accurate estimate of the intergenerational return to migration than specifications addressing only

contemporaneous covariates are included (child's sex, birth order, year of observation, and age cohort fixed effects and parent's sex and age group fixed effects). The finding are, however, unaltered when including the full set of parents' observable baseline characteristics.

observable differences between migrants and non-migrants previously employed in the literature, some potential sources of bias remain unaddressed. That is, this approach does not resolve the problem of unobserved heterogeneity across migrant and non-migrant siblings and does not account for time varying factors that may influence migration as well as child well-being. The validity of the research design depends on the presence of time-varying factors, such as wealth shocks, that may trigger a move and directly influence child outcomes and whether the selection into migration on unobservable individual characteristics among siblings is correlated with outcomes of their children. This section therefore reports the results of tests and alternative specifications that offer insights into omitted variable bias from time varying factors and the role of unobservable selection.

6.1. Selection on unobservable individual characteristics

6.1.1. Using selection on observables to assess bias from unobservables

First, the potential influence of selection bias arising from between-siblings unobserved heterogeneity is assessed following an approach proposed by Altonji et al. (2005) and refined by Oster (2017). This method is based on the idea of a proportional selection relationship on observed and unobserved variables. In this particular case, this assumption implies that if adding several relevant observed individual characteristics of the parents at baseline does not affect the coefficients of interest much, it is unlikely that there are many unobservable characteristics driving selection into migration between siblings that would completely cancel out the results. The selection on observables is evaluated by looking at coefficient movements when adding additional control variables and combining this with information about R-squared movements to assess their relevance. More specifically, Oster (2017) develops a measure (δ) that indicates how large selection on unobservable variables has to be, relative to selection on observables, to fully explain the estimated effect. Using the estimated coefficients and R-squared values from “uncontrolled” and “controlled” regression δ can be derived as follows:

$$\delta = \frac{\beta^C(R^C - R^U)}{(\beta^U - \beta^C)(R_{Max} - R^C)} \quad (3)$$

Whereas the controlled regression is estimated following equation (2), the uncontrolled regression includes only the child-specific characteristics and parent sibling fixed effects as controls. β^C stands for the estimated coefficient on whether the child is born to a migrant (to a certain destination) in the controlled regression. The estimated coefficient is denoted β^U in the uncontrolled regression. R^U and R^C are the respective R-squared values associated with these regressions. The procedure also requires making an assumption about R_{Max} , which is defined as the R-squared from a hypothetical regression that controls for all observed and unobserved covariates. In the presence of measurement error, imposing the theoretical maximum value of one ($R_{Max} = 1$) would appear to be an overly strong assumption as the outcome cannot be fully explained even if the full control set is included. Oster (2017) therefore proposes to set $R_{Max} = 1.3R^C$. As this analysis is useful only when the estimated coefficient in the controlled regression is statistically significant, Table 12 summarizes the

results pertaining to the regressions of height-for-age, weight-for-age, underweight, schooling-for-age, and age at school entry. More specifically, in order to fit to a homogenous-treatment-effect setup, the analysis is focused on the effect of being born to a parent who moved from rural Kagera to a city for the regressions of child growth indicators, being born after a parent moved to a city for the regressions of schooling-for-age, and being born to a rural-rural migrant for the regressions of school starting age.

The results demonstrate that when assuming that unobservables influence selection in a similar way as the observables, selection due to unobserved heterogeneity across migrant and non-migrant siblings would have to be two to 52 times larger compared to their observed differences in sex, age, relation to the household head, schooling-for-age and height in order to invalidate my interpretation of the relation between parental migration to cities and child growth and education. This is well-above the cut-off of one proposed by Oster (2017), suggesting that it is unlikely that the previously established effect of being born to a parent who moved from rural Kagera to a city is entirely driven by selection on unobservable individual differences between migrant and non-migrant siblings. The positive association of being born to a rural-rural migrant and school starting age, however, appears more likely to be biased by selection on individual unobservable characteristics.

Overall, it is important to acknowledge that the value of these findings depends entirely on the unverifiable assumption that the unobservables influence selection in a similar way to the observables. The next section therefore aims to more conclusively address the question of whether the findings in this paper are likely to be driven by unobserved heterogeneity between migrant and non-migrant siblings.

6.1.2. Between-sibling comparison of height

A different approach to assessing the likelihood that unobserved heterogeneity between siblings is driving the results is to restrict the comparison to children born to the same migrant parent. More specifically, the problem of confounds due to factors that are fixed to parents can be fully remediated by including parent fixed effects. The impact of parental migration is identified by exploiting the variation in the age of the children at the time of the move. In sum, the following equation is estimated:

$$Y_{i,t+r} = \alpha Exposure_{i,t+r} + \beta X_{i,t+r} + Parent_i + v_{i,t+r} \quad r = 1, 2 \quad (4)$$

$Parent_i$ denotes the parent fixed effects that will absorb all observed and unobserved fixed characteristics of the parents. The independent variable of interest, $Exposure_{i,t+r}$, indicates a child's exposure to the destination environment and is defined as the proportion of the child's life spent at the destination or the number of years the child lived at the destination relative to his or her age. A girl aged six who moved alongside her parents to a city at age three will, for instance, have been exposed to the city environment for half of her life. Her younger brother aged four on the other hand, will have been exposed for three quarters of his life. If living in a city has a causal effect, both

children will be affected. To the extent that this effect is cumulative, however, the younger brother should have benefitted more through his additional exposure.

Height can be considered as an indicator of cumulative growth faltering reflecting the interaction of nutrition and infection at early ages. If migration to cities has a causal effect on height, this should therefore be reflected in a difference between siblings depending on the duration of their exposure to cities. Table 13 summarizes the within-siblings estimates of the relation between exposure to a rural, urban, or town or city environment and height-for-age and the probability of being stunted. The results reveal a significantly positive effect of the time a child has been exposed to an urban environment on height-for-age. On average, a 10 per cent increase in the portion of a child's life spent in an urban area is associated with a difference of 0.04 standard deviations in height-for-age between siblings. Interestingly, this appears to derive from children who were exposed longer to a town environment. The sample of 72 children of parents who moved to cities with at least one sibling who was exposed to the destination for a different share of his or her life is, however, likely too small to draw meaningful conclusions.

Other outcomes studied in this paper are unlikely be influenced by the length of exposure to a certain environment. A child's weight, for instance, is very responsive to short-term changes. The effect of migration will therefore manifest itself in its entirety in the short term and thus for all siblings regardless of their exposure. Another potential caveat to this approach is the relatively limited occurrence of siblings with different exposure to the destination environment as the majority of children (and their siblings) were born at the destination and therefore have been exposed to the destination for their entire lives.

It is reassuring to see that this alternative strategy leads to the same conclusion for the impact of migration to urban areas on child growth. That is, finding a positive and significant association between exposure to an urban destination and height-for-age even after accounting for all parent characteristics that are fixed, supports the view that the estimates are not confounded by selection on parental characteristics that determine children's outcomes.

6.2. Time varying factors influencing migration and child well-being

A remaining issue of potential confounds due to unobservable factors is that the event that endogenously induced parents to move could also have had a direct effect on their children's outcomes independent of growing up in a different environment.

In part, this concern can be addressed by restricting the analysis to children of migrants born several years after their parents had moved. These children are arguably considerably less likely to have been affected directly by a temporary shock or event that endogenously triggered their parents' migration. The analysis is therefore conducted on the sample of children born more than one year after migration. The results of the regressions of health and education outcomes on parental migration status according to the destination are summarized in Tables 14 and 15 (column 1) respectively.

The findings with regards to the relation between parental migration and child health remain

unaltered. Children born at least two years after their parents moved to cities are significantly taller, heavier and less likely to be underweight than their non-migrant cousins growing up in rural Kagera. In line with the observation that the previously established effect derives largely from children born at the destination, the estimated coefficients in the regressions of height-for-age are substantially larger than when estimated on the full sample. The difference in height between children aged 5.5 born at least two years after their parent(s) moved to a city and their cousins growing up in rural Kagera amounts to approximately 3.6 cm. The estimated positive coefficient of parental migration to cities is not statistically significant in the regression of schooling-for-age. It seems likely that this is related to the reduction in the already much smaller size of the sample. Similarly, I find no evidence of a significant association between parental migration to a different rural area and the age at which children start school. This is in line with expectations as it was previously established that this effect derives from children moving alongside their parents.

Alternatively, parents' self-reported information on shocks can help rule out that the previously established relations are the result of bias arising from time varying factors. In the 2004 and 2010 survey, respondents were asked to indicate for each year since the previous survey whether they experienced any serious hardship or fortune in terms of wealth and living conditions. To mitigate (to some extent) the possibility that parents' migration was driven by a positive or negative shock, children whose parents indicated that the year in which they moved was "very good" or "very bad" are excluded (see Table 14- 15, column 2). Whereas the estimated coefficient for being the child of parents who moved to cities has lost its significance in the regression of height-for-age, there is an economically and statistically significant negative association with the likelihood of being stunted. That is, compared to their cousins growing up in rural Kagera, children of migrants to cities are 17 percentage points less likely to be stunted. The estimated coefficients in the regressions of weight-for-age and the likelihood of being underweight remain highly significant and have increased in terms of magnitude. Again, arguably due to a lack of precision as a result of the reduction in the sample size, I find no evidence of any statistically significant association between parental migration and schooling-for-age or school starting age.

7. Mechanisms

The results presented in this paper indicate that the systematic variation in child health and education outcomes across space in Tanzania is not merely the result of sorting. Rather, the existence of significant and economically important differences in the growth and schooling indicators of children whose parents moved to cities and the children of their parents' siblings who stayed in rural Kagera suggests that growing up in a city environment has a causal effect on child health and education. A question that then arises is what aspects of moving to and living in a city environment contribute to child health and education? Does increased access to safe drinking water affect child growth by reducing the risk of infectious diseases? Do increased income opportunities allow migrant parents to acquire more nutritious foods and/or other health inputs for their children? Or do people simply

adjust their child-care practices in response to being exposed to different behaviors in a city environment? In addition to describing the relationship between parental migration and child health and education outcomes in Tanzania, I therefore seek to understand how the previously established effects came to be. This will improve our understanding of the underlying determinants of child health and education outcomes necessary to identify specific policy solutions. While several studies have put forward potential mechanisms (e.g. Brockerhoff, 1994; Omariba and Boyle, 2010), to the best of my knowledge, there is no empirical evidence exploring the extent to which the proposed factors represent pathways through which migration affects the human capital of the next generation.

The analysis reported in this section aims to shed light on the role of i) income, ii) living conditions, (iii) access to clean drinking water,²³ (iv) fertility, and (v) education in shaping the established relation between parental migration to cities and child health and education. The corresponding mediator variables are (i) the logarithm of total real household consumption per capita, (ii) a dummy for whether the child is living in a dwelling with a non-earth floor,²⁴ (iii) a dummy for whether the child is living in a household where the main source of drinking water can be considered safe or improved, (iv) the number of children of the child's mother, and (vi) the highest level of education in the household (see Appendix A4 for a detailed explanation of the variables). As a first step, I explore whether parental migration significantly accounts for variations in these presumed mediators (see Table 16).

In line with Beegle et al. (2011), the results summarized in Table 16 clearly indicate that physical mobility goes hand in hand with economic mobility. That is, children of migrants who relocated to a city live in households with a consumption per capita that is 58 per cent higher than that of the households of their parents' siblings who stayed behind in their baseline villages in Kagera. In addition, they are 59.8 and 62.7 percentage points more likely to live in dwellings with a non-earth floor and have access to an improved water source. The results further confirm that moving to a city is associated with lower fertility. Children of migrants thus have fewer siblings with whom they share resources. Finally, these children live in significantly more educated households. On average, the highest level of education in their households is more than one grade higher. As these factors have been shown to be important contributors to child growth differentials (e.g. Van de Poel et al., 2007; Smith et al., 2005), it seems plausible that they play an important role in producing the overall positive effect of parental migration to cities on child growth.

In order to approximate how much of the improvement in child growth from parental migration to cities can be attributed to these channels, I rely on a method proposed by Acharya et al. (2016) to estimate what the effect of migration would have been had there been no differences in (i) income, ii) living conditions, (iii) access to clean drinking water, (iv) fertility, and (v) education in the household

²³It seems unlikely that access to clean drinking water directly affects schooling. As such, it will not be considered as a potential mediator variable for the effect of parental migration to cities on schooling-for-age.

²⁴It is worth noting that while floor material acts a proxy for household wealth, living in a dwelling with an earth floor also directly affects child health as this can facilitate the spread of infections and has been associated with increased risk of diarrhea (e.g. Woldemicael, 2001).

(see Appendix A3 for a more detailed explanation of this approach). These quantities of interest are referred to as the Average Controlled Direct Effects (ACDEs). Figure 4 provides, for each of the child growth outcomes, a graphical representation of, respectively, the total effect of parental migration to cities (Total effect) and the ACDE when accounting for each of the mediators (see also Appendix C, Table 7). The difference between the total effect, which represents the estimated effect of migration when not accounting for any of the potential mediator variables, and the ACDE is a measure of the role of the mediator in the mechanism.²⁵

These estimates reveal that household income and living conditions play an important role as a channel through which migration to cities affects child growth and schooling. As can be derived from Figure 4, the estimated effect of moving to cities decreases substantially both in terms of magnitude and statistical significance when accounting for the difference in income and whether the child is living in a household with a non-earth floor in all five regressions. Yet, the estimated returns to parental migration on child height and weight remain sizeable and statistically significant, suggesting that there are other mechanisms at play. The results further suggest that migration to cities influences child weight through improving access to clean drinking water. The portion of the variation in the weight of children of migrants who moved to a city and the children of their parents' siblings who stayed behind accountable to differences in access to clean drinking water is, however, small. Finally, it is worth noting that reduced fertility and higher education in migrant households do not appear to play a significant role in producing the overall effect of migration to cities on child growth or schooling.

8. Conclusions

There is growing recognition of the importance of migration as a livelihood strategy in developing countries. Internal migration is also believed to play a prominent role in the structural transformation and economic development of countries. The long-run consequences of the ongoing movements of the population are likely to be largely determined by the impact of migration on the human capital formation of children. In addition, evidence of large spatial heterogeneity in health and education outcomes in developing countries, raises the question whether children's outcomes are shaped by the environment in which they grow up or whether the variation across space is merely the result of sorting and the fact that these populations differ in many more respects than only the environment they reside in. If the former holds, moving can be expected to causally affect child outcomes. Children of migrants therefore constitute a particularly interesting group to study in order to answer this question. Yet, despite the magnitude and importance of the issue, we know surprisingly little about the consequences of migration for children of migrants in developing countries.

Drawing upon data from the Kagera Health and Development Survey, which traced household

²⁵Note that the role of the mediator (measured as the difference between the total effect and the ACDE) consists of both the indirect effect (measuring how strong a particular pathway is) and the interaction effect (telling us how much the mediator influences the direct effect of the treatment).

members who moved between 1991/94 and 2010, this paper presents an empirical study of the relation between parents' migration and children's health and education outcomes in Tanzania, a country characterized by stark differences in child development indicators and intensive migration. The impact of migration is identified using the variation in the health and education outcomes of children of migrants and the children of the migrants' siblings who stayed behind. This approach eliminates the component of migrant selection that takes place across families, thus allowing for more accurate estimates of the returns to migration. Furthermore, I rule out confounds due to a wide range of individual characteristics observed before migration that could influence the propensity to move as well as children's health and education.

The results reported in this paper show that parental migration has important implications for the development of future generations. The effects, however, vary according to the destination and timing of the move. More specifically, migration to urban areas and especially to large cities is associated with improved weight-for-age. Moreover, I find evidence of a large gain in height-for-age for children of migrants who were exposed to the city environment during their early childhood. Assuming that this difference persists through adulthood, the children of migrants who moved to cities are predicted to be approximately 3.5 cm taller. The offspring of relatively poorer migrants who moved to cities are also less likely to be underweight or stunted. Notably, the impact of moving to smaller secondary towns, which are a far more common destination, is subject to more variation. The coefficients are less precisely estimated and much smaller in terms of magnitude. Moreover, children whose parents moved to a different rural area – still the dominant migration stream in Tanzania and the rest of sub-Saharan Africa – appear to be similar to their non-migrant cousins in terms of their weight and height.

While the sample of children of school-going age is considerably smaller and the coefficients in the regressions of the education outcomes are less precisely estimated, I find a positive association between parental migration to cities and schooling-for-age. This, however, holds only for children born at the destination. In contrast, children moving alongside their parents to a different rural village experience a delay in their start of schooling. Given the substantial intra-rural migration flows in the country, a potential educational disadvantage for the children of rural-rural migrants could have detrimental consequences for future human capital development in Tanzania. At the very least, these results thus suggest that there is a compelling case for further work on these issues.

Beyond providing more robust evidence showing that the relation between parental migration and improved child growth and education indicators is not merely an artefact of selection, this paper also reveals that failing to address selection into migration across families gravely overstates the returns to migration. Specifically, a comparison of the “within-family” estimates and the “nave” estimates reveals that rural-urban migrants are drawn from families that have certain attributes contributing to improved child growth and education outcomes. While the differences between the nave and within-family estimates are attenuated by controlling for observable family characteristics they remain economically and statistically significant. Migrant and non-migrant families thus differ both in terms of observable and unobservable characteristics. This highlights the importance of

addressing household selection into migration for estimating the intergenerational returns to moving.

Furthermore, in addition to describing the relationship between parental migration and child health and education outcomes in Tanzania, I seek to understand how these effects come to be. This exploration of the underlying mechanisms indicates that an important part of the growth and education benefits is related to higher incomes and improved housing conditions for those who moved to cities. Yet, even after accounting for these differences, the estimated returns to parental migration on child height and weight remain sizeable and statistically significant. This shows that there are other mechanisms at play and again reveals a need for continued research efforts.

These findings have important implications. A growing body of evidence demonstrates the importance of child anthropometrics as predictors of long-term health, cognitive development, and labor market outcomes (e.g. Currie and Vogl, 2013). One extra centimeter of height has, for example, been associated with wage increases ranging from two to ten per cent in a developing country context (e.g. Haddad and Bouis, 1991; Vogl, 2014; Strauss and Thomas, 1998; Schultz, 2002). In particular in the context of a country where 3.3 million children under five risk not reaching their full physical and cognitive potential due to inadequate nutrition, the establishment of a large and robust positive association between migration and child growth supports the view that migration to urban areas and especially cities can play an important positive role in further development and poverty reduction through human capital accumulation. This is further reinforced by the demonstrated positive association between growing up in a city and schooling-for-age. As such, this paper demonstrates that returns to migration thus extend beyond migrants themselves and that migration can contribute to intergenerational mobility. In addition to improving their own welfare, migrant parents in Tanzania are able to enhance their children’s current well-being as well as their future earning potential by moving to cities. While it is important to acknowledge that with continued urban population growth comes the risk of overcrowding which can eventually outweigh the benefits of agglomeration in cities, these findings cast doubts on the desirability of policies restricting movements from rural to urban areas that are currently widely prevalent in developing countries (UNDP, 2013).

More broadly, this analysis contributes to debate on the origin of spatial inequalities in developing countries (e.g. Ravallion and Wodon, 1999) and sheds new light on the research on rural-urban labor productivity gaps (e.g. Hicks et al., 2017; Gollin et al., 2002). While the findings regarding selection into migration across households support the hypothesis that the population sorts itself geographically (e.g. Lagakos and Waugh, 2013; Young, 2013), the evidence of large returns to moving even when accounting for selection into migration across households and on observable pre-migration characteristics strongly suggest that growing up in a city environment causally affects child development. These findings therefore confirm the importance of childhood environments for development (e.g. Chetty et al., 2016; Chetty and Hendren, 2018; Chyn, 2018). Most importantly, this implies that further identification and understanding of the characteristics of the city environments that are conducive to human capital accumulation can provide important insights for designing more effective rural development strategies as well as health and education policies.

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Tables

Table 1: Parents' migration patterns

		2004			2010		
		Rural	Urban		Rural	Urban	
		Town	City		Town	City	
1991/94	Rural	238 (528)	100 (200)	41 (64)	473 (1354)	260 (594)	140 (276)
	Urban	20 (52)	9 (13)	2 (3)	33 (86)	26 (57)	17 (27)

Notes: Corresponding number of children in parentheses

Table 2: Summary statistics parents (in 1991/94)

	Stayed (N=1251)	Moved (N=873)		Rural-rural (N=473)		Rural-urban (N=400)		Rural-town (N=260)		Rural-city (N=140)	
Hh. size	7.695	8.234	***	7.820		8.723	***	8.904	***	8.368	**
Male hh. head (%)	80.256	76.861	*	77.801		75.75	*	76.923		73.571	*
Farmer hh. head (%)	81.615	75.945	***	79.281		72.000	***	75.769	**	65.000	***
Highest edu. (years)	6.643	6.913	***	6.564		7.325	***	6.961	**	8.000	***
Hh. cons. p.c. (2010 TZS)	318723	324843		301772	*	351808	***	349472	***	356142	**
Non-earth floor (%)	10.312	16.037	***	10.994		22.000	***	21.538	***	22.857	***
Electricity (%)	0.480	1.031		1.057		1.000		1.538	*	0.000	
Improved water source (%)	18.240	20.092		16.525		24.311	***	25.869	***	21.429	
Age	16.801	13.652	***	13.220	***	14.163	***	13.992	***	14.479	***
Female (%)	55.875	69.989	***	77.801	***	60.750	*	58.462	++	65.000	**
Married (%)	20.623	7.331	***	8.245	***	6.250	***	6.923	***	5.000	***
Child of hh. head (%)	54.277	54.441		54.334		54.500		52.692		57.857	
Schooling-for-age	-0.002	0.003	+	-0.069		0.088	+++	-0.022	+++	0.292	***

Notes: * p < 0.10, ** p < 0.05, *** p < 0.01 denote the significance of the T-stat. based on the comparison of each group of migrants to those who stayed. + p < 0.10, ++ p < 0.05, +++ p < 0.01 denote the significance of the T-stat. based on the comparison of the residuals after partialling out the sibling fixed effects.

Table 3: Summary statistics children (in 2004 and 2010)

	Stayed (N=6452)	Moved (N=3018)		Rural-rural (N=1884)		Rural-urban (N=1134)		Rural-town (N=794)		Rural-city (N=340)	
Hh. size	6.359	5.933	***	6.220	**	5.456	***	5.615	***	5.085	***
Male hh. head (%)	87.729	91.708	***	91.817	***	91.527	***	91.562	***	91.445	**
Highest edu. (years)	7.003	7.593	***	7.014		8.553	***	8.086	***	9.644	***
Hh. cons. p.c. (2010 TZS)	371321	484842	***	386634	**	647624	***	552137	***	870425	***
Non-earth floor (%)	17.515	38.909	***	19.552	**	71.062	***	60.683	***	95.280	***
Electricity (%)	2.879	16.827	***	4.795	***	36.814	***	26.422	***	61.062	***
Improved water source (%)	22.703	37.475	***	21.207		64.425	***	50.822	***	96.165	***
Age	6.751	5.693	***	5.649	**	5.767	***	5.649	***	5.884	***
Female (%)	49.440	49.702		48.937	**	50.970		50.756		51.471	

Notes: * p < 0.10, ** p < 0.05, *** p < 0.01 denote the significance of the T-stat. based on the comparison of each group of children of migrants to children of those who stayed.

Table 4: Parental migration and health and education (naive estimates)

	Height- for-age (≤ 19)	Stunting (≤ 19)	Weight- for-age (≤ 10)	Underweight (≤ 10)	Full vaccin. (1-5)	Good health (≤ 19)	Schooling- for-age (7-19)	School attend. (7-19)	School start. (3-19)
M^{Parent}	0.165*** (0.053)	-0.024 (0.017)	0.173*** (0.051)	-0.034** (0.017)	-0.008 (0.016)	0.011 (0.010)	0.014 (0.056)	-0.006 (0.017)	0.119* (0.059)
N	8574	8574	6667	6667	3622	8579	4034	4059	4128
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors at the level of the parent's village of origin in parentheses. Controls include the child's sex, birth order, year of observation, age cohort fixed effects, his/her parent's sex and age group fixed effects.

Table 5: Parental migration and health (within-family estimates)

	Height- for-age (≤ 19)	Stunting (≤ 19)	Weight- for-age (≤ 10)	Underweight (≤ 10)	Full vaccin. (1-5)	Good health (≤ 19)
M^{Parent}	0.064 (0.077)	-0.005 (0.027)	0.116* (0.068)	-0.026 (0.026)	-0.010 (0.031)	0.011 (0.018)
N	5658	5658	4732	4732	2640	5621
Controls	✓	✓	✓	✓	✓	✓
PSFE	✓	✓	✓	✓	✓	✓

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors at the level of the parent's village of origin in parentheses. Controls include the child's sex, birth order, year of observation, age cohort fixed effects, and his/her parent's sex, birth order, age group fixed effects and his/her parent's relation to the household head, schooling-for-age, marital status, height, and survey wave fixed effects in 1991/94. PSFE stands for parent sibling fixed effects.

Table 6: Parental migration by destination and health (within-family estimates)

	Height- for-age (≤ 19)		Stunting (≤ 19)		Weight- for-age (≤ 10)		Underweight (≤ 10)		Full vaccin. (1-5)		Good health (≤ 19)	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
M^{Parent}_{Rural}	-0.020 (0.089)	-0.024 (0.089)	0.010 (0.025)	0.011 (0.025)	0.043 (0.075)	0.039 (0.074)	-0.012 (0.028)	-0.011 (0.028)	0.003 (0.031)	0.003 (0.031)	0.016 (0.020)	0.016 (0.020)
M^{Parent}_{Urban}	0.214* (0.127)		-0.030 (0.049)		0.245** (0.100)		-0.052 (0.034)		-0.034 (0.049)		0.003 (0.024)	
M^{Parent}_{Town}		0.113 (0.131)		-0.002 (0.048)		0.149 (0.119)		-0.041 (0.039)		-0.042 (0.052)		0.012 (0.028)
M^{Parent}_{City}		0.512** (0.233)		-0.113 (0.088)		0.540*** (0.173)		-0.087** (0.043)		-0.013 (0.074)		-0.015 (0.049)
N	5658	5658	5658	5658	4732	4732	4732	4732	2640	2640	5621	5621
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
PSFE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors at the level of the parent's village of origin in parentheses. Controls include the child's sex, birth order, year of observation, age cohort fixed effects, and his/her parent's sex, birth order, age group fixed effects and his/her parent's relation to the household head, schooling-for-age, marital status, height, and survey wave fixed effects in 1991/94. PSFE stands for parent sibling fixed effects.

Table 7: Parental migration by destination and timing and health (within-family estimates)

	Height-for-age (≤ 19)		Stunting (≤ 19)		Weight-for-age (≤ 10)		Underweight (≤ 10)		Full vaccin. (1-5)		Good health (≤ 19)	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
$M_{Rural,before}^{Parent}$	-0.038 (0.109)	-0.041 (0.109)	0.046 (0.033)	0.047 (0.033)	0.009 (0.105)	0.007 (0.104)	0.019 (0.035)	0.019 (0.035)	-0.016 (0.043)	-0.015 (0.042)	0.048* (0.025)	0.048* (0.025)
$M_{Rural,after}^{Parent}$	0.013 (0.096)	0.010 (0.096)	-0.014 (0.028)	-0.013 (0.028)	0.073 (0.081)	0.069 (0.081)	-0.034 (0.033)	-0.033 (0.033)	0.003 (0.032)	0.003 (0.032)	0.001 (0.023)	0.001 (0.023)
$M_{Urban,before}^{Parent}$	0.173 (0.158)		-0.038 (0.059)		0.381*** (0.131)		-0.108** (0.051)		-0.055 (0.052)		-0.006 (0.033)	
$M_{Urban,after}^{Parent}$	0.254* (0.144)		-0.042 (0.055)		0.224** (0.109)		-0.044 (0.033)		-0.043 (0.055)		0.004 (0.027)	
$M_{Town,before}^{Parent}$		0.162 (0.166)		-0.033 (0.063)		0.292* (0.145)		-0.096 (0.060)		-0.037 (0.047)		-0.007 (0.036)
$M_{Town,after}^{Parent}$		0.120 (0.144)		0.000 (0.052)		0.117 (0.142)		-0.031 (0.039)		-0.059 (0.061)		0.015 (0.034)
$M_{City,before}^{Parent}$		0.249 (0.334)		-0.066 (0.089)		0.721*** (0.248)		-0.155*** (0.053)		-0.081 (0.096)		-0.001 (0.053)
$M_{City,after}^{Parent}$		0.583** (0.264)		-0.144 (0.096)		0.521*** (0.178)		-0.079* (0.047)		-0.007 (0.081)		-0.017 (0.053)
N	5606	5606	5606	5606	4694	4694	4694	4694	2623	2623	5572	5572
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
PSFE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors at the level of the parent's village of origin in parentheses. Controls include the child's sex, birth order, year of observation, age cohort fixed effects, and his/her parent's sex, birth order, age group fixed effects and his/her parent's relation to the household head, schooling-for-age, marital status, height, and survey wave fixed effects in 1991/94. PSFE stands for parent sibling fixed effects.

Table 8: Parental migration by destination and education (within-family estimates)

	Schooling-for-age (7-19)			School attend. (7-19)			School start. (3-19)		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
M^{Parent}	-0.042 (0.095)			0.003 (0.021)			0.167 (0.114)		
M_{Rural}^{Parent}		-0.044 (0.097)	-0.043 (0.098)		0.013 (0.026)	0.013 (0.026)		0.205* (0.119)	0.205* (0.118)
M_{Urban}^{Parent}		-0.038 (0.174)			-0.013 (0.032)			0.108 (0.181)	
M_{Town}^{Parent}			-0.150 (0.223)			-0.001 (0.042)			0.137 (0.181)
M_{City}^{Parent}			0.227 (0.191)			-0.041 (0.030)			0.051 (0.336)
N	2284	2284	2284	2307	2307	2307	2415	2415	2415
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
PSFE	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors at the level of the parent's village of origin in parentheses. Controls include the child's sex, birth order, year of observation, age cohort fixed effects, and his/her parent's sex, birth order, age group fixed effects and his/her parent's relation to the household head, schooling-for-age, marital status, height, and survey wave fixed effects in 1991/94. PSFE stands for parent sibling fixed effects.

Table 9: Parental migration by destination and timing and education (within-family estimates)

	Schooling-for-age		School attend.		School start.	
	(7-19)		(7-19)		(3-19)	
	(1)	(2)	(1)	(2)	(1)	(2)
$M_{Rural,before}^{Parent}$	-0.137 (0.138)	-0.139 (0.139)	0.004 (0.039)	0.004 (0.039)	0.419** (0.180)	0.421** (0.178)
$M_{Rural,after}^{Parent}$	0.025 (0.105)	0.028 (0.106)	0.031 (0.036)	0.031 (0.036)	-0.031 (0.110)	-0.031 (0.110)
$M_{Urban,before}^{Parent}$	-0.171 (0.192)		-0.020 (0.033)		0.153 (0.223)	
$M_{Urban,after}^{Parent}$	0.211 (0.197)		-0.026 (0.044)		0.079 (0.193)	
$M_{Town,before}^{Parent}$		-0.211 (0.247)		-0.008 (0.042)		0.173 (0.271)
$M_{Town,after}^{Parent}$		0.124 (0.321)		-0.026 (0.066)		0.129 (0.167)
$M_{City,before}^{Parent}$		-0.044 (0.235)		-0.063 (0.038)		0.110 (0.358)
$M_{City,after}^{Parent}$		0.348* (0.205)		-0.031 (0.038)		0.005 (0.368)
N	2255	2255	2277	2277	2385	2385
Controls	✓	✓	✓	✓	✓	✓
PSFE	✓	✓	✓	✓	✓	✓

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors at the level of the parent's village of origin in parentheses. Controls include the child's sex, birth order, year of observation, age cohort fixed effects, and his/her parent's sex, birth order, age group fixed effects and his/her parent's relation to the household head, schooling-for-age, marital status, height, and survey wave fixed effects in 1991/94. PSFE stands for parent sibling fixed effects.

Table 10: Comparing naïve estimates to within-family estimates (rural or urban)

	Height-for-age (≤ 19)		Stunting (≤ 19)		Weight-for-age (≤ 10)		Underw. (≤ 10)		Full vaccin. (1-5)		Good health (≤ 19)		Schooling-for-age (7-19)		School attend. (7-19)		School start. (3-19)	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
M_{Rural}^{Parent}	0.025 (0.060)	-0.043 (0.093)	-0.003 (0.019)	0.014 (0.025)	-0.005 (0.055)	0.035 (0.074)	0.007 (0.020)	-0.012 (0.028)	0.002 (0.024)	0.005 (0.032)	0.011 (0.015)	0.016 (0.019)	-0.056 (0.074)	-0.016 (0.021)	0.010 (0.029)	0.181** (0.089)	0.241** (0.112)	
M_{Urban}^{Parent}	0.526*** (0.097)	0.217 (0.131)	-0.098*** (0.032)	-0.031 (0.049)	0.442*** (0.073)	0.258** (0.099)	-0.077*** (0.024)	-0.054 (0.035)	-0.034 (0.029)	-0.036 (0.048)	0.008 (0.015)	0.003 (0.025)	0.307** (0.127)	0.046 (0.035)	-0.017 (0.035)	-0.108 (0.114)	0.126 (0.188)	
N	5658	5658	5658	5658	4732	4732	4732	4732	2640	2640	5621	5621	2284	2307	2307	2415	2415	
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	
PSFE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	
$M_{Rural(1)}^{Parent} - M_{Rural(2)}^{Parent}$	0.068 (0.072)	0.017 (0.023)		0.017 (0.023)	-0.040 (0.063)	-0.040 (0.063)		0.019 (0.021)	-0.002 (0.021)	-0.002 (0.021)	-0.004 (0.015)	-0.004 (0.015)	0.038 (0.086)	-0.026 (0.026)	-0.026 (0.026)	-0.060 (0.101)		
$M_{Urban(1)}^{Parent} - M_{Urban(2)}^{Parent}$	0.309*** (0.093)	-0.067** (0.030)		-0.067** (0.030)	0.184** (0.076)	0.184** (0.076)		-0.023 (0.021)	0.003 (0.025)	0.003 (0.025)	0.005 (0.017)	0.005 (0.017)	0.376*** (0.119)	0.063** (0.032)	0.063** (0.032)	-0.233 (0.127)		

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors at the level of the parent's village of origin in parentheses. Standard errors for the difference between $M_{Rural(1)}^{Parent}$ and $M_{Rural(2)}^{Parent}$, and $M_{Urban(1)}^{Parent}$ and $M_{Urban(2)}^{Parent}$ were obtained from 1000 bootstrapped replications. Controls include the child's sex, birth order, year of observation, age cohort fixed effects, and his/her parent's sex, birth order, age group fixed effects. PSFE stands for parent sibling fixed effects.

Table 11: Comparing naive estimates to within-family estimates (rural, town, or city)

	Height-for-age (≤ 19)		Stunting (≤ 19)		Weight-for-age (≤ 10)		Underw. (≤ 10)		Full vaccin. (1-5)		Good health (≤ 19)		Schooling- for-age (7-19)		School attend. (7-19)		School start. (3-19)	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
M_{Rural}^{Parent}	0.028 (0.060)	-0.047 (0.094)	-0.003 (0.019)	0.015 (0.025)	-0.004 (0.055)	0.031 (0.073)	0.007 (0.020)	-0.011 (0.028)	0.003 (0.024)	0.016 (0.019)	0.012 (0.015)	0.016 (0.019)	-0.054 (0.074)	-0.092 (0.107)	-0.016 (0.021)	0.010 (0.029)	0.181** (0.089)	0.242** (0.111)
M_{Town}^{Parent}	0.337*** (0.099)	0.107 (0.140)	-0.059* (0.033)	-0.002 (0.049)	0.362*** (0.080)	0.154 (0.118)	-0.066** (0.026)	-0.041 (0.040)	-0.050 (0.032)	0.013 (0.028)	0.001 (0.017)	0.013 (0.028)	0.121 (0.148)	-0.173 (0.229)	0.018 (0.046)	-0.015 (0.047)	0.044 (0.146)	0.169 (0.185)
M_{City}^{Parent}	0.979*** (0.099)	0.539*** (0.220)	-0.193*** (0.038)	-0.117 (0.086)	0.637*** (0.130)	0.572*** (0.161)	-0.104*** (0.028)	-0.095** (0.043)	0.000 (0.039)	-0.020 (0.049)	0.022 (0.021)	-0.020 (0.049)	0.718*** (0.089)	0.178 (0.223)	0.108*** (0.024)	-0.020 (0.029)	-0.370*** (0.128)	0.043 (0.396)
N	5658	5658	5658	5658	4732	4732	4732	4732	2640	2640	5621	5621	2284	2284	2307	2307	2415	2415
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
PSFE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
$M_{Rural(1)}^{Parent} - M_{Rural(2)}^{Parent}$	0.075 (0.072)	-0.018 (0.023)			-0.035 (0.063)	0.018 (0.021)	0.018 (0.021)	0.018 (0.021)	-0.002 (0.021)	-0.004 (0.015)	-0.004 (0.015)	-0.004 (0.015)	0.038 (0.086)	0.038 (0.086)	-0.026 (0.026)	-0.026 (0.026)	-0.061 (0.101)	-0.061 (0.101)
$M_{Town(1)}^{Parent} - M_{Town(2)}^{Parent}$	0.230** (0.103)	-0.056 (0.030)			0.208** (0.085)	-0.025 (0.024)	-0.025 (0.024)	-0.025 (0.024)	-0.006 (0.027)	-0.012 (0.020)	-0.012 (0.020)	-0.012 (0.020)	0.294** (0.148)	0.294** (0.148)	0.033 (0.043)	0.033 (0.043)	-0.125 (0.131)	-0.125 (0.131)
$M_{City(1)}^{Parent} - M_{City(2)}^{Parent}$	0.440** (0.171)	-0.076 (0.061)			0.065 (0.140)	-0.009 (0.038)	-0.009 (0.038)	-0.009 (0.038)	0.019 (0.043)	0.042 (0.030)	0.042 (0.030)	0.042 (0.030)	0.539*** (0.200)	0.539*** (0.200)	0.127** (0.027)	0.127** (0.027)	-0.412 (0.251)	-0.412 (0.251)

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors at the level of the parent's village of origin in parentheses. Standard errors for the difference between $M_{Rural(1)}^{Parent}$ and $M_{Rural(2)}^{Parent}$, $M_{Town(1)}^{Parent}$ and $M_{Town(2)}^{Parent}$, and $M_{City(1)}^{Parent}$ and $M_{City(2)}^{Parent}$ were obtained from 1000 bootstrapped replications. Controls include the child's sex, birth order, year of observation, age cohort fixed effects, and his/her parents sex, birth order, age group fixed effects. PSFE stands for parent sibling fixed effects.

Table 12: Assessing coefficient stability and unobservable selection

	<i>Controlled</i>		<i>Without parent's baseline characteristics</i>	
	M_{City}^C	R_{Max}	M_{City}^U	δ
Height-for-age (≤ 19)	0.458	0.553	0.440	-25.145
Weight-for-age (≤ 10)	0.507	0.564	0.485	52.490
Underweight (≤ 10)	-0.056	0.413	-0.050	-1.928
<hr/>				
	$M_{City,after}^C$	R_{Max}	$M_{City,after}^U$	δ
Schooling-for-age (7-19)	0.515	0.678	0.478	17.12
<hr/>				
	$M_{Rural,before}^C$	R_{Max}	M_{Rural}^U	δ
School start. (3-19)	0.371	0.927	0.417	0.145

Notes: The following controls are included in all regressions: the child's sex, birth order, year of observation, age cohort. R_{max} is the R-squared from a hypothetical regression that controls for all observables and unobservables. As suggested by Oster (2017), we set $R_{max}=1.3R^C$. Oster (2017)'s coefficient of proportionality is denoted δ and expresses the degree of selection on unobservables relative to observables that would be necessary to explain away the result. The uncontrolled regression includes only the child-specific characteristics and parent sibling fixed effects.

Table 13: Parental migration by destination and height (between-sibling estimates)

	Height- for-age (≤ 19)		Stunting (≤ 19)	
	(1)	(2)	(1)	(2)
$Exposure_{Rural}$	-0.001 (0.003)	-0.001 (0.003)	-0.001 (0.001)	-0.001 (0.001)
$Exposure_{Urban}$	0.004* (0.003)		-0.000 (0.001)	
$Exposure_{Town}$		0.006* (0.003)		-0.000 (0.001)
$Exposure_{City}$		-0.004 (0.006)		-0.001 (0.002)
N	2449	2449	2449	2449
Controls	✓	✓	✓	✓
PFE	✓	✓	✓	✓

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors at the level of the parent's village of origin in parentheses. Controls include the child's sex, birth order, year of observation, age cohort fixed effects. PFE stands for parent fixed effects.

Table 14: Parental migration and health (within-family estimates) time varying factors

	Height-for-age (≤ 19)		Stunting (≤ 19)		Weight-for-age (≤ 10)		Underweight (≤ 10)		Full vaccin. (1-5)		Good health (≤ 19)	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
M_{Rural}^{Parent}	-0.053 (0.098)	-0.010 (0.132)	0.012 (0.029)	0.000 (0.036)	0.033 (0.074)	0.169 (0.103)	-0.005 (0.030)	-0.037 (0.034)	-0.002 (0.036)	0.015 (0.039)	0.003 (0.021)	0.024 (0.037)
M_{Town}^{Parent}	0.027 (0.135)	0.302 (0.208)	0.005 (0.051)	-0.007 (0.059)	0.038 (0.138)	0.327** (0.150)	-0.012 (0.038)	-0.086 (0.057)	-0.044 (0.067)	-0.010 (0.048)	0.019 (0.036)	-0.013 (0.045)
M_{City}^{Parent}	0.753*** (0.271)	0.456 (0.311)	-0.146 (0.107)	-0.168* (0.087)	0.549** (0.225)	0.603** (0.251)	-0.047 (0.062)	-0.149** (0.063)	-0.009 (0.094)	0.090 (0.099)	-0.040 (0.059)	-0.000 (0.051)
N	4996	4575	4996	4575	4239	3871	4239	3871	2375	2177	5040	4545
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
PSFE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors at the level of the parent's village of origin in parentheses. Controls include the child's sex, birth order, year of observation, age cohort fixed effects, and his/her parent's sex, birth order, age group fixed effects and his/her parent's relation to the household head, schooling-for-age, marital status, height, and survey wave fixed effects in 1991/94. PSFE stands for parent sibling fixed effects. Model 1 reports the results of the regressions when children born before or less than two years after the move are excluded. In model 2, only children of migrants who did not report the year of migration as either very bad or very good are included.

Table 15: Parental migration and education (within-family estimates) time varying factors

	Schooling-for-age (7-19)		School attend. (7-19)		School start. (3-19)	
	(1)	(2)	(1)	(2)	(1)	(2)
M_{Rural}^{Parent}	0.002 (0.114)	-0.075 (0.236)	0.020 (0.031)	-0.084 (0.081)	0.065 (0.149)	0.221 (0.218)
M_{Town}^{Parent}	-0.103 (0.321)	-0.473 (0.288)	-0.095 (0.059)	0.091** (0.043)	-0.074 (0.217)	0.509 (0.375)
M_{City}^{Parent}	0.330 (0.230)	0.297 (0.326)	-0.039 (0.062)	0.003 (0.040)	-0.105 (0.458)	0.232 (0.537)
Observations	1903	1751	1920	1771	2017	1832
Controls	✓	✓	✓	✓	✓	✓
PSFE	✓	✓	✓	✓	✓	✓

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors at the level of the parent's village of origin in parentheses. Controls include the child's sex, birth order, year of observation, age cohort fixed effects, and his/her parent's sex, birth order, age group fixed effects and his/her parent's relation to the household head, schooling-for-age, marital status, height, and survey wave fixed effects in 1991/94. PSFE stands for parent sibling fixed effects.

Table 16: Parental migration and potential mediators

	Ln(const. p.c.)	Floor	Water	Fertility	Highest edu.
M_{Rural}^{Parent}	0.087** (0.039)	0.015 (0.038)	-0.043 (0.042)	0.056 (0.095)	0.113 (0.177)
M_{Town}^{Parent}	0.265*** (0.066)	0.304*** (0.065)	0.162* (0.083)	-0.103 (0.111)	0.767*** (0.283)
M_{City}^{Parent}	0.575*** (0.096)	0.598*** (0.071)	0.627*** (0.072)	-0.500*** (0.146)	1.164*** (0.296)
N	6114	6114	6114	6114	6114
Controls	✓	✓	✓	✓	✓
PSFE	✓	✓	✓	✓	✓

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors at the level of the parent's village of origin in parentheses. Controls include the child's sex, birth order, year of observation, age cohort fixed effects, and his/her parent's sex, birth order, age group fixed effects and his/her parent's relation to the household head, schooling-for-age, marital status, height, and survey wave fixed effects in 1991/94. PSFE stands for parent sibling fixed effects.

Figures

Figure 1: Overview of the structure of the Kagera Health and Development Survey

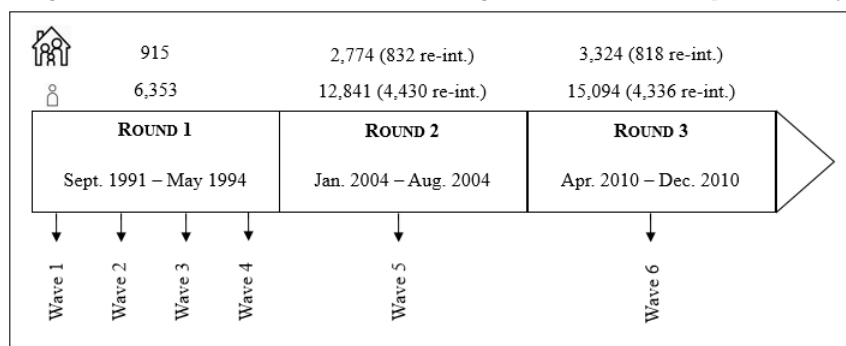
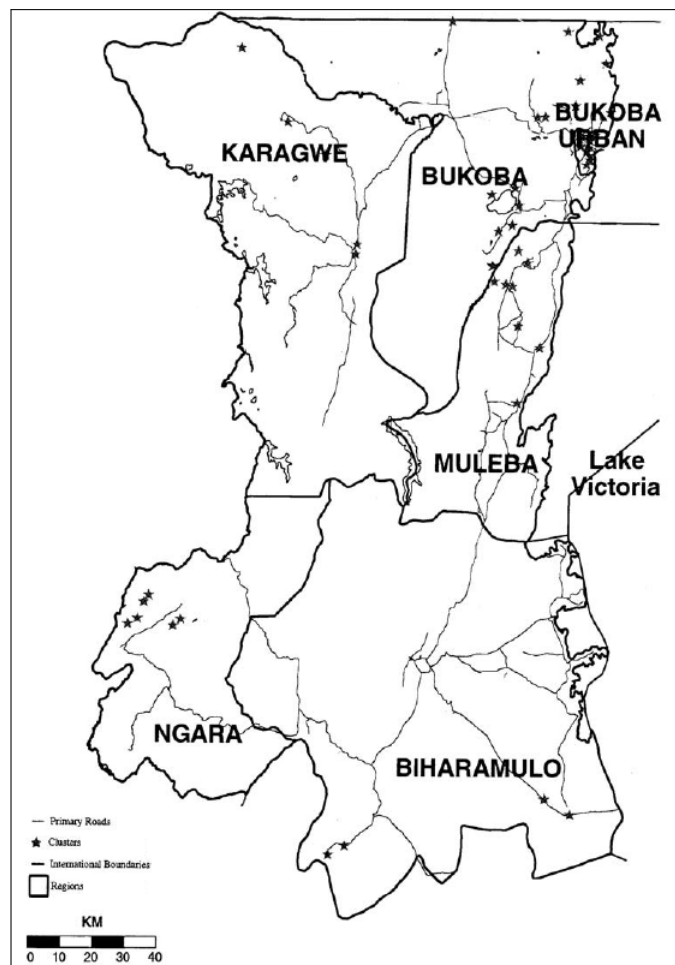
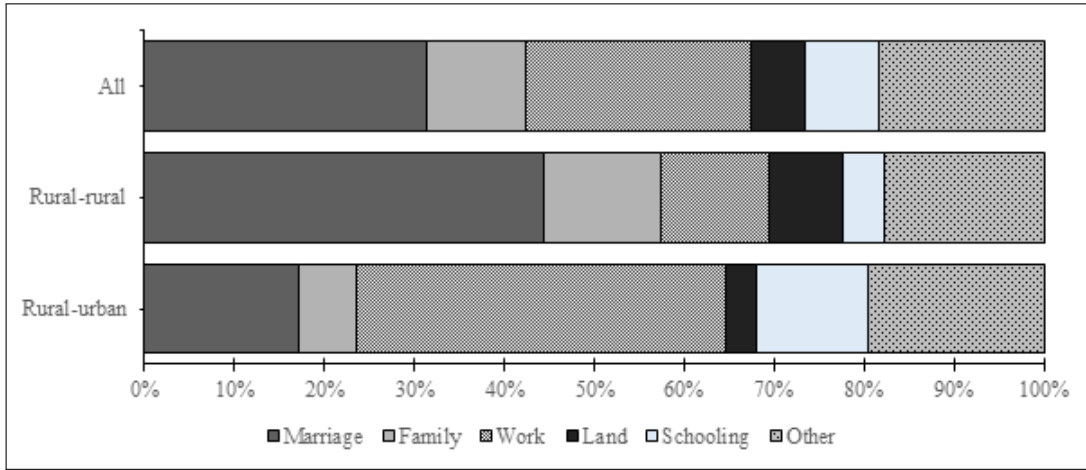


Figure 2: Clusters of households surveyed in Kagera in 1991/94



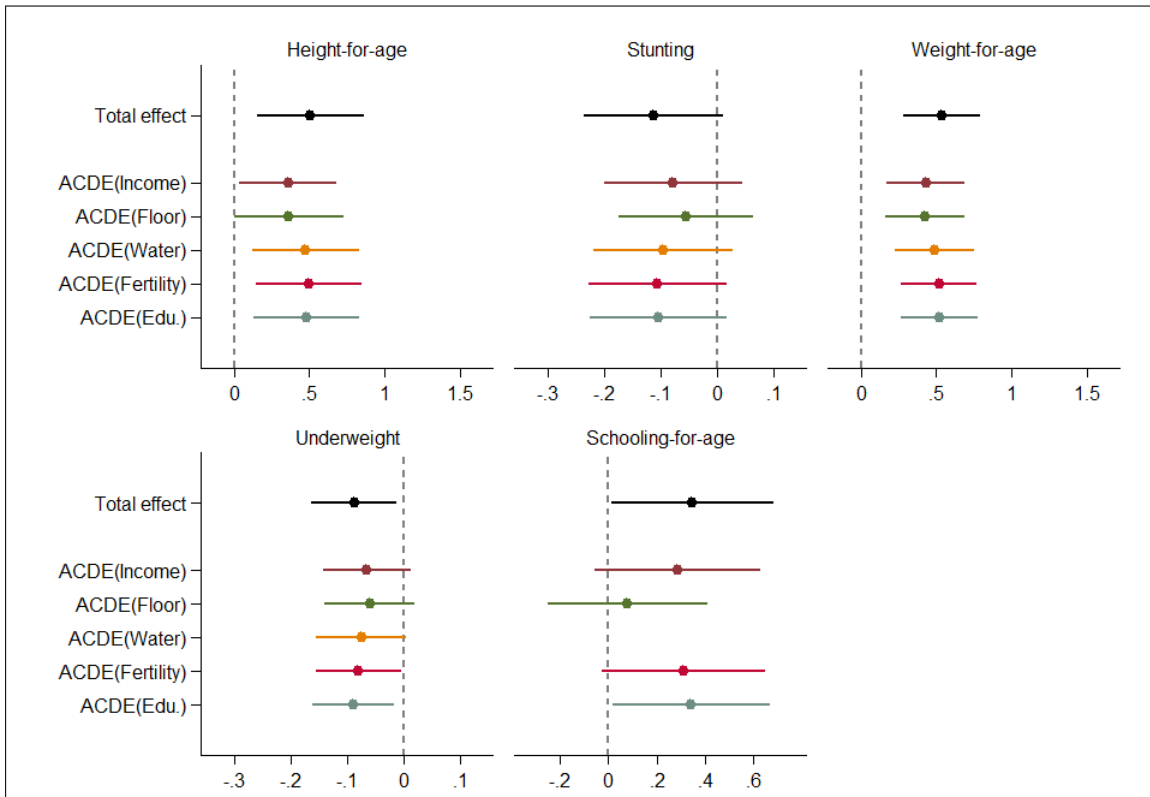
Note: As of 2010, Kagera is administratively divided into 8 districts (the district of Bukoba was split up and the districts of KyerwaMissenyi were newly created)

Figure 3: Main reported reason to move out of baseline (1991/94) community



Notes: This does not necessarily correspond to the motivation for moving to the location in which they are ultimately residing in 2004 and 2010 (that is the basis of the categorization of the destinations). The main reason to move to the current location was, however, not asked during the 2004 Survey.

Figure 4: Average Controlled Direct Effect of parental migration to cities ^a



Notes: ACDE(*mediator*) refers to the Average Controlled Direct Effect net of the effect taking place through the mediator variable, derived using the method proposed by Acharya et al. (2016). Horizontal spikes represent the 90% confidence intervals from 1000 bootstrapped replications. For a detailed summary of the differences between the total effect and the ACDEs see Appendix C, Table C7.

^a In the case of schooling-for-age, the mediation analysis is conducted for the effect of being the child of migrants born at a city destination.

Appendix A: Variables

Appendix A1: Linking children to parents and identifying siblings

Respondents were asked to report for each household member whether their natural mother and father were still alive and whether they were residing in this household. In addition, the respondents were required to indicate parents' names, which were then linked to a personal identification number if they were part of the survey. As the identity of parents (should) remain unchanged throughout time, information from different waves is used to impute missing values.

In addition, the following assumptions are made:

- When the sex of the identified “father” is reported as female and the sex of the identified “mother” as male, they are assumed to be switched.
- When information on the parents' identity is missing, but the relation to the household head is reported as “son or daughter”, the household head is assumed to be either the individual's mother or father depending on his/her reported sex. If there is only one spouse in the household, he or she is assumed to be the other parent.
- When information on the parent's identity is missing, but the individual is reported as the household head, the household member with relation to the household head reported as the “father or mother” is assumed to be the individual's father or mother depending on his/her reported sex.

Sibling groups are identified as individuals who identified the same person as their mother if only the mother's identity is known or the same person as their father if only the father's identity is known. If the parents' identities are unknown, but individuals report to be the household head and siblings of the household head, they are assumed to form a sibling group.

Appendix A2: Constructing years of schooling

Years of schooling were derived from the answers to the question whether the respondent has ever attended school and the reported highest grade completed. Those who negatively answered the first question are assumed to have zero years of education. The current schooling system in Tanzania consists of seven years of primary education, four years of secondary education and two years of advanced secondary education. Prior to independence, the schooling system involved eight years of primary school. For ease of comparison, this is equated to seven years of education. Adult education is equated to finishing primary school. Koranic education is counted as no years of education.

In summary, the information on highest grade obtained was transformed to years of education as follows:

Table 1: Constructing years of schooling)

Survey code	Grade	Years
Koranic	Koranic education only	0
P1	1st grade of primary education	1
P1	2nd grade of primary education	2
P2	3rd grade of primary education	3
P4	4th grade of primary education	4
P5	5th grade of primary education	5
P6	6th grade of primary education	6
P7	7th grade of primary education	7
P8	8th grade of primary education	7
Adult	Adult education	7
S1	1st grade of secondary education	8
S2	2nd grade of secondary education	9
S3	3rd grade of secondary education	10
S4	4th grade of secondary education	11
A1	1ste grade of advanced secondary education	12
A2	2nd grade of advanced secondary education	13
U1	1st grade of tertiary education	14
U2	2nd grade of tertiary education	15
U3	3rd grade of tertiary education (bachelor's degree)	16
U4	4th grade of tertiary education (master's degree)	17
U5	5th grade of tertiary education (master's degree)	18

Appendix A3: Average Controlled Direct Effects

A common approach to explore underlying causal mechanisms is to simply control for potential mediator variables in the regression to estimate the direct effect of the treatment. Yet, it has been demonstrated that simply conditioning on these variables can result in seriously biased estimates of direct effects leading to flawed conclusions regarding explanatory pathways. More specifically, the issue of intermediate variable bias may arise when there are intermediate confounders that are affected by the treatment and affect both the mediator and the outcome (e.g. Richiardi et al., 2013; Acharya et al., 2016; Vanderweele, 2010). When this occurs, conditioning on this variable can induce spurious correlations between the treatment and the outcome.

Acharya et al. (2016) propose a method to estimate the controlled direct effects without bias. More specifically, the outcome of interest is the average controlled direct effect (ACDE) which speaks to the question of what the average effect of the treatment would be if the mediator was fixed at a certain value for all units in the population. The ACDE is non-parametrically identified under the assumption of sequential unconfoundedness. That is, when there are (i) no omitted variables for the effect of the treatment on the outcome and (ii) no omitted variables for the effect of the mediator on the outcome, conditional on the treatment, pretreatment confounders, and intermediate confounders. It is important to acknowledge that this assumption is unverifiable and may not hold in this particular case.

The first stage of the sequential g-estimation proposed by Acharya et al. (2016) to obtain the ACDE estimates involves estimating the effect of the mediators (e.g. access to clean water) on the outcomes (e.g. height-for-age) conditional on the treatment (i.e. the dummy variables for migration)

and the other covariates:

$$Y_{i,t+r} = \alpha M_{i,t+r}^{Parent} + \phi_1 Ln(cons.p.c.)_{i,t+r} + \phi_2 Floor_{i,t+r} + \phi_3 Water_{i,t+r} + \phi_4 Fertility_{i,t+r} + \phi_5 Edu_{i,t+r} + \beta X_{i,t+r} + \gamma Z_{i,t}^{Parent} + S_i^{Parent} + u_{i,t+r} \quad (5)$$

Next, these estimates are used to purge the outcome variables of the effect of changes in the mediator variable to “demediate” them in other words by taking the estimated coefficient on the mediator variable, multiplying it by each unit’s value for the mediator variable, and subtracting that from the observed outcome. Note that this regression excludes any of the post-treatment variables from the first stage. The demediated outcomes for $Ln(cons.p.c.)_{i,t+r}$ thus correspond to:

$$\tilde{Y}_{i,t+r} = Y_{i,t+r} - \phi_1 Ln(cons.p.c.)_{i,t+r} \quad (6)$$

We then estimate the ACDEs by regressing these demediated outcomes on the dummy variables for migration as well as all (pre-treatment) covariates:

$$\tilde{Y}_{i,t+r} = \alpha M_{i,t+r}^{Parent} + \beta X_{i,t+r} + \gamma Z_{i,t}^{Parent} + S_i^{Parent} + u_{i,t+r} \quad r = 1, 2 \quad (7)$$

These estimates thus equate to what the effect of migration would have been had there been no differences in (i) income, (ii) housing conditions, (iii) access to clean drinking water, (iv) fertility, and (v) education in the household.

Appendix A3: Variable definitions

Variable name	Variable description
Height-for-age	<i>Height-for-age z-score</i> Standardized height-for-age z-score that indicates how many standard deviations a child (0-19 years) is away from the age- and sex-specific median height in a reference population of well-nourished children (WHO, 2007; 2011).
Stunting	<i>Stunting</i> A dummy variable equal to one when a child’s height-for-age is more than two standard deviations below the age- and sex-specific reference median.
Weight-for-age	<i>Weight-for-age z-score</i> Standardized weight-for-age z-score that indicates how many standard deviations a child (0-10 years) is away from the age- and sex-specific median weight in a reference population of well-nourished children (WHO, 2007; 2011).
Underweight	<i>Underweight</i> A dummy variable equal to one when a child’s weight-for-age is more than 2 standard deviations below the age- and sex-specific reference median.
Full vaccin.	<i>Full vaccination</i> A dummy variable equal to one when a child (0-5) was reported to have received three doses of vaccine protecting against tetanus, three doses of vaccine protecting against polio, one dose of vaccine protection against tuberculosis, and one dose of vaccine protection against measles.
Good health	<i>Reported good health</i>

	<p>A dummy variable equal to one when an individual's general health was reported as "good", on a 5 category ordinal scale ranging from "bad" to "excellent".</p> <p><i>Schooling-for-age z-score</i></p> <p>A child's years of education (see Section A.2) relative to the age-, sex- and survey-round-specific median years of education in this sample.</p>
Schooling-for-age	
School attend.	<p><i>School attendance</i></p> <p>Dummy variable equal to one when the child of primary and secondary school-going age (7-19) is reported to have attended school in the 12 months preceding the survey.</p>
School start.	<p><i>School starting age</i></p> <p>The age at which the child (3-19) is reported to have started school (including pre-primary school).</p>
M^{Parent}	<p><i>Child of at least one migrant parent</i></p> <p>Dummy variable equal to one when at least one of the child's parents who resided in rural Kagera during the 1991/94 round was found to live in a household in a different and distant (not-neighboring) location.</p>
M^{Parent}_{Rural}	<p><i>Child of at least one rural-rural migrant parent</i></p> <p>Dummy variable equal to one when at least one of the child's parents who resided in rural Kagera during the 1991/94 round was found to live in a household in a different and distant (not-neighboring) rural (as defined by the 2002 census classification) location than during the 1991/94 round.</p>
M^{Parent}_{Urban}	<p><i>Child of at least one rural-urban migrant parent</i></p> <p>Dummy variable equal to one when at least one of the child's parents who resided in rural Kagera during the 1991/94 round was found to live in a household in a distant (not-neighboring) urban (as defined by the 2002 census classification) location.</p>
M^{Parent}_{Town}	<p><i>Child of at least one rural-urban migrant parent</i></p> <p>Dummy variable equal to one when at least one of the child's parents who resided in rural Kagera during the 1991/94 round was found to live in household in a distant (not-neighboring) urban (as defined by the 2002 census classification) location outside of Dar es Salaam or Mwanza city (Ilemela and Nyamangana district) than during the 1991/94 round.</p>
M^{Parent}_{City}	<p><i>Child of at least one rural-urban migrant parent</i></p> <p>Dummy variable equal to one when at least one of the child's parents who resided in rural Kagera during the 1991/94 round was found to live in household in a different and distant (not-neighboring village) urban (as defined by the 2002 census classification) location in Dar es Salaam or Mwanza city (Ilemela and Nyamangana district).</p>
$M^{Parent}_{Rural(/Urban/Town/City),before}$	<p><i>Child born before a rural-rural(urban/town/city) move</i></p> <p>Dummy variable equal to one when at least one of the child's parents who resided in rural Kagera during the 1991/94 round was found to live in a household in a different and distant (not-neighboring) rural(/urban/town/city) location than during the 1991/94 round and the reported year of the parent's move is equal to or after the year of birth of the child.</p>
$M^{Parent}_{Rural(/Urban/Town/City),after}$	<p><i>Child born after a rural-rural(urban/town/city) move</i></p> <p>Dummy variable equal to one when at least one of the child's parents who resided in rural Kagera during the 1991/94 round was found to live in a household in a different and distant (not-neighboring) rural(/urban/town/city)location than during the 1991/94 round and the reported year of the parent's move is before the year of birth of the child.</p>
$M^{Parent}_{Rural(/Urban/Town/City),low}$	<p><i>Child of at least one rural-rural(/urban/town/city) migrant parent from the lower half of the initial income distribution</i></p> <p>Dummy variable equal to one when at least one of the child's parents who resided in rural Kagera during the 1991/94 round and whose household consumption per capita was below the (outcome variable-specific) sample median was found to live in a household in a different and distant (not-neighboring) rural(/urban/town/city)location than during the 1991/94 round and the reported year of the parent's move is before the year of birth of the child.</p>
$M^{Parent}_{Rural(/Urban/Town/City),high}$	<p><i>Child of at least one rural-rural(/urban/town/city) migrant parent from the lower half of the initial income distribution</i></p>

	<p>Dummy variable equal to one when at least one of the child's parents who resided in rural Kagera during the 1991/94 round and whose household consumption per capita was above the (outcome variable-specific) sample median was found to live in a household in a different and distant (not-neighboring) rural(/urban/town/city) location than during the 1991/94 round and the reported year of the parent's move is before the year of birth of the child.</p>
<i>Exposure_{Rural(/Urban/Town/City)}</i>	<p><i>Exposure to rural(/urban/town/city) destination</i></p> <p>The proportion of a child's life spent at a rural (/urban/town/city) destination. This is calculated as number of years a child has spent at the rural(/urban/town/destination), as derived by subtracting the reported year of migration from the year of the survey, divided by the age of the child and multiplied by 100.</p>
Child-specific controls	<p><i>Sex</i></p> <p>Reported sex (1= male, 2 = female).</p> <p><i>Child age cohort fixed effects</i></p> <p>Dummies for reported age in years (0-19).</p> <p><i>Schooling for age</i></p> <p><i>Birth order</i></p> <p>The birth order of the child, which was derived by ordering all children of the same mother who were part of one of the survey waves by age. If the identity of the mother is unknown, this variable reflects the order relative to other children of the same father.</p>
Parent's baseline characteristics	<p><i>Sex</i></p> <p>The child's parents reported sex (1= male, 2 = female).</p> <p><i>Age group fixed effects</i></p> <p>Dummy variables equal to one when the parent was between (i) 0 and 4, (ii) 5 and 15, (iii) 16 and 25, (iv) 26 and 35, (v) above 35 in 1991/94.</p> <p><i>Birth order</i></p> <p>The parent's birth order, which was derived by ordering all children of the same mother who were part of one of the survey waves by age. If the identity of the mother is unknown, this variable reflects the order relative to other children of the same father.</p> <p><i>Schooling-for-age</i></p> <p>The parent's reported years of education obtained (see Section A.2) relative to the age-, sex-specific median years of education in this sample in 1991/94 when the individual is below the secondary-school going age (i.e. 19), or the sex- specific median years of education in this sample in 1991/94 when the individual is aged above 19.</p> <p><i>Son or daughter</i></p> <p>A dummy variable equal to one when the parent was reported as the child of the household head in 1991/94.</p> <p><i>Head or spouse</i></p> <p>A dummy variable equal to one when the parent was reported to be the head of the household or his/her spouse in 1991/94.</p> <p><i>Marital status</i></p> <p>Dummy variable equal to one when the parent reported his/her marital status as married in 1991/94. Respondents below the age of 14 (legal age of marriage at the time) are assumed to be unmarried when information is missing. A dummy variable equal to one when this assumption was made is included in the regressions as well.</p> <p><i>Height</i></p> <p>Parent's height in 1991/94 expressed in cm. For individual above the age of 18 in multiple survey waves, information from other survey rounds is used to impute missing values.</p>
Ln[cons. pc.)	<p><i>Logarithm of total household consumption per capita</i></p> <p>The logarithm of real (adjusted for price differentials across time and space) total household consumption per capita.</p>
Floor	<p><i>Non-earth floor</i></p> <p>Dummy variable equal to one when the material of the floor of the building in which the household resides was not reported as earth.</p>
Water	<p><i>Improved drinking water source</i></p>

	Dummy variable equal to one when the main source of drinking water can be considered “improved” according to the according to the WHO/UNICEF Joint Monitoring Programme for Water supply, Sanitation and Hygiene. This excludes a well without pump or river or other sources.
Fertility	<i>Number of children born to the same mother</i> For children for whom the identity of the mother is unknown, this variable reflects the number of children born to the same father.
Education	<i>Highest level of education in the child's household</i> The highest grade obtained by any of the members of the household. This was converted to years of schooling based on the assumptions outlined in Appendix A2.

Appendix B: Robustness checks

I test the robustness of these results against several potential concerns. For the sake of brevity, the discussion here below is focused upon the regression specification including dummies equal to one when a child is born to at least one parent who moved to a different rural area, a secondary town, or a large city area.

First, the robustness of the results towards changes in the definitions of migrants and non-migrants is assessed. For the main analysis, individuals who stayed in their baseline village or a neighboring village were considered as non-migrants. A potential concern is that the results are confounded since some children in the control group are born to parents who moved, even if only to a destination close by. The results are highly similar in terms of magnitude and statistical significance when applying a less restrictive definition of migration and considering those who moved to a nearby village as migrants. In this case, the control group is thus restricted to children whose parent stayed in their exact baseline village. (see Appendix C, Tables 8-9, column 1). Similarly, the results are robust to adopting a more restrictive definition of migration and considering only those who moved to a different region as migrants (see Appendix C, Tables 8-9, column 2). It is worth noting that children of parents who moved outside of Kagera to secondary towns are, however, significantly less likely to have received all necessary vaccinations. Though the coefficient is only marginally significant, the magnitude is disconcerting. Compared to their cousins growing up in Kagera, these children are 14 percentage points less likely to have received all eight required vaccinations. This derives mainly from a reduction in the uptake of immunization against tetanus that requires multiple doses to be administered (see Appendix C, Table 10). It is worth noting that Kagera has consistently been among the best performing regions in terms of basic vaccination coverage. To the extent that this is, for example, related to supply-side factors specific to the region, it seems within reason that moving out of Kagera will affect immunization differently than moving within the region. Finally, the results are unaltered when excluding migrants who moved to Uganda (see Appendix C, Tables 8-9, column 3).

Because this is the impact of migration comparing within families, it nets out any transfers from migrants to those who stayed in rural Kagera. That is, if migrant parents, for example, sent back money to their origin households, then the estimates represent a lower bound of the impact of having a migrant parent since these remittances may relax liquidity constraints in the households of their siblings who stayed behind and allow them to invest more in health and education inputs. The results are, however, robust to controlling for the inverse hyperbolic sine transformation of the total value of gifts and loans (in cash or kind) received by the child's household from previous household members (see Appendix C, Tables 11-12, column 1). The estimated coefficients, however, increase slightly in terms of magnitude.

Finally, the massive influx of refugees from Burundi and Rwanda in Kagera in 1993 and 1994 represents a potential threat to the validity of the results presented in this paper. More specifically, if the refugee presence had a negative impact on the health and education outcomes of children in

the hosting communities in Kagera, the differences between children of migrants and the children of their siblings who stayed behind may reflect the deterioration of the latter group rather than or in addition to an effect of migration. Whereas several studies based upon the KHDS data indicate that the refugee presence has had a positive impact on the welfare of the local population (Maystadt and Verwimp, 2014; Maystadt and Duranton, 2018), Baez (2011) finds that childhood exposure to the refugee shock is associated with reduced height, schooling and literacy. The results reported in this paper do not, however, appear to be confounded by the effects of the refugee crisis. That is, I reach highly similar conclusions when restricting the analysis to children of parents originating from villages where no refugee camps were located within a 20km radius (see Appendix C, Tables 11-12, column 2) or even villages outside the two border areas that hosted the majority of the refugees, i.e., the districts of Karagwe and Ngara (see Appendix C, Tables 11-12, column 3).

Appendix C: Additional results

Table 1: Parental migration by destination and initial wealth and health (within-family estimates)

	Height-for-age (≤ 19)		Stunting (≤ 19)		Weight-for-age (≤ 10)		Underweight (≤ 10)		Full vaccin. (1-5)		Good health (≤ 19)	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
$M_{Rural,low}^{Parent}$	-0.048 (0.136)	-0.051 (0.136)	0.004 (0.042)	0.005 (0.041)	0.077 (0.104)	0.071 (0.104)	-0.033 (0.039)	-0.033 (0.039)	0.021 (0.035)	0.020 (0.035)	-0.002 (0.021)	-0.002 (0.021)
$M_{Rural,high}^{Parent}$	0.035 (0.122)	0.031 (0.122)	0.012 (0.039)	0.011 (0.039)	0.038 (0.104)	0.037 (0.103)	0.006 (0.036)	0.006 (0.036)	-0.012 (0.048)	-0.011 (0.048)	0.036 (0.028)	0.036 (0.028)
$M_{Urban,low}^{Parent}$	0.098 (0.197)		-0.036 (0.071)		0.217 (0.141)		-0.089 (0.055)		-0.065 (0.076)		-0.017 (0.030)	
$M_{Urban,high}^{Parent}$	0.336* (0.176)		-0.025 (0.063)		0.281** (0.130)		-0.016 (0.037)		-0.006 (0.050)		0.021 (0.035)	
$M_{Town,low}^{Parent}$		-0.004 (0.235)		0.015 (0.078)		0.076 (0.149)		-0.076 (0.061)		-0.090 (0.084)		0.010 (0.044)
$M_{Town,high}^{Parent}$		0.246 (0.148)		-0.022 (0.052)		0.234 (0.156)		-0.004 (0.045)		0.004 (0.041)		0.012 (0.036)
$M_{City,low}^{Parent}$		0.461 (0.280)		-0.208** (0.092)		0.702*** (0.241)		-0.135* (0.069)		0.004 (0.099)		-0.082 (0.062)
$M_{City,high}^{Parent}$		0.563* (0.322)		-0.044 (0.123)		0.444** (0.215)		-0.050 (0.053)		-0.021 (0.097)		0.034 (0.067)
N	5639	5639	5639	5639	4711	4711	4711	4711	2627	2627	5601	5601
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
PSFE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors at the level of the parent's village of origin in parentheses. Controls include the child's sex, birth order, year of observation, age cohort fixed effects, and his/her parent's sex, birth order, age group fixed effects and his/her parent's relation to the household head, schooling-for-age, marital status, height, and survey wave fixed effects in 1991/94. PSFE stands for parent sibling fixed effects.

Table 2: Exposure to the destination and health (within-family estimates)

	Height-for-age (≤ 19)		Stunting (≤ 19)		Weight-for-age (≤ 10)		Underweight (≤ 10)		Full vaccin. (1-5)		Good health (≤ 19)	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
$Exposure_{Rural}$	-0.000 (0.001)	-0.000 (0.001)	-0.000 (0.000)	-0.000 (0.000)	0.000 (0.001)	0.000 (0.001)	-0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
$Exposure_{Urban}$	0.002* (0.001)		-0.000 (0.001)		0.002** (0.001)		-0.000 (0.000)		-0.000 (0.001)		-0.000 (0.000)	
$Exposure_{Town}$		0.001 (0.001)		0.000 (0.001)		0.001 (0.001)		-0.000 (0.000)		-0.000 (0.001)		0.000 (0.000)
$Exposure_{City}$		0.005** (0.002)		-0.001 (0.001)		0.005*** (0.002)		-0.001 (0.000)		-0.000 (0.001)		-0.000 (0.000)
Observations	5498	5498	5498	5498	4631	4631	4631	4631	2622	2622	5461	5461
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
PSFE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors at the level of the parent's village of origin in parentheses. Exposure is defined as the share of life spent at the destination. Controls include the child's sex, birth order, year of observation, age cohort fixed effects, and his/her parent's sex, birth order, age group fixed effects and his/her parent's relation to the household head, schooling-for-age, marital status, height, and survey wave fixed effects in 1991/94. PSFE stands for parent sibling fixed effects.

Table 3: Parental migration by destination and timing and health (within-family estimates)

	Height-for-age (≤ 19)		Stunting (≤ 19)		Weight-for-age (≤ 10)		Underweight (≤ 10)		Full vaccin. (1-5)		Good health (≤ 19)	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
$M_{Rural, 2years}^{Parent}$	-0.157 (0.104)	-0.154 (0.104)	0.029 (0.038)	0.029 (0.038)	-0.024 (0.098)	-0.021 (0.099)	-0.003 (0.029)	-0.003 (0.029)	-0.007 (0.035)	-0.006 (0.035)	0.033 (0.023)	0.033 (0.023)
M_{Rural}^{Parent}	0.037 (0.095)	0.033 (0.095)	-0.011 (0.027)	-0.009 (0.027)	0.044 (0.081)	0.039 (0.080)	-0.010 (0.031)	-0.009 (0.031)	0.002 (0.032)	0.002 (0.032)	0.013 (0.023)	0.013 (0.023)
$M_{Urban, 2years}^{Parent}$	-0.035 (0.181)		-0.002 (0.041)		-0.069 (0.118)		0.000 (0.037)		-0.019 (0.054)		-0.022 (0.038)	
M_{Urban}^{Parent}	0.245* (0.144)		-0.034 (0.055)		0.267** (0.113)		-0.050 (0.035)		-0.033 (0.054)		0.006 (0.026)	
$M_{Town, 2years}^{Parent}$		0.020 (0.222)		0.004 (0.044)		-0.037 (0.150)		-0.010 (0.039)		-0.041 (0.063)		-0.071 (0.044)
M_{Town}^{Parent}		0.130 (0.161)		-0.007 (0.058)		0.158 (0.136)		-0.034 (0.039)		-0.037 (0.056)		0.026 (0.033)
$M_{City, 2years}^{Parent}$		-0.153 (0.256)		-0.021 (0.067)		-0.120 (0.222)		0.022 (0.057)		0.054 (0.053)		0.082 (0.056)
M_{City}^{Parent}		0.552** (0.232)		-0.108 (0.085)		0.571*** (0.182)		-0.093* (0.047)		-0.026 (0.081)		-0.032 (0.050)
N	5511	5511	5511	5511	4644	4644	4644	4644	2623	2623	5471	5471
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
PSFE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors at the level of the parent's village of origin in parentheses. Controls include the child's sex, birth order, year of observation, age cohort fixed effects, and his/her parent's sex, birth order, age group fixed effects and his/her parent's relation to the household head, schooling-for-age, marital status, height, and survey wave fixed effects in 1991/94. PSFE stands for parent sibling fixed effects.

Table 4: Parental migration by destination and vaccination (within-family estimates)

	Tetanus (3 doses)			Polio (3 doses)			Measles (1 dose)			TB (1 dose)		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
M^{Parent}	-0.013 (0.025)			-0.015 (0.024)			-0.004 (0.010)			0.005 (0.007)		
M_{Rural}^{Parent}		0.011 (0.025)	0.011 (0.025)		0.006 (0.022)	0.006 (0.022)		-0.011 (0.011)	-0.012 (0.011)		-0.001 (0.008)	-0.000 (0.008)
M_{Urban}^{Parent}		-0.057 (0.043)			-0.052 (0.043)			0.011 (0.019)			0.014 (0.011)	
M_{Town}^{Parent}			-0.061 (0.048)			-0.045 (0.048)			0.002 (0.021)			0.023* (0.012)
M_{City}^{Parent}			-0.048 (0.064)			-0.072 (0.065)			0.035 (0.026)			-0.007 (0.024)
N	2640	2640	2640	2640	2640	2640	2640	2640	2640	2640	2640	2640
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
PSFE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors at the level of the parent's village of origin in parentheses. Controls include the child's sex, birth order, year of observation, age cohort fixed effects, and his/her parent's sex, birth order, age group fixed effects and his/her parent's relation to the household head, schooling-for-age, marital status, height, and survey wave fixed effects in 1991/94. PSFE stands for parent sibling fixed effects.

Table 5: Comparing naive estimates to within-family estimates (rural or urban)

	Height-for-age (≤ 19)		Stunting (≤ 19)		Weight-for-age (≤ 10)		Underw. (≤ 10)		Full vaccin. (1-5)		Good health (≤ 19)		Schooling-for-age (7-19)		School attend. (7-19)		School start. (3-19)	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
M_{Rural}^{Parent}	0.031 (0.062)	-0.040 (0.095)	-0.006 (0.020)	0.013 (0.026)	-0.001 (0.056)	0.051 (0.072)	0.007 (0.020)	-0.015 (0.028)	0.000 (0.025)	0.005 (0.032)	0.010 (0.015)	0.016 (0.019)	-0.066 (0.075)	-0.095 (0.113)	-0.017 (0.023)	0.006 (0.030)	0.204** (0.095)	0.240** (0.117)
M_{Urban}^{Parent}	0.450*** (0.087)	0.201 (0.126)	-0.075** (0.030)	-0.027 (0.047)	0.395*** (0.069)	0.252** (0.099)	-0.067*** (0.024)	-0.054 (0.035)	-0.031 (0.031)	-0.037 (0.048)	0.001 (0.014)	0.003 (0.025)	0.214* (0.122)	-0.059 (0.182)	0.021 (0.035)	-0.019 (0.035)	-0.043 (0.118)	0.111 (0.185)
N	5616	5616	5616	5616	4702	4702	4702	4702	2620	2620	5579	5579	2261	2261	2284	2284	2393	2393
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
PSFE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
$M_{Rural(1)}^{Parent} - M_{Rural(2)}^{Parent}$	0.071 (0.074)	-0.019 (0.023)	-0.052 (0.063)	0.021 (0.022)	-0.004 (0.022)	-0.006 (0.015)	0.029 (0.088)	0.029 (0.088)	-0.023 (0.027)	-0.023 (0.027)	-0.023 (0.027)	-0.023 (0.027)	-0.023 (0.027)	-0.023 (0.027)	-0.023 (0.027)	-0.023 (0.027)	-0.023 (0.027)	-0.023 (0.027)
$M_{Urban(1)}^{Parent} - M_{Urban(2)}^{Parent}$	0.249*** (0.092)	-0.048 (0.030)	0.144* (0.075)	-0.128 (0.021)	0.005 (0.025)	-0.002 (0.018)	0.273** (0.119)	0.273** (0.119)	0.039** (0.032)	0.039** (0.032)	0.039** (0.032)	0.039** (0.032)	0.039** (0.032)	0.039** (0.032)	0.039** (0.032)	0.039** (0.032)	0.039** (0.032)	0.039** (0.032)

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors at the level of the parent's village of origin in parentheses. Standard errors for the difference between $M_{Rural(1)}^{Parent}$ and $M_{Rural(2)}^{Parent}$, and $M_{Urban(1)}^{Parent}$ and $M_{Urban(2)}^{Parent}$ were obtained from 1000 bootstrapped replications. Controls include the child's sex, birth order, year of observation, age cohort fixed effects, and his/her parents sex, birth order, age group fixed effects. PSFE stands for parent sibling fixed effects. In the specification without the PSFE, the logarithm of total household consumption per capita, whether the household is headed by a farmer, highest level of education in the household, sex of the household head are controlled for.

Table 6: Comparing naive estimates to within-family estimates (rural, town, or city)

	Height- for-age (≤ 19)	Stunting (≤ 19)	Weight- for-age (≤ 10)	Underw. (≤ 10)	Full vaccin. (1-5)	Good health (≤ 19)	Schooling- for-age (7-19)	School attend. (7-19)	School start. (3-19)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
M_{Rural}^{Parent}	0.034 (0.062)	-0.043 (0.095)	-0.006 (0.020)	0.014 (0.026)	-0.000 (0.056)	0.047 (0.072)	0.007 (0.020)	-0.014 (0.028)	0.001 (0.025)	0.004 (0.032)	0.010 (0.015)	0.016 (0.019)	-0.064 (0.074)	-0.092 (0.114)	-0.017 (0.023)	0.006 (0.030)	0.203** (0.095)	0.241** (0.117)
M_{Town}^{Parent}	0.297***	0.105 (0.140)	-0.046 (0.033)	-0.002 (0.049)	0.336*** (0.079)	0.154 (0.118)	-0.060** (0.026)	-0.041 (0.040)	-0.047 (0.034)	-0.043 (0.051)	-0.002 (0.016)	0.013 (0.028)	0.065 (0.143)	-0.173 (0.230)	0.001 (0.044)	-0.016 (0.047)	0.083 (0.149)	0.168 (0.185)
M_{City}^{Parent}	0.832*** (0.090)	0.494** (0.206)	-0.147*** (0.039)	-0.102 (0.079)	0.543*** (0.122)	0.550*** (0.164)	-0.083*** (0.028)	-0.095*** (0.043)	0.003 (0.042)	-0.019 (0.076)	0.008 (0.022)	-0.020 (0.050)	0.553*** (0.086)	0.224 (0.218)	0.064*** (0.024)	-0.025 (0.029)	-0.268** (0.126)	-0.002 (0.385)
N	5616	5616	5616	5616	4702	4702	4702	4702	2620	2620	5579	5579	2261	2261	2284	2284	2393	2393
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
PSFE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
$M_{Rural(1)}^{Parent} - M_{Rural(2)}^{Parent}$	0.077 (0.074)	-0.020 (0.023)	-0.047 (0.063)	0.021 (0.022)	-0.003 (0.022)	-0.006 (0.015)	0.028 (0.088)	0.028 (0.088)	0.028 (0.088)	0.028 (0.088)	0.028 (0.088)	0.028 (0.088)	0.028 (0.088)	0.028 (0.088)	0.028 (0.088)	0.028 (0.088)	0.028 (0.088)	0.028 (0.088)
$M_{Town(1)}^{Parent} - M_{Town(2)}^{Parent}$	0.192* (0.101)	-0.044 (0.030)	0.182** (0.081)	-0.019 (0.023)	-0.003 (0.027)	-0.015 (0.021)	-0.003 (0.023)	-0.015 (0.021)	-0.003 (0.023)	-0.003 (0.023)	-0.015 (0.021)	-0.015 (0.021)	-0.003 (0.023)	-0.015 (0.021)	-0.015 (0.021)	-0.015 (0.021)	-0.015 (0.021)	-0.015 (0.021)
$M_{City(1)}^{Parent} - M_{City(2)}^{Parent}$	0.338* (0.092)	-0.045 (0.063)	-0.007 (0.139)	0.011 (0.039)	0.023 (0.044)	0.028 (0.030)	0.028 (0.030)	0.028 (0.030)	0.028 (0.030)	0.028 (0.030)	0.028 (0.030)	0.028 (0.030)	0.028 (0.030)	0.028 (0.030)	0.028 (0.030)	0.028 (0.030)	0.028 (0.030)	0.028 (0.030)

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors at the level of the parent's village of origin in parentheses. Standard errors for the difference between $M_{Rural(1)}^{Parent}$ and $M_{Rural(2)}^{Parent}$, $M_{Town(1)}^{Parent}$ and $M_{Town(2)}^{Parent}$, and $M_{City(1)}^{Parent}$ and $M_{City(2)}^{Parent}$ were obtained from 1000 bootstrapped replications. Controls include the child's sex, birth order, year of observation, age cohort fixed effects, and his/her parents sex, birth order, age group fixed effects. PSFE stands for parent sibling fixed effects. In the specification without the PSFE, the logarithm of total household consumption per capita, whether the household is headed by a farmer, highest level of education in the household, sex of the household head are controlled for.

Table 7: Average Controlled Direct Effects of parental migration to cities ^a

	Height- for-age (≤ 19)	Stunting (≤ 19)	Weight- for-age (≤ 10)	Underw. (≤ 10)	Schooling- for-age (7-19)	Height- for-age (≤ 19)	Stunting (≤ 19)	Weight- for-age (≤ 10)	Underw. (≤ 10)	Schooling- for-age (7-19)
M_{City}^{Parent} (Total effect)	0.508** (0.214)	-0.112 (0.075)	0.537** (0.153)	-0.087* (0.046)	0.346* (0.203)	M_{City}^{Parent} (Total effect) - M_{City}^{Parent} (ACDE)				
M_{City}^{Parent} (ACDE(<i>Income</i>))	0.357* (0.196)	-0.078 (0.074)	0.430** (0.157)	-0.065 (0.048)	0.286 (0.208)	0.151*** (0.005)	-0.034** (0.014)	0.108*** (0.036)	-0.023** (0.012)	0.060 (0.049)
M_{City}^{Parent} (ACDE(<i>Floor</i>))	0.364* (0.220)	-0.054 (0.073)	0.423** (0.160)	-0.060 (0.048)	0.080 (0.200)	0.145*** (0.052)	-0.058*** (0.020)	0.114** (0.048)	-0.078* (0.015)	0.266*** (0.087)
M_{City}^{Parent} (ACDE(<i>Water</i>))	0.475** (0.217)	-0.095 (0.075)	0.489** (0.158)	-0.074 (0.048)		0.354 (0.041)	-0.018 (0.015)	0.048** (0.038)	-0.013 (0.014)	
M_{City}^{Parent} (ACDE(<i>Fertility</i>))	0.497** (0.214)	-0.105 (0.075)	0.518*** (0.155)	-0.079* (0.046)	0.310 (0.206)	0.012 (0.013)	-0.008 (0.007)	0.019 (0.013)	-0.008 (0.005)	0.036 (0.031)
M_{City}^{Parent} (ACDE(<i>Edu.</i>))	0.480** (0.196)	-0.104 (0.074)	0.522*** (0.155)	-0.089** (0.044)	0.342* (0.222)	0.028 (0.022)	-0.007 (0.005)	0.015 (0.017)	0.001 (0.006)	0.004 (0.032)
N	5639	5639	4711	4711	2249	5639	5639	4711	4711	2249
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
PSFE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: ACDE(*mediator*) refers to the Average Controlled Direct Effect net of the effect taking place through the mediator variable, derived using the method proposed by Acharya et al. (2016). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors at the level of the parent's village of origin in parentheses. For the ACDEs these are the standard errors from 1000 bootstrapped replications. Controls include the child's sex, birth order, year of observation, age cohort fixed effects, and his/her parent's sex, birth order, age group fixed effects. PSFE stands for parent sibling fixed effects. In the specification without the PSFE, the logarithm of total household consumption per capita, whether the household is headed by a farmer, highest level of education in the household, sex of the household head are controlled for.

^a In the case of schooling-for-age, the mediation analysis is conducted for the effect of being the child of migrants born at a city destination.

Table 8: Parental migration and health (within-family estimates) - robustness checks

	Height-for-age (≤ 19)			Stunting (≤ 19)			Weight-for-age (≤ 10)			Underw. (≤ 10)			Full vaccin. (1-5)			Good health (≤ 19)		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
M_{Rural}^{Parent}	-0.011 (0.068)	0.170 (0.192)	-0.045 (0.089)	0.015 (0.020)	-0.058 (0.055)	0.014 (0.025)	0.060 (0.060)	0.078 (0.135)	0.033 (0.075)	-0.002 (0.023)	-0.016 (0.051)	-0.011 (0.029)	-0.034 (0.039)	-0.012 (0.053)	0.001 (0.031)	0.003 (0.023)	0.002 (0.034)	0.015 (0.020)
M_{Town}^{Parent}	0.159 (0.111)	-0.005 (0.196)	0.103 (0.130)	-0.014 (0.043)	0.040 (0.062)	0.001 (0.048)	0.149 (0.101)	0.025 (0.145)	0.148 (0.120)	-0.031 (0.036)	0.020 (0.054)	-0.040 (0.039)	-0.057 (0.045)	-0.142* (0.071)	-0.048 (0.053)	0.018 (0.023)	-0.035 (0.037)	0.010 (0.029)
M_{City}^{Parent}	0.527** (0.235)	0.504** (0.236)	0.519** (0.248)	-0.114 (0.087)	-0.113 (0.087)	-0.105 (0.094)	0.554*** (0.174)	0.516*** (0.171)	0.564*** (0.184)	-0.086* (0.043)	-0.077* (0.041)	-0.094** (0.045)	-0.026 (0.076)	-0.021 (0.072)	-0.038 (0.074)	-0.016 (0.050)	-0.022 (0.049)	-0.020 (0.052)
N	5658	5658	5601	5658	5658	5601	4732	4732	4679	4732	4732	4679	2640	2640	2602	5621	5621	5555
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
PSFE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors at the level of the parent's village of origin in parentheses. a The following controls are included: the child's sex, birth order, year of observation, age cohort fixed effects, and the parent's sex, birth order, relation to the household head, schooling-for-age, marital status, height, survey wave fixed effects, and age group fixed effects in 1991/94. PSFE stands for parent siblings fixed effects. Model 1 reports the results of the regressions where children of those who moved to a nearby village are considered as children of migrants. In model 2, only children of at least one parent who moved outside Kagera are considered as children of migrants. Children of parents moving outside Tanzania are excluded in model 3.

Table 9: Parental migration and education (within-family estimates) - robustness checks

	Schooling-for-age (7-19)			School attend. (7-19)			School start. (3-19)		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
M_{Rural}^{Parent}	0.071 (0.124)	0.170 (0.343)	-0.051 (0.101)	0.018 (0.033)	0.021 (0.067)	0.007 (0.026)	0.008 (0.117)	0.157 (0.361)	0.230* (0.121)
M_{Town}^{Parent}	-0.052 (0.199)	-0.107 (0.262)	-0.147 (0.224)	0.031 (0.045)	0.005 (0.090)	-0.003 (0.042)	0.052 (0.156)	-0.056 (0.210)	0.147 (0.183)
M_{City}^{Parent}	0.251 (0.193)	0.238 (0.191)	0.207 (0.201)	-0.037 (0.030)	-0.044 (0.028)	-0.049 (0.032)	0.016 (0.332)	0.010 (0.324)	0.106 (0.366)
N	2284	2284	2269	2307	2307	2291	2415	2415	2394
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
PSFE	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors at the level of the parent's village of origin in parentheses. The following controls are included: the child's sex, birth order, year of observation, age cohort fixed effects, and the parent's sex, birth order, relation to the household head, schooling-for-age, marital status, height, survey wave fixed effects, and age group fixed effects in 1991/94. PSFE stands for parent siblings fixed effects. Model 1 reports the results of the regressions where children of those who moved to a nearby village are considered as children of migrants. In model 2, only children of at least one parent who moved outside Kagera are considered as children of migrants. Children of parents moving outside Tanzania are excluded in model 3.

Table 10: Parental migration by destination and vaccination (within-family estimates)

	Tetanus (3 doses)	Polio (3 doses)	Measles (1 dose)	TB (1 dose)
M_{Rural}^{Parent}	0.001 (0.042)	0.053 (0.038)	-0.022 (0.021)	-0.004 (0.020)
M_{Town}^{Parent}	-0.148** (0.070)	-0.093 (0.071)	-0.017 (0.031)	0.034** (0.017)
M_{City}^{Parent}	-0.052 (0.063)	-0.069 (0.064)	0.033 (0.026)	-0.009 (0.024)
N	2640	2640	2640	2640
Controls	✓	✓	✓	✓
PSFE	✓	✓	✓	✓

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors at the level of the parent's village of origin in parentheses. The following controls are included: the child's sex, birth order, year of observation, age cohort fixed effects, and the parent's sex, birth order, relation to the household head, schooling-for-age, marital status, height, survey wave fixed effects, and age group fixed effects in 1991/94. PSFE stands for parent siblings fixed effects.

Table 11: Parental migration and health (within-family estimates) - robustness checks continued

	Height-for-age (≤ 19)			Stunting (≤ 19)			Weight-for-age (≤ 10)			Underw. (≤ 10)			Full vaccinn. (1-5)			Good health (≤ 19)		
	(1)	(2)	(3)	(1)	(2)	(3)	1	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
M_{Rural}^{Parent}	-0.022 (0.089)	-0.004 (0.100)	0.031 (0.122)	0.009 (0.025)	0.008 (0.026)	0.007 (0.031)	0.043 (0.074)	0.035 (0.080)	0.081 (0.092)	-0.013 (0.028)	-0.019 (0.031)	-0.028 (0.037)	0.002 (0.031)	-0.001 (0.035)	0.015 (0.037)	0.015 (0.020)	0.012 (0.022)	0.029 (0.026)
M_{Town}^{Parent}	0.115 (0.130)	0.050 (0.134)	0.082 (0.146)	-0.004 (0.048)	0.016 (0.050)	0.010 (0.056)	0.153 (0.119)	0.168 (0.122)	0.220 (0.134)	-0.042 (0.039)	-0.039 (0.041)	-0.034 (0.046)	-0.043 (0.052)	-0.018 (0.059)	-0.029 (0.072)	0.009 (0.028)	0.004 (0.030)	0.009 (0.034)
M_{City}^{Parent}	0.520** (0.234)	0.526** (0.239)	0.465* (0.254)	-0.121 (0.087)	-0.117 (0.090)	-0.114 (0.095)	0.552*** (0.174)	0.564*** (0.170)	0.606*** (0.182)	-0.091** (0.042)	-0.093** (0.044)	-0.108** (0.045)	-0.015 (0.074)	-0.006 (0.075)	-0.008 (0.081)	-0.023 (0.048)	-0.015 (0.048)	-0.021 (0.053)
N	5658	4895	3937	5658	4895	3937	4732	4071	3264	4732	4071	3264	2640	2273	1837	5621	4867	3885
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
PSFE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors at the level of the parent's village of origin in parentheses. The following controls are included: the child's sex, birth order, year of observation, age cohort fixed effects, and the parent's sex, birth order, relation to the household head, schooling-for-age, marital status, height, survey wave fixed effects, and age group fixed effects in 1991/94. PSFE stands for parent siblings fixed effects. Model 1 reports the results of the regressions where the inverse hyperbolic sine transformation of the total value of gifts and loans (in cash or kind) received by the child's household from previous household members is included as a control. In model 2, children of parents originating from a village that was within 20km of a refugee camp are excluded. Children of parents originating from the Karagwe of Ngara districts in Kagera are excluded in model 3.

Table 12: Parental migration and education (within-family estimates) - robustness checks continued

	Schooling-for-age			School attend.			School start.		
	(7-19)			(7-19)			(3-19)		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
M_{Rural}^{Parent}	-0.033 (0.101)	-0.044 (0.103)	-0.081 (0.115)	0.014 (0.025)	0.016 (0.028)	0.017 (0.034)	0.201* (0.117)	0.146 (0.107)	0.159 (0.102)
M_{Rural}^{Parent}	-0.157 (0.218)	-0.178 (0.222)	-0.216 (0.238)	-0.002 (0.041)	-0.007 (0.043)	-0.011 (0.042)	0.153 (0.177)	0.057 (0.173)	0.109 (0.183)
M_{Rural}^{Parent}	0.303 (0.199)	0.241 (0.195)	0.190 (0.195)	-0.034 (0.029)	-0.041 (0.031)	-0.024 (0.029)	-0.028 (0.326)	0.013 (0.336)	0.206 (0.275)
N	2284	2016	1622	2307	2036	1635	2415	2129	1688
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
PSFE	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Cluster-robust standard errors at the level of the parent's village of origin in parentheses. The following controls are included: the child's sex, birth order, year of observation, age cohort fixed effects, and the parent's sex, birth order, relation to the household head, schooling-for-age, marital status, height, survey wave fixed effects, and age group fixed effects in 1991/94. PSFE stands for parent siblings fixed effects. Model 1 reports the results of the regressions where the inverse hyperbolic sine transformation of the total value of gifts and loans (in cash or kind) received by the child's household from previous household members is included as a control. In model 2, children of parents originating from a village that was within 20km of a refugee camp are excluded. Children of parents originating from the Karagwe of Ngara districts in Kagera are excluded in model 3.