

The long run effects of labor migration on human capital formation in communities of origin

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Many economists recognize that labor migration is one of the most direct strategies for escaping poverty. Yet, considerable debate remains over whether and how migration affects communities of origin over the long run. This paper provides new evidence of one channel through which circular labor migration has lasting effects on origin communities: by raising human capital formation of the next generation. We estimate the net effects of labor migration from Malawi to the South African mines using a difference-in-differences strategy, along with newly digitized Census and administrative data on access to mine jobs, and two plausibly exogenous shocks to the option to migrate. Twenty years after these shocks subsided, human capital is 4.8-6.9% higher among cohorts who were eligible for schooling in those communities of origin with the easiest access to migrant jobs. [133 words]

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1. Introduction

Many economists recognize that labor migration is conceptually one of the most direct strategies that poor individuals and families can use to improve their standards of living (Pritchett 2006, Banerjee and Duflo 2007, Clemens 2011). Indeed, a growing body of work documents how migration and access to migrant remittances affect individual and household-level outcomes in the short run.² Yet, considerable debate remains over whether and how migration affects communities of origin over the long run (Lucas, 2005, Constant and Zimmerman 2013). Hampering our ability to learn whether the short run effects of labor migration translate into lasting impacts on growth and development outcomes is the general lack of high quality data on migrant flows and challenging identification concerns (Clemens and McKenzie 2014) not easily addressed with existing research designs. This paper overcomes these challenges by leveraging data from historical migration episodes to provide the first credible estimates that adult labor migration raises human capital formation in origin communities over the long run. In so doing, we provide novel evidence on one specific channel through which migration positively affects inputs for growth in these communities.

Theory and existing empirical evidence on the short run impacts of migration suggest our finding is not obvious. Labor migration could affect the next generation's human capital formation in several conflicting ways (Antman 2013). For example: access to remittance income from migration could relax credit constraints, thereby directly increasing the demand for schooling (e.g. Yang 2008, Hanson and Woodruff 2003), although effects could depend on whether remittances are treated as temporary or permanent changes in income. Expected wage differentials between migrant and non-migrant jobs may incentivize kids to stay in school as the "brain gain" hypothesis suggests (Batista, Lacueste and Vicente 2012) or may undermine human capital accumulation by encouraging drop out (De Brauw and Giles, 2006; Gibson, McKenzie and Stillman 2011; McKenzie and Rapoport 2011). Losing a prime-aged adult even temporarily may cause families to substitute towards child labor and away from schooling (Antman 2011). Finally, large labor migration flows may have important effects on wages and prices in general equilibrium, generating positive or negative spillovers for education decisions in non-migrant households. Which of these factors dominate, and whether they are jointly large enough to alter human capital profiles of origin communities over the long run, are therefore empirical questions.

In this paper, we estimate the long run effects of labor migration on human capital formation in origin communities using the historical experience of circular labor migration in Malawi. This temporary, "there

² Important recent work in this area has been done by Yang (2008), Antman (2011), Beegle, De Weerd and Dercon (2011), Gibson, McKenzie and Stillman (2011) and (2013) and Theoharides (2014). All of these papers pay careful attention to identification concerns in settings where migrants are a selected group of individuals.

and back again” migration has long been a central feature in many developing countries. But credibly identifying the net effects of circular labor migration on long run outcomes is empirically challenging. Not only are high quality migrant flow data seldom available for developing countries, but endogeneity concerns plague estimates of migration impacts coming from comparisons of outcomes across high and low migration regions, for example: high migration regions may differ on measures of local economic activity that are also correlated with investments in education (Gibson, McKenzie and Stillman 2013). In the existing literature, researchers have cleverly used visa lottery designs and quasi-experimental variation in the value of remittances deriving from unanticipated exchange rate shocks to estimate how labor migration affects sending households in the short and medium-runs. These designs are unfortunately not well-suited to estimating long run effects of migration on communities of origin.

We overcome these empirical challenges by studying the long run effects of differential labor migration flows on sending communities in a context characterized by pre-existing spatial differences in the costs of accessing migrant jobs and time-varying exogenous shocks to the option to migrate. We implement a difference-in-differences strategy by combining newly digitized Census and administrative data for Malawi that capture migrant flows at sub-national level over time with spatial variation in migration costs, exogenous shocks to labor migration and remittances in sending communities and measures of long run educational attainment in these communities. We are aware of no other dataset that would allow a similar exploration of the impact of migrancy on long run community-level outcomes.³

Malawi provides a uniquely advantageous setting for answering our research question. Throughout the twentieth century, it was one of the primary suppliers of unskilled male workers for the South African Chamber of Mines. By the late 1970s, one in five Malawian men had ever worked abroad, typically on a series of two-year labor contracts that diverted up to two thirds of earnings to be paid only on return to Malawi. In Section 2, we explain how, since only men could be miners, there are no missing migrant households in our data. We describe how men on contract could not choose when to return, implying no self-selection of migration duration or return migrants. And we describe the system of deferred pay that ensured capital flowed back to rural areas and that the lion’s share of these flows was exogenously determined rather than driven by migrant motivations (Lucas and Stark, 1985).

³ A distinguishing feature of our work is that we can use Census data to get an accurate measure of migration prevalence at district-level. This is typically impossible to capture in general household surveys (De Brauw, Mueller and Lee 2013). Woodruff (2007) discusses studies that use household survey or Census data to estimate the effects of labor migration on origin communities in Mexico. Our work differs from these studies in that we use our data to estimate the impacts of specific historical migration episodes on outcomes measured many years later. In contrast, the Mexican studies estimate the causal effect of current labor migration by using historical migration networks to instrument for current migration.

Our approach exploits two sides of a natural experiment that generated exogenous variation in the opportunity to migrate from Malawi to the South African mines. In 1967, a new labor treaty eliminated historical recruiting quotas and led to an immediate three-fold increase in migration to South African mines. Seven years later, Malawi's president banned recruiting and recalled all workers, after a mining plane crash in April 1974 killed a group of Malawian workers. The ban remained in place until 1977, but labor migration flows never recovered to pre-1974 levels. The timing of these two key events interacted with pre-existing differences in migration costs at the district level imply differential childhood exposure to these mine labor migration shocks. This exogenous exposure forms the basis of our identification strategy.⁴

To measure the long run impacts of labor migration on origin communities, the first comparison in our difference-in-differences strategy estimates gaps in human capital stocks of cohorts that were age-eligible for primary school after 1967 between districts with and without access to historically-placed mine recruiting stations. These stations made it easier to migrate from some districts relative to others. We document how access to this historical recruiting network, in place since the 1930s, generated spatial variation in the size of labor migration shocks between 1967 and 1977. For each additional recruiting station, there are an additional 1,500 migrants, or 15 percentage points more migrants per district relative to pre-shock migration rates. Our second difference uses control cohorts who are too old to be in primary school after 1967 to net out any pre-existing differences in educational attainment across recruiting and non-recruiting districts. We use newly digitized Census data from 1977 and 1998 along with administrative data on the location of recruiting stations to estimate these models, controlling for cohort and district fixed effects, and for linear trends interacted with historical district-level covariates.

Our primary result indicates long-lasting effects of district-level exposure to shocks to circular labor migration on human capital stocks of the next generation. Twenty years after the migration shocks subsided, human capital among exposed cohorts is higher by between 4.8 and 6.9% in recruiting station districts, equivalent to between 0.12 and 0.18 years of schooling. We also estimate that 2 to 4% more adults have attained any primary schooling.⁵ Scaling these reduced form estimates using the first stage relationship between the presence of recruiting stations in a district and district-level migration flows, we find that for the average increase in migration between 1967 and 1977, eligible cohorts have between one third and one half a year more schooling by the late 1990s. This is a 13 to 18% increase, relative to mean

⁴ Our use of migration shocks measured at the level of the origin community is similar to the shock used in Theoharides (2014). In that paper, Theoharides estimates the short run impacts of labor migration demand shocks in the Philippines on enrollments in private secondary schools, also at district-level.

⁵ Importantly, we find no differences in the size of these education impacts across males and females. Because mining jobs were restricted to men, it seems unlikely that the education effects in our setting are explained by aspirations about earning a higher return to this education as a future labor migrant.

levels of schooling. Results are similarly large for access to primary school. Since there is a strong, positive relationship between human capital stocks of household heads and total household assets in Malawi, it is likely that these gains in total human capital generated through adult labor migration had large positive impacts on the well-being of subsequent generations.

Our analysis spans the early period of expanding labor migration from Malawi, the later period of the labor migration ban, and the periods directly before and after these shocks. The two shocks entailed potentially different long run effects of migration. Early treated cohorts experienced reductions in local male labor supplies along with increased remittance inflows, while later treated cohorts experienced similar cash infusions from accumulated migrant earnings, but no reduction in local labor supplies as adult migrants returned to Malawi. To allow each migration shock to have a different effect on long run human capital formation, we separate out cohorts eligible for schooling during the early expansion period (1967 to 1973) and cohorts eligible during the late labor ban period (1974 to 1977), all the while controlling for differences across recruiting and non-recruiting areas using the older age-ineligible cohorts. Consistently, we find that the effect of the migration shock in the labor ban years, after migrants return and adult labor is again available, is significantly larger than the effect of the migration shock during the labor expansion period, when migrants have left Malawi.

Because one of the ways in which the two migration shocks differed was in the presence or absence of adult male labor, we investigate whether the impacts on human capital formation vary with the local shadow value of child time, a potential substitute for missing labor. We show that educational attainment for all post-1967 cohorts is always higher in districts without agricultural plantations (non-estate districts) than in those with plantations (estate districts), where child labor was historically prevalent. We further show that in high labor migration districts with pre-existing agricultural estates, child labor increases significantly between 1967 and 1977. In contrast, in districts without estates, child labor falls in the wake of the migration shocks. While small sample sizes mean that these results are necessarily suggestive, they imply that the demand for substitute child labor in the wake of adult labor migration is one mechanism that may have muted the positive long run effects of migration on human capital formation in sending communities.

Our paper advances the migration and remittance literatures in several ways. By combining historical events with administrative and Census data, we provide the first credible estimates of the long run impacts of labor migration on human capital formation in communities of origin. Largely because of the nature of the data and empirical strategies available to researchers, the focus in much of the literature has been on measuring short and medium-run impacts of labor migration on school spending or on school

outcomes that vary across different types of migrant households, or across households with and without access to migrant networks.⁶ It is not clear from this work that the short run impacts on schooling choices necessarily result in differences in outcomes in the long run (Antman 2011), particularly at the level of the origin community. Despite being quantitatively smaller than the most credible estimates from the literature on the impact of remittances on school enrollments (e.g. Yang 2008, Theoharides 2014), the Malawian results suggest that we can generalize at least the sign of some of the short run results to the long run.

Our finding that eligible cohorts in migrant sending communities have higher levels of education two decades after the end of the high migration period has both historical and modern day relevance. Historically, many countries have had long-standing bilateral guest worker programs with similar features to the circular migration we describe in this paper (e.g. the US and Mexico; Germany and Turkey).⁷ Indeed, all Southern African countries were part of the pre-and post-colonial labor reservoir cultivated by the South African mines. Given the scale of the circular migration we document in Malawi, it is surprising that there is so little empirical evidence on the long run effects on sending economies of these labor recruitment institutions.⁸ The lessons we learn from this important yet little documented period in the economic history of Africa also have considerable relevance for existing and future planned guest worker programs in place in many low-income countries (Gibson, McKenzie and Rohorua 2013). Although some details of these modern programs may differ (e.g. deferred pay in worker contracts), modern guest worker programs also have limited-time work contracts, in-built circularity in migration flows and periodic labor bans that mirror the Malawian experience.⁹ The evidence in this paper suggests that there is scope for modern guest worker programs to have positive, and importantly, long-lasting impacts on human capital formation in communities of origin.

⁶ For example, Cox-Edwards and Ureta (2003) and Yang (2008) show that migrant remittances increase education spending and contemporaneous outcomes among children in El Salvador and the Philippines. Antman (2012) and Hanson and Woodruff (2003) use different empirical strategies to show that parental migration to the US raises schooling among Mexican girls. In contrast, Antman (2011) and McKenzie and Rapoport (2011) respectively show that parental migration increases work and reduces schooling in the short run among Mexican boys, and provides an option to migrate that reduces secondary schooling among male youth in Mexico.

⁷ From the late 1940s to the early 1960s, the Mexican Farm Labor Program (*Bracero*) allowed millions of guest workers from Mexico to work temporarily, on contract, in US agriculture. From the early 1960s to the 1970s, Germany's *Gastarbeiterprogramm* enabled Turkish workers (mainly men) into Germany on temporary contracts.

⁸ Lucas (1985) and Lucas (1987) are exceptions focusing on cross-country comparisons to get at aggregate effects of this system. Lucas (1985) notes that in the 1970s, 80% of working age males from Lesotho were employed on South African mines, as were 50% of men in Botswana and 15% in Mozambique.

⁹ Malawi has developed a new guest worker program that has drawn significant controversy (Mponda 2013). Labor bans have recently occurred in Nepal <http://www.cnn.com/2012/08/09/world/meast/nepal-migrant-workers/index.html>.

To begin, and as motivation for our empirical strategy, we present the background to mine migration from Malawi and local alternatives to migration. In Section 3, we describe the context of education in Malawi during the pre- and post-colonial period. Section 4 sets out our data while Section 5 presents the empirical strategy and addresses potential threats to validity. Section 6 presents our results and Section 7 concludes.

2. Labor Migration from Malawi: Background and Context

Malawi has a long tradition of cross-border labor migration, largely because it has always had high population densities, a lack of natural resources, few non-agricultural economic opportunities and it is centrally located in southern Africa. A bureaucratic infrastructure for labor exports was established early in the colonial period and our analysis takes advantage of massive and largely unexpected fluctuations in labor export flows and reverse flows of money in the post-colonial period. This section describes the early establishment of the mine labor recruitment system in Malawi and the source of exogenous shocks to labor exports between 1950 and 1990, and then compares the relative importance of earnings from mine work versus domestic earnings opportunities during the same period.

Circular labor migration from Malawi and labor migration shocks

Throughout the twentieth century, Malawians took advantage of employment opportunities in South Africa and Rhodesia (Zimbabwe and Zambia) to boost local incomes. Labor recruitment to both destination countries had become institutionalized by mid-century.¹⁰ The South African mining industry established a physical presence in Colonial Malawi (then Nyasaland), opening and operating a large network of recruiting stations by the 1930s. These stations were run by the Witwatersrand Native Labour Agency (WNLA, or Wenela), the South African Chamber of Mines' centralized labor recruitment organization that coordinated all recruitment activities outside of South Africa (Crush et al. 1991, pg40; Jeeves 1987).¹¹

We have collected and digitized administrative data on the location of Wenela stations as of 1937 and show their prevalence across the country in Figure 1. The thick black borders in the figure represent district boundaries, the red hatched areas indicate sub-districts with a Wenela station and white areas

¹⁰ Although migrant labor from Malawi to Rhodesia was prevalent prior to the 1950s, the scale of migration to Rhodesia was never as large as migration to South Africa post-1967, migrant and remittance flows were never as centralized or organized, and by the early 1960s, most of the jobs in Rhodesian agriculture had dried up.

¹¹ Decisions about where and when to set up recruiting practices in southern Africa in the first half of the 1900s were spearheaded by the mining industry's "labor czar", William Gemmill (Jeeves 1987). Gemmill presided over the expansion of Wenela into colonial Malawi during the 1920s and 1930s at a time when local agriculture was struggling with the Great Depression and the collapse of tobacco prices. He looked towards "Tropical Areas" to recruit labor, knowing that local employers could not compete with mine wages. (McCracken 2012, Jeeves 1987).

show sub-districts without a Wenela station. From this map, it is clear that each of the three main geographical regions (Northern, Central and Southern) had some access to a Wenela station.

The centralized and highly bureaucratic procedure of recruiting foreign labor allowed the mines to maintain a uniform wage across the industry thereby eliminating labor competition within the industry.¹² Nyasaland benefited from this system because Wenela agreements required mines to defer a large fraction of workers' pay and send it directly to Malawi, from where workers could withdraw their wage upon return. By setting up this system of circular labor migration, the government could be certain that a large share of incomes earned abroad were spent in Malawi, and that labor resources would not be forever lost to the country.

Between 1950 and 1990, migrant labor from Malawi to South Africa's gold mines rose from just over 10,000 men per year to a high of 120,000 men per year, and back down to almost zero (see Figure 2). To put these numbers in perspective, by 1977, one in five adult males had ever worked abroad. Most of these mineworkers were engaged on two-year contracts, after which they had to return home for some time before being allowed to reengage for subsequent contracts (Wilson 1972, pg 68, Prothero 1974 and Lucas 1985). Labor migration was therefore highly prevalent (we show below that almost all districts experienced some of this migration), circular, and long-term rather than seasonal.

Until 1967, national labor quotas restricted the number of workers Wenela could recruit from Nyasaland to a few thousand workers per year. European plantation owners in Nyasaland and in Southern Rhodesia (Zimbabwe) lobbied for these restrictions as a way to protect access to a cheap source of labor (Paton 1995, pg 46; Jeeves 1987). Between 1946 and 1959, the Wenela quota increased from 8,500 men to 20,000 men, which represented roughly 2% of the target population of working age males (Coleman 1972, Chirwa, 1992). However, by the 1960s opportunities for employment in Southern Rhodesia began to decline due to circumstances in that country (Clarke 1977, p. 31-32, Paton 1995, p. 47-48) and in 1967, Malawi's President Banda signed a new agreement with Wenela removing all quotas on recruitment of Malawian workers (Treaty Series No. 10/1967). Figure 2 shows the sudden 300% increase in the number of workers recruited by Wenela in the seven years following the abolition of quotas.

Because the 1967 labor agreement required Wenela to withhold two thirds of miner wages until the miners' return to Malawi, the massive increase in migration constituted a large positive income shock to the country. This fact will be important for interpreting our results, because it means we can be sure that

¹² Wilson (1972) and Lucas (1985) provide accounts of how Wenela and the local Native Recruitment Corporation (NRC) operated as labor monopsonists in the colonies, keeping wages low by hiring workers from Mozambique, Northern and Southern Rhodesia, Nyasaland, Lesotho, Swaziland, Tanzania, Angola, Botswana and South Africa.

at least two thirds of earnings returned to origin communities. Moreover, since miners themselves did not choose the value of their deferred pay, we do not need to be as concerned with the usual endogeneity problems associated with migrant motivations about whether to and how much to remit.¹³ To get a sense of the lower bound of how much a returning miner could bring home with them, we apply deferred payment shares to mine wage data and conservatively assume that voluntary remittances are zero.¹⁴ The total amount of income that a family could have received from a miner returning from a two-year contract in 1969 would have been about K276 (Malawian Kwacha), which is two-thirds of two years' worth of mining wages. By 1973, this had increased to K437 and by 1974 this number was K691. We provide a comparison of these deferred pay earnings with local agricultural wages below.

The post-1967 labor expansion came to an abrupt halt in April 1974, when a Wenela plane transporting miners back to Malawi crashed, killing 74 Malawian miners. In response, President Banda rescinded the labor agreement, banned Wenela recruiting, and recalled all Malawian migrant workers (Lucas 1985; Chirwa 1996). Between 1974 and 1977, mine employment fell dramatically (see Figure 2) from a high of over 120,000 men to zero. The initiation of the ban entailed a lump sum payout of deferred pay earnings for all returning miners. Wenela recruiters were able to restart operations in Malawi in 1977, and although there was a small increase in migration after this, employment levels for Malawians never returned to 1970 levels (see Figure 2). The South African mines had turned their strategy of recruitment inwards, substituting local labor for what they saw as unreliable foreign supplies (Crush 1986; Crush et al 1991, pg 129, Mariotti, 2015).

The unanticipated rise in external labor demand from 1967 to 1974 and sharp fall from 1974 to 1977 constitute the two sides of our natural experiment. The periods before 1967 and after 1977 represent relatively stable periods of low labor demand (or periods of slow growth in labor demand) from the foreign mining sector. In the empirical methods section, we define cohorts that are eligible for primary school during the migration shock periods, and cohorts who are either too young or too old to be in school during these shocks.

While mine wages may well have been attractive to Malawian farmers, labor migration was still costly and the complex logistics of getting access to a mining job in South Africa involved many steps.¹⁵ A

¹³ See Docquier and Rapoport (2006) and Yang (2011) for discussions on endogeneity of remittance flows.

¹⁴ We use mine wage data from Wilson (1972, p. 46) and Crush et al. (1991, p. 19). Mine wages increase substantially over the period, driven by increases in the international price of gold. Over the period of this study, K1 was worth about USD0.83.

¹⁵ This section draws on original colonial documents retrieved at Malawi's National Archives, including Governor's Memorandum on Labor Migrancy in Malawi (1956) and Provincial Office Memo (December 7 1961) and from Prothero (1974).

potential migrant needed to obtain official verification of no outstanding tax obligations from the local chief; then he needed to get similar approval from the local tax authority; following which he had to travel to a Wenela recruiting station. At the station, he had to pass a medical examination (mainly regarding a minimum weight requirement) and get ‘attested’ (approved for travel), after which he delivered the attestation documents back to the local district officer for processing of his foreign travel documents. The final step involved returning to the Wenela station to await transportation to a main Wenela depot for transfer to South Africa.¹⁶ The costs associated with signing up for mine work were therefore not negligible, and they varied depending on the distance to the nearest Wenela recruiting station.

In Section 4, we discuss how we use the spatial variation in the number of Wenela stations in a district as a proxy for (lower) labor migration costs that are exogenous to individuals and plausibly exogenous to the district, conditional on a set of baseline control variables. We postpone a more detailed analysis of location choice for Wenela recruiting stations, but note here that there would have been little available hard data to influence these location decisions made in the late 1920s and early 1930s. Our reading of the historical literature suggests that decisions about location placement were made after simple visual inspection of the potential of these areas for recruiting, without much regard to local economic conditions other than the labor supply potential of districts.¹⁷

Alternatives to labor migration: Employment in Malawi

As early as the 1940s and 1950s, the attraction of migrating to work on the South African mines was strong. The domestic economy had always been small, with few perceived opportunities for growth, and despite several attempts to develop large estates producing cash crops for export (first by the colonial government, and then taken over by the post-independence Banda government), wages in the local economy were always far below what workers could earn on the mines.¹⁸

¹⁶ Sources suggest that health and ability of measures we only used to sort workers into jobs, and not used to screen Malawians out of mine work entirely. Health restrictions on migrant workers were minimal and workers were recruited for many types of jobs. Once on the mines, a simple colored shape sorting test sorted workers into mechanical or non-mechanical jobs; a physical examination of eyesight, weight, height and stress-testing further sorted workers into very heavy, heavy or light work (Weyl 1981: p. 16).

¹⁷ Jeeves (1987) describes this process: “As early as 1928, the Chamber (of Mines) began to lay its plans for expansion into the north. In that year, Wenela sent one of its senior employees on tour into the trans-Zambesi. Travelling by auto, rail and ferry, P. Neergaard saw huge areas of Nyasaland, Northern Rhodesia, Tanganyika and northern Mozambique. He returned with glowing reports on the labour potential of these areas and the ease with which Wenela could establish itself in them.”

¹⁸ See Haviland (1953), Green (2007, 2013), McCracken (2012) for a discussion of the development of African and European agriculture in Malawi.

Malawians remaining at home had essentially two, not necessarily mutually exclusive, options for work in the agricultural sector. Workers could work for wages or as visiting tenants on the large tea, tobacco, cotton and sugar estates. Alternatively, they could work in the peasant smallholding sector (Kydd and Christiansen 1982) growing cash crops for export, or food crops to sell to estate laborers and visiting tenants who had no time or land to cultivate their own food.

Under Banda's leadership, the commercial agricultural estate sector grew substantially, underpinning Malawi's 6% annual growth rate from the late 1960s to the late 1970s. Employment on these estates also increased, and since estate wage-workers typically earned more than visiting tenants and smallholders (Chirwa 1997), this sector offered the most lucrative local alternative to mining. Yet, because estate wage earnings were always substantially lower than potential mine earnings (Chirwa 1997), it is not clear how the growth of opportunities for local employment and estate wages would have affected labor migration to South Africa (Weyl 1981, Chirwa 1997). Average annual earnings on local estates were 94.4 Kwacha in 1968, about 70% of what a returning migrant received in 1969. These local estate wages rose to 112 Kwacha in 1973 and to 126.80 Kwacha in 1974, but only represented about 37 % of a migrant's deferred pay in that year (Pryor and Chipeta 1990) due to faster growth in mine wages. Despite the growth of the estate sector immediately after independence, the low wages in this sector, challenges associated with being a visiting tenant, and the decline of the smallholding sector led many men to continue to seek employment with Wenela.¹⁹ The absence of these mineworkers from the Malawian economy, their abrupt return in 1974, and the substantial migrant earnings associated with each of these flows are likely to have impacted investments in education of the next generation through several channels. The next section discusses the background for these human capital investment decisions.

3. Education in Malawi in historical context

Levels of human capital accumulation in Malawi are low, although they have been increasing over time. Prior to independence in 1964, missionaries were responsible for education and emphasized vocational training above literacy.²⁰ Consequently, less than 6% of the population was literate in 1945. At independence in 1964, less than 35% of school-aged children were enrolled in primary school. Between 1959 and 1978 however, total student enrollment in primary school increased by 30% (Heyneman 1980). These increases occurred despite a lack of investment in primary school construction. The number of

¹⁹ Although there is debate about whether Banda wanted to stem the tide of migration to support the growth of the estate sector, the timing of the labor recall initiated by the plane crash, was clearly unexpected. Certainly the South African Chamber of Mines Annual Report of 1973 did not note any concerns with regard to existing labor recruiting practices (Chamber of Mines Annual Report 1973; Paton 1995, pg 54).

²⁰ Frankema (2014) notes that by 1938, the Nyasaland government's contribution to education was 8% of total spending on primary school, or less than two shillings per student.

primary schools did not increase between 1960 and 1992.²¹ By the early 1990s, primary school enrollment had grown to 50% of the relevant age range.²²

Before the mid-1990s, cost was an important constraint to attending school in Malawi. Average annual school fees were the same across the country: around 2.75 Kwacha for the first four years of primary school and 5.75 Kwacha for the next four years. We estimate that the tuition cost of sending three children to school was around 12 % of an agricultural worker's annual wage.²³ On top of tuition, parents were responsible for expenditures like textbooks, exercise books, writing materials and school uniforms (Heyneman 1980). Following the introduction of universal free primary education in 1994, primary school enrollment increased by 50%, indicating that school fees had indeed been a substantial impediment to enrollment even as late as the 1990s (World Bank and UNICEF 2009).

Apart from the direct costs of education, outside options to work and obligations within the family raised the opportunity costs of sending children to school. Children could be involved in home production, work on family farms doing subsistence farming (*mlimi*), or work for other farmers or landlords, including owners of estates producing tea and tobacco for export. Chirwa (1993) provides a few reasons to expect that outside options for child workers might have differed across space in Malawi, particularly along the dimensions of access to agricultural estate work. On agricultural estates that paid wages, children could expect to earn higher wages than in household production or agricultural production on the family plot, where wages were often zero. On agricultural estates with visiting tenant systems, tenant families may have required their children's assistance without pay, to meet landlord-specified production quotas. The shadow value of child labor on estates would therefore have been high, especially when coupled with absentee adult males.

Both the direct and indirect costs of education played a role in the demand for education between 1950 and 1990. Table 1 uses data from Census 1966 to provide some summary statistics on enrollment patterns among children age 10 to 14 just prior to the labor migration shock period.²⁴ Nationally, only 34% of these children were enrolled in school that year. There is interesting variation across districts, though. First, enrollment is significantly higher in Wenela (39%) relative to non-Wenela districts (24%). These pre-existing differences will be controlled for using district fixed effects in our empirical specification.

²¹ There were just over 2,000 primary schools for over 600,000 enrollees in 1974, and only 18 high schools in the country (Malawi Ministry of Education 1977). Data are from Malawi's Ministry of Education Annual Report (1960, p 37 Table 1) and Malawi's Statistical Yearbook (1995, p 57).

²² UNICEF: http://www.childinfo.org/files/ESAR_Malawi.pdf

²³ Average fertility during this period was seven to eight children per woman.

²⁴ The data used to construct this table are from National Census in 1966, and the nationally representative National Sample Survey of Agriculture (NSSA) conducted in 1967/1968. We digitized the data on school enrollment from the Census and child labor from the NSSA at district level for this relevant age group.

Second, school enrollment in 1968 is lower in districts with agricultural estates (Panel B, 27%) than in districts without estates (Panel C, 37%). These patterns are consistent with children having more alternatives to attending school in estate than in non-estate districts.

The means in Table 1 also indicate high prevalence of child labor for this young age group in the late 1960s. Around one in five children aged 10 to 14 were working for someone else or on family farms, with or without pay (Panel A, column 1). Since the definition of work used here does not include home production, these shares underestimate the fraction of children working.²⁵ The patterns of child labor across districts with and without estates are striking. In districts with agricultural estates (Panel B), child labor rates are high and approximately the same across Wenela and non-Wenela areas (18-19%). On the other hand, in districts without estates (Panel C), there is a significant 5 percentage point gap in child labor shares, with lower shares of children working in non-Wenela relative to Wenela districts (16% versus 21%). Before the labor migration shocks of the 1960s and 1970s, child labor was lowest in districts where the opportunity cost of going to school would have been lowest: in non-Wenela, non-estate districts. These differences suggest that we might expect heterogeneity in the long run effects of labor migration shocks among districts with and without agricultural estates.²⁶

4. Data and descriptive statistics

We collect and assemble data from several sources to analyze the impacts of labor migration on long run human capital attainment. We use both district level and individual level census data on human capital attainment and migration flows which we digitize as well as archival records on the location of Wenela stations. We supplement these data with data from various years of the Malawian Statistical Yearbooks. This section outlines key features of our data and highlights some of the variation in migration flows we use in our analysis. The Data Appendix contains further details on sample and variable construction.

Census data are from both the complete 1977 and 1998 population Census⁷ of Malawi and cover 24 districts across all three regions (North, Central and Southern regions). The 1998 Census is comprised of individual level data while the 1977 data are only available at more aggregated district-five year cohort-gender level. We collapse the 1998 data to the same district-cohort-gender groups as in the 1977 data and match these to the 1977 data, restricting the sample to adults ages 20 to 44 (in five-year age groups) in

²⁵ About 11-12% of children report working in home production in the 1977 Census, the first year for which these data exist. There are no significant differences in the prevalence of this home production work across Wenela and non-Wenela districts, or across estate and non-estate districts.

²⁶ Of course, it would be ideal to show what happens to child wages throughout the labor expansion and contraction periods. There are no historical data that we have been able to find that allows us to pursue this more direct strategy. Moreover, since the historical literature suggests that children were commonly employed in home production or on estates without being paid any market wage, it is not clear whether this ideal strategy would be informative.

1998 data. Because life expectancy in Malawi was only 46 years in the late 1990s (http://www.theglobaleconomy.com/Malawi/Life_expectancy/), we are concerned about mortality selection at ages over 45. We therefore use the 1977 Census data to construct a synthetic older cohort of adults, using those aged 20 to 44 in 1977 to represent the 41 to 64 year old cohorts in 1998. In section 5, we describe how we define which cohorts were age-eligible for primary school during the labor expansion years and during the labor ban years.

To proxy for the costs of getting access to mine work in South Africa, we collect administrative data on the location of Wenela recruiting stations prior to 1937. In our analysis, we use the number of Wenela stations in the district (results are similar if we use an indicator for at least one Wenela station in the district). Importantly, there are recruiting stations across the length of the country, so we can make within-region comparisons across cohorts facing higher versus lower migration costs. This allows us to create better counterfactuals by controlling for differences in outcomes that might arise because economic alternatives to mining differ across the Northern, Central and Southern regions.

Because historical Wenela stations provide a good pre-existing measure of differential migration costs, it is useful to get some insight into factors that might have been driving initial placement. Table 2 presents correlations between Wenela stations at the district level and a range of district-level historical and geographic variables. We use the number of Wenela stations in the district or an indicator for any Wenela station in the district as the outcome. There are several interesting correlations in this table. First, log population density measured in 1931 is negatively correlated with Wenela stations at the district level, although not significantly so in most cases. This could be because mine recruiters were unwilling to compete for male labor in areas where agricultural opportunities offered good outside options (i.e. where there were initially high densities of population on fertile land). Second, districts at higher altitude (i.e. more likely to be free of malaria), districts without estates, and districts in the Central region are more likely to have a Wenela station, although these relationships are seldom statistically significant. Notably, the level of historical literacy within a district does not appear to be a predictor of the location of a Wenela station. The implication is that the mining industry did not place additional value on local levels of human capital. Once region fixed effects are included, the message from Table 2 is that there is very little correlation between historical and geographic variables and the historical placement of the Wenela stations.

Our identification strategy rests on comparing outcomes across different cohorts from locations facing substantially different costs of signing up for mine work, proxied by the presence of a local Wenela station. Table 3 provides direct evidence that the number of Wenela stations in a district predicts

substantially higher labor outmigration in response to exogenous shocks to labor demand between 1966 and 1977. The first four columns show results from regressions of the change in the number of men working abroad between 1966 and 1977 on the number of Wenela stations in the district, region fixed effects, historical literacy and population density, an indicator for the presence of an agricultural estate in the district, and the interaction of estate districts with Wenela districts. The outcome is the difference between the number of men reporting that they returned from working abroad in the 10 years before 1977, reported at district-level in the 1977 Census, and the district-level stock of male migrants captured in the 1966 Census. Since the labor ban was still in place at the time of the 1977 Census, this variable gives us a complete picture of migration at the district level between 1966 and 1977. Exposure to one additional Wenela station in the district significantly raises the number of labor migrants by 1,532 men. Relative to the average change in migrants over the period (3,445) (Table 3, column 4), this is a 44% increase in migration for each additional station. This is a very large effect: relative to the initial mean stock of migrants in a district in 1966 (about 10,000), one additional recruiting station raises migration by 15 percentage points. Note that education levels within a district as measured by the local literacy rate in 1945 are not significant predictors of outmigration. If anything, higher historical literacy rates suggest less migration.

The second set of columns confirms this relationship for a modified outcome, measuring the migrant growth rate between 1966 and 1977. The migrant growth rate takes into account initial migration levels in 1966, and we see that the mean rate of migrant growth across all districts over these years is 30%. Relative to this mean, a district with one more recruiting station has 20 percentage points faster migration growth than the average district (Table 3, column 8), or an overall migrant growth rate of almost 50%. These are very large shocks to migration occurring over a very short period of time. In the next section, we explain how we use this spatial variation in access to mine work to distinguish between districts with high versus low exposure to the 1967 labor treaty and the 1974 labor ban.

Table 4 presents summary statistics for outcome and control variables at the district-cohort-gender level in Panel A and for historical and geographic variables at the district level in Panel B. To construct most of the variables in Panel B, we digitized historical Census data from 1931, 1945, 1966 and 1977. We present means for the full sample and for districts with and without a Wenela recruiting station along with the p value of the difference in means. Average population in a district is about 225,000.

Panel A shows that 63% of districts have at least one Wenela station and the average number of stations per district is 2.79. The fraction of cohorts eligible for primary school in either period, and in each of the

younger and older cohorts (these definitions will be described in more detail below) is balanced across Wenela and non-Wenela areas.

The middle part of Panel A gives an indication of the very low levels of education among adults in our sample. Average schooling among the adult sample (aged 20 to 64) is 2.56 years. Average education is 2.85 years in Wenela areas and only 2.07 years in non-Wenela areas; the share of adults who have ever been to primary school is 45% in Wenela areas and only 35% in non-Wenela areas. Both of these differences are strongly significantly different than zero and echo the differences in child enrollment patterns seen in Table 1.

Although migration was widespread across the country, the incidence of the migration shocks between 1967 and 1977 was concentrated in Wenela districts. Panel B of Table 4 shows that high shares of working age men report ever working abroad by 1977 (20%) in both Wenela and non-Wenela districts. However, the change in the number of men working as migrants is much higher in Wenela districts (5,060) relative to non-Wenela districts (752), and the migrant growth rate in WENELA areas is 46%, relative to a migrant growth rate of just 3% in non-Wenela areas between 1966 and 1977 (p value of this difference is 0.08). The final part of the table shows no statistically significant differences in local labor market opportunities across Wenela and non-Wenela areas just before the labor treaty was signed in 1967. Just over 60% of men were engaged in some form of wage work in both areas, and almost 40% were not earning any wage. Of course, our analysis will account for all pre-existing differences between Wenela and non-Wenela districts by using district fixed effects.

5. Empirical Strategy

Our aim is to estimate the long run impacts of migration on the intensive margin of the next generations' educational attainment (total years of schooling attained) as well as on the extensive margin (any primary school attained). These are relevant margins of adjustment for Malawi during this period, when there were only 50 government secondary schools across the country. Furthermore, focusing on primary education is important since enough education for functional literacy (around four years) is deemed to be necessary to have positive returns in agriculture (Foster and Rosenzweig 1996; Appleton 2000).

Our identification strategy exploits variation in childhood exposure to mine labor migration shocks induced by external labor shocks interacting with district-specific migration costs. Migration costs are proxied for by access to Wenela stations as shown in Figure 1. Figure 3 illustrates the variation in childhood exposure to the external labor shocks by defining eligibility for primary school enrollment across cohorts. We construct five-year birth cohorts for adults who are aged 20 to 64 in 1998 and have

therefore completed whatever primary schooling they are likely to get. We divide these cohorts into four broad groups to define pre-treatment, treatment and post-treatment cohorts.

The oldest cohorts born between 1933 and 1953 are too old for primary school by the start of the labor shock period, 1967. These cohorts range in age from 14 to 34 in 1967, and constitute our natural pre-treatment (control) group. The two middle groups in Figure 3 are the two cohorts eligible for primary school at some point during the labor migration shock. The group born between 1954 and 1963 is eligible for primary school during the early labor expansion period: they are aged 4 to 13 in 1967. The group born between 1964 and 1973 is eligible for primary school during the labor ban period, as they are all under the age of 10 in 1974. Finally, the youngest adult cohorts who are born between 1974 and 1978 are too young to be eligible for primary school between 1967 and 1977: the eldest in this cohort is three years old in 1977. Although these youngest cohorts are ineligible for schooling during the time of the labor migration shock, they may experience lingering effects of the shock, which is why we refer to this group as the post-treatment cohort, rather than a control cohort.

Defining eligibility using these five-year bins introduces some fuzziness into the treatment assignment. Although the pre-treatment cohort was clearly not eligible for primary school during the 1967 to 1977 period, not everyone who was eligible was exposed for the same length of time during the labor shock years. For example, the oldest individuals in the early treatment cohorts would have spent less time exposed to the labor expansion shock than the youngest in the same cohort. The coarse definition of cohort eligibility, necessitated by the form in which data are available in the 1977 Census, likely attenuates our estimates of the effects of migration on education, and we discuss this further in presenting our results.

Our main empirical model is

$$\begin{aligned} \bar{Y}_{asd} = & \beta_0 + \beta_1 \text{EarlyTreatment67-73}_{as} * \text{Wenela}_d + \beta_2 \text{LateTreatment74-77}_{as} * \text{Wenela}_d \\ & + \beta_3 \text{Post-Treatment}_{as} * \text{Wenela}_d + \beta_4 \text{Wenela}_d + X_{as}' \gamma + G_d' \pi + \lambda_d + \varepsilon_{asd} \quad (1) \end{aligned}$$

where \bar{Y}_{asd} represents the average years of schooling attained or the share of adults with any primary school by district-cohort-gender cell, $\text{EarlyTreatment67-73}_{as}$, $\text{LateTreatment74-77}_{as}$ and $\text{Post-Treatment}_{as}$ are binary variables denoting age-eligibility for primary school for males and females during each period of the labor shock years 1967 to 1977 and in the immediate post-treatment period as illustrated in Figure 3, and Wenela_d is a count variable of the number of historical recruiting stations in the district. X_{as} is a set of demographic controls including a complete set of five-year age cohort dummies and a female indicator. G_d contains district-specific historical variables that might influence the level and the changes in

education over time: the log of historical population density measured in 1931 and the share of literate adults in 1945. λ_d is a district fixed effect. In our most comprehensive specifications, we also include controls for a linear trend term interacted with two region fixed effects, and with historical district-level population density and historical district-level literacy rates.

There are three main parameters of interest in equation (1). β_1 and β_2 capture the difference in education gaps between age eligible and older pre-treatment cohorts in Wenela versus non-Wenela districts, or specifically, between districts with more versus fewer Wenela stations. β_1 estimates the education gap for cohorts eligible to be in school between 1967 and 1973 across districts with more versus fewer Wenela stations, controlling for this gap among the older age ineligible cohorts. β_2 estimates the education gap for cohorts eligible to be in school in the later period, between 1974 and 1977, across districts with more versus fewer Wenela stations, controlling for this gap among age ineligible older cohorts. Differences between β_1 and β_2 tell us about the relative education impacts of the labor expansion shock versus the labor ban. Additionally, β_3 tells us whether there is any persistence in the effects of the labor migration shocks among the youngest post-treatment cohorts.

Identification of these difference-in-differences parameters relies on exploiting district-level variation in the costs of accessing mine jobs proxied by the presence of historical Wenela recruiting stations combined with within-district level variation in age eligibility of different cohorts. In most studies of migration, drawing causal conclusions from comparisons of migrant and non-migrant families or individuals is difficult because the decision to become a migrant is seldom taken randomly. Differences in educational outcomes of children of migrants cannot necessarily be attributed to the impact of migration, if migrants take more risks, are poorer or more able, or simply more interested in investing in human capital. There are several reasons why, in our setting, these sources of selection bias are less of a concern.

First, we measure outcomes at the district level and include a host of controls in equation (1) that limit concerns about potential confounding of results from underlying unobservable or observable differences between districts, from short, sharp, differential shocks to the local economy, and from differential trends across districts.²⁷ The trend interaction terms also address concerns about mean reversion in education across districts. Second, we use the older pre-treatment cohorts to control for counterfactual differences in educational attainment across Wenela and non-Wenela districts. Parallel education pre-trends in this older comparison group support our identification assumption that, conditional on controls, non-Wenela

²⁷ For example, the Northern Region was the birthplace of formal schooling in Malawi (Heyneman 1980), originally established by missionaries. Including region-specific trends helps us to account for any differences in education trends across regions that stem from different initial conditions.

districts provide a valid counterfactual for Wenela districts. Third, migration to the mines was highly prevalent and there were migrants in every district and across all age groups. Differences across districts in migrant flows during the labor shock years had less to do with the characteristics of people who wanted to migrate, and more to do with how easy it was to get recruits from areas with pre-existing Wenela stations. Fourth, Wenela imposed few selection criteria on mine recruits, beyond requiring a physical fitness test, making it very unlikely that migrant selection on cognitive ability drives our results. Finally, our identification strategy relies on assumptions of no differential changes in school supply across Wenela districts: all of the effect comes from the changes in the demand-side. Since there was no massive school expansion program established in the post-independence period (see the discussion in Section 3), it is unlikely that changes in schooling access could confound the interpretation of our results.

One potential threat to validity of our results arises because proximity to Wenela stations in childhood (and hence $Wenela_i$) could be mismeasured. Our data do not contain information on birth district at the individual level, so we cannot be sure that a person's current district of residence is the district in which she went to school. This may pose problems if internal migrants have substantially more, or less, education than the average level of education of their origin and destination districts. To address this concern, we use information from the 1977 Census on the district, age and gender-specific prevalence of internal migration between 1967 and 1977 to create bounds for our education estimates in equation (1). In Appendix 2, we discuss how we implement this bounds analysis and we show that our results are robust to accounting for the two extreme types of composition effects driven by internal migration.

Conditional on our identification assumptions, β_1 and β_2 capture the causal effect of exposure to the migration expansion and subsequent contraction on long-run average educational attainment. We treat the expansion and contraction separately because they entail a different combination of shocks to the local district economy. In the earlier period between 1967 and 1974, men migrate to South Africa in increasing numbers, send back money in the form of remittances and collect cash via deferred payments when they circle back to Malawi after their two-year contracts. If children are required to substitute for missing male labor during the labor expansion, and if this effect is larger than the positive income effect (IE) from increased migrant earnings, we might see $\beta_1 < 0$. Alternatively, $\beta_1 > 0$ implies that the positive IE on educational investments dominates any crowd out of education during the labor expansion years. Note that any income effects are driven by miners responding to some combination of shocks to transitory and permanent income: we cannot know whether miners viewed the labor expansion as signaling permanently new opportunities to work in South Africa or not.

In the later period between April 1974 and 1977, migrants return to rural Malawi, almost overnight and bring home accumulated deferred pay and savings. With returning male migrants, children would no longer need to substitute for missing male labor. Given the size of the earnings payouts to returning migrants, the labor ban also implied a large positive shock to current income, which should serve to increase contemporaneous investments in education, as long as schooling is a normal good. However, if migrants viewed the labor ban as a negative shock to permanent income, we might see lower educational attainment among cohorts exposed to this labor contraction. Since there is no substitution effect in operation during the later period, $\beta_2 > 0$ implies that the positive transitory shock to income dominates any potentially negative impact through a reduction in permanent income. The reverse is true if $\beta_2 < 0$. Since remittance income was likely exhausted by the late 1970s and migrant labor flows never recovered to pre-1974 levels (see Figure 2), a finding of $\beta_3 > 0$ is consistent with at least some of the overall education response to the combined labor migration shocks being a response to a net positive permanent income shock.

To translate our reduced form estimates of the causal effect of exposure to migration shocks into human capital effects generated by the change in the actual number of migrant workers in each district, we pursue an instrumental variables (IV) strategy and scale our results using the following regression:

$$\begin{aligned} \bar{Y}_{asd} = & \alpha_0 + \alpha_1 \text{EarlyTreatment}_{67-73,as} * \Delta \text{Migrants}_d + \alpha_2 \text{LateTreatment}_{74-77,as} * \Delta \text{Migrants}_d \\ & + \alpha_3 \text{Post-Treatment}_{as} * \Delta \text{Migrants}_d + X_{as}' \phi + G_d' \kappa + \tau_d + \omega_{asd} \quad (2) \end{aligned}$$

To define the main treatment variables, we interact each of the cohort eligibility dummies with the change in the number of men who report working abroad between 1966 and 1977 (the same outcome used in the first four columns of Table 3: $\Delta \text{Migrants}_d$). We instrument for these interaction terms using $\text{EarlyTreatment}_{67-73,as} * \text{Wenela}_d$, $\text{LateTreatment}_{73-74,as} * \text{Wenela}_d$ and $\text{Post-Treatment}_{as} * \text{Wenela}_d$. All other controls are the same. The exclusion restriction is that, conditional on controls, the interaction of the district-level Wenela_d and eligibility dummies only predicts the interaction of the migrant flow variable with treatment dummies. Note that because the difference-in-differences design already provides an estimate of the causal effects of exposure to these migration shocks, the instrumental variables strategy is merely a re-scaling exercise. Estimates of α_1 , α_2 and α_3 evaluated at the sample mean of $\Delta \text{Migrants}_d$ tell us the following: at mean levels of migration during the shock years, how much more education is gained by each of the treatment cohorts, controlling for differences in education outcomes across high and low migrant shock districts using the older ineligible cohorts.

6. Empirical Results

Table 5 presents our main difference-in-differences results for long run effects of exposure to circular labor migration on the next generations' human capital attainment. The first three columns present results for years of education attained and the next three columns show results for the share of adults with any primary schooling. For each outcome, we first show results from the difference-in-differences comparisons including covariate controls that are fixed within a district-gender-cohort cell (female, age group dummies, region fixed effects, the log of historical population density in 1931 and the share of the adult population literate in 1931). We then include district fixed effects in a second specification, and finally a set of trend interaction terms (trends interacted with region dummies, with baseline population density and with baseline literacy rates) in our preferred specification. Robust standard errors are clustered at the district level. Since there are only 24 districts, we report significance levels throughout using the small sample t distribution.²⁸ The significance of our results is also robust to using randomization inference procedures to construct exact p values (see Appendix 3 for a description and results of this procedure). The last three lines of Table 5 present p values for tests of equality between each of the difference-in-differences parameters.

In the first specification, we can see that relative to the older pre-treatment cohorts, all of the post-1967 cohorts have higher educational attainment and are more likely to have ever been in primary school in Wenela compared with non-Wenela districts. These effects are large and statistically significant, and follow an inverted U-shaped pattern. For each additional Wenela station, cohorts eligible during the labor expansion gain 0.13 more years of education and are 1.4 percentage points more likely to have ever been to school (Table 5 columns 1 and 4). For each additional Wenela station, cohorts eligible during the labor contraction have 0.2 more years of education and are 2 percentage points more likely to have ever been to school. Among the youngest, post-treatment cohorts, for each additional Wenela station, education is 0.17 years higher and enrollment is 1.6 percentage points higher. The two sides of the migration shock appear to have had different impacts on educational attainment and access to primary schooling, with the largest impacts felt by cohorts in Wenela districts who were eligible for schooling during the labor ban period. In addition, the effects of the shock lingered as is evidenced by the higher educational attainment among the post-treatment cohorts.²⁹

²⁸ Relevant p values are taken from the t -distribution with degrees of freedom ranging from 23 (when there are no other controls in the regression) to 6 (when all controls are included and the sample is restricted to estate districts).

²⁹ In supplementary analysis (results available upon request) we see a continued decline in positive education impacts among even younger cohorts (ages 15-19 in 1998), suggesting a pattern of persistence in the shock that wears off over time.

Adding in district fixed effects increases standard errors a little (Table 5 columns 2 and 5) and adding in trend interaction terms reduces the size of the education impacts somewhat. However, even after including all of the controls in the regression, the impacts of exposure to labor migration shocks remain positive, robust, and significant. For each additional Wenela station in the district, cohorts eligible for school during the labor expansion and labor ban periods gain an additional 0.12 to 0.18 years of education and are between 1.1 and 1.6 percentage points more likely to have ever been in primary school (Table 5 columns 3 and 6). These estimates represent a 4.8 to 6.9% gain in total years of education and a 2.6 to 3.9% gain in the share with any primary school. Relative to the older pre-treatment cohorts, the youngest post-treatment cohorts in Wenela districts gain 0.14 more years of education (a statistically significant result) and access to any primary schooling is higher by 1 percentage point (not significant). Again, the pattern of coefficients suggests that the labor ban cohorts experienced the largest impacts of the labor migration shocks. These cohorts would have been eligible for school during the years in which miners were forced to return to Malawi and receive their lump-sum remittance and deferred pay payouts.

The last part of the table tests for the equality of outcomes across the three difference-in-differences parameters. The results of these tests are consistent with the pattern of coefficients. Regardless of the specification, we can reject the null that the effects of the migration shock are the same among the labor ban cohorts and each of the labor expansion and post-treatment cohorts at the 1%, 5% or 10% level. The labor ban cohorts in Wenela districts gain significantly more education than cohorts exposed to the labor expansion and cohorts eligible for schooling after the end of the labor ban, controlling for educational gains of similar cohorts in non-Wenela districts.

The fact that we cannot reject the null that the labor expansion cohorts and the immediate post-treatment cohorts experience the same impacts on education is most likely a function of the fuzziness of our definition of which cohorts are eligible for primary schooling in which years. We can only use five-year age cohorts in the 1977 Census, meaning that some of the oldest individuals in the labor expansion cohorts may not have been eligible for primary schooling. Because of this, we could be underestimating the average impact on education among these labor expansion cohorts in Wenela areas. At the same time, it is not uncommon to see the effects of an economic shock persist for a time after the shock subsides. Various reasons in the literature exist for why the effects of these labor migration shocks might persist in the youngest post-treatment cohorts. For example: increased nutrition in childhood could have increased the returns to investing in the youngest children (Hoddinott et al 2011), while increases in savings facilitated by remittance income could continue to relax credit constraints and finance schooling beyond the end of the labor ban years (Ashraf, Aycinena, Martinez and Yang, forthcoming).

To visualize our main finding, we use the full range of age groups across the 1977 and 1998 Census samples. In Figures 4 and 5, we show how differential exposure to the mining employment shocks enabled cohorts in Wenela districts to overtake exposed cohorts in non-Wenela districts, permanently altering the long run level of human capital of specific cohorts in Wenela districts, and how these effects start to fade in the post-treatment cohorts.

Each of the figures presents difference-in-differences coefficients from a regression of the relevant educational outcome on nine age dummies – one for each of the five year cohorts between ages 20 and 64 – and the interaction of these dummies with the number of Wenela stations in the district. In these regressions, we control for the full set of covariates from the most comprehensive specifications of Table 5, including controls for trend interactions. Each point along the solid black line is a coefficient on one of the age dummy terms interacted with the number of Wenela stations and the omitted category is the oldest cohort, age 60 to 64 in 1998. The black line therefore represents the gap in educational attainment (in Figure 4) or in primary school access (in Figure 5) across Wenela and non-Wenela districts, for each of the five-year age cohorts. The dotted lines represent the 95% confidence intervals for these coefficients. The older pre-treatment cohorts include those aged 45 to 60 in 1998 (plus the omitted group); the labor expansion cohorts are those aged 35 to 44 in 1998; the labor contraction cohorts are aged 25 to 34 in 1998; and the youngest post-treatment cohorts are aged 20 to 24 in 1998. Note that the estimated coefficients for each of these five-year age cohorts contribute to the weighted average effect estimated in Table 5. For example, among the labor ban cohorts, the estimated gap in educational attainment among Wenela and non-Wenela districts is about 0.17 in Figure 4, and is 0.179 in Table 5 (column 3).

Several features of these figures stand out. Looking at Figure 4, we see that the difference in educational attainment between Wenela and non-Wenela districts was essentially constant and zero among all of the pre-treatment cohorts. This parallel trend among the oldest control groups is reassuring, as it gives us no indication that Wenela districts were improving before those children eligible during the labor expansion period were able to be in school. In Figure 5, we see that Wenela districts actually had declining entry into primary school among the oldest control cohorts, relative to non-Wenela districts. The second feature to note about each of these figures is that educational attainment and access to primary schooling jumps up, starting with the 40 to 44 year old cohort which would have comprised children eligible for schooling at the start of the labor expansion period. Differences in educational attainment and access to primary schooling remain high, positive and statistically different than zero for all younger cohorts down to those aged 25 to 29 in 1998; this cohort would have been eligible for schooling at the very end of the labor contraction period. Finally, although we still see a positive difference in educational attainment and primary school access between Wenela and non-Wenela districts among the youngest, post-treatment

cohorts, the coefficients are smaller than during the labor ban years. This indicates that the effects of the migration shock are beginning to wear off in these cohorts.

Overall, both figures show that in the space of only ten years, exposure to mining employment shocks and concomitant migrant remittances enabled Wenela districts to overtake non-Wenela districts in their total amount of human capital, with long-lasting effect. Two decades after mine labor opportunities disappeared in Malawi, districts that had been most exposed to the labor shocks continued to have higher stocks of human capital among cohorts most exposed to these shocks. Adjustments occurred on both extensive and intensive margins: kids going to school stayed in school for longer, and there were more kids going to school overall, as indicated by the increase in share of adults with any primary schooling. These positive primary school enrollment effects account for between 7 and 10% of the total increase in enrollment rates between 1967 and 1978.³⁰

Whether this accumulation of human capital ultimately translated into growth effects at district level is a natural next question which we leave for future research. However, to show that there is likely some positive return to human capital in Malawi (that is, the quality of schooling is not so low that it is a worthless investment), we examine the cross sectional correlation between education and a measure of household well-being. Using recent data, we show in Appendix 4 that households with more educated heads accumulate significantly more assets than those with lower levels of education, regardless of the measure of education used and controlling for a host of demographics, household characteristics, and district fixed effects. While usual concerns about selection and measurement error caution us against interpreting the point estimates as exactly causal, the strong positive education gradient in household assets suggests that human capital formation and skills learned at school are indeed important for household well-being in Malawi.³¹ The long run effects of labor migration on human capital formation that we find in Table 5 likely had positive impacts on welfare in the most exposed districts and in the adult households of the most exposed cohorts.

Discussion of magnitudes

To get a better sense of how many additional migrants left Malawi during the labor expansion years, and how these numbers translate into education impacts, we can scale the difference-in-differences results using estimates from Table 3. From Table 3, we know how the presence of a recruiting station induced

³⁰ Heyneman (1980, Table 3) provides national enrollment numbers for Malawi in 1967 and 1978. We use Census 1977 data (Table 1, Population counts) to construct the total number of children ages 5 to 19 inclusive in each of the 1966 and 1977 Census years. We estimate the primary school enrollment rate in 1967 was approximately 20%, rising to about 35% in 1978.

³¹ Our results are consistent with findings in Chirwa and Matita (2009), who use Mincerian regressions to show that among wage earners working in urban areas of Malawi, the return to completed primary education is 5 percent.

migration flows out of districts between 1966 and 1977. Without any other controls, each Wenela station induces an additional 1,138 more labor migrants. From Table 5, we know that for each additional Wenela station, eligible cohorts have between 0.13 and 0.2 years more schooling, excluding other controls (column 1). Combining these pieces of information, we can construct an estimate of the impact of labor migration on education. At the mean level of labor migration shock in a district (3,445), total educational attainment increases by between 0.4 years ($0.13/1,138*3,445$) and 0.6 years ($0.2/1,138*3,445$).³²

Table 6 does this re-scaling more formally, using the instrumental variables strategy in equation (2) that controls for other potential confounders in a regression framework. For each outcome, we present IV results including all demographic and district level controls, and then add results including region-specific linear trends and trend terms interacted with baseline population density and literacy rates. First stage regression estimates appear in Appendix Table 1. Coefficients are presented for the mean district-level migration shock (3,445 additional men). Once all controls are included, we estimate that the mean level of migration in a district increases human capital formation among eligible cohorts by between 0.34 and 0.48 years (column 2), and these cohorts are between 3.2 and 4.3 percentage points more likely to have ever been to primary school (column 4). Compared with average levels of education and enrollment, these effects translate into an increase in education of between 13 and 18% for the mean migration shock in a district, and an increase in any primary schooling of between 7 and 10%. These results are significant at the 1% or 5% level.³³

Both the reduced form estimates of the impact of labor migration in Table 5 as well as the IV scaled results in Table 6 tell us how differential growth in migrant flows due to recruiting station placement affected the education profile of cohorts across the entire district. These estimates include any spillovers of labor migration to non-migrant households. There are reasons to expect that the return of remittances and receipt of deferred pay in lump sums several times larger than annual earnings would likely have had direct impacts on the local economy outside of migrant households. For example, families sharing resources within an extended family network could generate positive impacts for children of non-migrant households. Spending of this additional income could have generated local multiplier effects. And, general equilibrium impacts on wages of left behind workers represents another potential source of spillovers captured in our estimates. Theoharides (2014) is the only other paper we are aware of that takes into account potential spillovers from migrant to non-migrant households by estimating the impacts of labor migration demand shocks at the local labor market level. She estimates that the elasticity of high

³² This is similar to, although not exactly, the Wald estimate. The unit of observation in Table 3 is the district, while in Table 5 it is the district-gender-cohort group.

³³ These results are quantitatively similar using migrant growth rates as the main treatment variable.

school enrollment with respect to average migrant labor demand shocks in the Philippines (0.17). Our estimates of the elasticity of primary school enrollment are substantially lower at 0.07 to 0.1 (using the IV estimates in Table 6 and dividing by the average value of the education outcome: $0.032/0.41$ and $0.043/0.41$).³⁴

It is additionally useful to scale our estimates a third way, to compare with estimates of elasticity of school enrollment to migrant income. Yang (2008) reports what is arguably the most well-identified estimate of the elasticity of school spending with respect to remittance income within migrant households in the Philippines (0.55). We use his estimates to turn this elasticity into an elasticity of school enrollment with respect to remittance income: 0.44. For comparison, we compute a back-of-the-envelope estimate of the district-level income elasticity of enrollment in primary school in Malawi. The income elasticity of education is given by the percent change in enrollment divided by the percent change in migrant income at the district level. The percent change in income at district level is driven by the percent change in migrants multiplied by the percent change in income that these migrants bring home. The IV estimates evaluated at the mean change in migration during the period and divided by average enrollment gives us the percent change in enrollment for a percent increase in migrants. If we conservatively assume that every returning migrant brings home at least two thirds of his mine earnings income through the deferred pay system, this is equivalent to three times his annual income from estate work in 1974. Putting these two estimates together, we calculate an income elasticity of enrollment of 0.039 ($(0.032/0.41)/2$) during the labor expansion years and a higher elasticity of 0.052 during the labor contraction years ($(0.043/0.41)/2$).³⁵ The greater we assume the return of earnings was for migrant workers (i.e. the more voluntary remittances the migrants brought with them), the smaller these income elasticities become. Nevertheless, this elasticity is still far below the most credible estimate in the literature. One reason for these differences could be the very different context of outside options for child work in Malawi. In our final section, we discuss our evidence on how these outside options may have muted the effects of labor migration on investments in human capital of the next generation.

Exploring Mechanisms

For both education outcomes, we reject the null that β_1 and β_2 are the same at the 1% or 5% levels (p values range from 0.01 to 0.03 in Table 5). Relative to both the early treatment cohorts and the post-

³⁴ Theoharides (2014) estimates that for the average change in migrant labor demand (0.12 percentage points), enrollment rises by 1.2 percentage points, from a base of 56.8 percentage points. This gives an elasticity of 0.17 ($(1.2/56.8)/0.12$).

³⁵ Estimating a similar elasticity for the post-treatment cohorts is difficult, since it requires us to know how much income was saved out of remittances to be used for these later cohorts.

treatment cohorts, education impacts are always statistically larger for cohorts' eligible during the labor contraction period, when men have returned home and immediately after they collect their lump sum payouts. There are two not mutually exclusive interpretations for these differences. One possibility is that during the labor ban period, children no longer need to substitute for missing male labor, but the income effect still operates since men return home with deferred earnings and savings. Under this interpretation, we could use the difference between our estimates of β_1 and β_2 to back out the size of the substitution effect when labor migration is expanding. An alternative interpretation is that the total amount of income returning to Malawi in the expansion period is smaller than that returning in the contraction period, generating smaller initial impacts on education outcomes. Although total remittances and deferred payments from miners are roughly the same in the labor expansion and contraction periods (ZAR46 million in the early period and ZAR 50 million in the later period, Malawi Statistical Yearbook 1975), this translates into smaller annual flows between 1967 and 1973. Without district-disaggregated data on remittance flows per year, we cannot rule out that less total income per year is returning to each district in Malawi during the labor expansion period. For this reason, we do not calculate a numeric value for the size of the substitution effect present in the labor expansion period.

However, we offer two additional pieces of evidence to support the idea that there was indeed a substitution effect operating in the early labor expansion period, which was not present in the later labor ban period. First, we examine how our difference-in-differences estimates from Table 5 vary across districts with more versus fewer outside opportunities for child labor. Specifically, we look for treatment effect heterogeneity across districts with and without agricultural estates. Second, we ask directly whether there are contemporaneous impacts of the labor migration shock on the prevalence of child labor that differs across Wenela and non-Wenela districts, using data from 1968 and 1977 that bracket the massive shocks to migration. Because splitting the sample into estate and non-estate districts generates fairly small subsamples for testing, these results are necessarily suggestive.

As Section 3 outlines, children in rural Malawi may have worked in the home, on the family farm, or on estates. Especially in the case of estates, where tenant farmers were required to satisfy annual quotas of output to protect their land rights, the value of additional child labor would have been higher than in areas without estates throughout the twentieth century. And, since the estate sector was booming in the late 1960s and early 1970s, the value of child labor in these districts would have been even higher during the time of the two migration shocks. Thus, we expect the effects of labor migration on human capital to depend on these differences in local agricultural production technology.

Table 7 shows just these relationships. We estimate versions of equation (1) using the district level dataset from the combined 1977 and 1998 Census. We split the sample into districts with and without large tea, tobacco, sugar or cotton estates.³⁶ Although all areas experience the positive impacts of the migration shocks on educational attainment, the largest effects are seen among eligible cohorts in high exposure (Wenela) districts without large agricultural estates.³⁷ In these districts, cohorts eligible for primary school between 1967 and 1977 have between 0.14 and 0.21 more years of education and are between 1.4 and 2.1 percentage points more likely to be in school. In contrast, point estimates for the sample of estate districts are almost always smaller than for the non-estate sample. We do not have sufficient power to statistically distinguish between these coefficients across estate and non-estate subsamples in a pooled regression, which is why these results are more suggestive than conclusive.

Table 8 presents more direct evidence on the contemporaneous effects of these labor migration shocks on the prevalence of child labor. We regress the share of children aged 10 to 14 years working in 1968 or in 1977 on a 1977 year dummy, the number of Wenela stations in the district, the interaction of the 1977 dummy with number of stations, district fixed effects, and a range of demographic characteristics. These regressions allow us to ask whether on aggregate, child labor among children aged 10 to 14 changed differentially across districts with and without Wenela stations between 1968 and 1977. The results indicate that they do. For the full sample, districts with Wenela stations experienced a 1 percentage point decline in child labor among children aged 10 to 14 relative to districts without Wenela stations (Table 8 column 1). This decline only happens in the non-estate districts, where child labor declined by 1.3 percentage points. Among estate districts, the picture is very different. The share of children working rose by almost 20 percentage points in Wenela districts relative to non-Wenela districts. These effects make sense in the wake of the massive migration shocks of the 1960s and 1970s, and simultaneous growth in the estate sector.

Putting the results of Tables 7 and 8 together suggests that the local technology of production influenced the way in which circular labor migration affected families left behind. Exposure to mine employment shocks affected long run educational attainment differently in estate and non-estate districts. At the same time, exposure to these labor migration shocks differentially raised child labor in Wenela relative to non-Wenela districts, but only within estate areas. In non-estate areas, exposure to the labor migration shocks lowered rates of child labor over the 10-year period. We interpret this as evidence that where child labor

³⁶ See the Data Appendix for a discussion of how we define estate districts. Because of the coarseness of this measure, there is uncaptured variation within estate districts in the prevalence of estate lands out of total district agricultural lands.

³⁷ Recall from Table 3 that migration flows between 1967 and 1977 are not significantly different across estate and non-estate districts.

was less prevalent and arguably less valuable, (i.e. in non-estate districts), circular labor migration and concomitant remittance flows substantially increased investments in schooling. In estate districts, where child labor was a potentially valuable input into production, and even possibly a substitute for missing male labor, the effects of this migration on education of treated cohorts were muted.

7. Conclusions

Circular migration has long been considered one of the most immediate ways out of poverty for families, and potentially for whole sending communities. Despite its prevalence in most African labor markets, there is scant empirical evidence on this type of migration due to challenging identification concerns and data limitations. Our paper uses newly collected administrative data on exogenously determined access to migration opportunities and two waves of newly digitized Census data from 1977 and 1998 to provide direct evidence on one of the channels through which this migration can positively affect the lives of those left behind.

We show that the massive and unanticipated expansion and contraction of employment of Malawian men on South African mines over a ten-year period had lasting effects on human capital formation in rural communities of origin. This new evidence from Africa shows that the positive income effects of labor migration on the demand for education outweigh any potential negative substitution effects of this migration on the demand for child time in the labor market. However, there is some indication these long run impacts of labor migration on human capital accumulation of the next generation are muted, the higher the shadow value of child work is in a region.

These findings have broad relevance. The increase in educational attainment between 1967 and 1977 reflected in long run adult education outcomes twenty years later shows the extent to which a colonial era institution continues to play a role in Malawi's economy, long after independence and the end of formal recruiting. These lessons are relevant for many countries with historical, bilateral guest worker programs. Moreover, many modern guest worker programs share similar features of the Malawian migration experience, including limited-time work contracts and in-built circular migration. The evidence in our paper suggests that there is scope for these programs to have positive, long-lasting impacts on human capital formation in communities of origin, helping them to lay the foundations for economic growth by investing in the education of the next generation.

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Figure 1 The spatial distribution of Wenela mine recruiting stations across Malawi

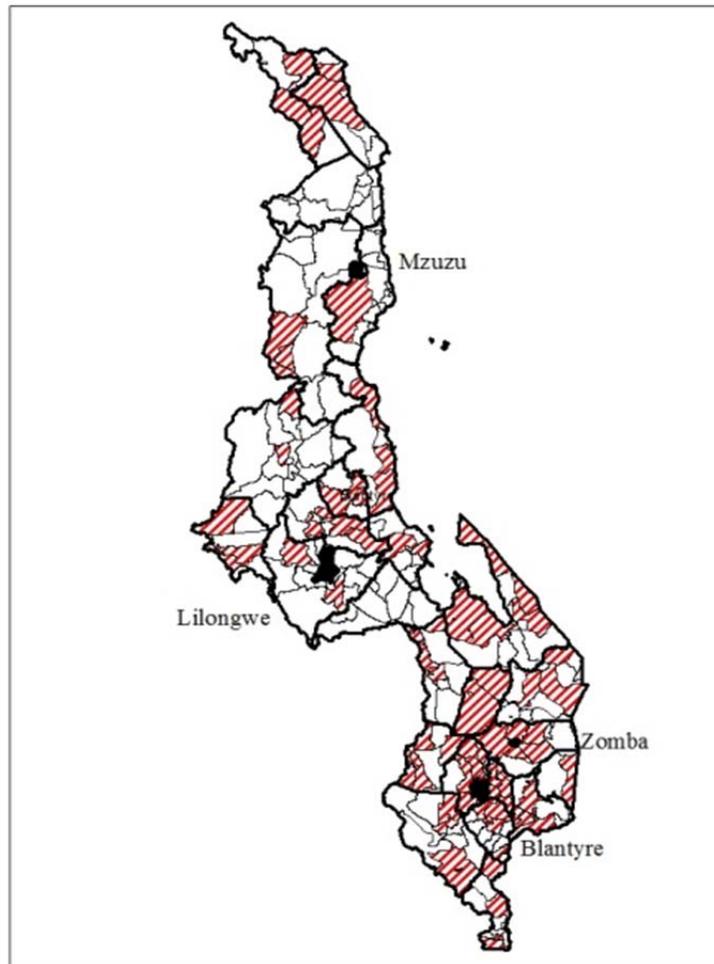


Figure 1 shows district boundaries (thick black lines), sub-district/traditional authority boundaries (thinner black lines) and the distribution of WENELA recruiting stations established by 1937 (red hatched areas) across the country. Malawi's four cities are shown as black shaded areas.

Figure 2: Annual recruitment of Malawian miners to South African mines, 1950-1994

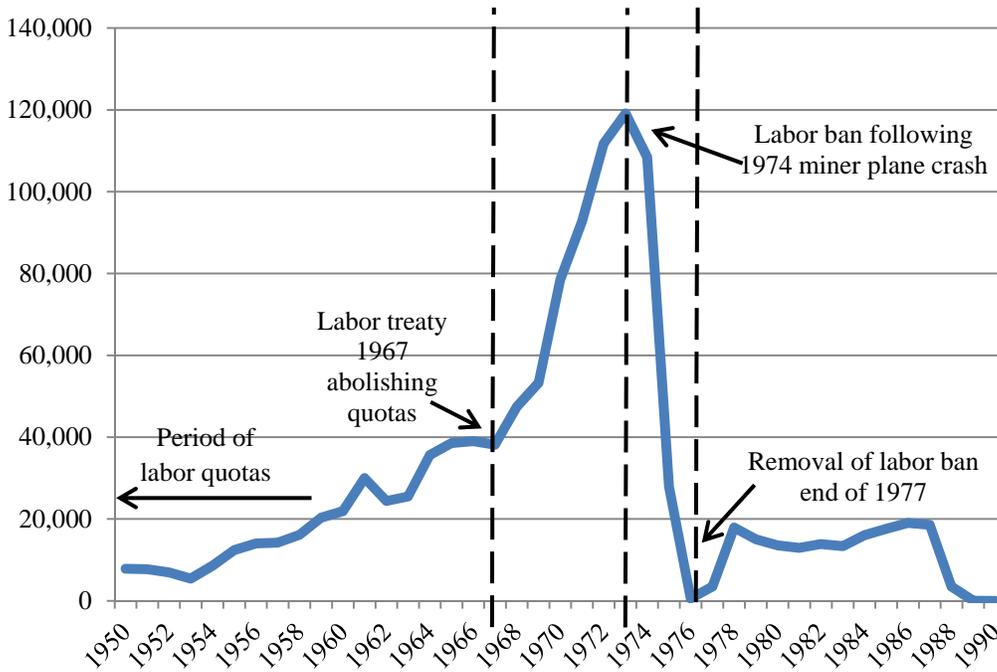


Figure 2 shows number of workers recruited to the South African mines in each year. The three dotted lines represent (from left to right) the abolition of labor quotas in August 1967, the moratorium on migration after the April 1974 Malawian plane crash and the legal resumption of mine migration in 1978.

Figure 3: Treatment assignment: Primary school eligibility in labor shock years 1967 -1977

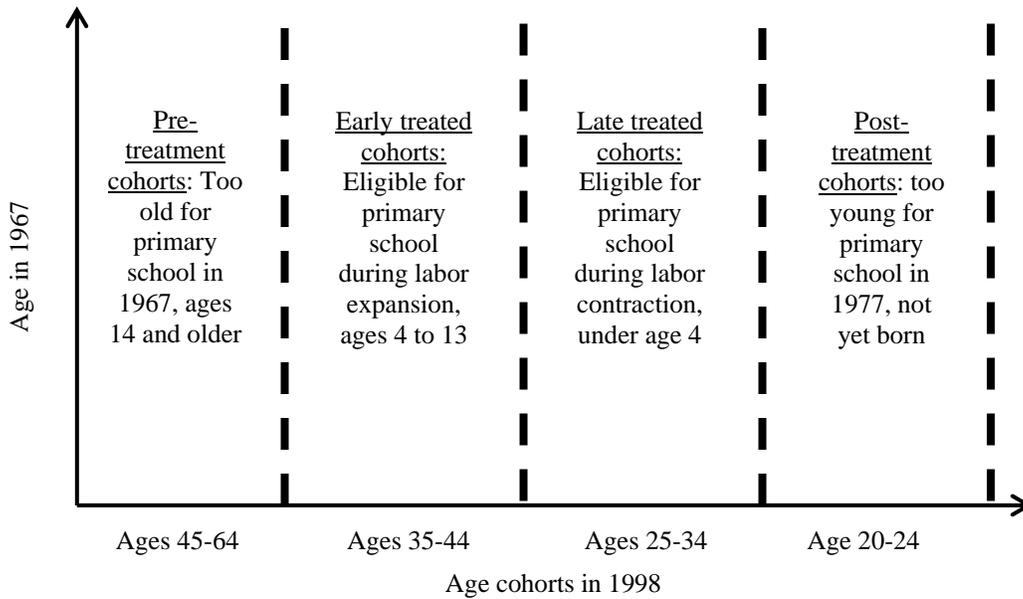


Figure 3 shows which cohorts are eligible for primary school during the labor shock years, 1967 to 1977. The horizontal axis shows age of cohorts in 1998, the vertical axis shows the range of ages for each of cohort in 1967.

Figure 4: Estimated differences in education by age group and Wenela status of district

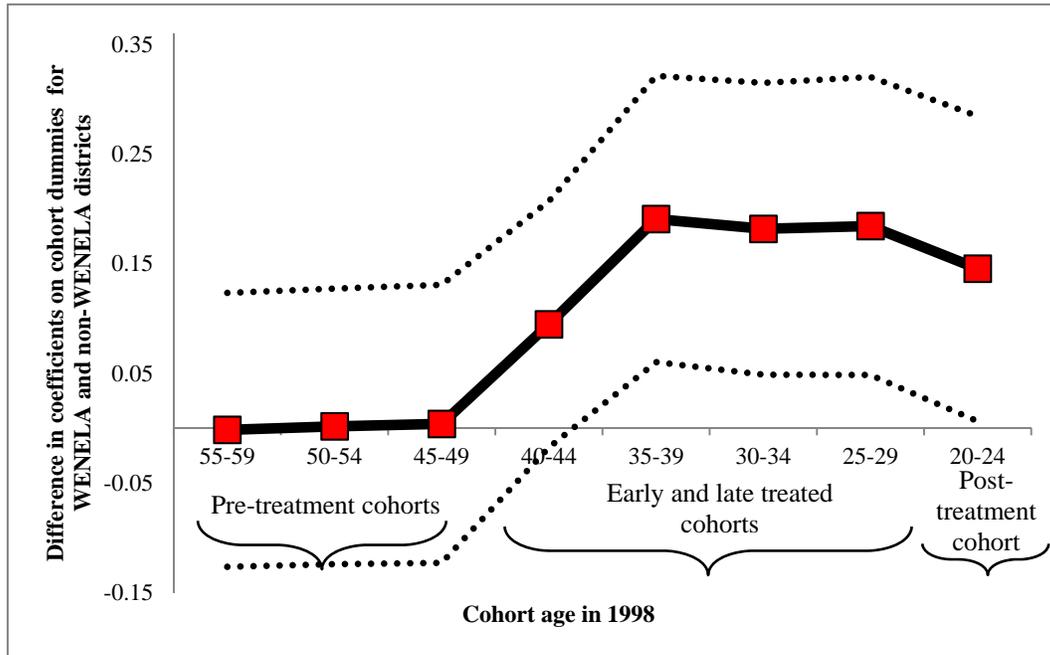
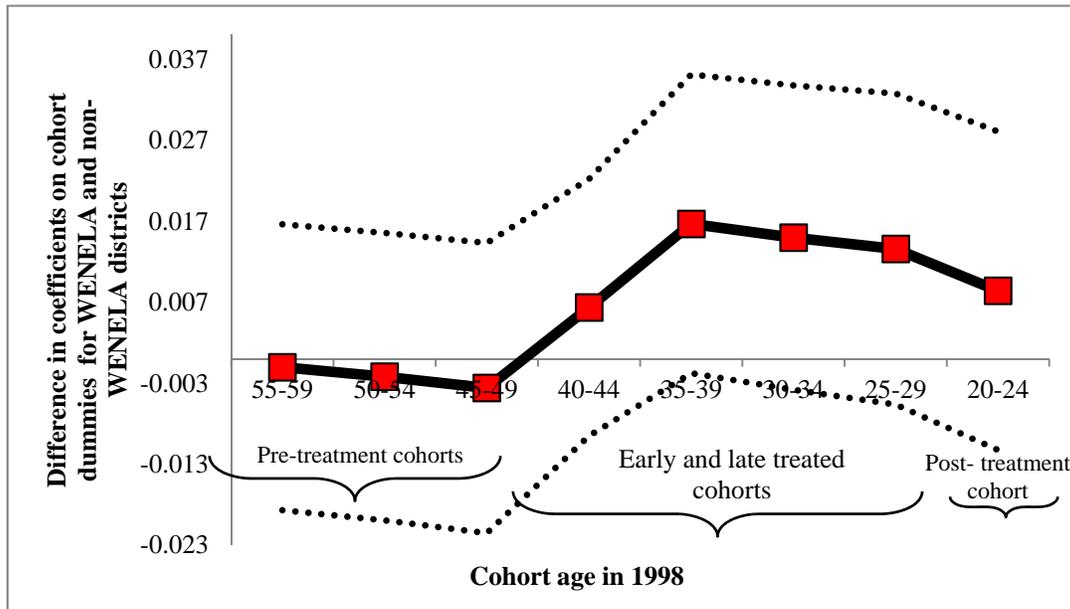


Figure 5: Estimated differences in primary schooling by age group and Wenela status of district



Figures 4 and 5: show estimated interaction term coefficients (solid line) and 95% confidence intervals (dotted lines) from a regression of education (figure 4) and primary schooling (figure 5) on nine age group dummies and their interaction with the number of WENELA stations in the district. Control include: female, log population density in 1931, share literate in 1945, district and region fixed effects, and trend terms interacted with region fixed effects, baseline population density and baseline literacy rates. The x-axis shows five-year age cohorts in 1998.

Table 1: Shares of children aged 10-14 working and share in school, 1966-1968

	Full sample	Wenela Recruiting Districts	Non-Wenela Districts	<i>p</i> value of difference
<i>Panel A: All districts</i>				
Share in school	0.34	0.39	0.24	***
Share working	0.19	0.21	0.17	**
N	46	30	16	
<i>Panel B: Districts with agricultural estates</i>				
Share in school	0.27	0.33	0.22	*
Share working	0.18	0.19	0.18	
N	16	8	8	
<i>Panel C: Districts without agricultural estates</i>				
Share in school	0.37	0.41	0.25	***
Share working	0.20	0.21	0.16	**
N	30	22	8	

Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively, and evaluated relative to the small sample *t* distribution to account for the small number of clusters. Enrolment data are from the 1966 Census. Work data are from the 1968 National Sample Survey of Agriculture and do not include home production. An observation is a district-cohort-gender group for the 10-14 age cohort.

Table 2: Historical and geographic predictors of Wenela recruiting station placement at district-level

	<i>Number of recruiting stations in the district</i>				<i>Any recruiting station in the district</i>			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log population density, Census 1931	-0.979 (0.595)	-1.206* (0.585)	-0.758 (0.639)	-0.176 (1.035)	-0.332*** (0.089)	-0.385*** (0.079)	-0.296*** (0.100)	-0.088 (0.094)
Estate district		-1.489 (1.126)	-0.891 (1.230)	-0.603 (1.535)		-0.343* (0.189)	-0.217 (0.174)	-0.109 (0.116)
Altitude (meters)^*100			0.288** (0.117)	0.125 (0.204)			0.07*** (0.020)	0.018 (0.019)
Literacy rate, Census 1945			2.678 (17.140)	10.010 (17.080)			-0.137 (2.380)	0.955 (1.250)
Central region				1.019 (2.128)				0.148 (0.151)
Southern region				-1.411 (3.961)				-0.619 (0.363)
Observations	24	24	24	24	24	24	24	24
R-squared	0.07	0.13	0.24	0.34	0.28	0.38	0.57	0.87
Mean of outcome	2.79	2.79	2.79	2.79	0.63	0.63	0.63	0.63

Robust standard errors in parentheses in all regressions. Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively, and evaluated relative to the small sample t distribution to account for the small number of clusters. Outcome is the number of recruiting stations in the district in 1937 or an indicator for any recruiting station in the district. All variables are measured at district level. Altitude is average altitude for each district and is a proxy for malaria risk.

Table 3: Wenela recruiting stations predict circular migration

	<i>Δ in number of migrants, 1966 to 1977</i>				<i>Migrant growth rate, 1966 to 1977</i>			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of Wenela stations	1,138** (457)	1,356*** (362)	1,357*** (354)	1,532*** (427)	0.129** (0.047)	0.170*** (0.038)	0.172*** (0.038)	0.191*** (0.040)
Central region		4,364 (3,359)	3,895 (3,816)	3,117 (4,390)		0.647* (0.344)	0.677* (0.367)	0.595 (0.395)
Southern region		1,431 (4,901)	479.8 (5,629)	-821.3 (6,274)		0.690* (0.376)	0.759* (0.423)	0.622 (0.439)
Mean Literacy rate in 1945		-7,114 (5,886)	-6,923 (6,425)	-6,577 (6,524)		-3.964 (4.954)	-4.024 (5.261)	-3.562 (5.286)
Log population density in 1931			827 (1,977)	1,293 (2,116)			-0.0627 (0.143)	-0.0136 (0.149)
Estates			374.1 (3697)	2,448 (4,762)			0.0659 (0.234)	0.284 (0.318)
Estate district*Number of Wenela stations				-702.3 (1,086)				-0.074 (0.064)
N	24	24	24	24	24	24	24	24
R2	0.15	0.41	0.41	0.42	0.29	0.60	0.60	0.62
Mean of outcome variable	3,445	3,445	3,445	3,445	0.30	0.30	0.30	0.30

Robust standard errors in parentheses in all regressions. Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively, and evaluated relative to the small sample *t* distribution to account for the small number of districts. Unit of observation is the district. Outcome in columns (1)-(4) is the change in the raw number (stock) of male migrants between 1966 and 1977, measured at district-level using Census data in 1966 and 1977. Outcome in columns (5)-(8) is the migrant growth rate between these years: (Migrant stock in 1977-Migrant stock in 1966)/Migrant stock in 1966. Number of Wenela stations is a count variable of all stations in the district in 1937 (mean is 2.79), estate is a dummy for whether the district contains a tea, tobacco, sugar or cotton plantation.

Table 4: Summary statistics for Census, Geographic and Administrative Data

	Full sample		Wenela Recruiting Districts		Non-Wenela Districts		<i>p</i> value of difference
	Mean	s.d.	Mean	s.d.	Mean	s.d.	
<i>Panel A: Outcomes and control variables for District-5 Year Age Group-Gender cells</i>							
<i>Variables measuring exposure to mining employment shocks</i>							
Number of Wenela stations	2.79	2.77	4.47	2.19	0.00	0.00	0.00
Any Wenela station	0.63	0.48	1.00	0.00	0.00	0.00	0.00
Early Treatment Cohorts: Primary school eligible in 1967-1973	0.30	0.46	0.30	0.46	0.30	0.46	0.50
Late Treatment Cohorts: Primary school eligible in 1974-1977	0.20	0.40	0.20	0.40	0.20	0.40	0.50
Post-Treatment cohorts: Primary school eligible after 1977	0.10	0.30	0.10	0.30	0.10	0.30	0.50
Pre-Treatment cohorts: Primary school eligible before 1967	0.40	0.49	0.40	0.49	0.40	0.49	0.50
<i>Education outcomes</i>							
Total years of education for adult sample	2.56	2.20	2.85	2.36	2.07	1.79	0.00
Share with any primary school for adult sample	0.41	0.29	0.45	0.30	0.35	0.25	0.00
Number of observations	480		300		180		
<i>Panel B: Geographic and historical district variables</i>							
Number of Wenela stations	2.79	2.83	4.47	2.26	0.00	0.00	0.00
Any Wenela station	0.63	0.49	1.00	0.00	0.00	0.00	0.00
Fraction of men ever been abroad by 1977~	0.19	0.06	0.20	0.05	0.18	0.06	0.27
Δ number of migrants, 1966-1977	3,445	8,282	5,060	9,672	752	4,519	0.16
Migrant growth rate, 1966-1977	0.30	0.68	0.46	0.78	0.03	0.37	0.08
Altitude: high malaria area=1	0.28	0.35	0.20	0.32	0.43	0.36	0.06
Population density in 1931	24.67	22.13	15.71	9.59	39.60	29.09	0.01
District contains an estate	0.21	0.41	0.27	0.46	0.11	0.33	0.17
English and vernacular literacy, share of youth in 1945	0.08	0.04	0.09	0.04	0.06	0.02	0.00
Fraction of men doing wage work (farm, cash or other) 1966	0.63	0.10	0.62	0.12	0.66	0.08	0.14
Fraction of men not earning any wage 1966	0.37	0.10	0.38	0.12	0.34	0.08	0.14
Number of districts	24		15		9		

Data in Panel A are means from 1998 micro data and the 1977 aggregate data, reported at the district-5 year age group-sex level. Data in Panel B are district level means from geographic data, aggregate Census data in 1931, 1945, 1966, 1977 and administrative data. *p* values are reported for the test of the difference in means across recruiting and non-recruiting station areas using robust standard errors and evaluated using the small sample *t* distribution to account for the small number of clusters. Estate is a dummy variable as described in the text.

Table 5: Long run effects of labor migration shocks on education: Difference-in-differences results

	<i>Total years of schooling attained</i>			<i>Share with any primary school</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
Early Treatment Cohorts*Num. Wenela stations (β_1)	0.137*** (0.044)	0.137*** (0.045)	0.124** (0.046)	0.014*** (0.004)	0.014*** (0.005)	0.011*** (0.004)
Late Treatment Cohorts*Num. Wenela stations (β_2)	0.202*** (0.064)	0.202*** (0.066)	0.179** (0.063)	0.020*** (0.006)	0.020*** (0.007)	0.016*** (0.005)
Post-Treatment Cohorts*Num. Wenela stations (β_3)	0.170*** (0.059)	0.170*** (0.061)	0.141** (0.066)	0.016*** (0.005)	0.016*** (0.005)	0.010 (0.006)
Num. Wenela Stations	0.008 (0.042)			-0.003 (0.004)		
Additional controls	Y	Y	Y	Y	Y	Y
District fixed effects	N	Y	Y	N	Y	Y
Trend interactions	N	N	Y	N	N	Y
N	480	480	480	480	480	480
R2	0.81	0.83	0.86	0.80	0.82	0.84
Mean of outcome variable	2.56	2.56	2.56	0.41	0.41	0.41
p value of F test $H_0: \beta_1=\beta_2$	0.01	0.01	0.01	0.01	0.01	0.03
p value of F test $H_0: \beta_1=\beta_3$	0.14	0.15	0.57	0.32	0.33	0.73
p value of F test $H_0: \beta_2=\beta_3$	0.02	0.02	0.07	0.01	0.01	0.06

Robust standard errors clustered at the district level. Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively, and evaluated relative to the small sample t distribution to account for the small number of clusters. Unit of observation is the district-5 year age group-gender cell. Vector of controls includes female dummy, age group dummies, two region fixed effects, the log of district-level population density in 1931 and the share of literate youths in 1945. In the final specification, trend terms are interacted with region fixed effects, baseline population density and baseline literacy rates. Number of Wenela stations in the district is a count variable. Sample includes adults ages 20 to 44 in 1977 and 1998 census.

Table 6: Scaling human capital effects for migrant outflows, IV results

	<i>Total years of schooling attained</i>		<i>Share with any primary school</i>	
	(1)	(2)	(3)	(4)
Δ Num. Migrants (1967-1977)*Early Treatment Cohorts (α_1)	0.415*	0.341**	0.042*	0.032***
	(0.212)	(0.125)	(0.022)	(0.011)
Δ Num. Migrants (1967-1977)*Late Treatment Cohorts (α_2)	0.612*	0.482***	0.061*	0.043***
	(0.316)	(0.162)	(0.032)	(0.014)
Δ Num. Migrants (1967-1977)*Post-Treatment Cohorts (α_3)	0.516*	0.347*	0.047*	0.024
	(0.287)	(0.168)	(0.026)	(0.016)
Additional controls, District FE	Y	Y	Y	Y
Trend interactions	N	Y	N	Y
N	480	480	480	480
Mean of outcome variable	2.56	2.56	0.41	0.41
p value of Chi2 test $H_0: \alpha_1=\alpha_2$	0.07	0.00	0.06	0.02
p value of Chi2 test $H_0: \alpha_1=\alpha_3$	0.25	0.96	0.36	0.63
p value of Chi2 test $H_0: \alpha_2=\alpha_3$	0.02	0.20	0.04	0.17

Robust standard errors clustered at the district level. Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively, and evaluated relative to the small sample t distribution to account for the small number of clusters. Unit of observation is the district-5 year age group-gender cell. Vector of controls includes age eligibility dummies, female, age group dummies, a Census year indicator, two region fixed effects, the log of district-level population density in 1931 and share of literate youth in 1945 and the interaction of density and literacy with trend. Instruments include: Number of Wenela stations in the district interacted with eligibility in 1967-1973, in 1974-1977, and post-1977 dummies. Coefficients evaluated at the mean change in number of migrants (3,445). Sample includes adults ages 20 to 44 in 1977 and 1998 census. See Appendix Table 1 for first stage for first stage regressions.

Table 7: Long run effects of labor migration shocks on education: Heterogeneous effects in estate and non-estate districts

	<i>Total years of education</i>		<i>Share with any primary school</i>	
	Districts with		Districts with	
	Estates	No Estates	Estates	No Estates
	(1)	(2)	(3)	(4)
Early Treatment Cohorts*Num. WNLA stations	0.097** (0.039)	0.149*** (0.044)	0.014* (0.006)	0.014*** (0.004)
Late Treatment Cohorts*Num. WNLA stations	0.147** (0.059)	0.215*** (0.054)	0.019* (0.009)	0.021*** (0.005)
Post-Treatment Cohorts*Num. Wenela stations	0.076 (0.065)	0.200*** (0.038)	0.010 (0.008)	0.018*** (0.003)
Additional controls, district FE and trend interactions	Y	Y	Y	Y
N	160	320	160	320
R2	0.86	0.86	0.85	0.84
Mean of outcome variable	2.30	2.69	0.39	0.42

Robust standard errors clustered at the district level. Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively, and evaluated relative to the small sample t distribution to account for the small number of clusters. Unit of observation is the district-5 year age group-gender cell. Estate denotes those districts which have substantial presence of tobacco and sugar estates, as described in the text. All regressions control for female, age group dummies, the log of district-level population density in 1931, the share of literate youths in 1945, historical literacy and population density interacted with a national trend, a full set of district and region fixed effects and region-specific trends. Number of Wenela stations is a count variable.

Table 8 : Effects of labor migration shocks on child labor shares

	Full sample	Estate districts	Non-estate districts
	(1)	(2)	(3)
Post Dummy (=1 in 1977)*Num. Wenela stations	-0.010** (0.004)	0.195*** (0.024)	-0.013*** (0.004)
Additional controls	Y	Y	Y
District FE	Y	Y	Y
Region trends	Y	Y	Y
N	92	32	60
R2	0.71	0.86	0.76
Mean of outcome variable	0.18	0.20	0.17

Robust standard errors clustered at the district level. Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively, and evaluated relative to the small sample t distribution to account for the small number of clusters. Unit of observation is the district-5 year age group-gender cell for the age group 10-14 years. Outcome is the share of children working in 1968 or in 1977. Vector of controls includes a dummy for the year of observation being 1977, female, two region fixed effects, the log of district-level population density in 1931, the share of literate youths in 1945 and historical literacy and population density interacted with post dummies. Number of Wenela stations in the district is a count variable.

Appendix Table 1: First stage estimates for IV scaling exercise

	<i>ΔMigrants (1967-1977)*Early Treatment Cohorts</i>	<i>ΔMigrants (1967-1977)*Late Treatment Cohorts</i>	<i>ΔMigrants (1967-1977)*Post-Treatment Cohorts</i>
	(1)	(2)	(3)
Early Treatment Cohorts*Num. Wenela stations	0.13** (0.047)	0.009 (0.007)	0.008 (0.005)
Late Treatment Cohorts*Num. Wenela stations	0.003 (0.002)	0.14*** (0.041)	0.013 (0.009)
Post-Treatment Cohorts*Num Wenela stations	0.004 (0.003)	0.022 (0.015)	0.14*** (0.041)
Additional controls, district fixed effects	Y	Y	Y
Trend interaction terms	Y	Y	Y
N	480	480	480
R2	0.47	0.45	0.40
F statistic on instruments (interaction term)	7.71	7.71	7.71

Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively, and evaluated relative to the small sample *t* distribution to account for the small number of clusters. Robust standard errors clustered at the district level. Unit of observation is the district-5 year age group-gender cell. Outcomes are eligibility dummies interacted with the change in the number (stock) of migrants between 1966 and 1977. All regressions control for female, age dummies, the log of district-level population density in 1931, the share of literate youths in 1945 and a full set of district fixed effects. Trends are interacted with baseline population density, baseline literacy, and region fixed effects.

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Appendix 1: Data

This appendix describes the main data sources used in the paper and the construction of main outcome and explanatory variables

1. *Education and demographic variables from 1998 Census*

- We use 100% microdata from the 1998 Census. These data are available from the Malawi National Statistics Office and from IPUMSI (<https://international.ipums.org/international/>).
- Variables include: total years of schooling attained for everyone in the data, current geographic location (region and district of the individual), age and gender. We create additional education variables: whether someone has attained any primary schooling, whether someone has completed primary schooling, and whether an individual reports being bilingual or not

2. *Education and demographic variables from the 1977 Census*

- We digitize aggregate data tables constructed from the 100% microdata of the 1977 Census, reported in *Malawi 1977 Population Census Final Report Volumes I and II*, Malawi National Statistics Office, Zomba
- Data are available at national, region and district level, and sometimes at district, sex and five-year cohort level.
- Variables we use include: total years of schooling attained by each gender-five year age group at district level, the share of each district-gender-five year age group cell that has ever been to primary school, and the cell counts for each district-gender-five year age cell.
We also use data on the number of men reporting a return from working abroad by district and five year age group, since the prior 1966 Census, and the number of boys and girls aged 10 to 19 who are employed outside the home, employed in the home, and enrolled in school

3. *Historic variables from older Census data*

- Aggregate tables presented at the district level are available from published reports for the 1931 (*Report on the Census of 1931*, Nyasaland Protectorate), 1945 (*Report on the Census of 1945*, Nyasaland Protectorate) and 1966 (*Malawi 1966 Population Census Final Report*, Malawi National Statistics Office, Zomba) Malawian Census. We digitized various tables from these reports and matched them to current definitions of district boundaries

- Variables include: the log of population density in 1931 and 1945, the share of youth who are literate (English and the vernacular) in 1945, the fraction of men employed in different sectors (farming/non-farming, working for wages/no wages, unemployed) in 1966, and the number of adult men who work abroad in 1966, reported at the district level

4. *Geographic variables*

- Altitude: we compute altitude for each point on the Malawian grid map using data from the national map seamless server (<http://seamless.usgs.gov/index.php>) and the Viewshed tool in ArcGIS. We aggregate these measures to district level.
- We define areas of high, medium or low malaria susceptibility based on standard measures of altitude: high malaria areas (altitude below 650m), medium malaria areas (altitudes between 650m and 1100m) and low malaria areas (altitudes over 1100m).
- We create a district boundary crosswalk that links districts over time (across Census waves) and across name changes. We assign variables measured in earlier years to later Census district boundaries in this way:
 - For districts that were eventually combined in later years, we add district level values together
 - For districts split apart in later years, we apportion district totals to split districts using the fraction of physical area that each split district accounts for within the total district.
- We identify which districts contain a large tea or tobacco plantation using information in Christiansen (1984). The FAO's crop suitability index measuring whether a district is highly suitable for tobacco or tea production significantly predicts this estate district indicator.

5. *Administrative data*

- Figure 1 is constructed using the location of Wenela/TEBA recruiting stations in 1937. We collected and digitized this historical data using a variety of sources. The main source included "Correspondence from the Secretariat, Zomba, Nyasaland 1935 (Circular number 8 1935, S1/169/35). We verified these stations were still open in later years using information from later Provincial Administration Reports (Northern Province: 7th December 1961 Ref. No. O.3.37 and Commissioner for Labour Circular, 25th March 1957)
- Figure 2 is constructed using national labor migration totals from a variety of sources including: Chirwa (1991 for years 1950-1958); Lipton (1980: for years 1959-1994); Crush, Jeeves and Yudelman (1991: pp234-235) and various years of TEBA Annual Reports

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Appendix 2: Bounding results for composition effects from internal migration

Internal migration poses one possible threat to the validity of our main results. Because neither the 1977 nor 1998 Census captures district of birth, we potentially mismeasure childhood exposure to Wenela recruiting stations among those people who move across districts after completing education, but before we see them in the relevant Census year. Internal migration flows are unlikely randomly allocated across districts. Without knowing more about differences in the magnitude and direction of migrant flows across districts, this possible misclassification of exposure to Wenela stations generates unpredictable biases in our estimates.

To see why, consider the following. Suppose all districts have the same average level of education before internal migration. If more educated adults move from non-Wenela to Wenela districts while less educated adults move in the opposite direction, this generates artificially positive differences in adult educational attainment across districts that we would ascribe to exposure to Wenela stations. As long as this sorting is constant over time, then the Wenela dummy in our regressions (as well as district fixed effects) controls for these observed differences in educational attainment driven by internal migration. If, however, internal migration flows differ by district as well as cohort then our results could be the result of complicated changes in the composition of population at the district level.

In the absence of individual level data on birth districts, we bound our effect sizes for possible composition changes induced by internal migration. We combine information on net migration rates from the 1977 Census with assumptions about possible levels of education of net migrants. First, we use 1977 Census data to construct the number of net migrants per person currently living in the district for each district in each five-year cohort and gender cell. We call this the net migration rate, or $NetMigRate_{asd}$. In our data, this number is always between -0.35 and 0.29.¹ We need to assume that this net migration rate is the same in 1977 and 1998, since the 1998 Census contains no information on district of birth. Second, we assume that all migrants – whether they show up as in- or outmigrants in a particular district – have

¹ Census 1977 counts the number of people in each district, cohort, and gender cell and enumerates how many of these individuals were born in each district. The net migration rate is computed as the difference between total in-migrants and total out-migrants divided by total current population in the district; it is the number of net migrants (in-migrants – out-migrants) per person living in the district. A 0.2 net migration rate means that for every person living in the district, there are 0.2 net in-migrants.

the same level of education and therefore we need only account for the potential education of net migrants, the difference between in- and outmigrants.²

We adjust our education variables (\bar{Y}_{asd}) measured at district, cohort, and gender level:

$$\begin{aligned}\bar{Y}_{asd}^{BOUND} &= \frac{N_{asd}\bar{Y}_{asd} - \text{NetMigrants}_{asd} * \bar{Y}_{as}^m}{N_{asd} - \text{NetMigrants}_{asd}} & (A.1) \\ &= \frac{N_{asd}\bar{Y}_{asd} - \text{NetMigRate}_{asd} * N_{asd} * \bar{Y}_{as}^m}{N_{asd} - \text{NetMigRate}_{asd} * N_{asd}} \\ &= \frac{\bar{Y}_{asd} - \text{NetMigRate}_{asd} * \bar{Y}_{as}^m}{1 - \text{NetMigRate}_{asd}}\end{aligned}$$

where $BOUND = \{upper, lower\}$, \bar{Y}_{asd}^{BOUND} represents the adjusted mean education outcome at district, cohort, and gender level, N_{asd} is total population in a district-cohort-gender cell, \bar{Y}_{as}^m is either the maximum or minimum value of the relevant education variable across all districts at cohort and gender level, and NetMigrants_{asd} is the total number of net migrants in a district-cohort-gender cell.

NetMigrants_{asd} is estimated by multiplying the total population in that district-cohort-gender cell with the net migration rate (NetMigRate_{asd}) for that cell. Each component of (A.1) comes from the relevant Census wave, except for NetMigRate_{asd} which is computed using 1977 Census data and applied to both Census waves. We estimate the main regression specifications for our sample after creating these adjusted education variables, one set for each of the extreme values of \bar{Y}_{as}^m , or

$$\begin{aligned}\bar{Y}_{asd}^{lower} &= \frac{\bar{Y}_{asd} - \text{NetMigRate}_{asd} * \bar{Y}_{as}^{max}}{1 - \text{NetMigRate}_{asd}} \text{ and} \\ \bar{Y}_{asd}^{upper} &= \frac{\bar{Y}_{asd} - \text{NetMigRate}_{asd} * \bar{Y}_{as}^{min}}{1 - \text{NetMigRate}_{asd}}.\end{aligned}$$

There are two notable features of equation (A.1). First, the adjustments we make for internal migration imply that \bar{Y}_{asd}^{upper} and \bar{Y}_{asd}^{lower} provide upper and lower bounds on mean education and average share of adults with any primary school across the entire sample. Second, despite these names, these adjustments do not imply that the difference-in-differences regressions using these new variables will produce estimates that contain our main education results. This is because in a closed system (i.e. the whole of Malawi) some districts are receiving districts ($\text{NetMigRate}_{asd} > 0$) while others are sending districts

² For example: if there are 110 in-migrants and 100 out-migrants to a particular district, and in-migrants and out-migrants have the same levels of education, the only change in composition that occurs as a result of this net migration is due to the additional 10 people who migrated into the district.

($NetMigRate_{asd} < 0$). In order for $\bar{Y}_{asd}^{upper} > \bar{Y}_{asd}$ or $\bar{Y}_{asd}^{lower} < \bar{Y}_{asd}$, the following equations should hold (note that in our sample, $1 - NetMigRate_{asd} > 0$ in all cases):

$$NetMigRate_{asd} * (\bar{Y}_{asd} - \bar{Y}_{as}^{min}) > 0 \quad (A.2)$$

$$NetMigRate_{asd} * (\bar{Y}_{asd} - \bar{Y}_{as}^{max}) < 0 \quad (A.3)$$

Since $\bar{Y}_{asd} \geq \bar{Y}_{as}^{min}$ and $\bar{Y}_{asd} \leq \bar{Y}_{as}^{max}$ in all districts, these equations are only satisfied for receiving districts that have $NetMigRate_{asd} > 0$. To see this, assume that we impute the minimum level of education for net migrants, Then, $\bar{Y}_{asd}^{upper} > \bar{Y}_{asd}$ is satisfied only in receiving districts because our adjustments take out the low levels of education of net in-migrants to create a higher adjusted mean education variable. For sending districts, where $NetMigRate_{asd} < 0$, the inequality in (A.2) is reversed and $\bar{Y}_{asd}^{upper} < \bar{Y}_{asd}$. Similarly, when we impute the maximum level of education for net migrants, equation (A.3) will only be satisfied in receiving districts; subtracting high levels of net in-migrant education generates $\bar{Y}_{asd}^{lower} < \bar{Y}_{asd}$. In sending districts, (A.3) is reversed, so $\bar{Y}_{asd}^{lower} > \bar{Y}_{asd}$.

Because we have both sending *and* receiving districts in our sample, and because rates of internal migration in 1977 are different across Wenela and non-Wenela districts (rates of in-migration are higher in Wenela districts, results not shown), our adjustments have different effects on the bounds values in specific Wenela and non-Wenela districts. More complicated patterns of net migration that vary across exposed and non-exposed cohorts and across Wenela and non-Wenela areas imply that adjustments for internal migration may generate in difference-in-differences estimates that do not bound our main result.³ Nevertheless, it is still a useful exercise to check whether internal migration modelled in this way appears to confound our results.

Appendix Table 2 displays results from difference-in-differences regressions estimated using the adjusted education variables, first including all controls and district fixed effects, and then adding in trend interactions with region fixed effects, baseline literacy and baseline population density. We compare the coefficients in this table with the main estimates in Table 5.

First, assuming net migrants have the maximum level of schooling in the district-cohort-gender cell for a given Census year, the presence of anyone new in a receiving district raises mean education and their absence from a birth district artificially deflates that district's average education. Adjusting for these

³ Crudely, if net migration rates are more likely positive in Wenela districts among exposed cohorts, we would be doing more "receiving district" adjustments in our core treatment groups and more "sending district" adjustments in our control groups.

educated net migrants, we still see large, positive impacts of exposure to treatment among exposed cohorts: those exposed during the labor expansion have 0.14 more years of education, while those exposed during the labor contraction have 0.2 more years of education. The education gap between Wenela and non-Wenela districts for the post-treatment group continues to be positive, at around 0.16 more years of education. Second, if we instead assume that net migrants are uneducated, removing them from our outcome measure in receiving districts and adding them back to sending districts reveals similar, large positive impacts of exposure to mine employment shocks. The difference-in-differences estimates in columns (3) and (4) imply that exposed cohorts in Wenela areas gained between 0.16 and 0.25 more years of education. Post-treatment groups continue to have about 0.22 more years of education in Wenela relative to non-Wenela areas, controlling for differences between these areas using the oldest pre-treatment cohorts. These bounds compare favorably to our main results in Table 5, 0.12 and 0.179 more years of education (Table 5, column 3).

Results are similar when we use the share with any primary school as outcome. In Table 5, directly exposed cohorts from districts with more Wenela stations are 1.1 to 2.6 percentage points more likely to have ever attended primary school. After adjusting for internal migration in Appendix Table 2 columns (5)-(8), these exposed cohorts from districts with Wenela stations are between 1.2 and 2.4 percentage points more likely to have ever been to primary school. All of our estimates are statistically different from zero at the 1, 5 or 10% level. Looking at the final three rows of the table, we see that we can strongly reject that the impacts of the migration shock on education are the same for the labor expansion and labor contraction cohorts, and we can reject that the impacts on primary school access for the labor contraction cohorts and the youngest post-treatment cohorts are the same. However, as in the case of our main results in Table 5, we cannot reject that the impacts for the post-treatment cohorts and the labor expansion cohorts are the same. We see the same inverted U-shaped pattern of coefficients in our bounded results as in the main results. The results of this bounding exercise suggest that selective internal migration and any resulting measurement error in $Wenela_d$ cannot account for our main effects.

Appendix 2 Table 1: Long run effects of labor migration shocks on education: Bounds for internal migration

Assumptions about migrant education:	<i>Total years of education</i>				<i>Share with any primary school</i>			
	Max. schooling		Min. schooling		Highest share with primary school		Lowest share with primary school	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Early Treatment Cohorts*Num. Wenela stations (β_1)	0.142*** (0.048)	0.136*** (0.044)	0.163** (0.059)	0.164** (0.063)	0.014*** (0.005)	0.012*** (0.004)	0.015*** (0.005)	0.014*** (0.005)
Late Treatment Cohorts*Num. Wenela stations (β_2)	0.202*** (0.067)	0.191*** (0.060)	0.251*** (0.084)	0.252** (0.090)	0.020*** (0.007)	0.016*** (0.005)	0.024*** (0.007)	0.021** (0.007)
Post-Treatment Cohorts*Num. Wenela stations (β_3)	0.168** (0.062)	0.153** (0.062)	0.220*** (0.076)	0.222** (0.088)	0.015** (0.005)	0.011* (0.006)	0.019*** (0.006)	0.015* (0.007)
District FE	Y	Y	Y	Y	Y	Y	Y	Y
Region trends	N	Y	N	Y	N	Y	N	Y
N	480	480	480	480	480	480	480	480
R2	0.83	0.86	0.83	0.85	0.824	0.84	0.82	0.83
Mean of outcome variable	2.53	2.53	2.59	2.59	0.41	0.41	0.41	0.41
p value of F test $H_0: \beta_1=\beta_2$	0.01	0.00	0.00	0.01	0.01	0.02	0.00	0.02
p value of F test $H_0: \beta_1=\beta_3$	0.22	0.56	0.02	0.09	0.56	0.69	0.10	0.76
p value of F test $H_0: \beta_2=\beta_3$	0.01	0.09	0.04	0.12	0.01	0.07	0.01	0.04

Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively, and evaluated relative to the small sample t distribution to account for the small number of clusters. Robust standard errors clustered at the district level. Unit of observation is the district-5 year age group-gender cell. Vector of controls includes female, age group dummies, two region fixed effects, the log of district-level population density in 1931 and the share literate in 1931, and historical density, historical literacy and region fixed effects interacted with a trend term. Number of Wenela stations in the district is a count variable. Outcomes are our estimates of the bounds on education and share in primary school, after accounting for maximum and minimum possible values of each variable for the number of net migrants in each age-gender cell. Details of variable construction are explained in the text. Sample includes adults ages 20 to 44 in 1977 and 1998 census.

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Appendix 3: Constructing exact p values for Table 5 results using randomization inference

Our empirical strategy exploits pre-existing spatial variation in migration costs across 24 districts within Malawi. The relatively small number of districts leads to a concern that standard inference procedures will over-reject the null hypothesis of zero impact of district-level exposure to the labor migration shock. We deal with this concern by presenting robust standard errors clustered at the district-level in Table 5 and obtain p values to indicate statistical significance using the small sample t distribution adjusted for the number of covariates that are constant within the cluster.

An alternative approach is to construct exact p values using randomization inference (Fisher, 1935; Rosenbaum 2002; see Cohen and Dupas 2010 for an example of how this is done in the context of a randomized controlled trial with 16 clusters and a single treatment). The idea behind this approach is as follows:

- We randomly assign the actual distribution of Wenela stations to districts and estimate the difference-in-difference models of Table 5 for this “false” assignment. The false allocation mimics the true distribution of stations
- Since there are over 1.3 million ways to allocate Wenela stations (the range of this variable is 0 to 10) to the 24 districts, we generate 1,000 different random assignments of stations to districts and estimate the difference-in-differences regression for each allocation.
- We compute the empirical distribution of t statistics for each of the three main parameters (β_1 , β_2 and β_3) generated by these 1,000 false assignments
- We compare the actual t statistics from Table 5 to the empirical distribution of test statistics for each parameter and compute the probability of observing a t statistic in the tails of this distribution. The resulting p values, denoted randomization inference p values are presented in the table below.

In all cases, we can reject the null of zero impact at the 10% level. In most cases, for the estimates of β_1 and β_2 , we can also reject the null of zero impact at the 5% level.

References

- Cohen, Jessica and Pascaline Dupas, 2010 “Free distribution or cost-sharing? Evidence from a randomized malaria prevention experiment”, *Quarterly Journal of Economics*, Vol. 125 (1): 1-45
- Fisher, Ronald A. 1935 The Design of Experiments London: Oliver and Boyd.
- Rosenbaum, Paul R. 2002 Observational Studies New York: Springer-Verlag.

Appendix 3 Table 1: Randomization inference p values for difference-in-differences estimates of Table 5

p value for:	<i>Outcome is: Years of schooling</i>			<i>Outcome is: Share with any primary school</i>		
$H_0: \beta_1=0$	0.009	0.001	0.001	0.006	0.001	0.016
$H_0: \beta_2=0$	0.008	0.009	0.012	0.007	0.013	0.011
$H_0: \beta_3=0$	0.013	0.013	0.031	0.008	0.009	0.084
Other controls?	Y	Y	Y	Y	Y	Y
District FE?	N	Y	Y	N	Y	Y
Trend interactions	N	N	Y	N	N	Y

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Appendix 4: Correlations between human capital formation and household well-being in Malawi

Given concerns about the quality of education provided in poor countries, it is worthwhile asking whether there is any evidence that more years of schooling and more skills learned in school are valuable in Malawi. We use data from the 2004 Malawi Integrated Household Survey to estimate the relationship between total assets owned by the household (as a summary measure of household well-being) and human capital attainment of the head of the household. There are too few wage earners in the data to examine the relationship between individual education and wages.

We restrict the sample to all rural households in the survey where the head is between the ages of 26 and 60 in 2004, leaving us with 85% of the initial survey. We calculate total assets owned by the household (the range is 0 to 31; the mean is 6.9) and regress this index on four different measures of human capital of the household head: total years of schooling attained, whether the head has been to primary school, and self-reported literacy in English and Chichewa (the person reports that they can read a one page letter in the relevant language). Using the education level of the head of household (rather than the maximum level of education in the household) is a meaningful way to characterize how much human capital the household has access to, since there are very few three-generation households in the sample. The literacy measures reflect skills acquired in the lower levels of primary school: 88% of household heads with four years of completed education report being literate in Chichewa, and literacy rates increase from 47% to 72% between two and three years of total schooling.

Appendix 4 Table 1 presents the results of regressing our asset index on the four human capital measures for three specifications: one without other controls, a second adding controls for the age and gender of the household head, household size, historical district characteristics (literacy, population density) and region fixed effects, and a third that includes district fixed effects. Results indicate a robust, large and positive relationship between each human capital measure and the household asset index. In the first three columns of the table, we see that an additional year of schooling is correlated with about one third more total assets (columns 1-3), which is a 5-6% return per year of schooling. For a household head with any primary school, assets are 7% higher (columns 4 and 5). Returns to literacy are particularly large: having a literate household head raises total assets in the household by over two; a 30% gain. While usual concerns about selection and measurement error caution us against interpreting these point estimates as exactly causal, the strong positive relationship between human capital attained in childhood and measures of household well-being in adulthood provide some evidence that education, and skills learned at school, are indeed valuable in Malawi.

Appendix 4 Table 1: Correlation between household asset index and human capital of household head

	<i>Outcome: Total assets owned by household [mean=6.9]</i>											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
<i>Human capital of household head</i>												
Years of education	0.380*** (0.023)	0.362*** (0.023)	0.361*** (0.019)									
Any primary school				0.253 (0.165)	0.471*** (0.148)	0.473*** (0.146)						
Literate in English							2.531*** (0.129)	2.217*** (0.130)	2.185*** (0.130)			
Literate in Chichewa										2.435*** (0.148)	2.061*** (0.166)	2.005*** (0.147)
N	6,771	6,771	6,771	6,771	6,771	6,771	6,794	6,794	6,794	6,794	6,794	6,794
Effect size: % of mean assets	6%	5%	5%	4%	7%	7%	37%	32%	32%	35%	30%	29%
Additional controls?	N	Y	Y	N	Y	Y	N	Y	Y	N	Y	Y
District FE?	N	N	Y	N	N	Y	N	N	Y	N	N	Y

Robust standard errors clustered at the district level. Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively. Unit of observation is the household; household head's level of education is the main regressor in each specification. Sample includes all rural households where the household head is between the ages of 26 and 60 in 2004. Additional controls include age of household head, whether the head is female, household size, the log of district-level population density in 1931, the share of literate youths in the district in 1945, and region fixed effects. All regressions are weighted using household weights. Data are from the 2004/2005 Malawi Integrated Household Survey.