Does labor migration affect human capital in the long run? Evidence from Malawi

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Abstract: Circular labor migration within and across national borders is a central feature of labor markets across Africa, and yet little is known about impacts on sending communities. This paper estimates the net effects of labor migration on long run human capital accumulation in Malawi. Malawi is one of many labor reservoirs in the region that historically supplied workers to South African mines, and where the potential for children to substitute for missing male labor is large. We study the effects of large circular migration shocks generated by two events in this country: the removal of labor migration quotas in 1967, and a ban on mine labor in 1974. We use proximity to historic mine recruiting stations to capture spatial variation in exposure to the resulting expansion and contraction of foreign employment. Using newly digitized Census data, we compare differences in educational attainment across high and low migration shock areas, among adult cohorts eligible and ineligible for primary school between 1967 and 1977. Both expansion and contraction of labor migration had large, positive and lasting impacts on education of those left behind. Age eligible cohorts with the greatest exposure to migration shocks gain 0.08 to 0.135 more years of schooling and are 2 to 3 percentage points more likely to have ever attended school. These positive effects of labor migration income are significantly smaller where local agricultural production offered more opportunity for child labor. [233 words]

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1. Introduction

Circular labor migration within and across national borders is a central feature of labor markets across Africa. Yet, because this temporary, "there and back again" migration is difficult to model and measure, the empirical evidence on how it affects rural sending regions is sparse (Lucas 2005). Addressing this substantial gap in our knowledge of how labor markets in low-income countries operate is important for two reasons. First, labor migration may well be one of the most direct strategies that poor households have available to them to improve their standard of living in the short run (Clemens 2011, Pritchett 2006). Knowing to what extent this is true could provide new insights for development policy. Second, widespread circular migration and related flows of money have the potential to generate long term change in rural economies. Investigating and documenting such long run changes can lead to a better understanding of how local economies find pathways out of poverty.

In this paper, we study one particular way that circular labor migration can have lasting effects on sending communities: through its impact on human capital accumulation. We ask two related research questions. First: does adult labor migration increase or reduce total human capital attained by the next generation? The answer to this question is not obvious. While migrant remittances can relax credit constraints and enable families to send kids to school, migration possibilities could incentivize kids to stay in school (brain gain) or to drop out (Gibson, McKenzie and Stillman 2011; McKenzie and Rapoport 2011). And, in settings where child labor is prevalent, as is the case in most of Africa, losing a working-aged adult even temporarily may crowd out schooling by increasing the demand for child labor. Whether this crowd out occurs depends to some extent on how valuable children are in production, or how substitutable they are for adult labor. To investigate whether circular labor migration can crowd out schooling, we ask a second question: Do the long run impacts of labor migration depend on local technologies of production that provide different outside options for child work?

We use the historical experience of Malawi to answer these questions by estimating the net effects of circular labor migration on human capital accumulation in rural sending communities. Malawi is one of many labor reservoirs that supplied unskilled male workers to the South African mines through much of the twentieth century. By 1977, one in five adult men had ever worked abroad. We study the effects of large circular migration shocks generated by two events in this country. In 1967, a new labor treaty between Malawi and South Africa removed a binding labor quota and opened the way for a 300% increase in the flow of Malawians to South African mines. Access to these foreign jobs was halted after a mining plane crash in April 1974 killed a group of Malawian miners. The crash precipitated a four-year ban on all mine labor migration, leading to the immediate return of over a hundred thousand men. We use both sides of this natural experiment to test for the net effect of this migration on human capital attainment, and to understand more generally the role that local agricultural production technologies may play in determining the net effect of this migration.

To identify long run impacts on education, we compare differences in human capital accumulation across areas facing plausibly exogenous and differential costs of accessing these mine jobs between cohorts that are age eligible and age ineligible for primary school between 1967 and 1977. We use the location of mine recruiting stations as a pre-determined measure of differences in the costs of labor migration, since they were established long before the 1967 labor treaty and 1974 labor ban. We analyze the periods of labor expansion and labor contraction separately, since they entail different combinations of shocks that could have different impacts on education. In the early period, a surge in labor migration entailed sharp reductions in local male labor supplies and large inflows of remittances. During the labor ban period, returning migrants increased local labor supplies and brought with them their accumulated earnings. While labor migration income flowed back to rural Malawi in both periods, migrants were missing in the first period and present in the second period. We examine differences in the education impacts between these two periods to shed some light on the mechanisms through which labor migration affects sending communities. We then estimate difference-in-differences regressions separately for estate and non-estate districts to provide further evidence that circular labor migration has different impacts on sending communities depending on whether children have outside options for work.

We collect and digitize Census data from 1977 and combine it with the 1998 Census to measure completed education outcomes among adults. A great advantage of using Census data is that we can accurately measure migration prevalence at the district level, something that is often difficult to capture in household surveys (De Brauw, Mueller and Lee 2013). We also collect and digitize administrative data on the location of historic mining recruiting stations to provide district-level variation in exposure to the rapid expansion and contraction of foreign employment and earnings. Being proximate to a recruiting station substantially lowers migration costs, and we show that this proximity predicts significantly higher flows of men out of a district between 1967 and 1977. Having more recruiting stations in a district implies a lower cost of migration, which is the main spatial variation used in the analysis. Our identification assumptions are that there are no contemporaneous shocks to education in either recruiting or non-recruiting areas, and that in the absence of these differential migration shocks, cohort trends in educational attainment would be the same across districts (we show they are in the pre-period). We control for many observable and unobservable confounders across recruiting and non-recruiting districts using historic and geographic control variables that could have affected historical station placement, trend terms interacted with baseline historic variables, district fixed effects and region-specific trends.

We show that in the space of ten years, exposure to mining employment shocks enabled districts with recruiting stations to overtake districts without recruiting stations in their total amount of educational attainment, with long-lasting effects. The main results suggest that greater exposure to this international labor migration contributed to a 3.4 to 5.2% gain in total years of education and a 2.1to 3.1% increase in any primary schooling attendance. These primary school enrollment effects account for up to one fifth of the total increase in enrollment rates between 1967 and 1978.

To translate our reduced form estimates of the causal effect of district-level exposure to these labor migration shocks into human capital effects generated by additional migrant workers, we pursue an instrumental variables (IV) strategy. We measure the change in migrant flows between 1967 and 1977 at the district level, and instrument for these flows (interacted with cohort eligibility dummies) using the number of recruiting stations in a district (interacted with eligibility dummies). The IV estimates suggest even larger impacts of labor migration on district-level education outcomes in the long run. For a 1% increase in labor migrations, education increases by between 1 and 1.5 years, which is a 40 to 50% increase relative to mean levels of schooling. Results are similarly large for primary school enrolment. Because we estimate effects at the district level (rather than household level), these results include any spillovers from migrant to non-migrant households within the district. Our back-of-the-envelope estimate of the migrant income elasticity of education lies between 0.2 and 0.3.

Several pieces of evidence suggest that these long run impacts of circular migration on education of the next generation are muted when there are more outside options for children to work. First, we find that the positive effect for cohorts eligible for primary school during the labor expansion, when men are missing from local labor markets, is significantly smaller than the impacts for cohorts eligible during the labor ban years, when repatriated migrants collected their earnings at home. Second, we find that the effects on education are significantly larger in non-estate districts than in estate districts, in both periods. These non-estate districts offer fewer viable alternatives to school for children compared with estate districts where kids had the opportunity to produce export crops on estate plantations. An important implication of these results is that the long run benefits of short run, circular labor migration depend on the technology of production of the local economy, and in particular, the local shadow value of child time.

Malawi provides an advantageous setting for dealing with the twin challenges of identification and data quality in migration research. First, there is only a single selection problem to consider, since only adult men, not households, could move to South Africa (Gibson, Mckenzie and Stillman 2011). Second, migrants were required to return home after two years and required to have up to two thirds of their earnings diverted into deferred pay. We can therefore be sure that flows of capital back to rural areas were substantial and that the large majority of remittances were exogenously determined by the rules of the mine labor contract rather than by migrant motives to remit. Third, we compare outcomes across districts with historically different and plausibly exogenous access to mine jobs through recruiting stations. Although labor migration to South Africa was important in all districts (as we will show), proximity to a mine recruiting station substantially reduced the costs of migrating from Malawi as we explain in Section 3. A fourth benefit of our approach is that using 100% Census data allows us to estimate the effects of migration on entire districts, capturing any spillovers across migrant and non-migrant households arising from remittance sharing across households, local multiplier effects of migrant spending, and general equilibrium effects of migration shocks on local wages.

Our paper contributes to several literatures. First, we contribute to a child labor literature that focuses on understanding how local economic shocks crowd out or crowd in schooling (Edmonds 2009). This literature generally finds that in the wake of transitory income shocks that change the value of children's time, child labor increases and enrollment falls. In contrast, shocks to wealth or permanent income raise investments in schooling and reduce child labor (Edmonds and Schady 2012, Kruger 2007, Kruger et al 2012). Because earnings from labor migration may have components of both transitory and permanent income shocks, and because adult labor migration is almost never the source of income shocks in child labor studies, it is difficult to predict what should happen to human capital in response to circular labor migration. We provide new evidence that even when men do move away, replacement income in the form of remittances or deferred pay does improve education outcomes, but to a lesser extent when children have outside options.

Second, we provide new results that contribute to a relatively small literature on circular labor migration that almost never focuses on African countries, and is seldom able to measure long run impacts of such migration. Existing research provides mixed evidence on the human capital benefits of labor migration (e.g. Antman 2012, Cox-Edwards and Ureta 2003, De Brauw and Giles 2008, Gibson et al 2011, McKenzie and Rapoport 2011, Theoharides 2013, Yang 2008), often measuring the short run impacts of remittances in countries where child labor is less common (e.g. the Philippines, El Salvador and Mexico).[2] Longer run effects of labor migration flows at the district level have not been established. Yet, despite very different outside options for children in Malawi, our estimate of the income elasticity of educational attainment (0.2-0.3) falls squarely between the most credible estimates of this elasticity from

[2] Cox-Edwards and Ureta (2003, for El Salvador), Yang (2008, for the Philippines) and Theoharides (2013, for the Philippines) find that migrant remittances or migration flows increase education spending and outcomes. McKenzie and Rapoport (2011) show that Mexican adult migration to the US reduces schooling among Mexican youth. Theoharides (2013) shows that labor migration demand shocks increase investments in private schooling in the Philippines. Antman (2012) shows that the timing of parental migration to the US increases total educational attainment of Mexican girls.

the literature (Yang 2008 estimates an elasticity of 0.44; Theoharides 2013 estimates a related elasticity of 0.17).

Third, the evidence from Malawi's historical experience is relevant for a significant number of African countries that were also part of South Africa's labor reservoir. During the colonial and early post-colonial periods, the South African Chamber of Mines recruited large numbers of men from Mozambique, Zambia, Zimbabwe, Tanzania, Angola, Lesotho, Swaziland and Botswana in addition to Malawi. Given the scale of this circular migration, it is perhaps surprising that there is so little evidence on the effects of this mass migration on sending countries.[3] Our results are also relevant for any low- income country experiencing substantial outflows of labor migrants and inflows of remittances. To understand the potential long run impacts of this migration on education, we argue it is essential to know more about the characteristics of local labor markets of sending communities, specifically whether children have outside opportunities to work. Additionally, abrupt cessations of foreign guest worker programs may have unintended negative consequences for human capital outcomes in contexts where the net effect of adult labor migration is positive (e.g. as in Theoharides 2013).

We begin by setting out some facts about education in Malawi and child labor during the pre- and post-colonial period. To motivate our empirical strategy, Section 3 presents the background to mine migration from Malawi and local alternatives to migration. Section 4 sets out our data while Section 5 presents the empirical strategy and addresses potential threats to validity. Section 7 presents our results and Section 8 concludes.

2. Education and child labor 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. Consequently, less than 6% of the population was literate in 1945. At independence in 1964, enrollment of school-aged children was under 35%. 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 primary schools did

[3] Lucas (1985) and Lucas (1987) are exceptions that focus on cross country comparisons to get at aggregate effects of this system. He 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.

not increase between 1960 and 1992.[4] By the early 1990s, primary school enrollment had grown to 50% of the relevant age range (UNICEF: http://www.childinfo.org/files/ESAR_Malawi.pdf).

Throughout our analysis period, cost was an important constraint to attending school. Average annual school fees were the same across the country, and until the early 1980s were around 2.75 Kwacha for lower primary and 5.75 Kwacha for upper primary school.[5] Although these amounts appear low, the cost of sending 3 children to school was around 12 % of an annual wage. In addition, parents were responsible for other expenditures such as textbooks, exercise books, writing materials and school uniforms (Heyneman 1980). Following the introduction of universal free primary education in 1994, enrollment increased by 50%, indicating that school fees had indeed been a substantial impediment to enrollment (World Bank and UNICEF 2009). Table 1 shows the proportion of children aged 10 – 19 enrolled in school in 1977 – the only year for which we can compute these rates. At the national level almost 40 % of these children were enrolled in school that year. There is however significant variation in school enrolment rates by district; the enrolment rate in WENELA districts was substantially higher (45%) than that in non-WENELA districts (31%).

Apart from the direct costs of education, children's outside options and obligations also played a role in the demand for education between 1950 and 1990. Table 1also shows the alternatives children had with respect to the use of their time. Apart from attending school, children could work on family farms doing subsistence farming (*mlimi*), in the household, or for other farmers or landlords, including owners of estates producing tea and tobacco for export. Column 1 of panel A of the table shows that rates of child labor in 1977 – again, the only year for which we can compute these rates – were indeed high. Around four in ten children aged 10 to 19 were working for pay for someone else, working without pay on family farms, or working in home production. Furthermore, while there is no variation in the rates of home production across districts (13 %), we see substantial variation in the share of children working outside the home across WENELA (25 %) and non-WENELA (34 %) districts in 1977.

In panels B and C we investigate the origins of these employment differences. One of the main factors distinguishing between high and low child labor districts is the presence of agricultural estates producing crops for export.[6] By the late 1970s, a third of all 10 to 19 year olds in estate districts were working and this number is the same for both WENELA and non-WENELA districts (34 %). In contrast the share was

[4] 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).
[5] Lower primary consists of the first four years of primary school, upper primary the next 4 years (Heyneman 1980).
[6] In the next section, we discuss in more detail the evolution of these estates between 1950 and 1990.

only 25% in non-estate districts with much lower employment in WENELA districts (22 %) than non-WENELA districts (34 %). Strikingly, the non-WENELA districts show the same child labor rates in estate and non-estate districts (column 3). There are at least two reasons for the high employment rates in the estate districts. The first is that the returns to child labor are higher on agricultural estates where children could expect to earn a higher wage relative to what children engaged in household or agricultural chores on the family plot could expect to earn, which was often nothing (Chirwa 1993). Girls were employed as tea or tobacco leaf pickers, while boys were often involved in running errands and sending messages to workers in different parts of the large plantations (Chirwa 1993). The second reason is that families who worked on these estates as tenants may have required their children's assistance without pay, to meet landlord specified quotas. The shadow value of child labor on estates would therefore have been high, especially when coupled with absentee adult males.

We exploit variation in migration incidence across WENELA and non-WENELA districts in both estate and non-estate districts to determine whether migration of males from these districts led to an increase in educational attainment or whether it led to an increase in children working as they substitute for missing males. A comparison of columns 2 and 3 in panels B and C suggests that in districts with easier access to migration and fewer alternative options for children, the incidence of child labor was lower than in districts where migration was more difficult (explained in the next section) and where children had outside options. Note that these outside options existed because of the local technology of production, and were unrelated to the extent of migration. Furthermore, educational enrolment mirrors this result. Enrolment is substantially higher in high migration-few outside option districts than it is in high migration-more outside option districts, at 47 % compared with 39 %. In contrast the school enrolment rate in low migration districts is the same regardless of the availability of outside options at around 32 %. Table 1 provides merely one snapshot in time and in the results that follow we investigate whether long run differences in education outcomes between WENELA and non-WENELA districts depend on the local technology of production.

3. Labor Migration from Malawi: Background and Context

Malawi is today one of the poorest and most densely populated countries in Africa and the world. Over half of the population lives in poverty, commensurate with its 170th (out of 186) ranking on the Human Development Index (United Nations Human Development Report, 2013). The agricultural sector currently employs around half of the labor force and 85% of the population lives in rural areas. Malawi's central location in southern Africa, high population densities, lack of natural resources, and few non-agricultural economic opportunities have all contributed to the historical importance of labor exports in this country's economic profile.

The bureaucratic infrastructure for labor exports had already been established early in the colonial period and our analysis takes advantage of massive and largely unexpected fluctuations in these labor export flows and concomitant remittances in the post-colonial period. To motivate our empirical strategy, the next sections describe the early establishment of the mine labor recruitment system in Malawi, the source of shocks to labor exports between 1950 and 1990, and the relative importance of earnings from mine work versus domestic earnings opportunities during the period.

Circular labor migration from Malawi and labor migration shocks

Throughout the twentieth century, Malawians took advantage of employment opportunities on Rhodesian farms and South African mines to boost local incomes. By mid-century, labor recruitment to both destination countries had become institutionalized. The South African mining industry established a physical presence in Malawi, opening and operating a network of recruiting stations by 1937. These stations were run by the Witwatersrand Native Labour Agency (WNLA, or WENELA), the mining industry's centralized labor recruitment organization that coordinated all recruitment activities outside of South Africa (Crush et al. 1991, pg40; Jeeves 1987).[7]

The centralized and highly bureaucratic procedure of recruitment of foreign labor allowed the mines to maintain a uniform wage across the industry thereby eliminating labor competition within the industry.[8] Colonial Malawi (then 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.

[7] WNLA, or WENELA, merged with the Native Recruitment Corporation, NRC, in 1977 to form The Employment Bureau of Africa or TEBA. Many of the 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 1930s at a time when local agriculture was struggling with the Great Depression and the collapse of tobacco prices (McCracken 2012, Jeeves 1987).

[8] Wilson (1972) and Lucas (1985) provide accounts of how the WENELA and the NRC operated as labor monopsonists in the colonies, keeping wages in the industry low by hiring workers from Mozambique, Northern and Southern Rhodesia, Nyasaland, Lesotho, Swaziland, Tanzania, Angola, Botswana as well as South Africa.

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 1).[9] To put these numbers in perspective, at the peak of labor migration, around 20% of the adult male population was missing from the country. 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. Workers could reengage for subsequent contracts after spending some time at home (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 recruitment agreements restricted the number of workers WENELA could recruit from Nyasaland to a few thousand workers per year. These restrictions were lobbied for by European plantation owners in Nyasaland and in Southern Rhodesia (Zimbabwe) as a way to protect access to a cheap source of labor (Paton 1995, pg 46; Jeeves 1987).[10] Between 1946 and 1959, the WENELA quota increased from 8,500 men to 20,000 men, which represented a relatively small (around 2%) share 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 1 shows the sudden 300% increase in the number of workers recruited by WENELA in the seven years following the new agreement.

This massive increase in employment constituted a large positive income shock to the country. The 1967 labor agreement also required WENELA to withhold two thirds of miner wages until the miners' return to Malawi. This fact will be important for interpreting our results, because it means we can be sure that at least two thirds of earnings were returned to sending communities. Moreover, since miners themselves did not choose the value of their deferred pay, we do not need to be concerned with the usual endogeneity problems associated with migrant remittance choice. In 1967, the value of deferred pay and remittances paid by WENELA was K2.114 million (Malawi Statistical Yearbook 1972, Table 7.9) and 16,907 miners returned from Malawi that year. If we conservatively assume that voluntary remittances are zero, then the

[9] Prior to the 1950s, migrant labor from Malawi to Rhodesia was also prevalent. However, the number of men leaving for Rhodesia never reached the numbers migrating to South Africa; the system of recruitment and remittance flows was never as centralized or organized as it was for WENELA; and by the early 1960s, most of the employment in Rhodesian agriculture had dried up.

[10] The Southern Rhodesian government forced WENELA to restrict the amount of workers WENELA would recruit from Nyasaland (Jeeves 1987). This restriction remained in place until Southern Rhodesia reduced its reliance on the Nyasaland labor supply (Clarke 1977).

total amount of income that a family could have received from a miner returning from a two-year contract would have been about 342.71 Kwacha.[11]

The 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 1) 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, after which, this source of income earning opportunity would have been shut off for potential miners. WENELA recruiters were allowed to restart operations in Malawi in 1977, but employment levels for Malawians never returned to 1970 levels. 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 labor demand (or periods of slow growth in labor demand) from the foreign mining sector. In the empirical methods section, we describe how the periods before and after the employment shocks define our comparison cohorts.

While mine wages may well have been attractive to Malawian farmers, labor migration was still costly. By 1937 WENELA was well established throughout Nyasaland with recruiting stations set up in many districts across the country. We have collected and digitized administrative data on the location of these WENELA stations as of 1937 and show their prevalence across the country in Figure 2. The red hatched areas in the figure represent sub-districts that had a WENELA station; the white areas show sub-districts without a WENELA station; and the thick black borders represent district boundaries. Each of the three geographical regions had some access to a WENELA station.

However, the logistics of getting access to a mining job were neither simple nor costless. The procedure for getting from Malawi to South Africa involved many steps.[12] A man 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

[11] Authors' calculations using mine wages, number of miners and total monies sent back to Malawi.
[12] 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).

'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 going back to the WENELA station to await transportation to a main WENELA depot for transfer to South Africa (Prothero 1974). The costs associated with signing up for mine work were therefore not negligible. They also varied depending on the distance to the nearest WENELA recruiting station.

We use this spatial variation in the number of WENELA stations in a district as our 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 discussion of location choice for WENELA recruiting stations to Section 4, but note here that the time these decisions were being made in the late 1920s and early 1930s, there would have been been little available hard data to influence these choices. Most likely, location placement decisions were made after simple visual inspection of the potential of these areas for recruiting.[13]

Alternatives to labor migration: Employment in Malawi

Our discussion of labor migration has so far ignored domestic economic opportunities for Malawians. To understand how mass circular migration of adult male labor along with related flows of remittance income could have affected educational investments of children, we next describe the changing context of local economic opportunities in rural Malawi between 1950 and 1990.

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.

Malawians remaining at home had essentially three, not necessarily mutually exclusive, options for work in the agricultural sector. Workers could work for wages on the large tea, tobacco, cotton and sugar estates, or as visiting tenants on these farms. Any remaining income earners were absorbed into the peasant smallholding sector (Christiansen and Kydd, 1982) where they also grew cash crops for export, or

[13] 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."

food crops to sell to estate laborers and visiting tenants who had no time or land to cultivate their own food.

Under Banda the 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, however it is not clear how opportunities for local employment and estate wages would have changed the prevalence of labor migration to South Africa (Weyl 1981, Chirwa 1997). Although it is possible that Banda wanted to stem the tide of migration to support the growth of the estate sector, the timing initiated by the crash, was clearly unexpected. [14]

Estate wage-workers typically earned more than visiting tenants and smallholders (Chirwa 1997). Average annual earnings on local estates were 94.4 Kwacha in 1968 rising to 112 Kwacha in 1973 and to 126.80 Kwacha in 1974 (Pryor and Chipeta 1990).[15] In contrast, average miner earnings were at least twice as much as the average agricultural wage in the late 1960s. Miners could earn 183 Kwacha per year in 1966 (Wilson 1972, pg 46) and this grew to 375 Kwacha in 1974 (Crush et al 1991, pg. 19). Mine earnings were therefore always significantly higher than they would have been relative to working on an estate farm in Malawi (Chirwa 1997). Despite the growth of the estate sector immediately after independence and the opportunities for wage employment and tenancy status, the low wages, the challenges associated with being a visiting tenant, and the lack of growth in the small holding sector led many men to continue to seek employment with WENELA.

4. Data and descriptive statistics

To analyze impacts of labor migration on human capital attainment, we collect and assemble data from several sources. We use both district level and individual level census data on human capital attainment and migration histories which we digitize as well as archival records on the location of WENELA stations. We supplement this data with data from various years of the Malawian Statistical Yearbooks. We briefly outline key features of our data in this section and highlight some of the variation in migration flows that we will use in our analysis. The Data Appendix contains further details on sample and variable construction.

Our census data are from the complete 1977 and 1998 population Census' of Malawi and cover 24 districts across all three regions (North, Central and Southern regions). Average population in a district across the period is about 225,000. The 1998 Census is comprised of individual level data while the 1977

[14] 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).
[15] In 1974 K1 (Malawian Kwacha) was worth USD1.22

data are aggregated to district-cohort-sex level. We collapse the 1998 data to the same district–cohort-sex group and match these to the 1977 data, restricting the sample to adults ages 20 to 49 (in five-year age groups) in the 1998 data and 20 to 44 in the 1977.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 these upper ages. 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 65 year old cohorts in 1998. We define which cohorts were age-eligible for primary school during the labor expansion years and during the labor contraction years in more detail in the next section.

To proxy for the costs of getting access to mine work in South Africa, we collect data from administrative records 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 comparisons across exposed and non-exposed cohorts within regions and within districts. This allows us to create better counterfactuals by controlling for differences in outcomes that might arise because economic alternatives to mining differ across large districts and larger regions.

Although the locations of WENELA stations set up long before the period of analysis provide us with a good pre-existing measure of differential migration costs, it is still useful to get some insight into factors that might have been driving placement and that could be potential confounders in our analysis. Table 2 presents correlations between WENELA stations at the district level and a range of historical and geographic variables. We use the number of WENELA stations in the district as the outcome. Interestingly, 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). Certainly, we see some evidence of this in the negative coefficient on the estate dummy: in districts that have large tea or tobacco plantations later in the period, the chances of having a WENELA station in 1937 are lower (although never significantly so). Districts at higher altitude (i.e. more likely to be free of malaria) and districts in the Central region are more likely to have a WENELA station, although these relationships are not always significant. Notably, the level of 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. Overall, Table 2 suggests that there is very little historical and geographic correlation with the 1937 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. These costs of signing up are proxied for by the presence of a local WENELA station. Table 3 provides evidence that the number of WENELA stations in a district predicts substantially higher labor outmigration. The first four columns show results from a regression of the log of the number of men who report working abroad between Census 1967 and Census 1977 on the number of WENELA stations in the district, WENELA region fixed effects, historical literacy and population density, and an indicator for the presence of an agricultural estate in the district as well as the interaction of estate districts with WENELA districts. Exposure to one additional WENELA station in the district significantly raises the flow of labor migrants by 8.1%. It is important to note here that education levels within a district as measured by the local literacy rate in 1945 are not significant predictors of outmigration. If anything, higher literacy rates suggest lower rates of migration.[16] The second set of columns confirms this relationship for a modified outcome, measuring the change in number of migrants per household at the district level between 1967 and 1977. 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 five-year district-cohort-sex level in Panel A and for historical and geographic variables at the district level in Panel B. Much of the data for Panel B comes from newly digitized historical Census data (from Census 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.

Panel A shows that 63% of districts have at least one WENELA station and the average number of stations per district is 2.8. The fraction of cohorts eligible for primary school in either period, and in each of the younger and older comparison cohorts 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 adults aged 20 to 44 (across eligible and ineligible cohorts) is 2.56 years. Average education is 2.85 years in WENELA areas and only 2.08 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.

[16] Importantly, although the coefficient on the WENELA*Estate district interaction term is fairly large, there are no significant differences in the differential flow of migrants out of WENELA and non-WENELA districts across estate and non-estate areas. If anything, migration from estate districts with WENELA stations is higher.

In Panel B of Table 4, we see some differences between WENELA and non-WENELA areas. In WENELA areas, a higher fraction of men report ever working aboard by 1977 (37%). However, the share of men in non-WENELA areas who have ever worked abroad is also high (31%). Population density in 1931 is significantly lower in WENELA districts and the fraction of districts with a tea or tobacco estate is higher among WENELA districts than among non-WENELA districts, although not significantly so.

The last part of this table gives us some idea of how local economic opportunities differed across areas. Using variables constructed from the 1966 Census, we see that men are more likely to be working for a wage in 1966 in non-WENELA areas, and are more likely to report not earning any wage in 1966 in the WENELA areas (earning a wage could include self-employment on own farm, and unemployment). Rates of wage work in farming are similar in both areas, with men more likely to work in non-farm sectors for a wage in the non-WENELA areas. In the face of such differences in economic opportunities at the district level, it will be particularly important to control for district fixed effects in our analysis.

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. Primary education up to around four years of schooling, equivalent to the level of functional literacy, is also important since it is likely to have positive returns in agriculture (Appleton 2000; Foster and Rosenzweig 1996).

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 2. 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 between the ages of 20 and 65 in 1998 and have therefore completed whatever primary schooling they are likely to get. We define four cohorts: the oldest cohort born between 1933 and 1953 is too old for primary school by the start of the labor shock period, 1967. This group ranges in age from 14 to 34 in 1967. The youngest adult cohorts are born between 1974 and 1978, and hence are too young to be eligible for primary school between 1967 and 1997. The eldest in this cohort is only three years old in 1977. These youngest and oldest cohorts together constitute the ineligible group for much of our analysis, although we split them apart in presenting some of our results.

The two middle groups in Figure 3 are our two eligible cohorts. 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 contraction period, as they are all under the age of 10 in 1974. Defining eligibility using the five-year bins introduces some fuzziness into the treatment assignment. Although the control group of ineligibles 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. This coarse definition of cohort eligibility likely attenuates our estimates of the effects of migration on education.

We estimate the following empirical model:

$$\bar{Y}_{asd} = \beta_0 + \beta_1 Eligible67\text{-}73_{asd} * WENELA_d + \beta_2 Eligible74\text{-}77_{asd} * WENELA_d + \beta_3 Eligible67\text{-}73_{asd} + \beta_4 Eligible74\text{-}77_{asd} + \beta_5 WENELA_d + G_d{'}\pi + X_{as}{'}\gamma + \lambda_d + \varepsilon_{asd} \quad (1)$$

where $\bar{Y}_{asd}$ is average years of schooling attained or share of adults with any primary school by district-cohort-sex cell, *Eligible67-73* and *Eligible74-77* are binary variables denoting age-eligibility for primary school during each period of the labor shock years 1967 to 1977 as illustrated in Figure 2, and $WENELA_d$ is a count variable of the number of recruiting stations in the district by 1937. G_d contains district-specific historical variables and the interaction of these variables with a linear trend term: the log of historical population density measured in 1931 and the share of literate adults in 1945. X_{as} is a set of demographic controls (age group and sex) and λ_d is a district fixed effect.

The main parameters of interest in equation (1) are those associated with the difference-in-differences terms, *Eligible67-73*WENELA* and *Eligible74-77*WENELA*. β_1 and β_2 capture the differential difference in education gaps between age eligible and age ineligible 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 and younger 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 cohorts. Differences between β_1 and β_2 tell us about the relative education impacts of the positive migration shock versus the labor ban.

Identification of β_1 and β_2 relies on 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. Of course, districts with different numbers of WENELA stations may differ on dimensions other than exposure to the migration shocks, for example, they could be wealthier districts, or have more schools, or more land for cultivation. Using the ineligible cohorts allows us to control for counterfactual differences in educational attainment across WENELA and non-WENELA districts. As with any difference-in-differences design, our identification assumption is that, conditional on controls, non-WENELA districts should provide a valid counterfactual for WENELA districts. We will show parallel education pre-trends in the older comparison cohorts to support this assumption.

Equation (1) contains several controls that further limit concerns about confounding factors arising from underlying unobservable and observable differences between districts and from short, sharp, confounding and differential shocks to the local economy. Since geography, agricultural conditions, weather and population density vary much more down the length of the country and across regions and districts, we eliminate many potentially confounding factors controlling for region and district level fixed effects. To make sure that our estimates are not driven by differential trends across areas with baseline characteristics, we control for interaction terms between a linear trend and historical population density in 1931, historical literacy rates and region indicators.[17] This strategy also addresses concerns about mean reversion in education across districts.

Our identification strategy also 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 Section 2), 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 $WENELA_d$) 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. To address this concern, we use information from the 1977 Census on the district, age and sex-specific prevalence of internal migration between 1967 and 1977 to create bounds for our education estimates in equation (1). Implementing these bounds does not change any of our main results. Web Appendix 2 further discusses how we implement this bounds analysis and the results of this exercise.

Conditional on our identification assumptions, β_1 and β_2 capture the causal effect of exposure to the migration expansion and subsequent labor contraction on long-run average education. We treat these two

[17] For example, the Northern Region was the birthplace of formal schooling in Malawi (Heyneman 1980), originally established by missionaries. It is plausible that these different initial conditions with respect to education could influence subsequent trends in education across regions.

periods 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. During these six years, men are missing from the local economy, and money flows back. If $\beta_1<0$, this implies that for these years the negative substitution effect (SE) of children for missing male workers outweighs the positive income effect (IE) from increased migrant earnings. Alternatively, if $\beta_1>0$, the positive IE on educational investments dominates any crowd out of education during the labor expansion years. Note that because we cannot know whether miners viewed the labor expansion as signaling permanently new opportunities to work in South Africa, the IE may be driven by a combination of temporary positive shocks and from a permanent positive shock to lifetime wealth.

In contrast, in the later period between April 1974 and 1977, the missing men all return to rural Malawi, almost overnight. Returning migrants bring home their accumulated deferred pay and other savings, and the payout of this money occurs in 1974. Until the end of the labor ban period in 1977, children would no longer need to substitute for missing male labor. Given the size of the earnings payouts to returning migrants, the labor ban implied a large positive and temporary shock to income, which should serve to increase 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 SE 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 $\beta_2<0$.

In the final part of our analysis, we test for the size and signs of β_1 and β_2 across estate and non-estate samples. This allows us to learn about how the net effects of this labor migration differ depending on whether kids had significant outside options for work available to them at the time, based on local technologies of production. As noted in section 2, the tea, tobacco, sugar and cotton estates provided children with more viable outside options for work. Our hypothesis is that if there are any substitutions between missing male labor and child workers, we should see evidence of smaller net effects of access to South African mine work on the long run educational attainment of adults from estate districts, compared with adults in non-estate districts. We therefore test whether $\beta_{1,estate} < \beta_{1,non\text{-}estate}$ and $\beta_{2,estate} < \beta_{2,\ non\text{-}estate}$..

To aid interpretation of results, it is worth noting two additional features of the empirical setting of migration from Malawi, and the implementation of our district-level measure of exposure in equation (1). First, because mine migration from Malawi is so prevalent, no districts are completely untouched by the labor expansion and contraction. However, as Table 4 shows, districts with at least one WENELA station experience a much larger increase in the number of men migrating abroad between 1967 and 1977, so we can estimate the differential impact of additional migration (and return flows of men, and money) across high and low shock intensity districts.

Second, β_1 and β_2 capture the total impact of these migration shocks on the district. This includes any direct or indirect spillovers from migrant to non-migrant households.[18] Direct spillovers could occur if migrants share earnings across households. Indirect spillovers would arise from local multiplier effects of returned income, or through general equilibrium wage impacts in the wake of departing and returning migrants. Given the size of migrant flows from the country as well as the average deferred pay amounts collected by migrants upon return, many of these indirect effects are likely, and could be very large.

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

$$\bar{Y}_{asd} = \alpha_0 + \alpha_1 Eligible67\text{-}73_{asd} * \Delta LnMigrants_d + \alpha_2 Eligible74\text{-}77_{asd} * \Delta LnMigrants_d + \alpha_3 Eligible67\text{-}73_{asd} + \alpha_4 Eligible74\text{-}77_{asd} + \alpha_5 \Delta LnMigrants_d + G_d' \kappa + X_{as}' \phi + \tau_d + \omega_{asd} \quad (2)$$

To define the main treatment variables, we interact the cohort eligibility dummies with the change in log number of male migrants at the district level between 1967 and 1977 ($\Delta lnMigrants67\text{-}77_d$).[19] We construct $\Delta lnMigrants67\text{-}77_d$ by subtracting 1977 district-level data on the stock of men in each district who returned from working abroad since 1967 from 1966 district-level data on the stock of adult men working abroad in 1966. This difference measures how many additional migrants left and returned to Malawi over the ten year period and varies only in the cross-section. Since the labor ban was still in place in 1977, we have a complete picture of migration at the district level. We instrument for these interaction terms using $Eligible67\text{-}73_{asd}*WENELA_d$ and $Eligible73\text{-}74_{asd}*WENELA_{d.}$ All other controls are the same. The exclusion restriction is that conditional on controls, the interaction of the district-level $WENELA_d$ and the eligibility dummies only predicts the interaction of the migrant flow variable with the eligibility dummies. Estimates of α_1 and α_2 tell us: for 1% more labor migration during the shock years (which means 1% more men leaving, and 1% more men returning), how much more education do eligible cohorts gain, controlling for differences in education outcomes across high and low migrant shock districts using ineligible cohorts.

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[18] Theoharides (2013) is one of the only other papers we are aware of that captures this total impact of migration shocks, including all spillovers from migrant to non-migrant households.

[19] We cannot use migrant flows in the early versus late periods, as we only have a measure of migrant stocks from 1966 and 1977 Census data, from which we generate the change in migrant flows measure at district-level.

6. Empirical Results

i. Long run education effects of exposure to mine employment shocks

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 four columns present results for years of education attained and the next four columns show results for the share of adults with any primary schooling. For each outcome, we show results from the raw difference-in-differences comparisons without any controls, then include baseline and demographic controls as in equation (1), then include district fixed effects, and finally include region-specific trends in our most comprehensive specification. Robust standard errors are clustered at the district level and since there are only 24 districts, we report significance levels using the small sample *t* distribution.[20] *p* values for the test of equality of β_1 and β_2 appear in the last line of the table.

Without any controls, we can see that educational attainment and any primary school enrolment is higher among cohorts in WENELA districts eligible for primary school during the labor expansion and during the labor contraction periods. These effects are large and statistically significant. For each additional WENELA station, individuals eligible in the early period gain 0.1 more years of education and are 1.1 percentage points more likely to have ever been to school. For each additional WENELA station, individuals eligible in the later period gain 0.16 more years of education and are 1.7 percentage points more likely to have ever been to school. Education is also higher on average (by 0.06 of a year) for each additional WENELA station in the district, and eligible cohorts have more education on average than the ineligible younger and older control cohorts.

Columns (2) to (4) show that adding in our set of controls contributes a great deal to the variation in outcomes. Even with these controls in the regression though, the impacts of exposure to the labor migration shocks between 1967 and 1977 remain positive, robust, and significant. We briefly describe magnitudes here for models with all controls (columns 4 and 8), and postpone further interpretations to the next section. For each additional WENELA station in the district, cohorts eligible for school during the expansion and contraction of mining employment gain an additional 0.089 to 0.135 years of education and are between 1.1 and 1.7 percentage points more likely to have ever been in primary school. These estimates represent a 3.4 to 5.2% gain in total years of education and a 2.1 to 3.1% gain in the share with any primary school. For both outcomes, once all controls are included, we can reject that $\beta_1 = \beta_2$ at the 5% or 10% level (*p*-values are 0.04 and 0.06 for total education and share with any primary school,

[20] 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).

respectively). Cohorts in WENELA districts exposed to the labor contraction gain significantly more education than cohorts in WENELA districts exposed to the labor expansion, controlling for educational gains of similar cohorts in non-WENELA districts.

As noted in the previous section, internal migration between childhood and adulthood could have led to a spurious relationship between measures of exposure to mine employment shocks in childhood and education outcomes measured in adulthood. This is particularly the case if internal migrants have substantially more, or less, education than the average level of education of their origin and destination districts. Web Appendix 2 shows that our results are robust and still large and positive after accounting for the two extreme types of composition effects driven by internal migration.

Instead of using only two cohort dummies and one control group to measure education gaps between eligible and ineligible cohorts across WENELA and non-WENELA areas, we can use the full range of age groups across the 1977 and 1998 Census samples to illustrate the long run human capital effects of exposure to circular migration shocks. Figures 4 and 5 show how differential exposure to these 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 in WENELA districts.

Figure 4 presents coefficients from a regression of total years of education 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, controlling for gender, demographics and baseline controls, district and region fixed effects. Vertical lines demarcate our treatment and comparison cohorts: those too old for primary school in 1967 (ages 45 to 64 in 1998), those eligible for primary school during the labor expansion (ages 34 to 44 in 1998) and the labor contraction (ages 25 to 34 in 1998), and those too young for primary school before 1977 (ages 20 to 24 in 1998). The black line in Figure 4 represents regression-adjusted education levels among cohorts from districts with the mean number of WENELA stations; the dotted line represents regression-adjusted education levels among cohorts in districts with no WENELA stations.

Several important features of the figure stand out. First, the trends in educational attainment are parallel for the pre-1967 comparison cohorts and for the post-1977 cohorts. Second, education is increasing over time in both WENELA and non-WENELA areas, with large increases beginning in the cohort eligible for primary school in the 1967 to 1974 period (we explicitly do not control for trend in this figure so that we can see the trend over time). To understand the increase in education from just over one year to over four years in non-WENELA areas, we need to recall that even non-WENELA districts were likely affected by the expansion and contraction in foreign employment – only to a lesser extent than WENELA districts.

Our identification strategy relies on using WENELA stations to isolate *larger* migration flows in some districts relative to others. The figure clearly shows how this greater exposure to the same employment shocks enabled WENELA districts to overtake non-WENELA districts in the accumulation of human capital. While WENELA areas begin with *lower* average education among the older cohorts relative to non-WENELA areas, WENELA areas start to overtake average education levels in cohorts 40-44 years and 35-39 years who are eligible for primary school just as employment on the mines begins to expand.[21] Education levels in WENELA areas remain above education levels in non-WENELA areas for the period of labor contraction.

To better illustrate these cohort-specific changes in this education gap between WENELA and non-WENELA areas, we plot the interaction term (i.e. the difference between the two lines in Figure 4) by five-year age cohort in Figure 5, along with confidence intervals for these differences. The black line clear shows education levels in the WENELA areas overtaking education levels in the non-WENELA areas starting with the 40-44 year old cohort, and continuing until the youngest cohort (age 20-24), when the differences are no longer statistically significant. Together, Table 5 and Figures 4 and 5, 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. Even after mining labor opportunities disappeared in Malawi, districts that had been most exposed to the labor shocks continued to invest in higher levels of education in the youngest cohorts, although no longer at increasing rates across cohorts.

ii. Discussion of magnitudes

Our evidence suggests that exposure to circular labor migration allowed rural communities with better access to mining jobs to invest in education and overtake communities with less access to mining jobs in terms of human capital accumulation. Districts with easy access to mine jobs continued to have cohorts with higher levels of education, even after the cessation of migration. The difference-in-differences results in Table 5 show that these long run education effects were large: exposure to the labor expansion increases total years of schooling among eligible cohorts by about 0.089 years, while exposure to the labor contraction raises total years of schooling by an even larger 0.135 years. Adjustments occur on both extensive and intensive margins: kids going to school stay in school for longer and there are more kids going to school overall, as indicated by the increase in share of adults with any primary schooling. These

[21] While it may be somewhat tempting to attribute the increasing trends in education that start with the 45-49 year old cohorts as being the result of a post-independence exuberance, this reason cannot explain the *difference* in educational attainment across WENELA and non-WENELA areas.

positive primary school enrollment effects account for between 13% and 20% of the total increase in enrollment rates between 1967 and 1978.[22]

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 in Table 5, we can scale the reduced form difference-in-differences results using the estimates from Table 2. From Table 2, we know how the presence of a recruiting station induced migration flows out of districts between 1967 and 1977. Without any other controls, each WENELA station induces 8% more labor migrants. From Table 5, we know that for each additional WENELA station, total years of schooling is higher among eligible cohorts by between 0.1 and 0.16 of a year, excluding other controls. Combining these pieces of information, we can construct the Wald estimate for the impact of labor migration on education. In districts with 1% more labor migration, total educational attainment increases by between 1.25 (0.1/0.08) and 2 (0.16/0.08) years.[23]

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 trends. F-statistics from each of the first stage regressions are over 11. Once all controls are included, we estimate that for each 1% increase in migrants at district level, eligible cohorts gain between 1.043 and 1.53 more years of education and are between 10.5 and 14.5 percentage points more likely to have ever been to primary school. Compared with average levels of education and enrolment, these effects translate into an increase in education of between 40 and 60%, and an increase in any primary schooling of between 25 and 35%. More children are going to school, and more of them stay in for longer, in districts with more outmigration. These results are significant at the 1% level.

Another way to think about these results is in terms of the number of households affected. Table 2 showed us that each additional WENELA station raised the share of households with a migrant worker by 2.3%. In numbers, this represents an additional 1,430 migrants across about 30,000 households. Now suppose we assume assumption that all of these education gains are generated through migrant households only with no change in the education of kids in non-migrant households. This implies that on average, children in migrant households eligible for primary schooling between 1967 and 1973 attain 3.8

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[22] 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.

[23] This is not quite the Wald estimate, since the unit of observation in Table 2 is the district, while in Table 5 it is the district-cohort-sex group.

more years of education (0.135/0.0233). Similarly, children in migrant households eligible for primary schooling between 1974 and 1977 attain 5.7 more years of schooling (0.135/0.0233).

Clearly, these numbers are far too higher to be sensible, and the assumption that migration of this scale has no spillover effects to non-migrant households is implausible. 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, especially if families share resources within the extended family network. Local multiplier effects from the spending of this additional income as well as general equilibrium impacts on wages of left behind workers are another potential source of local spillovers. This implies that labor migration likely affected more children than those from migrant households. While the coefficients on number of recruiting stations in Table 2 reflect how much additional migration occurred in districts with more WENELA stations, these coefficients do not capture the percent change in individuals and households affected by this migration. For this reason, we prefer either the reduced form estimates of the impact of this labor migration, or the IV scaled results that tell us how an entire district is affected by differential growth in migrant flows due to recruiting station placement. Both of these measures capture any spillover effects of labor migration to non-migrant households.

Because rural Malawi presents a very different context for studying the effects of labor migration on sending communities, it is useful to compare our estimates of the education effects to other estimates in the literature. We can do this by computing a back-of-the envelope estimate of the district-level income elasticity of schooling. Note that because we estimate a positive impact of exposure to the labor ban period on long run human capital ($\beta_2>0$), we treat this elasticity as a current income elasticity rather than a permanent income elasticity. The income elasticity of education is given by the percent change in schooling divided by the percent change in income at the district level. The IV estimates of α_1 and α_2, divided by average education gives us the percent change in years of education for a 1% increase in migrants. If we assume that every returning migrant bring home at least two thirds of their mine earnings income through the deferred pay system, this is equivalent to three times their annual income from estate work, or a doubling of income. Putting these two estimates together, we calculate an income elasticity of 0.2 ((1.043/2.56)/2) during the labor expansion years and a higher elasticity of 0.3 during the labor contraction ((1.53/2.56)/2).[24] The greater we assume the return of earnings was for migrant workers, the smaller these income elasticities become. Despite very different outside options for children in Malawi, our estimates fall squarely between the migration literature's most credible estimates of this income

[24] For the early period, the percent change in education is 1.043/2.56, while the percent change in income is 2, giving an elasticity of (1.043/2.56)/2=0.17. Similar calculations apply to the later period.

elasticity. For comparison: Yang (2008) estimates an elasticity of school enrollment with respect to remittance income in migrant households in the Philippines of 0.44, while Theoharides' (2013) estimates the elasticity of high school enrollment with respect to average migrant labor demand shocks, also in the Philippines of 0.17.[25]

iii. Mechanisms

Both difference-in-differences results of Table 5 and IV results of Table 6 show that exposure to the labor expansion and labor contraction increased human capital attainment of children eligible for primary schooling during the period. The positive estimate of β_1 implies that any positive income effects of this labor migration dominate any potential negative substitution of child for adult labor. The positive estimates of β_2 imply that workers did not treat the later labor contraction period as a negative shock to permanent income.

For both the years of education outcomes and the primary school enrolment outcome, we can reject the null that β_1 and β_2 are the same at the 1%, 5% or 1% level (*p* values range from 0.00 to 0.06). We also find that $\beta_2 > \beta_1 > 0$: are statistically larger for cohorts' eligible during the labor contraction period, when men have returned home. There are two possible interpretations for this difference. One possibility is that during the labor contraction period, the substitution effect is no longer in operation, since men have returned home, while the income effect is present, 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 amount of income returning to Malawi in the first period is smaller than that returning in the second period. Although total remittances and deferred payments from miners are roughly the same in both periods (ZAR46 million in the early period and ZAR 50 million in the later period, Malawi Statistical Yearbook 1975) per year 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 income is returning to each district in Malawi during the labor expansion.

Evidence from our final set of results is, however, supportive of a substitution effect operating in the early labor expansion period. We look at how our difference-in-differences estimates vary across districts with

[25] Yang (2008) estimates an elasticity of education spending with respect to remittance income of 0.55 in his sample of Filipino migrant households. We use his regression results to estimate that an elasticity of enrollment with respect to remittance income of 0.44. Theoharides (2013) 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).

more opportunities for child work versus fewer outside options. This exercise also helps us see what factors mitigate the long run impacts of circular migration on educational attainment.

As Section 2 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, providing opportunities for kids to work during the period. In particular, 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.[26] All of the positive long run education effects we estimate for eligible cohorts in high exposure districts are driven by variation in access to mine work among districts without large agricultural estates.[27] In these districts, cohorts eligible for primary school between 1967 and 1977 have between 0.10 and 0.156 more years of education and are between 1 and 1.5 percentage points more likely to be in school. These estimates are large, and statistically significant, and we can reject $\beta_1=\beta_2$ for this non-estate sample at the 1% level.

In contrast, point estimates for the sample of estate districts are smaller than for the non-estate sample. We can, however, also reject the equality of $\beta_1=\beta_2$ for the estate sample at the 5% level (*p* value is 0.05). Comparing across samples, we can reject that $\beta_{1,estate}=\beta_{1,non\text{-}estate}$ for years of education but not for primary schooling, and we can reject that $\beta_{2,estate}=\beta_{2,nonestate}$ for both outcomes (*p* values are 0.011 and 0.03).

These differential effects of exposure to mine employment shocks on long run educational attainment in estate and non-estate districts link up well with the cross-sectional variation in rates of child labor across estate and non-estate districts that we say in Table 1. Our results indicate that where child labor was less valuable, (i.e. in non-estate districts), circular labor migration and concomitant remittance flows substantially increased investments in schooling. 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

[26] See the Data Appendix for a discussion of how we define estate districts. Because of the coarseness of this measure, there is variation within estate districts in the prevalence of estate lands out of total district agricultural lands.
[27] Recall from Table 3 that migration flows between 1967 and 1977 are not significantly different across estate and non-estate districts.

education are harder to discern, and much more muted in the estate areas of the country. The general lesson we draw from this evidence is that the local technology of production strongly influences the effects that circular labor migration has on families left behind.

7. Conclusions

We have used two waves of complete Census data from 1977 and 1998 to 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 accumulation of rural sending districts. 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, labor migration has larger impacts on human capital when the local technology of production provides fewer outside options for child work. Said differently, a high shadow value of child work tends to mute the long run impacts of labor migration on human capital accumulation of the next generation.

These results have broad relevance. Circular migration is considered one of the most immediate ways out of poverty for families, and potentially for whole sending communities. Our paper uses newly digitized Census data and administrative data on access to migration opportunities to provide direct evidence on one of the channels through which this migration can positively impact the lives of those left behind, even in contexts where child labor is highly prevalent. Given the centrality of human capital accumulation in the development process, our results suggest that better access to foreign work opportunities and foreign incomes could enable poor, rural communities to lay the foundations for economic growth by investing in the education of the next generation.

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Figure 1: Annual recruitment of Malawian miners to South African mines, 1950-1994

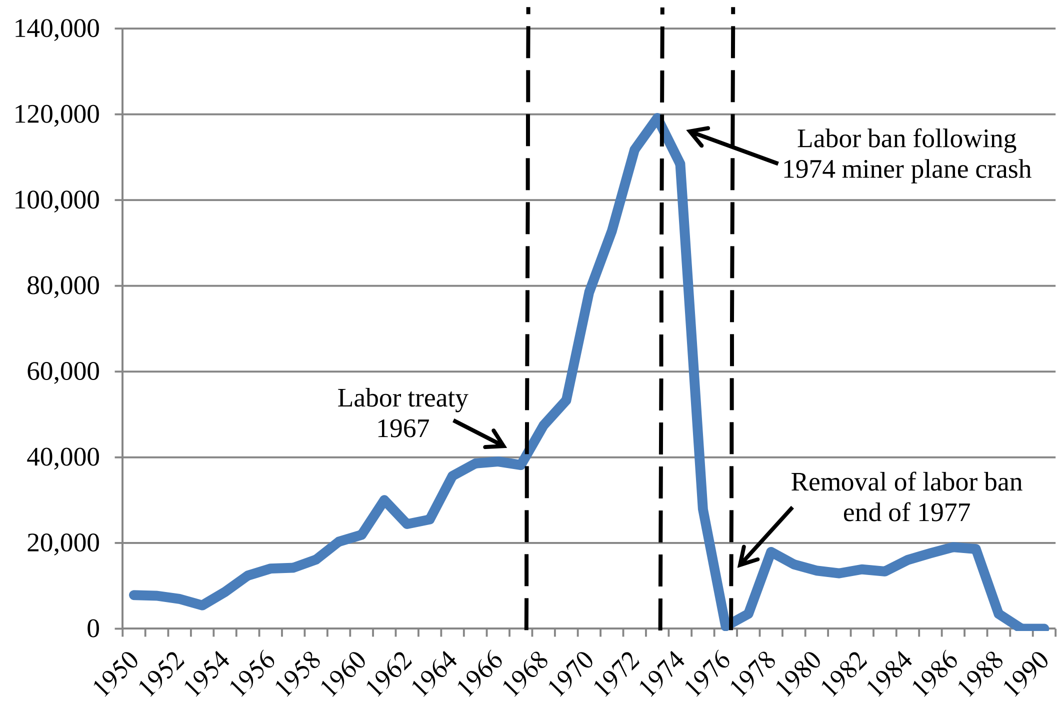

Figure 1 shows number of Malawian workers recruited to work on mines in South Africa in each year, 1950-1994. The three dotted lines represent (from left to right) the initiation of the new recruiting agreement in August 1967, the moratorium on migration to South Africa after the April 1974 Malawian plane crash and the legal resumption of mine migration to South Africa in 1978.

Figure 2 The spatial distribution of mine recruiting stations across Malawi

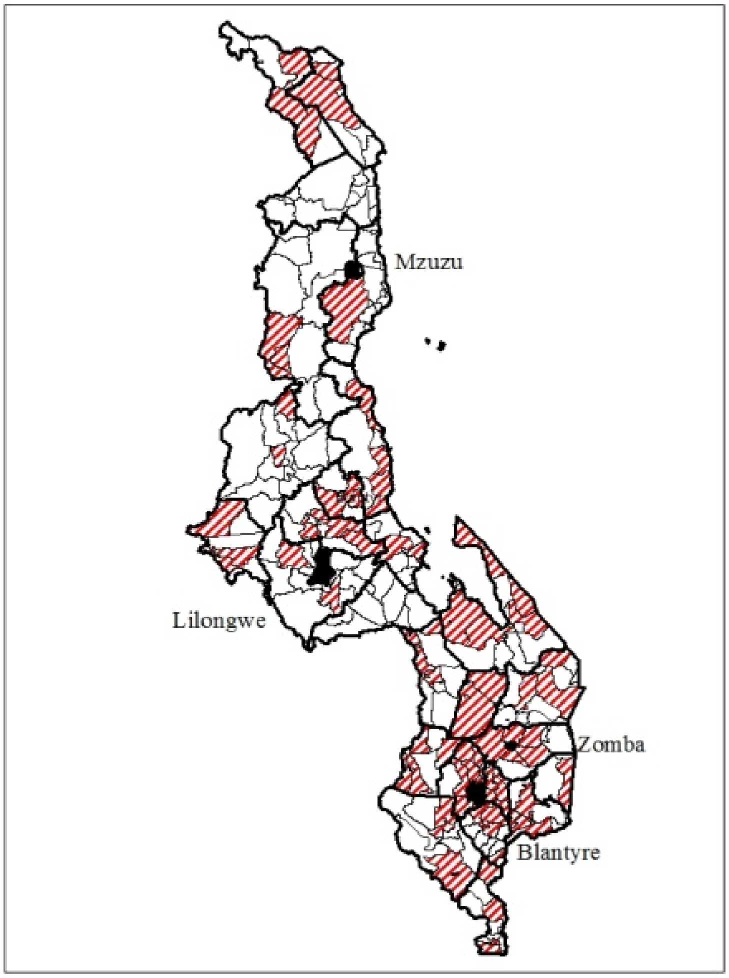

Figure 2 shows district boundaries (thick black lines), sub-district/traditional authority boundaries (thinner black lines) and the distribution of WNLA recruiting stations established by 1937 (red hatched areas) across the country. Malawi's four cities are shown as black shaded areas.

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

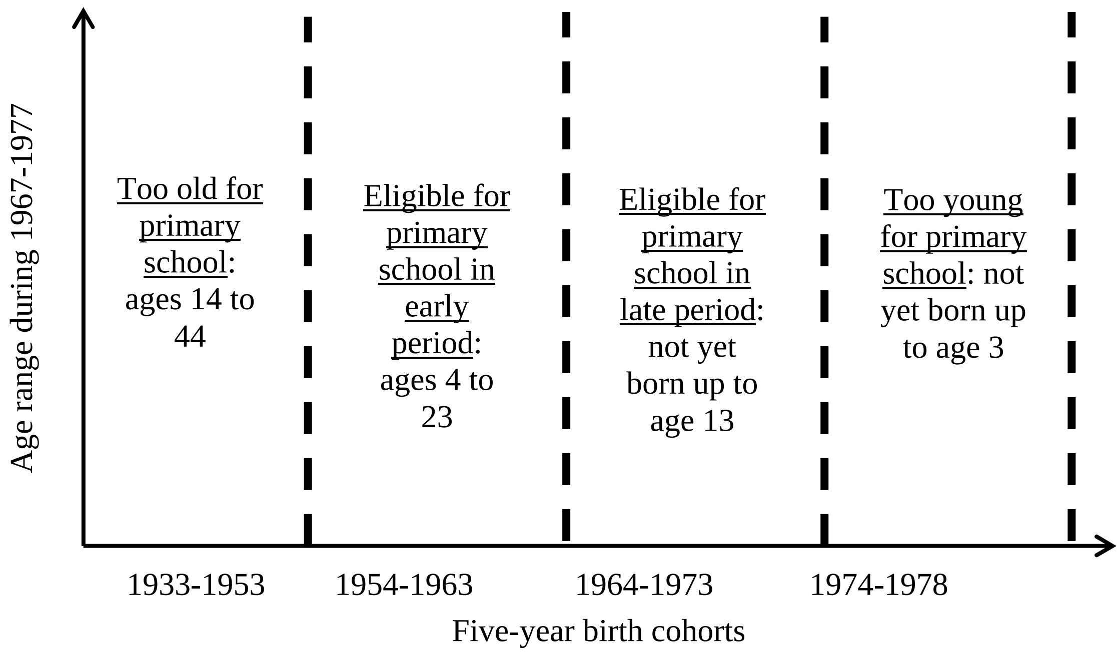

Figure 3 shows how cohorts are determined to be eligible and ineligible for primary school during the labor shock years, 1967 to 1977. The horizonal axis shows year of birth, the vertical axis shows the range of ages between 1967 and 1977. Our four groups are the older comparison group, the group eligible for primary school during the early period, the group eligible during the later, labor ban period, and the youngest ineligible group. All individuals are between 20 and 65 in 1998.

Figure 4: Mean education by age group and Wenela status of district

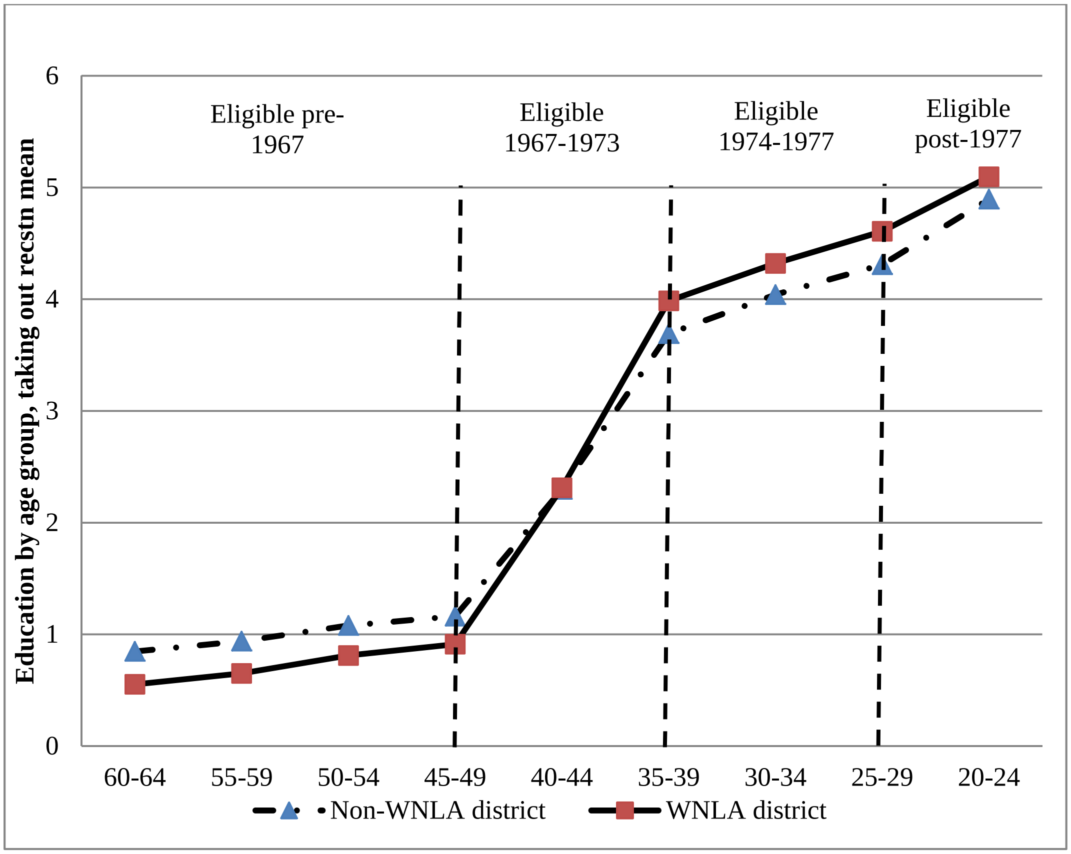

Figure 4 shows age group coefficients from a regression of total years of education on nine age group dummies and their interaction with number of WNLA stations in the district, controlling for female, log population density in 1931, share literate in 1931 and region fixed effects. The solid line shows the interaction term coefficients evaluated at the mean number of WNLA stations in a district (2.79).

Figure 5: Differences in mean education by age group and Wenela status of district

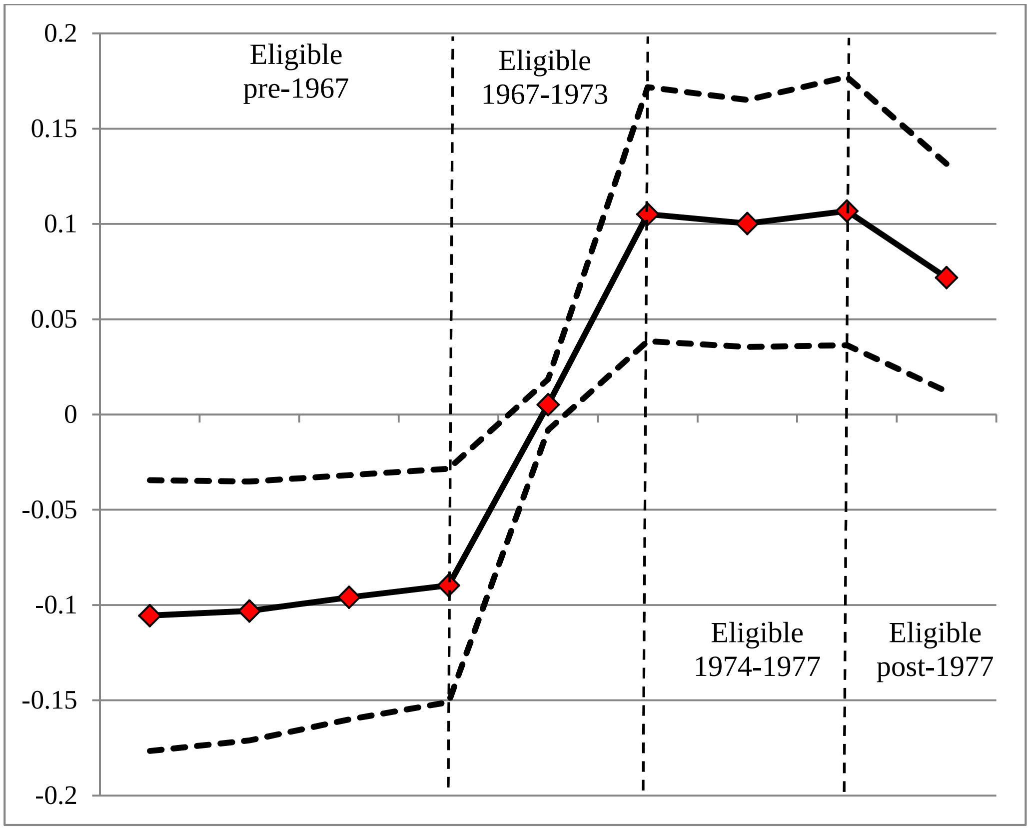

Figure 5: shows estimated interaction term coefficients and 95% confidence intervals from a regression of total years of education on nine age group dummies and their interaction with a WNLA station indicator and other control variables as in Figure 4.

Table 1: Shares of children in school and share working in districts with and without mine recruiting stations

	Full sample	Wenela Recruiting Districts	Non-Wenela Districts	*p* value of difference
		Panel A: All districts		
Share in school	0.39	0.45	0.31	***
Share working: Overall	0.42	0.38	0.47	*
Share working: for wages or as *Mlimi*, as employee or employer	0.29	0.25	0.34	***
Share working: in home production	0.13	0.13	0.13	
Share not in school/working	0.19	0.18	0.21	*
N	108	64	44	
		Panel B: Districts with agricultural estates		
Share in school	0.34	0.39	0.31	
Share working: Overall	0.47	0.47	0.48	
Share working: for wages or as *Mlimi*, as employee or employer	0.34	0.34	0.34	
Share working: in home production	0.13	0.13	0.14	
Share not in school/working	0.18	0.14	0.21	**
N	40	16	24	
		Panel C: Districts without agricultural estates		
Share in school	0.42	0.47	0.32	***
Share working: Overall	0.38	0.35	0.47	*
Share working: for wages or as *Mlimi*, as employee or employer	0.25	0.22	0.34	***
Share working: in home production	0.13	0.13	0.13	
Share not in school/working	0.19	0.19	0.21	
N	68	48	20	

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. Data are from the 1977 Census and cover rural areas of all districts. An observation is a district-five year age cohort-gender group for cohorts 10-14 and 15-19 (employerment questions are not asked of children younger than 10). The share not in school/working includes: unemployed/inactive, independents, and no data.

Table 2: Historical and geographic correlates of Wenela recruiting stations at district level

	Number of recruiting stations in the district				*Any recruiting station in the district*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log population density, Census 1931	-0.979	-1.206*	-0.758	-0.176	-0.332***	-0.385***	-0.296***	-0.088
	(0.595)	(0.585)	(0.639)	(1.035)	(0.089)	(0.079)	(0.100)	(0.094)
Estate district		-1.489	-0.891	-0.603		-0.343*	-0.217	-0.109
		(1.126)	(1.230)	(1.535)		(0.189)	(0.174)	(0.116)
Altitude (meters)^*100			0.288**	0.125			0.07***	0.018
			(0.117)	(0.204)			(0.020)	(0.019)
Literacy rate in 1945			2.678	10.010			-0.137	0.955
			(17.140)	(17.080)			(2.380)	(1.250)
Central region				1.019				0.148
				(2.128)				(0.151)
Southern region				-1.411				-0.619
				(3.961)				(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. Log population density and the estate dummy 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

	Ln Number of new men working abroad between 1967 and 1977				*Average Δ in number of men working abroad between 1967 and 1977 per household*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of Wenela stations	0.082**	0.106***	0.106***	0.0817**	0.03**	0.0351***	0.0356***	0.0233*
	(0.029)	(0.022)	(0.022)	(0.031)	(0.011)	(0.010)	(0.010)	(0.013)
Central region		0.654**	0.723**	0.875**		0.259*	0.294*	0.370**
		(0.290)	(0.328)	(0.354)		(0.136)	(0.146)	(0.160)
Southern region		0.572*	0.703*	0.907**		0.162	0.233	0.335
		(0.300)	(0.341)	(0.400)		(0.145)	(0.158)	(0.192)
Literacy rate in 1945		-2.588	-3.077	-4.581		-1.625	-1.817	-2.57
		(3.302)	(3.520)	(4.177)		(1.349)	(1.467)	(1.800)
Log population density in 1931			-0.128	-0.275			-0.0647	-0.139
			(0.126)	(0.210)			(0.067)	(0.108)
Estates			-0.0855	-0.561			-0.0185	-0.257
			(0.193)	(0.460)			(0.080)	(0.196)
Estate district*Number of Wenela stations				0.122				0.0609
				(0.083)				(0.038)
N	24	24	24	24	24	24	24	24
R2	0.20	0.58	0.59	0.64	0.14	0.52	0.55	0.61

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. Unit of observation is the district. Outcome in columns (1)-(4) is the change in the (log) number 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 change in the number of male migrants per household in each district between 1967 and 1977. Number of WNLA stations is a count variable of all stations in the district in 1937, estate is a dummy for whether the district contains a tea, tobacco, sugar or cotton plantation.

Table 4: Summary statistics for Census data

	Full sample		Wenela Recruiting Districts		Non-Wenela Districts		*p* value of difference
	Mean	s.d.	Mean	s.d.	Mean	s.d.	
Panel A: Outcomes and control variables for District-5 Year Age Group-Sex cells							
Variables measuring exposure to mining employment shocks							
Number of WNLA stations	2.79	2.77	4.47	2.19	0.00	0.00	0.00
Any WNLA station	0.63	0.48	1.00	0.00	0.00	0.00	0.00
Eligible for primary school in 1967-1973	0.30	0.46	0.30	0.46	0.30	0.46	0.50
Eligible for primary school in 1974-1977	0.20	0.40	0.20	0.40	0.20	0.40	0.50
Younger comparison cohorts: Eligible after 1977	0.10	0.30	0.10	0.30	0.10	0.30	0.50
Older comparison cohorts: Eligible before 1967	0.40	0.49	0.40	0.49	0.40	0.49	0.50
Education outcomes							
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
Control variables							
Female	0.50	0.50	0.50	0.50	0.50	0.50	0.50
Log Population density 1931	2.90	0.77	2.58	0.58	3.42	0.76	0.00
English and vernacular literacy, youth in 1945	0.08	0.03	0.09	0.04	0.06	0.02	0.00
Number of observations	480		300		180		
Number of districts	24		15		9		

…continued

Table 4 (continued): Summary statistics for Census data

	Full sample		Wenela Recruiting Districts		Non-Wenela Districts		*p* value of difference
	Mean	s.d.	Mean	s.d.	Mean	s.d.	
Panel B: Geographic and historical district variables							
Any Wenela station	0.63	0.49	1	0	0	0	0.00
Number of Wenela stations	2.79	2.83	4.47	2.26	0	0	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 ln number of migrants, 1967-1977	0.14	0.52	0.24	0.58	-0.03	0.37	0.09
Area (km squared)	3,145	2,249	4,301	2,320	2,743	1,849	0.04
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
Population density 1966	59.27	50.56	35.73	22.54	98.51	60.74	0.00
District contains an estate	0.21	0.41	0.27	0.46	0.11	0.33	0.17
English and vernacular literacy, 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 with a farm wage 1966	0.34	0.14	0.36	0.15	0.30	0.13	0.16
Fraction of men with a cash wage 1966	0.16	0.10	0.13	0.08	0.21	0.10	0.02
Fraction of men with another source of wage 1966	0.14	0.04	0.13	0.05	0.15	0.04	0.19
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, 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

	Total years of schooling attained				*Share with any primary school*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Eligible 1967-1973*Num. Wenela stations	0.103***	0.079**	0.079**	0.089**	0.011***	0.008***	0.008***	0.009***
	(0.033)	(0.034)	(0.035)	(0.035)	(0.003)	(0.003)	(0.003)	(0.003)
Eligible 1974-1977*Num. Wenela stations	0.168***	0.111**	0.111**	0.135**	0.017***	0.011**	0.011**	0.013***
	(0.052)	(0.052)	(0.053)	(0.054)	(0.005)	(0.004)	(0.004)	(0.004)
Eligible 1967-1973	0.978***	-0.671***	-0.671***	-0.699***	0.154***	-0.090***	-0.090***	-0.091***
	(0.130)	(0.110)	(0.112)	(0.119)	(0.016)	(0.010)	(0.010)	(0.011)
Eligible 1974-1977	2.391***	-0.366**	-0.366*	-0.433**	0.355***	-0.036**	-0.036**	-0.039**
	(0.219)	(0.173)	(0.176)	(0.194)	(0.026)	(0.015)	(0.015)	(0.016)
Num WNLA Stations	0.066***	0.061*			0.007**	0.002		
	(0.021)	(0.033)			(0.003)	(0.003)		
Additional controls	N	Y	Y	Y	N	Y	Y	Y
District FE	N	N	Y	Y	N	N	Y	Y
Region trends	N	N	N	Y	N	N	N	Y
N	480	480	480	480	480	480	480	480
R2	0.38	0.90	0.92	0.93	0.36	0.92	0.94	0.94
Mean of outcome variable	2.56	2.56	2.56	2.56	0.41	0.41	0.41	0.41
p value of F test H_0: $\beta_1=\beta_2$	0.01	0.12	0.13	0.04	0.01	0.10	0.11	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-sex cell. Vector of controls includes female, age group, a Census year indicator, two region fixed effects, the log of district-level population density in 1931, the share of literate youths in 1945 and literacy interacted with a national trend. Number of WNLA 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

	Total years of schooling attained		*Share with any primary school*	
	(1)	(2)	(3)	(4)
ΔLn Migrants (1967-1977)*Eligible 1967-1973	0.91**	1.043***	0.097**	0.105***
	(0.327)	(0.321)	(0.034)	(0.034)
ΔLn Migrants (1967-1977)*Eligible 1974-1977	1.21**	1.531***	0.124***	0.145***
	(0.461)	(0.443)	(0.040)	(0.038)
Eligible 1967-1973	-0.576***	-0.594***	-0.080***	-0.081***
	(0.102)	(0.112)	(0.011)	(0.011)
Eligible 1974-1977	-0.224	-0.268	-0.021	-0.024
	(0.157)	(0.182)	(0.015)	(0.017)
Additional controls, District FE	Y	Y	Y	Y
Region trends	N	Y	N	Y
N	480	480	480	480
R2	0.91	0.92	0.93	0.93
Mean of outcome variable	2.56	2.56	0.41	0.41
p value of Chi2 test H_0: $\beta_1=\beta_2$	0.09	0.00	0.10	0.02
First stage F-statistic for instruments:	11.39	12.44	11.39	12.44

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-sex cell. Vector of controls includes female, age group, a Census year indicator, two region fixed effects and the log of district-level population density in 1931. Instruments include: Number of Wenela stations in the district interacted with eligibility in 1967-1973 and 1974-1977 dummies. Sample includes adults ages 20 to 44 in 1977 and 1998 census.

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

	Total years of education		*Share with any primary school*	
	Districts with		Districts with	
	No estates	Estates	No estates	Estates
	(1)	(2)	(3)	(4)
Eligible 1967-1973*Num. WNLA stations	0.101**	0.067**	0.010**	0.010**
	(0.041)	(0.018)	(0.004)	(0.003)
Eligible 1974-1977*Num. WNLA stations	0.156**	0.096***	0.015***	0.012**
	(0.055)	(0.018)	(0.005)	(0.004)
N	320	160	320	160
R2	0.93	0.94	0.94	0.95
Mean of outcome	2.69	2.30	0.42	0.39
p value of F test H_0: $\beta_1=\beta_2$	0.006	0.007	0.005	0.036

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-sex 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 and age squared, the log of district-level population density in 1931, the share of literate youths in 1945, literacy interacted with a national trend, a full set of district fixed effects and region-specific trends. Number of Wenela stations is a count variable.

Appendix Table 1: First stage estimates for IV scaling exercise

	*Δ Ln Migrants (1967-1977)*Eligible 1967-1973*		*Δ Ln Migrants (1967-1977)*Eligible 1974-1977*	
	(1)	(2)	(3)	(4)
Eligible 1967-1973*Num. Wenela stations	0.082***	0.082**	0.004	0.002
	(0.029)	(0.029)	(0.002)	(0.002)
Eligible 1974-1977*Num. Wenela stations	0.002	0.001	0.091***	0.088***
	(0.001)	(0.001)	(0.027)	(0.026)
Eligible 1967-1973	-0.091	-0.090	-0.01	-0.007
	(0.117)	(0.117)	(0.007)	(0.007)
Eligible 1974-1977	-0.005	-0.003	-0.113	-0.105
	(0.003)	(0.003)	(0.119)	(0.115)
Additional controls, district fixed effects	Y	Y	Y	Y
Region Trends	N	Y	N	Y
N	480	480	480	480
R2	0.467	0.468	0.442	0.469
F statistic on instruments (interaction term)	11.39	12.44	11.39	12.44

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-sex cell. All regressions control for female, age dummies, the log of district-level population density in 1931, the share of literate youths in 1945 and literacy interacted with a national trend, and a full set of district fixed effects.

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

	Total years of education				*Share with any primary school*			
Assumptions about migrant education:	Max. schooling		Min. schooling		Highest share with primary school		Lowest share with primary school	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Eligible 1967-1973*Num. Wenela stations	0.088*	0.098**	0.097*	0.109**	0.009***	0.010***	0.009**	0.010**
	(0.035)	(0.036)	(0.048)	(0.049)	(0.003)	(0.003)	(0.004)	(0.004)
Eligible 1974-1977*Num. Wenela stations	0.118*	0.143**	0.152*	0.183**	0.012**	0.013***	0.014**	0.016**
	(0.051)	(0.053)	(0.078)	(0.082)	(0.004)	(0.004)	(0.006)	(0.006)
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	0.92	0.93	0.92	0.92	0.94	0.94	0.94	0.94
Mean of outcome variable	2.53	2.53	2.59	2.59	0.41	0.41	0.41	0.41
p value of Chi2 test H_0: $\beta_1=\beta_2$	0.11	0.03	0.09	0.04	0.11	0.07	0.08	0.06

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-sex cell. Vector of controls includes female, age group, a Census year indicator, two region fixed effects, the log of district-level population density in 1931 and the share literate in 1931, and historical density and literacy 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-sex cell. Details of variable construction are explained in the text. Sample includes adults ages 20 to 44 in 1977 and 1998 census. Comparison cohorts are older and younger cohorts.

Web Appendix 2: Bounding results for composition effects from internal migration

Not for publication

An important potential threat to validity of our results comes in the form of internal migration. One drawback to using Census data is that neither the 1977 nor 1998 Census indicate district of birth for adults with different levels of education. This means that 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 fix ideas: suppose all districts have the same average level of education before internal migration. If adults with more education move from non-WENELA to WENELA districts while less educated adults move in the opposite direction, this would generate artificially positive differences in adult educational attainment across districts that we would ascribe to exposure to WENELA stations. If this type of sorting is constant over time, then the WENELA dummy in our regressions controls for these differences in internal migration rates. If, however, internal migration flows differ by district as well as by cohort, this could still threaten the validity of our results by generating 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 values 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

[1] Census 1977 counts the number of people in each cohort, district and sex 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.

in a particular district – have the same level of education and therefore we only need to 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 cohort, sex and district level:

$$\bar{Y}_{asd}^{BOUND} = \frac{N_{asd}\bar{Y}_{asd} - NetMigrants_{asd} * \bar{Y}_{as}^{m}}{N_{asd} - NetMigrants_{asd}} \qquad \text{(A.1)}$$

$$= \frac{N_{asd}\bar{Y}_{asd} - NetMigRate_{asd} * N_{asd} * \bar{Y}_{as}^{m}}{N_{asd} - NetMigRate_{asd} * N_{asd}}$$

$$= \frac{\bar{Y}_{asd} - NetMigRate_{asd} * \bar{Y}_{as}^{m}}{1 - NetMigRate_{asd}}$$

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

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 the main estimates in Table 5. This is because in a closed system (i.e. the whole of Malawi) some districts are receiving districts ($NetMigRate_{asd}>0$) while others are sending districts ($NetMigRate_{asd}<0$). 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):

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

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

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[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.

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, $\bar{Y}_{asd}^{upper} > \bar{Y}_{asd}$ 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. 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 contrast, for sending districts where $NetMigRate_{asd} < 0$, the inequalities in (A.2) and (A.3) are reversed. For such districts, it is possible that $\bar{Y}_{asd}^{lower} > \bar{Y}_{asd}$ and $\bar{Y}_{asd}^{upper} < \bar{Y}_{asd}$.

Because we have both sending *and* receiving districts in our sample, and because rates of internal migration 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. Even more complicated patterns of net migration varying across exposed and non-exposed cohorts, and across WENELA and non-WENELA areas, imply that any 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 a region-specific trend term for each outcome. We compare the main coefficients in this table with the coefficients in Table 5. Overwhelmingly, the positive human capital effects of exposure to the employment expansion and contraction in WENELA districts are still evident. All of our estimates are statistically different from zero at the 5 or 10% level, and we can reject $\beta_1 = \beta_2$ at the 5% or 10% level in all cases, once all controls are included.

If we assume net migrants have the maximum level of schooling in the cohort-sex cell for a given Census year, the presence of anyone new in a receiving district "brings the average up" and their absence from a birth district artificially deflates that district's average education. Adjusting for these educated net migrants, we still see large, positive impacts of exposure to treatment among exposed cohorts: those exposed during the labor expansion have 0.098 more years of education, while those exposed during the labor contraction have 0.143 more years of education. If we instead assume that net migrants are uneducated, removing them from our outcome measure in receiving districts and

[3] Crudely, if net migration rates are more likely to be 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.

adding them back to sending districts reveals similar, large positive impact 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 0.10 and 0.18 more years of education. These estimates compare favorably to our main results in Table 5 which lie between 0.08 and 0.135 more years of education (Table 5, column 4).

Results are similarly robust when we use the share with any primary school as outcome. In Table 5, exposed cohorts from districts with more WENELA stations are 1.1 to 1.7 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 0.09 and 1.6 percentage points more likely to have ever been to primary school. The results in Appendix Table 2 suggest that internal migration and any resulting measurement error in $WENELA_d$ is unlikely to drive our results.

Data Appendix

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 data from the 100% sample of the 1998 Census microdata. 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, district and TA 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% sample of the 1977 Census and reported in *Malawi 1977 Population Census Final Report Volumes I and II*, Malawi National Statistics Office, Zomba
   - Data are available at national, region or district level, and sometimes at district, sex and five year age group 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

3. *Historic variables from older Census data*
   - Aggregate tables presented at the district level are available from published reports for the 1945 and 1966 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 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 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 created a district boundary crosswalk that links districts over time (across Census waves) and across name changes. We assign earlier variables to the 1977 and 1998 Census district boundaries in this way:
    - For districts that were eventually combined we add district level values together
    - For districts that were split apart, 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*

- We collected and digitized historical information on the location of WNLA/TEBA recruiting stations in 1937. The main source included "Correspondence from the Secretariat, Zomba, Nyasaland 1935 (Circular number 8 1935, S1/169/35). We also used information from later Provincial Administration Reports to verify these stations were still open in later years (Northern Province: 7th December 1961 Ref. No. O.3.37 and Commissioner for Labour Circular, 25th March 1957)
- Figure 1 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
- We collect and digitize district-level totals on the number of foreign travel identification documents issued to men for 1958-1966 from the *Malawi Ministry of Labour Report 1963-1967* (1969)