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Refugee camps – a lasting legacy? Evidence on long-term health impact

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JEL classification: I15, O10, O15, J13

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July, 2019

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Abstract

This paper examines the long-term impact of refugee camps on the health of local residents in Tanzania. Taking height-for-age z-score (HAZ) as a proxy for health, the paper exploits the fact that different birth cohorts were exposed to different stages of the camps' lifecycle. Temporal variation through birth cohorts is combined with geographic variation in a difference-in-difference estimation approach. First, the paper examines the generation that were children at the opening of the camps and are now adults (as of 2012). It finds a negative and localised health effect that has persisted into adulthood. The result is comparable to a 2.9% to 5.9% reduction in adult hourly earnings. However, those that were exposed for a longer duration were less affected suggesting that subsequent economic development around camps mitigated the initial adverse effect. Second, this paper compares the subsequent generation that was born once the camps were already in operation, and those born after camps closed. It finds no observable difference in the HAZ score between those born during camps operation and in the post-camp period.

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1. Introduction

Conflict is often accompanied by displacement of individuals within and across national borders. In the host regions, the immediate aftermath of such forced migration shocks is typically characterised by pressure on existing resources. Of the 70.8 million forcibly displaced individuals globally, a majority live in refugee camps in developing countries (UNHCR, 2019). These camps are intended as an emergency response allowing humanitarian organisations to deliver emergency shelter, food, water and medical care. However, owing to the increasingly protracted nature of conflict, these camps often evolve into long-term settlements, lasting for several years and becoming loci of continued resource inflows such as humanitarian aid, infrastructural investments and evolving local trading.

Previous micro-level analysis has found that proximity to camps has an effect on local labour market outcomes, household consumption, human capital and the wealth of host communities (Alix-Garcia, 2009; Ruiz & Vargas-Silva, 2015, Maystadt & Verwimp 2014; Maystadt & Duranton 2019; Baez, 2011). Of these studies, however, only a few have looked at the long-term impact (Maystadt & Duranton 2019; Ruiz & Vargas-Silva, 2015). Even so, even these studies tend to focus on cohorts that were alive at the beginning of the refugee influx and are also based on a sample of camps that were in operation for a relatively shorter period.

Other long-term impacts on host communities, including the impact on cohorts of individuals born in the period the camps were in operation and thus exposed to the later evolution of the relationship of camps to local communities, and indeed those born after camps closed, are still under-researched. This paper contributes to this gap in the literature using data arising from the refugee influx from Rwanda and Burundi into Tanzania in the period 1993-2012.

The paper uses height-for-age z-score (HAZ) as a proxy for health, to explore one impact of the fact that different birth cohorts were exposed to different stages of refugee camps. The temporal variation through birth cohorts is coupled with variation in distance to refugee camps to create a difference-in-difference estimation approach.

This paper is closely related to the work of Baez, 2011 who studies the impact of exposure to refugee camps on human capital in Tanzania. However, the perspective and methodology of this paper differ in two important respects. First, whereas Baez, 2011 solely examined the effect on the health of individuals aged 10-15 who were exposed to the refugee influx in their childhood, this paper examines whether the effect of childhood exposure, if any, lasts into adulthood i.e. age 20 and above. Qualitative evidence shows that, in the immediate aftermath of the refugee influx, the increase in population was associated with an increase in deforestation, communicable diseases and pressure on resources. However, in subsequent years, there were some positive spill overs. Locals are reported to have benefitted from roads and healthcare–services provided to camp populations (Whitaker, 1999). Other studies also found that locals around refugee camps experienced an increase in consumption levels and assets, although the effects are heterogeneous across economic activities (Alix-Garcia, 2009; Maystadt & Verwimp, 2009; Ruiz & Vargas, 2015). In light of these possible positive spill overs, and because poor health in childhood may still be recouped in the growth window, the first objective is to establish whether the effect of camp exposure is discernible in later life.

Secondly, while previous research has focused on the initial phases of refugee arrival, this paper considers another juncture that is crucial for host regions – the departure of refugees and closure of camps. By comparing the generation of local children that were born after the refugee influx to those born after camps closed, I examine the effect of having been exposed to the camps relative to being born in a post-camp era. These later cohorts have not previously been studied. The closure of the camps could have led to a loss of positive spill overs which may have negatively affected those born after the camps closed. On the other hand, the departure of refugees and closure of camps could have alleviated pressure on local resources to the benefit of those born after camps closed. This paper seeks to examine what the effects were.

Another difference of the present paper to other studies is the use of a different dataset (the World Bank Living Standards Measurement Survey (LSMS) rather than the Kagera Health Demographic Survey (KHDS)), which combined with new data on camps in the Kigoma region, improves upon the geographic scope of previous studies.

The paper finds a negative, albeit localised, effect of exposure to camps discernible through to adulthood, for individuals that were children at the time of the refugee influx. The effect is a reduction in HAZ of 0.29 between those who lived within 50km of a camp and those that were more than 50km away. The result is comparable to a 4.3% difference in adult hourly earnings. However, those that were exposed for a longer duration were less affected. These results are robust to different measures of exposure and provide evidence that the negative effect of camps may have dissipated over time. Among the later generation who are still children (at the end of the period of study), the results indicate that there is no observable difference in the HAZ between those born during camp operations and in the post-camp period.

The remainder of the paper is structured as follows: section 2 discusses the contextual background, section 3 provides an overview of the pertinent literature, section 4 describes the data used and section 5 outlines the empirical methodology. The results are discussed in section 6. Section 7 provides some robustness checks while section 8 concludes.

2. Context

In 1993, civil war erupted in Burundi following the assassination of the president. In October of the same year, about 250,000 Burundians fled across the border into Tanzania. A few months later in April 1994, the plane carrying the president of Rwanda and the new president of Burundi was shot down. Within 24 hours, 250,000 refugees from Rwanda fled into Tanzania and more continued to pour in over the subsequent months of 1994. Based on UNHCR sources, Maystadt (2014) notes that the scale and pace of the influx was unprecedented. In total, Tanzania received more than 800,000 refugees from Burundi and Rwanda in the short period. It is estimated that the refugees represented more than a third of the local population in the two recipient regions of Kagera and Kigoma (Maystadt, 2014; Whitaker, 1999; Adisa,1996). Prior to the refugee influx, Kagera and Kigoma being among the remotest regions of Tanzania, were also among the poorest regions (Green, 1995).

The sudden nature of the events and the scale of the inflow caught the UNHCR and the government of Tanzania off guard. The refugees settled near the border because the limited means of transportation and the terrain of the region limited their mobility. Maystadt (2014) documents how the Tanzanian Ministry of Home Affairs and the UNHCR chose campsites but that by the time the UNHCR and the government of Tanzania moved into action in 1994, it was deemed too costly to move them farther away from the border. In addition to avoiding relocation costs, the Ministry and UNHCR also wanted to minimize the cost of repatriation when the situation in the sending countries became more peaceful (Maystadt, 2014; Lupala 2015). Map 1 shows the location of the refugee camps.

The sudden population shock caused a pressure on natural resources. The presence of refugees led to an increase in demand for firewood and water and resulted in deforestation and land degradation of some regions close to the camps (Whitaker, 1999; Green 1995). Relief agencies reported outbreaks of diseases such as malaria, cholera and dysentery in local areas (Eriksson et al., 1996). Whitaker (1999) further documents events during this period. He notes that the refugee influx was also accompanied by a proliferation of humanitarian aid agencies and expatriate workers. Relief agencies hired local labour and many local employees from government hospitals and schools are reported to have left their positions in favour of the higher salaries from aid organizations. Due to the demand for housing by expatriate workers, housing prices increased. However, foreign workers also created a demand for goods such as chocolate and cheese and enterprising locals took advantage of the new market opportunities (Whitaker, 1999).

Although the refugees were kept in camps, evidence shows that refugees interacted with the local population. They are reported to have worked as labourers in neighbouring villages weeding, harvesting, clearing land, tending livestock or fetching water and firewood. Commercial centres also developed around the camps. There were daily markets and farmers who had previously traded across the border before the camps now sold their products in nearby camps. According to the World Food Program that distributed aid, refugees traded about 75% of their aid receipts (Whitaker, 1999).

During the duration of the camps, vast sums of money were injected into Kagera and Kigoma. By 1999, it was reported that more than $15 million had been spent on improving roads, airstrips and communication infrastructure in Kagera region alone. Locals around the camps also gained access to health facilities in the refugee camps. Before the arrival of the refugees, Tanzania had just introduced a cost-sharing scheme requiring locals to start paying part of their health care costs, but at refugee health facilities, locals continued to access health services free of charge. In the event of referrals, free transportation was provided for both refugees and the local population to district hospitals (Lupala, 2015; Whitaker, 1999). Locals also benefitted from UNHCR-installed water outlets in the camps and surrounding villages (Wolfcarius, 2008).

In 1996, when the security situation improved, most Rwandan refugees left and camps that had hosted them closed down. The return of Burundian refugees was more gradual because of the continued poor security situation in Burundi. In 2002, the UNHCR and the Tanzanian government officially launched a repatriation program to facilitate the return of Burundian refugees. Repatriation was initially limited to relatively safe areas in northern Burundi. In subsequent years, camp closures continued as they emptied out upon the return of the refugees. In 2006, about 350,000 refugees still remained. Mass repatriation efforts reduced the figures even further over the following three years. By 2009, only about 50,000 Rwandan and Burundi refugees remained – the lowest figure for the first time in 15 years (See figure 1). Several UN sources viewed 2009 as the first "camp free" year in the Kagera region. In Kigoma, by 2009 camps had all but closed down with the exception of three that housed the remaining refugees (Yang-Yang, 2014). Figure 1 shows the number of Rwandan and Burundian refugees in Tanzania until 2014.

Figure 1: Number of Refugees from Rwanda and Burundi in Tanzania, 1961-2014

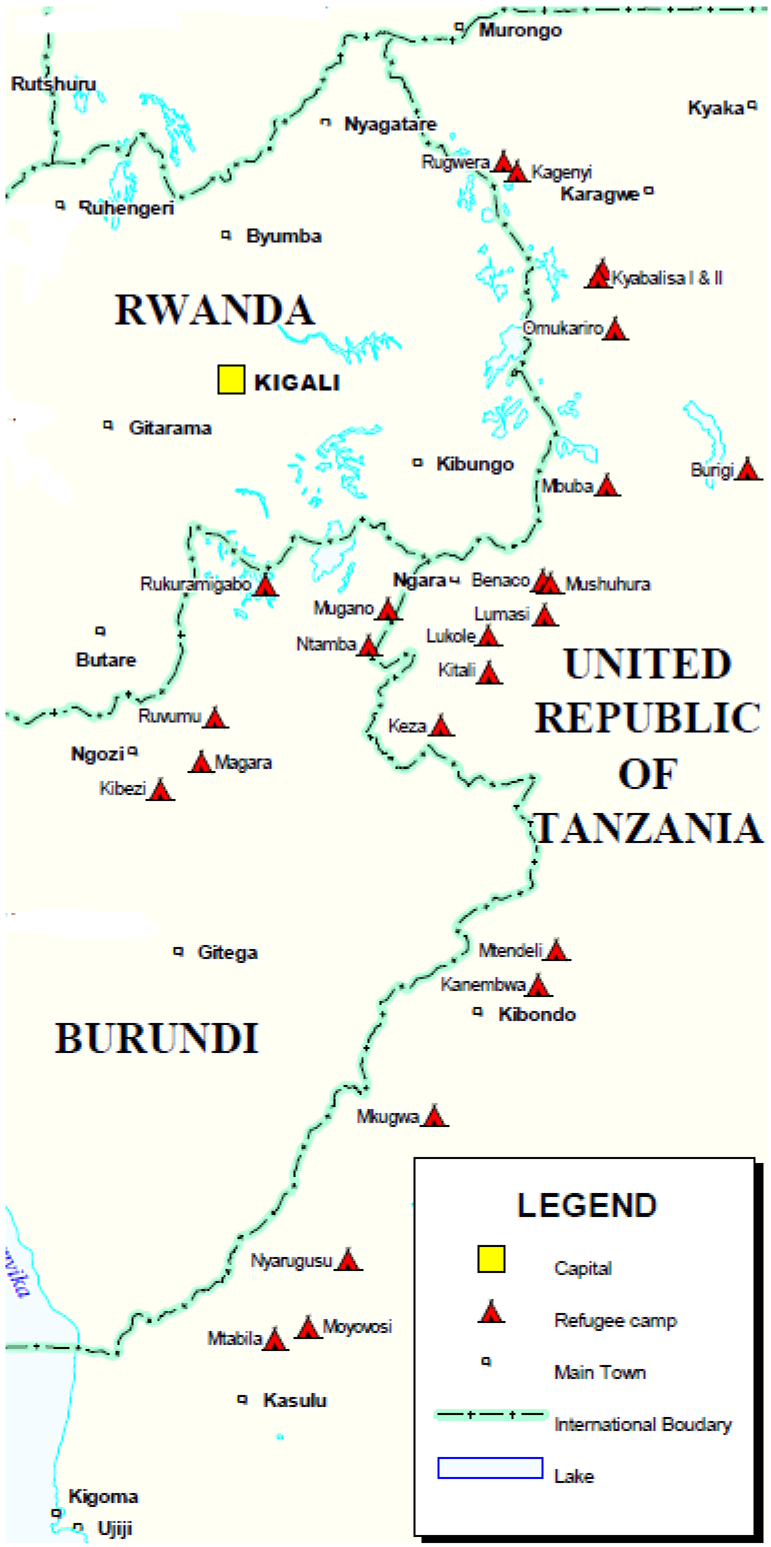
Source: UNHCR 2019

The presence of the refugees had so altered local conditions that the departure of refugees was predicted to result in a vacuum. Whitaker (1999) noted "if, over the course of refugee presence, local farmers learned to change production patterns to cater to new local market demands, then such farmers could face food insecurity and loss of income after camp disbandment…"

After camps closed, premises and facilities were handed over to local district authorities. The UNHCR together with the local government established a programme for the rehabilitation and reconstruction of former campsites referred to as the Joint Programme J.P. 6.1 Transition from Humanitarian Assistance to Sustainable Development in North-western Tanzania. It was intended to address the gap left by the withdrawal of humanitarian agencies. The programme which ran from 2008 to 2011 with a budget of over US $10 million had three areas of intervention: Wealth Creation, Social Services and Governance, and Sustainable Management of Natural Resources. A component of the programme was the training of health personnel. Some former campsites were transformed into schools, others health centres and one became a military training centre (Lupala, 2015; Han, 2009; United Nations Tanzania, (n.d.).

Of the 22 camps, nine closed in 1996, one in 2005, eight in 2007 and 2008, one in 2009, two in 2012 and one was active as of the time of the survey in 2012. The data allow me to compare the health of the youngest generation alive when the nearest camp was in operation to that of the generation after the closest camp closed in order to examine how the youngest generation exposed to camp compares to those that were born after the camp closed.

Map 1: Refugee camps in Tanzania



Source: UNHCR (1999)

3. Literature review

Health shocks may arise from an increase in, or lack of, employment opportunities. Forced migration constitute a labour supply shock in recipient regions. Braun and Mahmoud (2014) argue that the magnitude of the effect depends on the degree of substitutability between migrants and locals. Using data from the 1950s after the expulsion of millions of Germans from Eastern Europe to West Germany after World War II, they find that the forced migration shock reduced the employment rate of the local workforce. In this case the forced migrants and natives are very close substitutes: the forced migrants spoke the same language (German) and had been educated in German schools. Similarly, in this context, we can note that Rwandans and Burundians are not linguistically distant from the locals in the Kagera and Kigoma region and could therefore be considered close substitutes.

Calderon and Ibanez (2009) find that the labour market effects are largest for low-skilled workers. They examine the impact of the Colombian conflict on regions within Colombia that hosted the internally displaced population. They distinguish between high and low-skilled workers in recipient regions and find a general decline in wages, but that it is greatest among the low-skilled.

In Tanzania, there is evidence that the forced migration shock had an impact on host regions' labour market and goods market. Ruiz and Vargas (2015, 2016) examine the impact of the forced migration shock on the type of employment held by the locals. They find that exposure to the refugee shock resulted in locals having a higher likelihood of working in household farms or tending household livestock and a lower likelihood of working outside the household as employees. They find that the low likelihood of being employed outside the household was particularly strong among Tanzanians that had been doing casual work before the shock where they faced competition from refugees. Their sample is composed only of individuals that were alive at the beginning of the refugee influx.

A second potential impact on health arises via consumption goods and wealth. Alix-Garcia and Saah (2009) examine the impact of proximity to refugee camps and access to aid on the prices of Tanzanian agricultural goods in adjacent markets between 1992 and 1998. The authors find that there was an increase in the prices of those agricultural goods that were staples of the refugee diet and that were not provided as part of the aid ration. Whitaker (1999) provides extensive and useful qualitative insights into these price effects that resulted from the composition of aid rations to the refugees. Food aid was typically in the form of maize, cooking oil and beans. However, the diet of Rwandans and Burundians is primarily cassava and green bananas while Tanzanians prefer maize. Refugees therefore sold their aid goods to locals in exchange for cassava and plantain. As a result, the prices of bananas and cassava increased sharply and there were reports of locals endangering the food security of their households by selling large amounts of their food stocks in order to take advantage of the high prices of these goods. In contrast, the price of maize declined as the local markets were flooded with maize from the refugees and local farmers were unable to sell their own maize produce. These authors also assess the impact of the refugee presence on short-term household wealth. They find, overall, an increased incidence of wealth indicators such as radios and bicycles in rural households closer to the refugee camps which they attribute to the price impact arising from the change of diet. The wealth effects therefore differ across households. Rural residents living near refugee camps benefitted from selling their stock of agricultural products. On the other hand, the authors explain that because urban households are more likely to be buyers of agricultural goods for consumption, they are affected by the high prices and therefore experience negative wealth effects.

Similar findings arise from Maystadt and Verwimp (2014), who study the impact of the forced migration shock on the welfare of households in the Kagera region of Tanzania between 1994 and 2004. They find an overall positive albeit heterogenous impact on household consumption. Agricultural workers were worse off and the authors suggest it is due to increased competition from refugees for those jobs. They also find that, despite the reported surge in local entrepreneurship, those self-employed in non-agricultural activities experienced a relative welfare drop perhaps because of increased competition from other local entrepreneurs who came from other Tanzanian regions. In a subsequent paper, Maystadt and Duranton (2019), use data from 2004 and 2010 and still find that overall, the local population had higher consumption levels. They provide evidence that this effect is driven by infrastructural investments in the regions, specifically roads built to serve the refugee camps.

One study directly focuses on the health of the local population. Baez (2011) finds a negative impact of the refugee influx on the human capital of local Tanzanian children. First, the author examines the impact of hosting refugees on the health of 0-5-year old children in 1996, less than two years after the arrival of the refugees. To do so, he compares the children in more affected areas with those in less affected areas in 1992 and 1996. He finds a 15 to 20 percentage point increase in the incidence of infectious diseases, an increase of roughly 7 percentage points in mortality for children under five and a decline in HAZ of 0.3 standard deviations. Second, the author employs a double difference comparing individuals in highly affected areas with those in lesser-affected areas at the onset in 1994, (when they are 0-5 years old) and in 2004 (when they are 10-15 years old). He still finds a negative effect on health.

These results warrant further discussion. Individuals are believed to keep growing until age 20 (see, for example, Moradi, 2010). Empirical evidence shows that children that are lagging behind in growth can catch up to their peers and that this “catch up growth” happens in puberty. Indeed, evidence from Tanzania corroborates this phenomenon (Hirvonen, 2014). At the time that Baez (2011) observes individuals in 2004, (when they are 10-15 years of age) they have not attained full adult stature. Therefore, whether the effect of exposure in childhood, if any, was permanent and lasting into adulthood is an important question and a gap in the literature. This is particularly true if some of the positive spill-over effects discussed above take a longer time to arise than the short-run negative shock arising from the mass influx of refugees. Hence, using more recent data from 2012, I link adult health outcome (proxied by HAZ) with data on the year of opening and closing of the nearest camp during an individual’s growth window (defined as age 0-19). Exposure to refugee presence was determined both by proximity to the camp and by the age of the individual during the operational years of the nearest camp. This approach will capture the long-term impacts on the health of individuals that were children at the beginning of the influx and are now adults as well as the health of later cohorts with shorter exposure. A similar strategy has been used to study the impact of school construction during childhood on education and earnings in adulthood (Duflo, 2001). Such cohort analyses rely on two types of variation – variation in the cohorts that are exposed to a treatment, and geographic variation in the treatment. Implementing such a cohort dimension by exploiting years of camp operation during an individual’s childhood is the novelty of this paper. Beyond shedding light onto whether the effect of childhood exposure is discernible in adulthood, this paper can go a step further and examine whether the impacts are heterogeneous by the duration of the exposure during the growth window. Having a longer exposure could have afforded more opportunity to benefit from any positive effects but it could also imply more exposure to the negative effects.

A further and principal contribution of this paper is that it attempts to examine the effect on individuals that have previously not been the focus of any study in this literature - the groups born during and after camp operations. The aforementioned studies all examine impacts on individuals that were alive at the beginning of the refugee influx. The effect on the generation of individuals that are born during the refugee era or after camps close is a question that has yet to be considered in this literature. Given that the migration shock was associated with effects on the labour market, the goods market, household consumption and household wealth, it is reasonable to hypothesise that the children born during the refugee era would fare differently from those born after camps closed. On one hand, if we take the evidence that refugee presence exerted a pressure on resources, those born after camps closed when this pressure is alleviated, may have better outcomes than those exposed to camps. On the other hand, if refugee presence generated positive spillovers, those born after camps closed may be worse off. Two studies outside the economics literature provide support for the latter hypothesis. Lupala (2015) conducted a case study on the impact of closing the Mtabila refugee camp in Tanzania on the livelihoods of surrounding residents. The author conducted qualitative interviews with 198 households in three villages near Mtabila refugee camp after its closure. Villagers reported that the closure of the camp had led to loss of services such as health centres, schools, water supply and routine road maintenance. They also reported that after the closure of the camp there had been a decline of agricultural production, which they note had been dependent on unskilled refugee labour. Similarly, Han (2009) conducted qualitative interviews in three villages near two refugee camps in Tanzania (Kanembwa and Karago) after they closed. Among her findings, she notes that while initially the demand for construction materials by NGOs for building camp facilities led to increases in prices of construction materials in the towns that hosted the refugees, the high prices persisted even after the camps closed (Han, 2009). Although Lupala, 2015 and Han 2009 are descriptive qualitative studies and do not aim to draw causal links, they nevertheless hint at possible impacts of closing refugee camps. This paper intends to contribute to this gap in literature.

This paper also differs from the aforementioned existing studies in its use of the LSMS. Previous studies on Tanzania have primarily used the KHDS. The KHDS is a panel dataset, conducted in 1991, 1992, 1993, 1994, 2004 and 2010. It contains individual level data on education, health and anthropometrics in addition to data on household activities, household expenditure as well as community level information for 49 villages in the Kagera region of Tanzania. Although the KHDS has been instrumental for generating evidence where none existed, a key limitation is that it only contains data on one region of Tanzania. As is discussed further in the data section, one advantage of using the LSMS over the KHDS is that the LSMS allows for the study of the impacts on the Kigoma region, which was also affected by the forced migration shock. This is made possible by new data on camps in the Kigoma region. Previous empirical studies have used only camps in the Kagera region.

As a measure of exposure to the refugee presence, the literature uses geographic variation in refugee presence generated by the shock. Baez (2011), classifies the western districts that border Rwanda and Burundi as treatment districts, whereas the eastern part of Kagera are used as controls. In the same paper, the author also uses distance of villages from the border of Rwanda. The use of the district level variation is potentially problematic as the Eastern district of Kagera also hosted refugees. In view of this, subsequent work (Ruiz and Vargas, 2015; Maystadt and Verwimp, 2014, Maystadt & Duranton, 2019) proposed an intensity index based on distance to all camps weighted by an estimate of the population of the camps as a less noisy measure of refugee presence. In this paper, I will provide a further refinement presenting analysis based on distance to the nearest camp.

In addition, I will also present analysis using the intensity index with a modification that allows the index to not only be village-specific but also cohort-specific.

4. Data description

4.1 Birth cohorts

This analysis uses the 2012 Living Standards Measurement Survey (LSMS) on Tanzania to look at health outcomes across defined birth cohorts. The LSMS is a nationally representative household survey that collects information on a wide range of topics including income, consumption, education, health, labour and other socio-economic characteristics. I limit the sample to the two administrative regions of Kagera and Kigoma where the camps were located and the three surrounding regions of Mara, Mwanza and Shinyanga.[2] I thereby retain 3934 individuals.

This analysis uses only the 2012 data. The 2014 wave of the LSMS, data collection for which was completed in 2015, is the most recent but it is not preferred – there was renewed outbreak of violence in Burundi in 2015 that caused a new movement of refugees into Tanzania. Although the magnitude was small compared to that of 1993, at least two camps that had been closed were reopened. Although not a concern for individuals that were already height mature by the times camps closed, it may be a concern for younger individuals that were interviewed in 2015. Using the 2012 round avoids this concern altogether. Because the LSMS is not available pre-1993 (i.e. before the camps opened), I use the 2012 wave and construct birth cohorts for temporal variation.

[2] Given the potential channels through which refugee presence may affect host communities discussed in the literature review and context sections, it is unlikely that individuals beyond those regions would have interacted with the camps. In the methodology section, I discuss how I farther limit the sample by distance.

I restrict the sample to individuals that are between ages 0-49 at the time of the survey in 2012. Age 49 is chosen as the upper limit because the normal process of ageing typically begins at this point and height shrinkage or loss of stature begins (Cline et al., 1989; Moradi, 2010). Height growth typically occurs between age 0-19 and most individuals attain final adult height by age 20 (See Moradi, 2010). I therefore exploit the fact that different age groups had differing exposure to the refugee influx. I define two broad groups based on survey year, year of birth and camp operation dates:

i) *Adults at the time of the survey*: At the time of the survey in 2012, *all* these individuals are adults i.e. they are above 19 years of age. However, individuals who were age 0-19 at camp opening were susceptible to the effects of the refugee influx as they were still children. On the other hand, individuals who were age 20-30 at camp opening were already height mature at the time of the influx – they may have been affected in other ways such as in the labour market, but not height-wise. As will be discussed further in the empirical strategy, comparison of adults in the survey who were children at camp opening and those who were mature at camp opening will be the basis for examining the long-term impact of exposure to camps.

ii) *Children at the time of the survey*: These individuals are still children at the time of the survey. They were either (i) exposed to a camp at some point in their childhood or (ii) they were born after the camps closed. Comparison of children born before camps closed and those born after camps closed will form the basis for examining how, in this generation of individuals that are still children, individuals born during the camp era fare relative to those born after camps closed.

For each village, I calculate how long each cohort would have been exposed to each camp in childhood. The result is a by village by camp cohort-specific duration measure for which a given camp (c) was operational during an individual of village v of cohort k's childhood (age 0-19).

Table 1 illustrates how various cohorts were exposed to the refugee camp presence, by way of illustration, using one camp, Lukole. In the analysis however, this is done for each camp.

Table 1: Cohort exposure (time dimension) to camp presence

Subgroup	Description	Sample type	Age at survey	Age in 1993	Year of birth	length of exposure to camp Example: Lukole A
			Alive at the beginning of the influx			
1	Mature at the beginning of the influx (1993)	Mature at survey. Long-run effect – placebo	39-49	20-30	1963 - 1973	0
2	Child at the beginning of influx (1993)	Mature at survey. Long-run effect	32-38	13-19	1974	1
					1975	2
					1976	3
					1977	4
					1978	5
					1979	6
					1980	7
			26-31	7-12	1981	8
					1982	9
					1983	10
					1984	11
					1985	12
					1986	13
			20-25	0-6	1987	14
					1988	15
					1989	15
					1990	15
					1991	15
					1992	15
			Born during the refugee era or after camps closed			
3	Not alive at the beginning of the influx (born during the refugee era or after camps closed)	Child at survey. Young generation.	13-19	N/A	1993	16
					1994	15
					1995	14
					1996	13
					1997	12
					1998	11
					1999	10
			7-12	N/A	2000	9
					2001	8
					2002	7
					2003	6
					2004	5
					2005	4
			0-6	N/A	2006	3
					2007	2
					2008	2
					2009	1
					2010	0
					2011	0
					2012	0

Note: Based on the age at survey (2012) individuals are first split into those that are adults and those that are still children. The adult sample (subgroup 1 and 2) is used to examine the long-run effect in the old generation, while the child sample (subgroup 3) is used to examine the effect within the young generation. Within each sample, the year of birth, year of opening and closing of each camp are further used to determine duration of exposure of a particular cohort to a particular camp. Duration is defined as the number of years that a cohort was exposed during its childhood window of age 0-19. The example shows the number of years of exposure to Lukole A camp. (E.g. Lukole A was opened in 1993 and closed in 2008. An individual born in 1987 would have been 6 years old when it opened, therefore, upto and including age 19, they would have had 14 years of exposure.) The cohort exposure dimension is then combined with variation in distance from a camp to an individuals' village. Age bands (0-6; 7-12; 13-19) are chosen to align with growth spurts.

4.2 Location of clusters/villages[3]

The geographic coordinates of clusters are obtained from the LSMS. The sample comprises 194 clusters. For confidentiality purposes, the LSMS does not provide the precise geographic coordinates of a household. Rather, the LSMS assigns each household in a cluster the average of the GPS coordinates of households in that cluster, randomly offset within a range of 0-5km. Because the empirical strategy relies on exposure by distance from camps (see section on methodology), the offsetting introduces measurement error in distances constructed using cluster locations. However, as the offset is random, the measurement error should be random therefore mitigating concern regarding bias in estimated effects. One approach that is suggested for further mitigating this concern is to use distance bands (Perez-Heydrich et. al., 2013) and this paper will present results that use this approach.

4.3 Camps

Distance to camps will form the basis for estimating exposure to refugee presence. I discuss this distance measure in the next subsection. Information on these camps is compiled from various sources. The geographic coordinates of the camps in Kagera are obtained from Maystadt (2014). The geographic coordinates of the camps in Kigoma, and the opening and closing dates of camps in both Kagera and Kigoma are obtained from Yang-Yang (2014) and from the UNHCR field office in Tanzania. For the population of the camps, I use estimates from Maystadt (2014) and various UNHCR reports. I use the highest population estimate available as a proxy for the size of the camp. The total camp sample comprises 22 camps, 11 in Kagera and 11 in Kigoma. The population across camps ranges from 2,155 to 350,000 over these years.

[3] Clusters are typically defined by village boundaries (LSMS, 2012).

4.4 Measures of exposure to camps

In addition to exposure to refugee presence by cohorts described above, I construct measures of exposure to refugee presence as follows.

i. Nearest camp analysis

First, I consider only the nearest camp. After excluding observations that do not have a HAZ score (outcome variable), of the individuals that have a HAZ score and are in my sample of interest 0-49-year olds, 31 individuals were more than 430km away from a camp. After excluding these outliers, I end up with a final sample of 3,934 individuals.

Table 2: Summary statistics of distance to the nearest camp

	Mean	Std. Dev.	Min	Max	p10	p25	p50	p75
Distance to nearest camp (km)	173.9	112.6	2	404.2	21.1	78.5	168.0	278.5
Observations	3934							

Instead of a continuous measure of distance to the nearest camp, I define a discrete treatment variable where I classify individuals having a camp within 20km as the treatment group and those farther than 20km as the control group. I then vary this threshold to 50km, 80km, 100km and 170km. The choice of the thresholds is motivated by the reality of accessibility in North Western Tanzania as well as by the distribution of the distance to nearest camp variable (see Table 2). For instance, given that one of the ways locals interacted with camps was access to services, there is a limit to what is a feasible distance to travel. The 20km also corresponds to the 10^{th} percentile of the distance to the nearest camp, while 80km and 170 km correspond to the 25^{th} and 50^{th} percentile respectively.

ii. Intensity of all camps

Second, I consider all camps and create an intensity exposure index. Specifically, following the approach of Maystadt and Verwimp (2014) and Ruiz and Vargas-Silva (2015, 2016), I create a *camp exposure intensity index* by weighting the inverse distance from each village to each refugee camp by the population of each camp ($Population_c$) as a proportion of the population of all camps. The inverse of the distance is taken to reflect the inverse relationship between intensity of exposure and distance – villages closer to the camp experienced greater intensity than those farther away. Weighting by population ensures proportionality so that the possible effect of smaller camps is not exaggerated and that of larger camps underestimated.

I deviate from Maystadt and Verwimp (2014) and Ruiz and Vargas-Silva (2015, 2016) in that I weigh each camp by the duration for which a camp was open during an individual's childhood (which varies by cohort) rather than weighting by the total number of years the camp was operational (which is fixed). As discussed above, cohort exposure Table 1 provides an illustration of how duration of exposure to each camp for each cohort is determined based on the year of birth, year of opening and year of closing of the camp. As a result, whereas the intensity index in Maystadt and Verwimp (2014) and Ruiz and Vargas-Silva (2015, 2016) is fixed by village, I introduce time variation.

The by village by cohort intensity exposure variable (I_{vk}) is thus:

$$I_{vk} = \left(\sum_{c=1}^{22}\left[\left(\frac{1}{distance_{v,c}}\right)\left(Duration_{v,k,c}\right)\left(\frac{Population_c}{\sum_{c=1}^{22} Population_c}\right)\right]\right)$$

Each of the above measures will be incorporated into the estimation equation discussed in the methodology section below.

4.5 Outcome variable (Height-for-age-z score (HAZ))

The outcome variable of interest is height for age z-score – HAZ. HAZ is defined as the difference, expressed in standard deviation units, between an individual's height and the median height of a healthy and well-nourished population of the same age and gender ("reference population"). I use the WHO 2007 standard as the reference population as it is the most recent and also contains a more diverse pool of ethnic and cultural backgrounds than previous standards (WHO, 2007; de Onis et al, 2007). The average HAZ in Tanzania is -1.5 (DHS, 2015).

HAZ is a reliable indicator of long-run nutritional status (WHO, 1995; Akresh, 2014; Thomas, Lavy & Strauss, 1996). Because it reflects household income and consumption, it is considered a good non-monetized measure of welfare and has been used to estimate effects of childhood exposure to crop failure, (Akresh et al., 2011), violence (Akresh et al., 2012) and agricultural price shocks (Coigneau & Jedwab, 2012). The effects of economic shocks experienced in childhood are reflected in HAZ (Akresh et al., 2011; Coigneau & Jedwab, 2012; Micklewright & Ismail, 2001). Furthermore, height is associated with labour market outcomes and educational attainment (Thomas & Strauss, 1997; Schultz, 2002; Alderman et al., 2006). Previous cohort studies have found that low HAZ in childhood is associated with lower earnings in adulthood (Victora et al., 2008; Hoddinot et al., 2011; Galasso et al., 2016). Having a HAZ of less than – 2 ("stunting"), is of particular concern as it impairs cognitive development and has grave implications for later socio-economic outcomes (WHO, 1995)

To obtain the HAZ, I use individuals' height measurement and age from the LSMS. Rather than the self-reported age in years, for better precision, I calculate an individual's age in months as the difference between the month of birth and the month when the height measurement was taken.

Table 3 provides the mean of the HAZ between individuals within 100km of a camp and those farther than 100km, across age cohorts. Individuals within 100km of a camp on average have a statistically significant lower HAZ than those in villages far away. A key point is that effect is also statistically significant for the oldest cohort i.e. individuals age 39-49 at the time of the survey. Since these individuals were mature before 1993, the fact that we observe a statistically significant difference indicates that villages closer to areas that later became camps were worse off than those farther away even before the refugee influx.

Table 3: Differences in means of HAZ between those within 100km of a camp and those farther than 100km but less than 430km, by age groups

	(1) Full mean	(2) Within 100km mean	(3) More than 100km mean	(4) diff
Age 39-49	-0.84	-1.08	-0.75	0.32**
Age 32-38	-0.80	-1.08	-0.69	0.40**
Age 26-31	-1.04	-1.52	-0.84	0.68***
Age 20-25	-0.93	-1.46	-0.73	0.74***
Age 13-19	-1.41	-1.72	-1.29	0.43***
Age 7-12	-1.49	-1.88	-1.33	0.55***
Age 0-6	-1.40	-1.81	-1.23	0.58***
Observations	3934	1134	2800	3934

5. Empirical methodology

I exploit temporal variation (through birth cohorts) and spatial variation (through distance thresholds) and use a difference-in-difference with birth cohort fixed effects model.

Identification of the effect of camp presence and the effect of closing camps on HAZ could be challenged by endogeneity in camp locations. The crucial identification assumption relied upon is common trends – there could be differences in levels of HAZ between locations as long as trends would have been the same in the absence of camps. Regarding location of the camps, the existing empirical studies on Tanzania (see literature review), rely on the fact that the refugees were pushed into Tanzania by the internal conflict within their own countries, which is unlikely to have been affected by socio-economic outcomes in Tanzania (Alix-Garcia, 2009, Ruiz and Vargas 2015, 2016). The refugees settled where they could and by the time the UNHCR responded, it was deemed infeasible, cost-wise, to relocate them far from the borders (Maystadt & Verwimp, 2014). With regard to camps closing, this was largely exogenous to the socioeconomic conditions in Tanzania. Camps closed because refugees left. According to the UNHCR, the possibility of return was dependent upon the security situation in their countries. In the robustness section, I conduct a test of pre-trends to explore the validity of the identification strategy.

5.1 Impact of exposure to camp presence on adults who were children at the opening of camps (long-term effect)

Comparison of adults that were children at the opening of camps to those that were height mature forms the basis of examining the long-term impact of exposure to camp presence on those who were children at the opening of camps. These individuals (subgroups 1 and 2 in Table 1) are observed as adults in 2012. I use the following equation:

$$HAZ_{ivk} = \beta_0 + \beta_1 (TREAT_d) + \beta_2 (TREAT_d * Child\_open) + X_i + \delta_k + \theta_j + e_{ivk} \qquad (1)$$

The dependent variable, HAZ_{ivk}, is the height-for-age z-score of an individual i in village v born in year k. $Child\_open$ is a dummy variable that indicates whether or not an individual was a child at camp opening. $TREAT_d$ is a dummy indicating whether the individual's village is within d km of the nearest camp. I estimate the equation using different distance thresholds (20km, 50km, 80km, 100km and 170km respectively). Construction of the distance thresholds are described in section 4.4. X_i controls for gender. θ_j and δ_k are region fixed effects and year of birth fixed effects respectively. e_{ivk} is a random idiosyncratic error term. Identification of the impact of the refugee shock comes from comparing individuals who were children at the opening of the camps and those who were not, together with the variation in the exposure of villages. The difference-in-difference estimator of interest is β_2

As an alternative measure of exposure, I use the intensity index (I_{vk}) discussed in section 4.4, to estimate the following:

$$HAZ_{ivk} = \beta_0 + \beta_1 (I_{vk}) + X_i + \delta_k + \theta_j + e_{ivk} \quad (2)$$

5.2 Impact on younger generation

Identification of the effect of having been exposed to a camp for the younger generation comes from comparing children born before camps closed to those born after camps closed and by variation in the exposure of villages. These individuals are subgroup 3 in Table 1.

I estimate a variant of the long-term exposure equation 1 in which I use a discrete binary variable C_i, which takes the value of 1 if the nearest camp was operational at some point in the individual's childhood (regardless of duration) and 0 otherwise. In subsequent analyses I refine the variable further by using different categories of duration of exposure during childhood.

To examine the aggregate exposure to all camps in the young generation using the intensity index (I_{vk}), I estimate equation 2 for the young generation.

6. Results and Discussion

In the following sections, I present the results for adults who were children at the opening of the camps (long-term effect), followed by results on the younger generation.

6.1 Impact of exposure to camp presence on adults who were children at the opening of camps (long-term effect)

Table 4 shows the results of the baseline specification equation (1) when treatment is defined using the discrete distance thresholds. The table shows a negative effect of camp exposure and the effect appears to be stronger when treatment is defined closer to the camp (a decline in the HAZ by 0.4 standard deviations) and tapers off as the distance threshold is increased. When the distance threshold is increased to 170km there is no statistically significant effect, suggesting that the effect of exposure was localised.

Turning to the effect identified in the literature, Baez (2011), finds a worsening of HAZ by 0.3 standard deviations in childhood. In adulthood, I find a decline ranging between 0.2-0.4 standard deviations depending on how strictly the treatment threshold is defined. Abstracting from the differences in methodology, dataset and cohorts between this paper and Baez, 2011, the findings in this section confirm a negative effect of camp exposure on the health of this older generation and provide first evidence that the negative effect lasted into adulthood.

Viewing the results in the context of the HAZ of Tanzania may better contextualise this finding. Considering an average individual in Tanzania, a decrease in HAZ of 0.2-0.4 from the average of -1.5, is a non-trivial effect. This implies a reduction of the HAZ to a range of -1.7 to -1.9 which would tip the individual closer to stunting (HAZ of less than -2).

The magnitude of the effect can also be expressed in terms of earnings using existing estimates of the association between HAZ and labour market outcomes in developing countries. One such estimate, though not from Tanzania, suggests that a 1 standard deviation increase in HAZ at 3 years raises hourly earnings by 14.8% (Hoddinot et al., 2011). Using this as a simple guide would translate the magnitude of the effect to a 2.9% to 5.9% reduction in adult hourly earnings for an individual that was 3 years of age.

It is interesting to point out the negative coefficient on the *male* variable in Table 4 and indeed in all regression tables. The results indicate that being male is associated with a lower height-for-age z-score than being female. This is also confirmed by other data on Tanzania. As an example, in the 2015 Demographic Health Survey for Tanzania, males have an average HAZ of -1.5 compared to -1.4 for females.

Table 4: Long-term impact of exposure to camps on adults who were children in 1993 (using distance thresholds)

	HAZ				
	(1)	(2)	(3)	(4)	(5)
treat20*childopen	-0.400**				
	(0.161)				
treat50*childopen		-0.285**			
		(0.126)			
treat80*childopen			-0.195*		
			(0.114)		
treat100*childopen				-0.236**	
				(0.105)	
treat170*childopen					-0.061
					(0.104)
male	-0.320***	-0.319***	-0.315***	-0.316***	-0.324***
	(0.048)	(0.047)	(0.048)	(0.047)	(0.047)
Observations	1,313	1,313	1,313	1,313	1,313
R-squared	0.145	0.145	0.146	0.147	0.153
Birth year fixed effects	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes

Note: The individuals in this sample are those in subgroups 1 and 2 in exposure Table 1. “treat d” is a dummy variable that takes 1 for individuals within d km of a camp and 0 otherwise. All models include a constant term and the base treatment term. Robust standard errors in parentheses, clustered at village level. *** p<0.01, ** p<0.05, * p<0.1

6. 1.1 Does the effect vary by duration of exposure?

Because the result above only considers whether an individual was a child when the camp opened regardless of the duration of the exposure, I explicitly examine whether the effect varies by length of exposure. It should be noted that year-of-birth fixed effects do not fully control for the effect of duration because individuals born in the same year may have different durations based on which camp is nearest.

On average individuals in the adult sample were exposed to a camp in their childhood for 3 years. I classify individuals into three categories of exposure based on the duration of exposure – 0 years of exposure (no exposure), 1 to 2 years ($duration_{short}$) and 3 or more than 3 years ($duration_{long}$). The 0 years of exposure serves as the reference group. This can be thought of an extension of equation 1 above where I now use different levels instead of the binary $Child\_open$ variable. Specifically, I interact duration with the discrete distance thresholds to examine if treatment varies by duration.

$$HAZ_{ivk} = \beta_0 + \beta_1 (TREAT_d) + \beta_2 (TREAT_d * duration_{short}) + \beta_3 (TREAT_d * duration_{long}) + X_i + \delta_k + \theta_j + e_{ivk} \quad (1.1)$$

The results in Table 5 reveal interesting insights into the effect of duration of exposure. First, when treatment is restricted to areas in close proximity to the camps (where treatment is defined as being within 20km or 50km of a camp), the effect of being exposed for a shorter duration is greater than that of being exposed for a longer duration relative to the reference group (i.e. exposure of 0 years). In other words, when considering the effect of being in very close proximity to a camp, being exposed for a shorter duration was more detrimental than being exposed for a longer duration.

The opposite is true when the distance is increased to include areas farther from the camps (where treatment is defined as being within 80km or 100km away from the nearest camp). In this case, the effect of being exposed for a shorter duration relative to 0 years is not statistically significant. Rather, it is the effect of being exposed for a longer duration relative to 0 years that is statistically significant.

Why is it that for areas closest to the camps short exposure in childhood is more detrimental than longer exposure? This appears to be consistent with the anecdotal evidence discussed in the context section that areas around the camp received investment in infrastructure and an influx of humanitarian aid. This result seems to suggest that some cohorts may have benefitted from the economic development around the camps – children that were exposed for a shorter duration in childhood would have had a shorter span of benefit whereas those that were exposed for a longer window in childhood would have benefitted the longest from such investments. When the threshold is widened to areas farther away from the camps, exposure for a shorter time had no statistically significant effect, supporting the earlier finding that the negative effect was localised. However, longer exposure is statistically significant suggesting that the benefits of aid around camps were also localised. This is a new result in the literature.

Table 5: Long-term effect of camp exposure by distance thresholds from nearest camp and duration of exposure

	HAZ				
	(1)	(2)	(3)	(4)	(5)
treat20	0.220				
	(0.193)				
treat20*duration short	-0.618**				
	(0.282)				
treat20*duration long	-0.323*				
	(0.176)				
treat50		0.120			
		(0.159)			
treat50*duration short		-0.347*			
		(0.186)			
treat50*duration long		-0.243			
		(0.149)			
treat80					
			-0.107		
treat80*duration short			(0.118)		
			-0.0372		
treat80*duration long			(0.123)		
			-0.337**		
treat100				-0.130	
				(0.121)	
treat100*duration short				-0.0688	
				(0.112)	
treat100*duration long				-0.393***	
				(0.129)	
treat170					-0.279**
					(0.116)
treat170*duration short					-0.00945
					(0.112)
treat170*duration long					-0.121
					(0.123)
male	-0.321***	-0.320***	-0.313***	-0.313***	-0.323***
	(0.0477)	(0.0466)	(0.0476)	(0.0474)	(0.0476)
Observations	1,313	1,313	1,313	1,313	1,313
R-squared	0.146	0.145	0.148	0.150	0.154
Birth year fixed effects	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,313	1,313	1,313	1,313	1,313

Notes: The individuals in this sample are those in subgroups 1 and 2 in exposure Table 1. "treat d" is a dummy variable that takes 1 for individuals within d km of a camp and 0 otherwise. All models include a constant term. The reference group is those that have 0 duration of exposure. Robust standard errors in parentheses, clustered at village level. *** p<0.01, ** p<0.05, * p<0.

6. 1.2 Does the effect vary by age at exposure?

While the results in Tables 4 and 5 suggest that there is a negative effect on HAZ as a result of exposure to the refugee shock for those adults that were closest to camps, an important question is whether certain ages were more affected. To investigate this, I consider whether the effect of the refugee shock is heterogeneous across age groups. The differentiation of exposure by age group makes it possible to examine whether being exposed to the refugee shock had a differential effect at a certain critical age group compared to another. I group those who were children at the beginning of the influx into three age categories 0-6, 7-12, and 13-19 based on their age at the opening of the nearest camp. This age grouping aligns with the timing of growth spurts and is constructed with reference to the relationship between age and growth (Akresh, 2017, Case and Paxson, 2008). To test for the possibility of heterogeneous effects across age groups, I estimate the following regressions that build on equation 1. The oldest age group, age category 13-19 serves as the reference group.

$$HAZ_{ivk} = \beta_0 + \beta_1 (TREAT_d) + \beta_2 \left(TREAT_d * ageatexposure_{0\_6}\right) + \beta_3\left(TREAT_d * ageatexposure_{7\_12}\right) + X_i + \theta_j + e_{ivk} \qquad (1.2)$$

The results in Table 6 show that the effect is larger for the younger age groups. Relative to those who were age 13-19 at the time of the refugee shock, age groups 0-6 (exposed in early childhood) were more negatively affected than were those who were age 7-12. When treatment is defined as being within 20km of a camp, being in the youngest group (0-6) is associated with a reduction of HAZ of 0.59 relative to the oldest group while being in the middle age group (7-12) relative to the oldest is associated with a reduction of HAZ of 0.48.

In the previous subsection, the result showed that exposure for short duration was more detrimental. This subsection finds that the effect was larger for the youngest age group relative to those who were older.

Although these findings may seem counterintuitive, the two findings are not inconsistent. This is because age at exposure does not necessarily correlate with duration of exposure because they vary by camp. To illustrate this point, consider an individual who was in the youngest age group and whose nearest camp was Lukole A (closed in 2008). Such an individual would have been exposed for a long duration. On the other hand, an individual in the youngest age group whose nearest camp was Omukariro (closed in 1996) would have been exposed for a short duration.

Table 6: Long-term effect of camp exposure across age groups

	HAZ				
	(1)	(2)	(3)	(4)	(5)
treat20	0.242				
	(0.186)				
Age 7-12 at opening*treat20	-0.481***				
	(0.173)				
Age 0-6 at opening*treat20	-0.591**				
	(0.257)				
treat50		0.168			
		(0.165)			
Age 7-12 at opening*treat50		-0.395**			
		(0.156)			
Age 0-6 at opening*treat50		-0.438***			
		(0.159)			
treat80			0.0305		
			(0.133)		
Age 7-12 at opening*treat80			-0.357**		
			(0.138)		
Age 0-6 at opening*treat80			-0.380***		
			(0.130)		
treat100				-0.0704	
				(0.138)	
Age 7-12 at opening*treat100				-0.395***	
				(0.123)	
Age 0-6 at opening*treat100				-0.367***	
				(0.121)	
treat170					-0.133
					(0.125)
Age 7-12 at opening*treat170					-0.263***
					(0.0873)
Age 0-6 at opening*treat170					-0.347***
male	-0.330***	-0.325***	-0.321***	-0.322***	-0.343***
	(0.0619)	(0.0615)	(0.0618)	(0.0612)	(0.0619)
					(0.0991)
Observations	910	910	910	910	910
R-squared	0.153	0.154	0.157	0.162	0.167
Birth year fixed effects	No	No	No	No	No
Region fixed effects	Yes	Yes	Yes	Yes	Yes

Notes: The individuals in this sample are those in subgroups 1 and 2 in exposure Table 1. “treat d” is a dummy variable that takes 1 for individuals within d km of a camp and 0 otherwise. All models include a constant term. The reference group is those that were age 13-19 at exposure. Robust standard errors in parentheses, clustered at village level. *** p<0.01, ** p<0.05, * p<0.

6. 1.3 Intensity analysis

The results presented thus far only consider the effect associated with the nearest camp. To investigate the aggregate effect of all camps, I present the results of equation 2 that uses the by village by cohort intensity index described in section 4.4. The descriptive statistics of the intensity index are provided in the Appendix.

Table 7 shows the results where I estimate the aggregate effect of all camps only on those that were exposed. Column 1 presents the results of the effect of proximity and duration whereas column 2 present the results that include the population of the camps as weights. I find that intensity has a negative and statistically significant effect on HAZ. The magnitude increases when I use the intensity index that is adjusted for camp population implying that it is not just distance to camps and duration that have an effect but also the size of camps. This would be consistent with the reports that refugee labourers competed with locals for low wage jobs and other resources – the bigger camps would have had more competition for resources. Additionally, it would also be consistent with reports of disease outbreaks at the beginning of the influx – bigger camps would have been more susceptible to epidemics.

To interpret the result more easily, we can consider the aggregate effect of exposure for a child that had average exposure to all camps relative to one that had the least exposure i.e. lower intensity index. The coefficient in column 2 suggests that exposure to camps for an individual of average exposure relative to the least exposed is associated with a decrease in HAZ of 0.09.[4]

[4] The calculation is obtained by multiplying the coefficient on *intensity by* the difference between the average and minimum of the intensity index I_{vk} . See Table A4 in the Appendix.

Table 7: Intensity analysis – adult sample

	HAZ	
	(1)	(2)
Intensity	-0.147***	
	(0.0523)	
Population adjusted intensity		-5.549**
		(2.141)
male	-0.319***	-0.319***
	(0.0476)	(0.0477)
Observations	1,313	1,313
R-squared	0.148	0.150
Birth year fixed effects	Yes	Yes
Region fixed effects	Yes	Yes

Note: The individuals in this sample are those in subgroups 1 and 2 in exposure Table 1. All models include a constant term. Robust standard errors in parentheses, clustered at village level. *** p<0.01, ** p<0.05, * p<0.1

6.2 Impact of exposure to camp presence on the younger generation

The results do not show any statistically significant effect of camp exposure on the health of the younger generation i.e. the last two birth cohorts across all distance thresholds (see Table 8). In other words, those who have had the nearest camp operational at some point in childhood are not statistically differentially affected relative to those born after the camp closed. This is in contrast to the results for the older generation in Table 5, where there was a significant negative effect across all distance thresholds except the farthest.

I conduct a similar exercise as that described in section 6.1.1 to examine whether, for the young generation, there is a differential effect of exposure by duration of exposure. I re-estimate equation 1.1 on the younger sample. In the younger generation, the effect of being exposed for a short duration (1 to 2 years) relative to 0 years is not statistically significant irrespective of how I define treatment distance threshold (see Table 9). The effect of being exposed for a longer duration relative to 0 years is also not significant at all treatment thresholds.

Even when using the intensity index, the results (Table 10) show no statistically significant effect among the young generation, again corroborating the previous findings, which found a significant effect in the older cohorts but none in the younger generation. In other words, there is no statistically significant effect between those born when camps were operational and those born after their closure, whether using distance thresholds or the intensity index.

Table 8: Impact of exposure to camps on young generation (using distance thresholds)

	HAZ				
	(1)	(2)	(3)	(4)	(5)
treat20* C_i	-0.232				
	(0.252)				
treat50* C_i		0.0493			
		(0.226)			
treat80* C_i			-0.225		
			(0.181)		
treat100*C_i				-0.0745	
				(0.171)	
treat170*C_i					-0.103
					(0.127)
male	-0.305***	-0.303***	-0.308***	-0.305***	-0.302***
	(0.0446)	(0.0450)	(0.0456)	(0.0459)	(0.0447)
Observations	2,621	2,621	2,621	2,621	2,621
R-squared	0.152	0.155	0.159	0.158	0.152
Birth year fixed effects	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes

Note: The individuals in this sample are those in subgroup 3 in the exposure Table 1. "treat d" is a dummy variable that takes 1 for individuals within d km of a camp and 0 otherwise. All models include a constant term and the base treatment term. Robust standard errors in parentheses, clustered at village level. *** p<0.01, ** p<0.05, * p<0.1

Table 9: Effect of camp exposure on younger generation by distance thresholds from nearest camp and duration of exposure

	HAZ				
	(1)	(2)	(3)	(4)	(5)
treat20	0.0983				
	(0.189)				
treat20*duration short	-0.352				
	(0.255)				
treat20*duration long	-0.185				
	(0.277)				
treat50		-0.264*			
		(0.145)			
treat50*duration short		0.0128			
		(0.218)			
treat50*duration long		0.0707			
		(0.261)			
treat80			-0.252**		
			(0.126)		
treat80*duration short			-0.0701		
			(0.166)		
treat80*duration long			-0.346		
			(0.224)		
treat100				-0.366***	
				(0.127)	
treat100*duration short				0.0288	
				(0.158)	
treat100*duration long				-0.174	
				(0.222)	
treat170					0.0386
					(0.110)
treat170*duration short					-0.0395
					(0.118)
treat170*duration long					-0.159
					(0.166)
male	-0.306***	-0.304***	-0.305***	-0.304***	-0.300***
	(0.0446)	(0.0450)	(0.0454)	(0.0460)	(0.0448)
Observations	2,621	2,621	2,621	2,621	2,621
R-squared	0.153	0.155	0.160	0.159	0.153
Birth year fixed effects	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes

Note: The individuals in this sample are those in subgroup 3 in exposure Table 1. “treat d” is a dummy variable that takes 1 for individuals within d km of a camp and 0 otherwise. All models include a constant term and the base treatment term. Robust standard errors in parentheses, clustered at village level. *** p<0.01, ** p<0.05, * p<0.1

Table 10: Intensity analysis– young generation

	HAZ	
	(1)	(2)
Intensity	-0.102	
	(0.0916)	
Population adjusted intensity		-4.863
		(3.176)
male	-0.301***	-0.302***
	(0.0448)	(0.0450)
Observations	2,621	2,621
R-squared	0.153	0.154
Birth year fixed effects	Yes	Yes
Region fixed effects	Yes	Yes

Note: The individuals in this sample are those in subgroup 3 in exposure Table 1. All models include a constant term. Robust standard errors in parentheses, clustered at village level. *** p<0.01, ** p<0.05, * p<0.1

7. Robustness checks

7.1 Testing for pre-trends

To explore the validity of the identification assumption, I conduct a test for pre-trends. Specifically, I test for whether there were differential trends by distance to the nearest camp ($distance_{vc}$) for individuals who were mature in 1993 (subgroup 1 in Table 1). I assume the refugee shock took effect at an earlier date (placebo treatment), by splitting the subgroup halfway into those that were age 20-25 years and those that were age 26 to 30 years in 1993. I use a "placebo-after" dummy that denotes whether or not an individual was born after the placebo treatment and estimate the following equation.

$$HAZ_{ivk} = \beta_0 + \beta_1 (distance_{vc}) + \beta_2 (Placeboafter * distance_{vc}) + X_i + \delta_k + \theta_j + e_{ivk} \quad (3)$$

Since this placebo treatment precedes the actual refugee shock and the individuals considered here were already mature at the time of the influx, the difference-in-difference estimator β_2 should be statistically insignificant. Table A5 in the Appendix shows that this is the case.

7.2 Balancing test

The results may also be undermined if there were compositional changes in the treatment and control areas over time. To test the validity of this assumption, I use the sample of individuals born between 1993 and 2012 and test whether a series of covariates changed by cohort in treated versus untreated regions. I estimate covariate balance regressions of this form:

$$Covariate_{ivk} = \beta_0 + \beta_1 (TREAT_d) + \beta_2 (TREAT_d * C_i) + \delta_k + \theta_j + e_{ivk} \quad (4)$$

I test this for household size, the gender of the individual, whether the household is rural, education of the household head, age of the household head, gender of the household head and whether the household head's occupation is agriculture. Under the hypothesis that there were no compositional changes, we would expect β_2 to be 0.

I find the difference in difference is not significant for all the covariates across all treatment thresholds except for the covariate for gender of the respondent and the likelihood of having a male head. However, even the likelihood of having a male head is significant only at the 10% (see Table A6 in the Appendix). I take this as evidence that there were no compositional changes, on those observables, over time between treatment and control areas. I include gender as a control in all regressions.

7.3 Alternative measures of exposure

7.3.1 Continuous distance

To exploit the full variation in distance to camps, I re-estimate the baseline specification 1 using the continuous distance from an individual's village to the nearest camp as an alternative to the discrete distance thresholds. I also use the log of the distance as another alternative measure.

For the older generation, the effect of having been exposed to camps in childhood is negative and statistically significant (see Table A7 in the Appendix). Note the coefficients when using continuous distance and log distance are positive because the relationship between HAZ and distance is inverse – the negative effect is reduced as one moves away from the camp. To interpret the result more easily, we can consider the effect of moving 10km *away* from the nearest camp. The result suggests that this is associated with a small improvement in HAZ of 0.008.[5] However, the effect is only statistically significant at the 10% (see Table A7 in the Appendix).

Although when using continuous distance, the effect of having been exposed to camps in the young generation, is negative, it is smaller in magnitude to that in the older generation (see Table A8 in the Appendix).

7.3.2 Distance bands

To examine the localisation of the effect suggested by the results, I classify distance to the nearest camp into three categories namely, 0-20 kilometres, 20-100 kilometres and greater than 100 kilometres. Under this classification, 18 villages lie within 20km of a camp, 37 villages lie between 20 - 100km of a camp, and 139 villages are more than 100km but less than 430km from a camp. I

[5] The calculation is obtained by multiplying the coefficient on the interaction term of $distance_v * Child\_open$ by 10km.

use this classification to assess the effect associated with being near or at an intermediate distance to a camp relative to being far from a camp.

I use the following equation, which is a variant of baseline equation 1:

$$HAZ_{ivk} = \beta_0 + \beta_1 (distance0\_20km * Child\_open) + \beta_2 (distance20\_100km * Child\_open) + X_{iv} + \delta_k + \theta_j + e_{ivk} \quad (5)$$

The results also suggest that the effect was localised (See Table A9 in the Appendix). The effect between those who were nearest and farthest is a reduction in HAZ of 0.36. The effect between those that are at the intermediate distance (20-100 km) and farthest (more than 100km) is lower at 0.2. I also tested the p-values for equality between the distance categories. Specifically, the test for whether there is a differential effect between those in 0-20km and 20-100km is significant. The results indicate a decreasing gradient – exposure was associated with negative impact on the HAZ and the effect is stronger for areas closest to camps relative to those farthest away.

I also estimate the analogous regression for the younger generation where the indicator variable denotes whether the nearest camp was operational at some point in childhood or whether the individual was born after the camp closed. In contrast to the result from the old generation, In the young generation, I find no statistically significant effect of camps on those who were closest (0-20km) relative to those who were farthest (more than 100km) or on those at an intermediate distance relative to those farthest (see Table A10 in the Appendix). This finding corroborates the previous results of a lack of statistically significant effect in the younger generation using either the distance thresholds or intensity index.

8. Conclusions

This paper investigated the long-term impact of refugee camps on the health of local residents. First, it considered individuals who were children at the beginning of the refugee influx and examined the effect on their health as adults. The findings show that for this generation, camp exposure has had a negative impact on their adult health. While the existing literature shows a negative effect in childhood, this paper provides evidence that this effect lasted into adulthood.

However, the effect is lower for those individuals who were exposed for a longer duration. This finding is consistent with reports of the nature of the refugee crisis at the time – both the government of Tanzania and the UNHCR were not prepared for the scale and swiftness of the crisis. Outbreaks of disease, pressure on resources, deforestation and land degradation were reported. In these circumstances, those who were exposed for the shortest duration would have been exposed to the worst of the situation. On the other hand, camp presence was followed by a substantial inflow of humanitarian aid and infrastructure investment. To the extent that there was economic development around the camps, the findings suggest that those who were exposed for a longer duration in their childhood would have benefitted from this economic development the longest, possibly affording them the opportunity to recover from the initial adverse effect.

A concern of policy relevance is the magnitude of the observed negative effect. I find an effect of between -0.2 and -0.4 standard deviations in HAZ when using discrete distance thresholds and this effect is significant at the 10%. Using existing estimates of the association between height and labour market outcomes, a simple back-of-the-envelope calculation would translate the effect to a 2.9% to 5.9% reduction in adult hourly earnings.

However, when it comes to the younger generation, those who were exposed to the refugee presence do not fare any differently from those born in the post-camp era. As noted above, the finding that those who were exposed for the longest duration were less negatively affected, suggests that relief efforts, the associated investment in the region and the resultant local trading opportunities may have reduced the adverse effects of the camps over time. The expectation would then be that when camps close, there would be an adverse effect because relief efforts cease and local economic opportunities that were hitherto generated by the camps shrink. Consequently, later cohorts' health could be affected by the closure relative to those that benefited from aid and opportunities in the later stages of the camps' existence. However, I find that this is not the case - the cohorts born after the camps are not worse off than those cohorts born when the camps were in operation. This is possible given that one channel through which health was potentially affected was due to initial disease burden, poor sanitation, deforestation and land degradation. Over the years and by closing, these issues had been curbed hence those exposed to the camps in the younger generation are not different from those born in the post-camp period. Secondly, it suggests that the camp rehabilitation and reconstruction programme described earlier that was established in 2008 to fill the gap left by the withdrawal of humanitarian agencies, mitigated the adverse effects from the loss of benefits that would otherwise have been left by the closure of the camps. However, as health is only one dimension, the efficacy of this program is deferred to future research.[6]

Some limitations of the analysis should be noted. Although I provide support for the robustness of the identification strategy, I acknowledge that it is not able to address completely the possibility of unobserved time-variant characteristics that may be related to the camps. The results presented are also conditional on survival since only living individuals are observed. The results are therefore likely a lower bound estimate of the actual effect. Similarly, because self-reported age is used to

[6] This paper has suggested humanitarian aid and infrastructure generated by the camps as a plausible channel through which local health was affected. It has found no effect on health upon closure of camps suggesting that the rehabilitation and reconstruction programme may have mitigated the potential adverse effects from camp closure. However, the efficacy of this programme clearly warrants further empirical investigation as at the time of writing, it has not been possible to obtain the data required to do so.

calculate HAZ, the results obtained are likely to be an underestimate of the true effect (Akresh et al, 2011).

Nevertheless, the findings of this paper remain insightful for policy in the current climate of unprecedented refugee flows and in view of the climate-induced forced displacement that is predicted to occur in the future. A major challenge currently confronting several countries and one that is likely to remain relevant is how to sustainably support host regions. This paper's findings demonstrate that the onset of forced displacement flows is a critical window – conditions at the onset could have persistent effects. The results also show that a long-term perspective is necessary as the impact on host regions and their residents change over time. This is an important consideration to ensure the development of these regions can be sustained even after the withdrawal of humanitarian and relief efforts.

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Appendix

Table A1: Summary statistics of Intensity Index (Entire sample)

	mean	sd	min	max
Intensity	0.53	0.77	0.00	5.65
Population adjusted intensity	0.02	0.02	0.00	0.20
Observations	3934			

Table A2: Summary statistics of Intensity Index (Adult sample)

	mean	sd	min	max
Intensity	0.61	0.85	0.00	5.65
Population adjusted intensity	0.02	0.02	0.00	0.20
Observations	1313			

Table A3: Summary statistics of Intensity Index (Young generation sample)

	mean	sd	min	max
Intensity	0.48	0.72	0.01	5.59
Population adjusted intensity	0.01	0.02	0.00	0.15
Observations	2621			

Table A4: Least exposed cohort versus average exposed cohort (Intensity index)

		coefficient	Mean of intensity Index	Minimum of intensity index	Difference in intensity	Associated change in HAZ
1 unit increase in intensity (Adult sample)	Table 7	-0.147	0.61	0.00	0.61	-0.09
1 unit increase in intensity (Young sample)	Table 10	-0.102	0.48	0.01	0.47	-0.05

Robustness checks results

Table A5: Placebo test

	HAZ
Dist. Nearest camp	0.0011
	(0.001)
Dist. Nearest camp * Placeboafter	0.0002
	(0.001)
male	-0.3054***
	(0.102)
Observations	305
R-squared	0.110
Birth year fixed effects	Yes
Region fixed effects	Yes

Note: The individuals in this sample are those in subgroup 1 in exposure table 1 in the appendix. All models include a constant term. Robust standard errors in parentheses, clustered at village level. *** p<0.01, ** p<0.05, * p<0.1

Table A6: Balance test

	DID (treat20*Ci)	DID (treat50*Ci)	DID (treat80*Ci)	DID (treat100*Ci)	DID (treat170*Ci)
Household size	-0.635	0.0728	-0.353	-0.312	0.168
Child is male	-0.139*	-0.0308	-0.102**	-0.130***	0.0148
Rural household	0.0221	-0.0313	0.0503	-0.0485	0.0195
Education of household head	0.419	-0.28	-0.0967	-0.0519	0.0267
Age of household head	3.179	1.61	2.57	1.775	0.194
Household head is male	0.00914	0.127*	0.0949*	0.0834	0.0275
Household head's occupation is agriculture	-0.0774	-0.0498	-0.00529	0.00331	0.0652

Note: The individuals in this sample are those in subgroup 3 in exposure table 1 in the appendix. All models include a constant term, treat d level effect, region fixed effects and year of birth fixed effects. Ci indicates if individual was born before or after camp closed. Robust standard errors in parentheses, clustered at village level. *** p<0.01, ** p<0.05, * p<0.1

Table A7: Long-term effect of camp exposure, by continuous distance to the nearest camp

	HAZ	
	(1)	(3)
Dist. Nearest camp	0.0015***	
	(0.001)	
Dist. Nearest camp*childopen	0.0008*	
	(0.000)	
log Dist. Nearest camp		0.1332**
		(0.053)
log Dist. Nearest camp *childopen		0.0540**
		(0.026)
male	-0.3201***	-0.3165***
	(0.047)	(0.047)
Observations	1,313	1,313
R-squared	0.159	0.153
Birth year fixed effects	Yes	Yes
Region fixed effects	Yes	Yes

Note: The individuals in this sample are those in subgroups 1 and 2 in exposure table 1. All models include a constant term. Robust standard errors in parentheses, clustered at village level. *** p<0.01, ** p<0.05, * p<0.1

Table A8: Impact of exposure to a camp in childhood on young generation, by continuous distance to the nearest camp

	HAZ	
	(1)	(2)
Dist. Nearest camp	0.0004	
	(0.001)	
Dist. Nearest camp*C_i	0.0006**	
	(0.000)	
log Dist. Nearest camp		0.1074
		(0.083)
log Dist. Nearest camp*C_i		0.0128
		(0.014)
male	-0.3030***	-0.3035***
	(0.045)	(0.045)
Observations	2,621	2,621
R-squared	0.153	0.154
Birth year fixed effects	Yes	Yes
Region fixed effects	Yes	Yes

Notes: The individuals in this sample are those in subgroups 7 to 11 in exposure table 1 in the appendix. C_i is a dummy that takes 1 if the nearest camp was operational at some point in childhood and 0 otherwise. All models include a constant term. Robust standard errors in parentheses, clustered at village level. ***p<0.01, ** p<0.05, * p<0.1

Table A9: Long-term effect of camp exposure, by distance bands from nearest camp

	HAZ
(0-20km) * childopen	-0.362**
	(0.143)
(20-100km) * childopen	-0.234**
	(0.096)
male	-0.316***
	(0.048)
Observations	1,313
R-squared	0.148
Birth year fixed effects	Yes
Region fixed effects	Yes

Notes: The individuals in this sample are those in subgroups 1 and 2 in exposure table 1 in the appendix. All models include a constant term. The reference group is those more than 100km away from a camp. Robust standard errors in parentheses, clustered at village level. *** p<0.01, ** p<0.05, * p<0.

Table A10: Impact of camp exposure on younger generation, by distance bands from nearest camp

	HAZ
(0-20km) * C_i	-0.315
	(0.276)
(20-100km) * C_i	-0.266
	(0.234)
male	-0.308***
	(0.0453)
Observations	2,621
R-squared	0.154
Birth year fixed effects	Yes
Region fixed effects	Yes

Notes: The individuals in this sample are those in subgroup 3 in exposure table 1 in the appendix. C_i is a dummy that takes 1 if the nearest camp was operational at some point in childhood and 0 otherwise. All models include a constant term. The reference group is those more than 100km away from a camp. Robust standard errors in parentheses, clustered at village level. *** p<0.01, ** p<0.05, * p<0.