The Development Push of Refugees : Evidence from Tanzania*

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March 2018

Abstract

We exploit a 1991–2010 Tanzanian household panel to assess the effects of the temporary refugee inflows originating from Burundi (1993) and Rwanda (1994). We find that the refugee presence has had a persistent and positive impact on the welfare of the local population. We investigate the possible channels of transmission, underscoring the importance of a decrease in transport costs as a key driver of this persistent change in welfare. We interpret these findings as the ability of a temporary shock to induce a persistent shift in the equilibrium through subsequent investments rather than a switch to a new equilibrium in a multiple-equilibrium setting.

Keywords: Refugees; Tanzania; Multiple equilibrium; Roads.

JEL Classification: I32; O18; Q54

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*We thank Jenny Aker, Jennifer Alix-Garcia, Olivier Bakewell, Michael Clemens, Mathias Czaika, Kalle Hirvonen, Michael Lipton, Anna Maria Mayda, Maria Navarro Paniagua, Walter Steingress, Jacques Thisse, Mathias Thoenig, Mathis Wagner, and participants in the Migration and Development Conference (Oxford), the LICOS seminar (Leuven, Belgium), the Sussex seminar (Brighton, UK), the IFPRI seminar (Washington DC), the Graduate Institute economic seminar (Geneva), the Lancaster seminar, the Centre for Research in Economic Development workshop (Namur, Belgium), the Centre for the Study of African Economies conference (Oxford), the Royal Economic Society meeting (London) and conference (Brighton) and the Households in Conflict Network workshop (Berkeley, CA, US) for their comments and suggestions. We are also indebted to the World Bank and Kathleen Beegle for granting access to confidential geographic data of the Kagera Health and Demographic Surveys. We specially thank Joachim De Weerdt (Economic Development Initiatives) and Kalle Hirvonen (IFPRI) for their help with the data and for sharing some of their own constructed data. Jose Funes provided valuable research assistance with GIS data. The first author is grateful to the Centre for Institutions and Economic Performance (LICOS), KU Leuven and the International Food Policy Research Institute (IFPRI) for their support during his post-doctoral position. J.-F. Maystadt acknowledges financial support from the IFPRI Strategic Innovation Funds, the CGIAR Research Program on Policies, Institutions, and Markets, and the KU Leuven research fund (Methusalem). The authors declare that they have no relevant or material financial interests that relate to the research described in this paper.

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1 Introduction

What are the long-term effects of temporary population shocks? Are these effects, if any, caused by a switch of equilibrium in a multiple-equilibrium setting, or are they the consequence of post-shock investments that shift the supply curve and thus the equilibrium? In the case of a shift in equilibrium, what are the investments that drive this shift? To answer these questions we exploit a 1991–2010 Tanzanian household panel to assess the effects of the inflow of temporary refugees originating from Burundi (1993) and Rwanda (1994). We find that the refugee presence has a persistent and positive impact on the welfare of the local population. We investigate the possible channels of transmission, underscoring the importance of a decrease in transport costs. We interpret these findings as the ability of a temporary shock to induce a persistent shift of the equilibrium through subsequent infrastructure investments rather than an immediate switch to a new equilibrium in a multiple-equilibrium setting.

These findings are important because large population shocks occur frequently and are often the source of considerable social tensions. After World War II, the newly-established United Nations High Commissioner for Refugees (UNHCR) recognized the existence of 400,000 refugees. The decolonization period, as well as the resurgence of civil wars after the end of the Cold War, led to a rapid increase in the number of people seeking protection in foreign countries, including the mass flights of Kurds from Northern Iraq, refugees fleeing inter-ethnic violence in former Yugoslavia, and the more than 2 million Rwandans fleeing to former Zaire, Tanzania, Burundi, and Uganda in 1994. **UNHCR (2016) reported about 22.5 million refugees in developing countries at the end of 2016, of which 84 percent were hosted in developing countries.** The recent surge in asylum seekers from Syria and North Africa in the European Union makes the question of the socio-economic consequences of population inflows even more pressing.

Importantly, about 70 percent of refugees have that status for more than five years so their presence may have far-reaching consequences on their local hosts, as they interact with the host economies. Furthermore, most refugees are hosted by their neighboring countries, not necessarily facing much better economic conditions. The Horn of Africa offers a recent example (UNHCR, 2012). Repeated violence, combined with a severe drought in 2011, is responsible for more than 1 million Somali refugees, who are almost exclusively hosted in neighboring countries such as Kenya, Ethiopia, Yemen, Djibouti and Eritrea. Recent conflicts in Syria have also been followed by the inflow of hundreds of thousands of people hosted mainly in neighboring countries such as Turkey, Lebanon, Jordan, or Iraq.

These patterns of forced migration flows into neighboring countries have led some scholars to argue that such population shocks may explain the existence of conflict spillovers by creating political and social tensions in hosting countries (Azam and Hoeffler, 2002; Salehyan, 2008). Montalvo and Reynal-Querol (2007) have also warned against the risk of malaria propagation in refugee-receiving countries. However, these cross-country analyses face the challenges of distinguishing the causal impact of refugees from that of other conflict spillovers and identifying specific channels of transmission. Assessing the consequences of major flows of forced migrants across areas of the same country that have been differently exposed to the presence of refugees should allow for a better identification of these channels and will better inform policies to accompany these shocks in the future.

Furthermore, whether the changes in the host economy after the departure of refugees result from a switch to a new (and better) equilibrium or from a shift in the existing equilibrium is of fundamental policy importance. The existence of multiple equilibria may justify extensive policy experimentations to attempt a jump to a better equilibrium. If it is instead the same equilibrium that shifts, it becomes important to understand the precise drivers of this shift and perform some cost-benefit analysis when public investment is involved.

To answer the questions raised above, three main challenges need to be overcome. The first is to find a large temporary population shock. Our work exploits one of the largest inflows of refugees in modern times. About 1 million refugees were forced to leave Burundi in 1993 and Rwanda in 1994 to be hosted in the neighboring region of Kagera in Tanzania. All refugees from Rwanda were repatriated in 1996, and by 2004 most refugees from Burundi had moved back to their country of origin or relocated into a neighboring region.

The second challenge is to find appropriate data tracking the local population over a long period of time. By surveying exactly the same households between 1991 and 2010, the Kagera Health and Development Survey dataset provides the opportunity to assess the impact of refugees up to 14 years after the bulk of them were forced to repatriate.

The third main challenge is to develop a suitable estimation strategy. We argue and empirically show that such refugee inflows can be considered as a natural experiment. This characterization allows us to demonstrate the exogeneity of the economic improvements in the Kagera region even long after the refugees' departure. We then show that these improvements are best interpreted in the context of a lowering of trade costs following road construction to serve refugee camps.

Our work contributes to the literature on the long-run effects of shocks and the identification of multiple equilibria. Since the seminal paper of Davis and Weinstein (2002), it has become common to exploit exogenous variation in bombing intensity in war episodes to investigate that issue (Brakman et al., 2004; Miguel and Roland, 2011). Those papers have tended to reject the existence of multiple equilibria, observing a return to pre-existing patterns of economic activity and population distribution (Brakman et al., 2004; Davis and Weinstein, 2002), poverty levels, population density, infrastructure, and human capital (Miguel and Roland, 2011). However, that there is a persistent equilibrium in some cases is not enough to dismiss the notion of multiple equilibria. An alternative approach is to investigate the path dependence resulting from historical events. Bleakley and Lin (2012) showed that even though the historical advantages linked to the proximity to portage sites have become obsolete over time, such a proximity has still contemporaneous consequences on the distribution of population and economic activity. This may suggest that there were initially multiple equilibria. Then, after one equilibrium was chosen it turned out to be extremely persistent. While this interpretation is interesting, the evidence is indirect.[1]

Showing a large change over a period of time is necessary, but not sufficient for multiple equilibria to play a role, since one also needs to prove that there was no change in the fundamentals underlying the perhaps unique equilibrium. We show that in the case of the Kagera region the large changes that occurred after the arrival of the refugees and persisted after their departure can be explained to a great extent by new roads built to serve the refugee camps.

Our work is also related to the literature on migration and refugees. The consequences of migration flows on labor market outcomes and ultimately on the welfare of individuals in hosting communities have been investigated mainly in developed countries (Card, 1990; Borjas, 1999; Angrist and Kugler, 2003; Manacorda et al., 2012; Ottaviano and Peri, 2012, Docquier et al., forthcoming, are prominent examples). In developing countries, the issue has been explored from the perspectives of the migrants (Rosenzweig, 2007; Beegle et al., 2011; Grogger and Hanson, 2011), their countries of origin (Adams and Page, 2005; Hanson, 2009), or the households directly linked to migrants (Woodruff and Zenteno, 2007;

[1] In a different vein, Redding et al. (2011) claimed evidence for multiple equilibria by showing that the division of Germany and its reunification led to a shift in the location of the main airport hub. This finding is ambiguous because the airport infrastructure is largely the outcome of government decisions. We also note important changes in the political fundamentals.

Yang, 2008). As reviewed by Ruiz and Vargas-Silva (2013), an emerging literature also seeks to assess quantitatively the consequences of forced migration on the host population (Alix-Garcia and Saah, 2010; Baez, 2011; Maystadt and Verwimp, 2014; Ruiz and Vargas-Silva, 2016, 2015; Del Carpio and Wagner, 2015). However, much of that literature has focused on the short-run impact on the hosting economy. Maystadt and Verwimp (2014) focus on the short-run and distributional impact of refugees on the hosting labor markets. Alix-Garcia and Saah (2010) and Baez (2011) investigate the short-run consequences in terms of food prices and child health outcomes, respectively. Kreibaum (2016), Taylor et al. (2016), and Alix-Garcia et al. (2017) provide evidence in other contexts for Uganda, Rwanda, and Kenya, respectively. The literature reaches a relative concensus of considerable benefits for the host population, although with possible redistributive effect. More recently, Del Carpio and Wagner (2015) assess the impact of Syrian refugees in Turkey focusing on the short-run labor markets adjustments, through displacement and occupational upgrading effects. Our paper differs from these papers by investigating the long-run and persistent consequences of hosting refugees, more than 10 years after most refugees left. Similar to our paper, Ruiz and Vargas-Silva (2015, 2016) exploit the same data to assess the consequences of hosting refugees. However, the same authors focus on the labor markets and cannot explain why such effects persist overtime. None of the above papers addresses the hysteresis effect found in this paper. Sarvimaki (2011) is an exception. He underscored the role of agglomeration economies to explain the long-run impact of forced migrants on Finnish hosting areas. As far as we know, our paper is the only one dealing with the persistent impact of forced migration in a developing country.

Finally, our paper is part of a recent literature that explores the effects of transportation infrastructure following Banerjee et al. (2012), Faber (2014), Ghani et al. (2016), Storeygard (2016), Baum-Snow et al. (forthcoming), Donaldson (forthcoming), Jedwab et al. (2017), and Jedwab and Moradi (2016) in developing countries or Baum-Snow (2007), Michael (2008), Duranton and Turner (2012) and Duranton et al. (2014) in developed countries. We also contribute to an earlier literature that assesses the welfare improvements of road accessibility using household data (Jacoby, 2000; Jacoby and Minten, 2009; Khandker et al., 2009). Our main innovation here is to use a panel of households to limit the possible biases caused by changes in the composition of population after the construction of the new infrastructure.[2] Our interpretation of road construction as a "historical accident" also echoes Jedwab et al.'s (2017) use of the

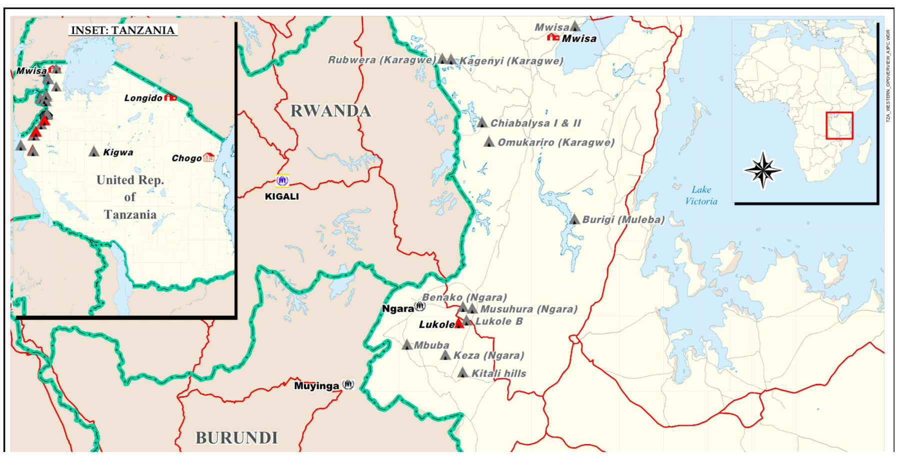
[2] Gonzalez-Navarro and Quintana-Domeque (2016) also use a panel of households but with a very different objective in mind. They assess the effect of paving urban roads on local property values.

construction of the colonial railroad in Kenya as a natural experiment.

The paper is organized as follows. Section 2 describes how the massive refugee inflows of 1993 and 1994 may help to explain the shift of equilibria observed in the region of Kagera in Tanzania between 1991 and 2010. By distinguishing between two periods (1991–2004 and 1991–2010), Section 3 shows that the impact of hosting refugees does not fade away over time, indicating a persistent and positive impact on households' welfare. Section 4 investigates possible channels of transmission. Section 5 concludes.

2 Background

Figure 1: The Kagera region and the location of refugee camps



Source: UNHCR Regional Spatial Analysis Lab (Nairobi) and fieldwork geographic coordinates.

The Kagera region is a remote region in northwestern Tanzania of about 30,000 square kilometers. As shown by the map in Figure 1, the Kagera region is located between Lake Victoria, Uganda, Rwanda, and Burundi. The number of inhabitants amounted to about 1.5 million people in the early 1990s. Kagera is one of the poorest regions of the country in terms of annual income per capita with an average of 149,828 Tanzanian shillings (Tzs, that is, US$166 in 2001), representing less than 65 percent of the annual income per capita of the country (Tanzania, NBS, 2003) .

Starting on October 21, 1993, between 250,000 and 300,000 Burundians fled into Tanzania following the assassination of the president of Burundi. A second influx of 250,000 refugees came from Rwanda over 24 hours on April 28, 1994 (Rutinwa, 2002), after the crash of the plane carrying the presidents of Rwanda and Burundi, which triggered the Rwandan genocide. This was largest and fastest exodus the UNHCR had ever witnessed. Over the next two months, it was followed by nearly another million refugees, fleeing Rwanda. In 1995, there remained about 800,000 refugees in Kagera. The majority, who originated from Rwanda, were forced to leave in 1996. Repatriation of the refugees from Burundi was more progressive. Their number continuously decreased to about 70,000 in 2004. The last camp (Lukole) was closed in June 2008.

The unanticipated and localized nature of the events provides a tool to isolate the impact of the refugee influx from other factors. As witnessed by a local aid worker, "They came very unexpectedly. The local population was never expecting such a thing. Just overnight, so many people were around. ... They came like a swarm of loco bees" (personal communication, May 6, 2008). Alix-Garcia and Saah (2010) also underlined the unexpected nature of the refugee flow following political assassinations.

Importantly, the influx of refugees in October-November 1993 was so sudden that refugees stayed close to local communities without formal assistance until April 1994. Their poor health conditions limited their ability to move very far away from where they originally crossed the border and, to protect them, borders had to be enforced by the military. The unexpected nature of the shock, together with the sheer number of refugees, prevented anyone, be it the Tanzanian government or UNHCR, from directing the refugees to the one or more locations across the region initially designated to host them. Instead, UNHCR and the Ministry of Home Affairs had to site a small number of city-sized camps within a very small radius of where the refugees had initially arrived. As can be seen in Figure 1, contrary to international law recommendations and to the guidelines of the UNHCR *Handbook for Emergencies*, this siting resulted in camps located very close to the borders.[3] That Tanzania was caught unprepared and had difficulty finding a place for hundreds of thousands of refugees removes, to a large extent, a potential problem of endogeneity. We discuss this issue further in Section 3. Furthermore, a new refugee policy implemented by the Tanzanian government restricted the movement of the refugees to 4 kilometers around the camps. With a permission, refugees could go beyond that limit to work or to trade with the local population but they had to come back overnight to keep their entitlement to services such as free food delivery. These movement restrictions, coupled with geographical features limiting the spatial spread of the impact (Baez, 2011), provides an exceptional framework to identify the local effects of refugees.

[3]Two exceptions appear on Figure 1: the camps of Burigi and of Mwisa. Both are special "protection camps" that were populated by only 10,000 refugees in 1995, compared with 350,000 for the largest camp.

According to people interviewed in the Kagera region, refugees are reported to have affected the local economy through various channels.[4] First of all, the labor market was disrupted. While agricultural workers faced fiercer competition from refugees working in the fields, non-agricultural workers benefited from increased job opportunities provided by non-governmental organizations (the Red Cross, CARE, Tanganyika Christian Refugee Service, Norvegian People's Aid and so on) and UN agencies (UNHCR, World Food Programme). New varieties of goods (particularly non-food items) were introduced to meet international workers' different tastes. Farmers selling their products on the local market benefited from cheaper labor and higher crop prices. Agricultural production was reported to have doubled in some villages close to large refugee camps. Several businesses also mushroomed around the refugee camps. In turn, they attracted entrepreneurs from other regions. Second, upon the arrival of refugees, surging prices on the goods markets resulted from a new demand from the humanitarian sector and the refugees themselves (Alix-Garcia and Saah, 2010), while adverse health impacts were also documented (Baez, 2011). Environmental degradation and security concerns were also reported during the refugee crisis (Berry, 2008). As we discuss below, the construction of refugees camps was also accompanied by significant infrastructure development.

3 The Effect of Refugees

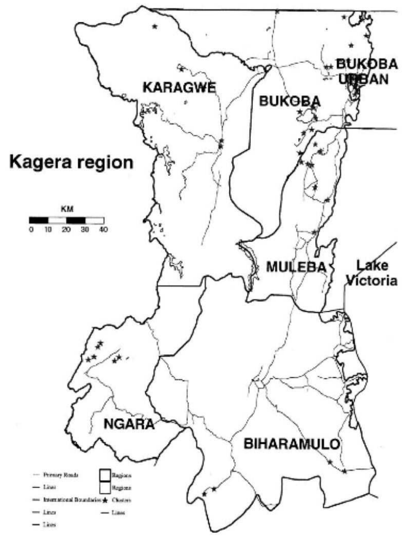
3.1 Data and Identification Strategy

We use the Kagera Health and Development Survey (KHDS) dataset collected by Economic Development Initiatives and the World Bank (Beegle et al., 2006; De Weerdt et al., 2010). Based on the World Bank Living Standards Measurement Study standards (Grosh and Glewwe, 1995), KHDS provides comprehensive information on several dimensions of individual and household well-being, such as levels of consumption, income, and assets. It also documents some community and facilities characteristics, such as the availability of public services and so on.

In four waves, the KHDS interviewed 915 households and their members from fall 1991 to January

[4]Two months of iterative field research (Udry, 2003) fed the quantitative analysis presented in this paper. In order to refine some of our hypothesis, we conducted about 30 interviews, gathered data (notably refugee camp location and population), and collected some reports to better understand the economic environment of the region and the issues (management, interaction between refugees and local people) related to the refugee presence.

Figure 2: Villages surveyed in the Kagera Health and Development Surveys



Source: Beegle et al. (2006)

1994. Households originated from 51 randomly selected (with geographical stratification) Kagera communities (Figure 2). The survey was initially stratified based on four economic zones and the division between high- and low-mortality rates within each economic zone. Such stratifications into 8 zones (denoted strata hereafter) aimed at ensuring relatively appropriate sampling of households with adult mortality but has been shown to provide a representative sample in terms of basic welfare and other indicators for the region of Kagera (Beegle et al., 2011). An important feature of this survey is that great efforts were made later to trace the whereabouts of individuals from the original 915 households. The field team achieved recontact rates above 90 percent about 10 and 16 years later, in 2004 and 2010. The addition of 2010 data does not only pave the way for a simple extension of previous works (e.g. Baez 2011, Maystadt and Verwimp 2014) since it allows to assess the impact of refugees several years after refugees returned to their country of origin. An important limitation of the 2010 data is that they do not contain information about income and village characteristics. Further description of the data can be found in Appendix A.

These data are particularly rich for assessing the impact of the refugee inflows of 1993–1994 on the local population. First, the first wave of the KHDS was undertaken before October 21, 1993, the date of the Burundi President's assassination and the start of the refugee crisis in the Kagera region. Therefore, the data should allow us to distinguish the effect of the refugee inflows from some initial differences between villages or households. Second, the location of the different villages throughout all the region allows us to introduce a key heterogeneity in our sample, depending on whether the households were living in a village close to a refugee camp or not. Third, we exploit waves 5 (2004) and 6 (2010) to assess the persistent nature of the temporary shock on the welfare of the local population.

By exploiting both time and spatial variations in the way households traced over time have been affected by the refugee inflows originating from Burundi (1993) and Rwanda (1994), we estimate the effect of the refugee presence, along with other explanatory variables defined at household or village level, on real consumption:

$$\log\left(\frac{C_{h,t}}{P_{v(h,t),t}}\right) = \beta_0 + \beta_1 RI_{v(h,t),t} + \alpha_h + \alpha_t + \alpha_s * time + \epsilon_{h,t} \quad (1)$$

where $C_{h,t}$ denotes nominal consumption by household h in year t; $P_{v(h,t),t}$ is the price level in village v in year t, where household h lives during the same year; $RI_{v(h,t),t}$ is an index measure of refugee inflow; and α_t, $\alpha_s * time$, and α_h are time, strata-time fixed effects (with strata defined at the original location), and household fixed effects, respectively.

Let us now discuss these variables in turn. Our dependent variable is defined as the real consumption per adult equivalent. Consumption data are only fully comparable for the years 1991, 2004, and 2010, so that we mainly use waves 1, 5, and 6 of the KHDS for our analysis. The adult equivalent transformation is applied using the method proposed by Collier et al. (1986) for Tanzania. More information about the construction of this variable is given in Appendix A. In our robustness checks, we also use alternative dependent variables such as the consumption of food and non-food items. To understand the channels through which these effects are working we also estimate regressions using price indices as dependent variables.

The explanatory variable of interest measures the way each household was affected by the refugees in 1993–1994. To construct the refugee index we use information on both the population of refugee camps and the distance between the villages where households live and the refugee camps. The estimated number of refugees per camp in 1995, the peak of the refugee presence, was collected through fieldwork. More specifically, we sum the refugee population weighted by inverse distance: $\sum_{c=1}^{13} \frac{pop_c}{d_{v,c}}$, where c goes from 1 to 13 refugee camps and v from 1 to 51 villages. The resulting variable is continuous, takes the value zero in 1991, and for the sake of assessing the persistent impact of the refugee presence is the same for 2004 and 2010. We then log this quantity (and add 1 to deal with the zero values in 1991) to obtain our refugee index, $RI_{v,t}$. Our decision to use a log is motivated by the fact that six villages appear to be particularly exposed to the refugee presence (with value equivalent to more than 20,000 refugees in the vicinity or 200,000 at an average distance of 10 kilometers). We refer to these six villages as "high-refugee areas." In the absence of strong priors about the exact functional form needed to measure refugee exposure, we explore a number of alternatives in our robustness checks.

We also construct climatic variables with monthly rainfall data in total millimeters, averaged over the growing periods of the last two years and transformed into anomalies. Appendix A provides more information about the construction of that variable. Climatic variables are constructed at the level of the village of origin to avoid introducing bias because of selective migration between areas with different climate characteristics.[5] These data are available from the Tanzania Meteorological Agency for 1980 to 2010. In Section 4, we will also make use of other village-level data, based on the community questionnaire of the KHDS (distance to health services, secondary school, number of social services and non-governmental organizations, village population) or secondary data (road accessibility, distance to borders, and bilateral trade data). The construction of these variables is postponed to Section 4.

In some specifications, we also augment the above specification with household characteristics to assess the sensitivity of our results to possible changes in characteristics among the households. Household characteristics include the age, its square, and the level of education of the head; a dummy indicating whether the household head has a chronic illness; dummies indicating the sex and marital status of the household head; the average education level of the household members; dummies for split-off households (such as a child identified in 1991, who creates a new household by 2004 or by 2010); and the log of the size of the household. Given the risk of bad controls (Angrist and Pischke, 2009), we remain cautious when interpreting these specifications.[6]

In our main results, both clustered as well as spatially robust standard errors are reported. For the former, we first cluster the standard errors at the initial village level, to account for correlation within villages

[5]Given the links between weather variations and migration in Tanzania (Hirvonen, 2016) or elsewhere in Africa (Marchiori et al., 2012; Dillion et al., 2011), weather variables can only be considered as exogeneous if we restrict the construction of weather anomalies on the village of origin or based on the sample of individuals who have not migrated.

[6]One concern may be that household characteristics such as the level of education of the head or household size may change as a result of the presence of the refugees, be correlated with the changes in real consumption per adult equivalent, and therefore introduce some endogeneity.

Table 1: Descriptive statistics for main results (mean values)

	Real consumpt. (2010 Tzs)	Age (head)	Education (head)	Chronic illness (head)	Size of household	Split-off household	Mean educ. of household	Rainfall 2-year average
1991								
High-refugee areas	313,472	46.0	3.2	0.20	6.42	0.00	1.81	139.48
Other areas	437,320	49.2	4.4	0.18	7.59	0.00	2.25	161.75
All	424,680	48.9	4.3	0.19	7.47	0.00	2.21	159.53
2004								
High-refugee areas	405,836	41.7	4.7	0.31	4.95	0.55	2.68	90.25
Other areas	559,921	45.0	5.5	0.28	5.38	0.54	3.61	146.86
All	543,301	44.6	5.4	0.28	5.33	0.54	3.51	140.91
2010								
High-refugee areas	611,488	41.1	5.1	0.29	4.92	0.52	3.03	109.38
Other areas	709,479	42.8	6.0	0.22	4.71	0.58	4.06	131.40
All	698,358	42.6	5.9	0.23	4.74	0.57	3.95	128.90

Notes: Real consumption is expressed in adult equivalent terms and in 2010 Tanzanian shillings (Tzs). Average monthly rainfall during the growing periods of the last two years is expressed in millimeters.

(Moulton, 1986; Bertrand et al., 2004). We then cluster the standard errors at a higher level —the strata level in line with the original sampling stratification —to deal with spatial correlation beyond the boundaries of the villages, as well as serial correlation in the observations from households over time. Given the small number of strata (8) which might produce underestimated intra-group correlation, we turn to 1,000 replications of wild bootstrap (percentile-t method), known to resist to heteroskedasticity (Cameron et al., 2008; Cameron and Miller, 2015).[7] For spatially robust standard errors, we adjust standard errors for spatial and time dependency of an unknown form (Conley, 1999) by adopting Colella et al. (2018) procedure.[8] We assume that spatial dependency disappears beyond a cutoff point of 90 kilometers, which comprises the average distance of 88 kilometers between the 51 original villages of our sample. We then experiment with lower and higher cutoff points at 60, and 120 kilometers.

Figure 3 gives a first indication that high-refugee areas experienced an increase in real consumption per adult equivalent between 1991 and 2004, but the increase is even stronger between 1991 and 2010.[9]

[7]To compute the standard errors, we follow Cameron and Miller (2015:344) according to which "a conservative estimate of the standard error equals the width of a 95% confidence interval, obtained using asymptotic refinement, divided by 2×1.96."

[8]Colella et al. (2018) argue to correct for a non-marginal mistake in Hsiang (2010)'s widely-used code. In our case, both codes produce the same estimated standard errors.

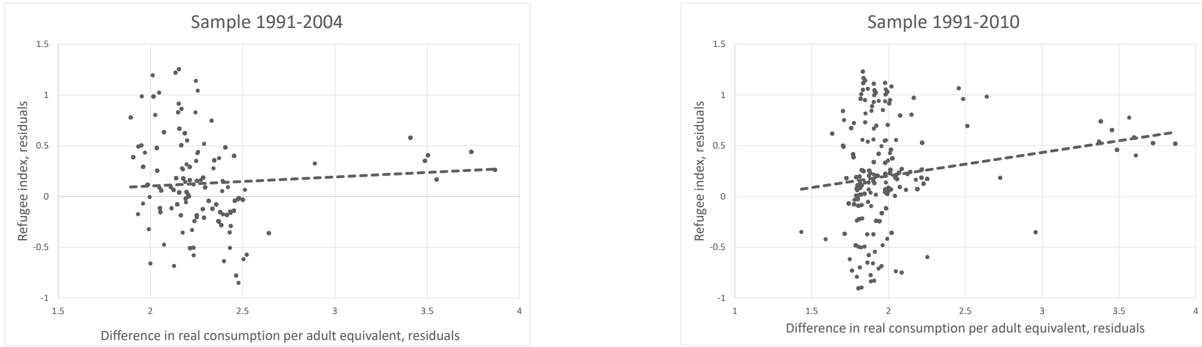
[9]Figure 3 is obtained by retrieving the residuals from year-specific regressions of each variable on village fixed effects and strata-specific time trends. Similar pictures are obtained by repeating the exercise using the village-level means. Note that our results are robust to dropping one-by-one the faster-growing villages depicted on the top-right quadrants of Figure 3. The

According to Table 1, which presents summary statistics, refugee-hosting areas also differed from other areas in other respects. In particular, they appear to have been poorer, less educated, and less prone to rain-fed agriculture in 1991. These differences indicate that refugee camps were located in initially less favorable locations. Although political motivations, the health status, and the limited mobility of the refugees have been argued to reduce the potential selection of the most attractive locations for refugee camps, our summary statistics point at potentially negative sorting, inasmuch as refugees happened to arrive in poorer areas. Such negative sorting is also obvious when regressing the presence of refugees on the initial real consumption per adult equivalent. The refugee presence is negatively and significantly associated with the initial level of welfare. Similar results are obtained when we augment the model with the 1991-1993 growth in outcomes or when the sample is restricted to households who were living in the two border areas, i.e. the districts of Karagwe and Ngara. The detailed results are provided in Table B.3 of Appendix B. Clearly, refugee camps appear to be systematically located in poor and low growth-potential areas, even when comparing areas close to the borders.

The summary statistics of Table 1 also underscore the importance of households fixed effects and time-varying village characteristics in our estimating equation (1). The initial differences stress the importance of controlling for potential changes in the composition of groups by tracing exactly the same households and controlling for observed and unobserved characteristics. In particular, the household fixed effect, α_h, controls for any unobserved permanent differences between households. The time dummy, α_t, controls for time-varying events affecting all households. When strata-year fixed effects are introduced, we exploit within-strata variations in the exposure to refugees.

The sample comprises 3,314 households, including households who had migrated within and outside of Kagera by 2004 and 2010. Due to missing consumption data, 414 households are excluded. Six households are excluded due to missing geographic coordinates and the resulting impossibility of linking them to weather data. The sample is reduced to 2,456 households when we exclude migrants. The sample of households followed between 1991 and 2004 includes 2,770 households, of which 155 households are dropped due to missing consumption data. Appendix A provides more detailed information on the construction of the sample. Our results are also shown to be robust to a change in the definition of the sample. Including migrants in the sample has the advantage of accounting for native displacement. This

robustness of our results to alternative samples is described in Section 3.3.



(a) (b)

Figure 3: Change in real consumption and the presence of refugees

matters because displaced natives are likely to form a selected subsample (Hatton and Tani, 2005; Card, 2005). As documented by Table A.2 in Appendix A, migration rates are markedly lower in high-refugee areas compared with other areas. However, a similar selection problem may occur because of attrition. Table A.2 in Appendix A reports lower attrition rates in high-refugee areas. This is unlikely to be an artefact of the data since the attrition rates for the whole sample closely match the rates provided by De Weerdt et al. (2010). These differences in attrition rates highlight the importance of household fixed effects, which allow us to focus on within-household variation.

3.2 The Impact of Hosting Refugees

Panels A and B of Table 2 report our main results regarding the effect of refugees over 1991–2004 and 1991–2010, respectively. In panel A, column 1 regresses log real consumption per adult equivalent in 1991 and 2004 for all households in the KHDS data on the refugee index of their village, a time dummy, and a household fixed effect. The coefficient on the refugee index, which we can interpret, with a slight abuse of language, as an elasticity, is rather low, around 0.02. Column 2 adds time-varying location characteristics —that is, average monthly precipitation over the growing periods of the last two years —to the specification of column 1. The coefficient on the refugee index increases to 0.05 and becomes significant at reasonable levels of confidence. When strata-year fixed effects are added, the coefficient of the refugee index increases at 0.08. This is of course in stark contrast with the fact that on average refugees arrived in areas that were initially much poorer than those that did not host refugees. At the

Table 2: Main results: Refugees and consumption

Panel A	(1)	(2)	(3)	(4)	(5)
Dep. var.	Log real consumption per adult equivalent, 1991 and 2004				
Refugee Index	0.019	0.045	0.076	0.078	0.066
(Cluster-village)	(0.025)	(0.020)*	(0.024)**	(0.026)**	(0.028)**
(Cluster-strata)	(0.025)	(0.020)*	(0.024)**	(0.026)**	(0.028)**
(Wild bootstrap-strata)	(0.024)	(0.018)**	(0.020)***	(0.023)***	(0.023)***
(Conley-90 km cutoff)	(0.010)*	(0.003)***	(0.010)***	(0.005)***	(0.007)***
(Conley-60 km cutoff)	(0.013)	(0.011)***	(0.012)***	(0.011)***	(0.010)***
(Conley-120 km cutoff)	(0.012)	(0.005)***	(0.012)***	(0.009)***	(0.009)***
Household controls	No	No	No	No	Yes
Rain	No	Yes	No	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Strata time trends	No	No	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	4,670	4,670	4,670	4,670	4,670
R-squared	0.159	0.167	0.181	0.181	0.244
Panel B					
Dep. var.	Log real consumption per adult equivalent, 1991 and 2010				
Refugee Index	0.068	0.079	0.193	0.195	0.185
(Cluster-village)	(0.041)	(0.041)*	(0.059)***	(0.057)***	(0.067)***
(Cluster-strata)	(0.041)	(0.042)	(0.054)***	(0.049)***	(0.057)***
(Wild bootstrap-strata)	(0.034)**	(0.048)*	(0.059)***	(0.044)***	(0.048)***
(Conley-90 km cutoff)	(0.025)***	(0.018)***	(0.045)***	(0.039)***	(0.043)***
(Conley-60 km cutoff)	(0.031)**	(0.018)***	(0.038)***	(0.034)***	(0.039)***
(Conley-120 km cutoff)	(0.023)***	(0.023)***	(0.043)***	(0.037)***	(0.043)***
Observations	4,912	4,912	4,912	4,912	4,912
R-squared	0.314	0.314	0.327	0.327	0.391

Notes: *, **, ***: significant at 10%, 5%, and 1%, respectively. The same regressions are estimated in both panels. The sample excludes households migrating outside of Kagera. R-squared retrieved from regressions with standard errors clustered at the village level.

same time, this is consistent with the summary statistics of Table 1, which shows that real consumption per capita increased faster in high-refugee areas. Adding climatic characteristics to the specification in column 4 slightly increases the coefficient. Overall, our results suggest a positive effect of refugees in 2004, 10 years after their arrival and 8 years after the departure of a large majority of them.[10]

[10]This does not prevent negative effects around the time of their arrival, of course. Note that Maystadt and Verwimp (2014) found a lower coefficient of about 0.06–0.07. With our sample, a similar coefficient may be obtained by using their larger consumption basket in the definition of the real consumption per adult equivalent, dropping the strata-year fixed effects and introducing their additional time-varying village characteristics (reported natural and epidemic disasters). For comparability between our two samples, 1991–2004 and 1991–2010, we do not allow for these alternative specifications in Table 2, because these additional data are not available in the last round of the KHDS.

Panel B of Table 2 replicates the specifications of panel A but uses 2010 household data instead of 2004 data. The impact of refugees, though a large majority have been gone for more than 10 years, is stronger, as soon as strata-year fixed effects are introduced. The importance of introducing fixed effects is illustrated by the increase in the coefficient in column 3. From panel B, it is clear that the impact of refugees is still observed in 2010, more than 10 years after most refugees left. The impact remains significant and at about 0.19. Adding rainfall-based controls and household characteristics to the specification leaves previous estimates virtually unchanged. The specifications used to obtain results presented in column (3) are therefore considered as our main specifications on which robustness checks will be based on.

Interestingly, such elasticity is of higher magnitude as the long-term impact (about 0.09) of population flows on wages found by Sarvimaki (2011) in the case of Finland. Adopting a general equilibrium perspective in the US context, Ottaviano and Peri (2012) found a much lower long-term average positive effect of immigration on native wages of about 0.6 percent. The comparability of migrants in the United States and refugees in Tanzania can obviously be called into question. But the difference of magnitude is puzzling enough to motivate further investigation on the channels of transmission in the next section.[11]

3.3 Robustness Checks

The above results rely on a number of identifying assumptions and specification choices. We therefore examine their robustness to (1) the existence of a pre-refugee trend; (2) the role of unobserved time-varying location characteristics; (3) changes in the sample of households followed over time; (4) alternative specifications of the dependent variable; and (5) alternative definitions of our main variable of interest, the refugee index. All results are shown using standard errors clustered at the village or at the strata levels but are robust to the use of Conley (1999) standard errors.

[11] Among the significant coefficients not reported in Table 2, we find strong negative effects for non-married heads of households and households having a head with a chronic illness, as should be expected. We also find a positive effect for split-off households. Split-off households are new households created as of 2004 and 2010 by previously surveyed household members. We also find a coefficient of around 0.08 for the average education of the household. This coefficient is typical of extant findings in the literature for apparent returns on education in Sub-Saharan Africa (Psacharopoulos, 1994; Schultz, 1999). A positive deviation in rainfall during the last two growing seasons has a positive impact on real consumption, as expected in an economy that largely depends on rain-fed agriculture (Beegle et al., 2011). As expected, the effect of rainfall disappears when strata-year fixed effects are introduced, stressing the importance of controlling for unobserved changes across economic zones.

Robustness to differential growth trends. We assume that households affected by the presence of refugees would have followed a similar trajectory in terms of real consumption per adult equivalent if refugees had not landed in Kagera. We can construct the same variables as above for an additional pre-refugee year to conduct a "placebo" test and explore whether differences in outcomes can be explained by the "refugee presence" when refugees were not yet present. Based on the sample of households followed between 1991 and 1993, column 1 of panel A in Table 3 suggests that the positive effect of the refugee index on real consumption per adult equivalent cannot be explained by changes occurring before the refugees arrived.[12] Adding rainfall variations, strata-year fixed effects and household characteristics leave that conclusion virtually unchanged.

Nonetheless, the lack of significant coefficients may simply reflect the reduction of the sample to about 770 households followed between 1991 and 1993. We investigate this issue further by introducing future split-off households in the sample. Over-sampling those households whose members will be followed in a larger proportion by 2004 and 2010 confirms that our results may not be attributed to a trend existing before the refugees arrived. Detailed results are provided in Table B.4 of Appendix B.[13] Another concern is that we may attribute to the presence of refugees the effects of a convergence process stronger in high-refugee areas compared with others. Panel B of Table 3 augments the regressions presented in panel A with the interaction term between the presence of refugees and the initial real consumption averaged at the initial village level. Initially richer villages were actually growing faster compared with other villages within high-refugee areas. Such a pre-existing trend points to the lower-bound nature of our estimates.

Robustness to geography. We cannot be certain that our identification strategy is not picking up unobserved time-variant characteristics, somehow related to the presence of refugees. We know that refugee camps are strongly correlated with proximity to the borders. One concern may be that our variable of interest captures unobserved time-varying characteristics, related to the distance to the borders with Rwanda and Burundi. At the cost of removing relevant variation, equation (1) can be augmented with an inter-

[12]We acknowledge that the comparison between 1991 and 1993 consumption data is not perfect because those data were collected based on different recall periods. Despite dividing the 1991 consumption data by 2 as suggested by Bengtsson (2010), we cannot exclude the existence of reporting errors due to different recall periods (Beegle et al., 2012). There is, however, no obvious reason to believe the measurement error introduced by such a difference of recall periods may be different between high-refugee areas and other areas. We also note that the inclusion of the fixed effects implicitly controls for trend differences prevailing prior to 1991. In any case, controlling for the changes in real consumption between 1991 and 1993 and between 2004 and 2010, does not largely alter the coefficient of interest presented in Panel B of Table 2

[13]Over-sampling the future split-off households in panel A of Table 2 also gives similar point estimates.

Table 3: Placebo test (parallel trend assumption)

Panel A	(1)	(2)	(3)	(4)	(5)
Dep. var.	Log real consumption per adult equivalent, 1991 and 1993				
Placebo $RI_{v,t}$	-0.026	-0.356	-0.235	-0.014	-0.011
	(0.059)	(0.087)***	(0.118)*	(0.156)	(0.151)
	[0.053]	[0.077]***	[0.123]*	[0.179]	[0.172]
Household controls	No	No	No	No	Yes
$Rain_{v,t}$	No	Yes	No	Yes	Yes
Time fixed effect	Yes	Yes	Yes	Yes	Yes
Strata time trends	No	No	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	1,140	1,140	1,140	1,140	1,140
R-squared	0.987	0.988	0.989	0.989	0.990
Panel B					
Dep. var.	Log real consumption per adult equivalent, 1991 and 1993				
Placebo $RI_{v,t}$	-0.658	-0.674	-0.523	-0.567	-0.564
	(0.078)***	(0.080)***	(0.213)**	(0.207)***	(0.206)***
	[0.082]***	[0.087]***	[0.212]***	[0.224]**	[0.215]***
Placebo $RI_{v,t}$	0.086	0.076	0.078	0.070	0.070
$\times$ Initial $\text{cons}_{v,1991}$	(0.009)***	(0.010)***	(0.017)***	(0.015)***	(0.015)***
	[0.010]***	[0.011]***	[0.017]***	[0.017]***	[0.017]***
Observations	1,140	1,140	1,140	1,140	1,140
R-squared	0.990	0.990	0.990	0.990	0.990

Notes: Standard errors in parentheses are clustered at the initial village level. Standard errors in brackets are clustered at the strata level, using wild bootstrap method (Cameron et al. 2008). *, **, ***: significant at 10%, 5%, and 1%, respectively. The same regressions are estimated in both panels. R-squared retrieved from regressions with standard errors clustered at the village level.

action term between the distance to the border(s) and a time dummy.[14] Panel B of Table 4 reports the coefficient of the refugee index in this augmented model. We find that this augmented model provides even stronger results by 2010, although less precisely estimated. At equal distance to the border, doubling the presence of refugees would increase real consumption per adult equivalent by 7 percent by 2004 and 24 percent by 2010. Given the location of the regional capital in the eastern part of Kagera, our results are also unlikely to be driven by a distinct trend in urban versus rural areas. The KHDS defines an urban community based on the assessment of the community leader in the first round of the survey. Panel C of Table 4 reports the coefficient of the refugee index when excluding urban areas. The coefficient of interest

[14]That variable is actually used by Baez (2011) and Ruiz and Vargas-Silva (2015) as a proxy for the refugee inflows in Kagera while assessing the impact on child health outcomes. We believe that the proposed refugee index is a less noisy measurement of the presence of refugees.

Table 4: Robustness to alternative samples and dependent variables

Dep. var.	Log real consumption per adult equivalent					
	1991 - 2004			1991 - 2010		
	(1)	(2)	(3)	(4)	(5)	(6)
	Coef. RI	clustered SE village	clustered SE strata	Coef. RI	clustered SE village	clustered SE strata
A. Main results	0.076	(0.024)**	[0.020]***	0.193	(0.059)***	[0.059]***
		N=4,670			N=4,912	
B. Controlling for distance to borders*D_t	0.066	(0.018)***	[0.022]***	0.237	(0.127)*	[0.130]*
		N=4,670			N=4,912	
C. Excluding urban areas	0.090	(0.022)***	[0.018]***	0.180	(0.064)***	[0.062]***
		N=3,788			N=4,090	
D. Including migrants	0.084	(0.019)***	[0.015]***	0.159	(0.052)***	[0.049]***
		N=5,230			N=5,788	
E. Including only migrants	0.052	(0.031)*	[0.023]**	0.207	(0.062)***	[0.080]***
		N=1,616			N=2,594	
F. Minimum value from 51 regressions (dropping 1 village)	0.066	(0.020)***	[0.021]***	0.138	(0.093)	[0.097]
		N=4,590 (village 32)			N=4,830 (village 32)	
G. Maximum value from 51 regressions (dropping 1 village)	0.082	(0.020)***	[0.018]***	0.216	(0.053)***	[0.039]***
		N=4,568 (village 39)			N=4,806 (village 11)	
H. Using food consumption as dep. var.	0.062	(0.019)***	[0.017]***	0.129	(0.063)**	[0.068]*
		N=4,670			N=4,912	
I. Using non-food consumption as dep. var.	0.098	(0.032)***	[0.038]**	0.349	(0.089)***	[0.062]***
		N=4,670			N=4,912	
J. Excluding self-produced consumption from dep. var.	0.078	(0.021)***	[0.018]***	0.204	(0.058)***	[0.047]***
		N=4,589			N=4,911	

Notes: Only the coefficient for the Refugee Index (denoted RI) is reported. Standard errors in parentheses are clustered at the initial village level. Standard errors in brackets are clustered at the strata level, using wild bootstrap method (Cameron et al. 2008). *, **, ***: significant at 10%, 5%, and 1%, respectively. The same specification with time, household and strata-specific time trends is estimated in both samples.

remains largely in the same order of magnitude when we exclude households living in urban areas.

Robustness to sample definition. The households migrating outside of the region of Kagera by 2004 or by 2010 are excluded in the sample of our main results. Although migration rates are markedly lower in high-refugee areas compared with other areas (as documented by Table A.2 in Appendix A), selective migration may occur as a result of the inflows of refugees. Possible native displacements could bias the estimation of the impact of refugees on the population of interest. However, Panel D of Table 4 reports the coefficient of interest, when including those households who have migrated outside Kagera. We find even a lower coefficient by 2010. That confirms that we are unlikely to capture the confounding effect of unobserved characteristics between migrants of high-refugee areas versus those of other areas. In fact, migrants themselves have improved their welfare by 2010 in refugee-hosting areas, compared with migrants from other areas (Panel E of Table 4). The magnitude of the coefficients is of similar order. Although migration may be a consequence of the refugee-induced welfare gains, it confirms that migrants from refugee-hosting areas do not bias our results. Our results are also largely unchanged when dropping one village at a time. The minimum and maximum values of the coefficient of interest shown in panels F and G of Table 4 feature relative stability to that sensitivity test. The efficiency of the results are only sensitive to dropping one particular village for our results by 2010 but the magnitude of the coefficient remains fairly large at 0.14.[15]

Robustness to the choice of dependent variables. Our results are robust to alternative dependent variables. In panels H and I of Table 4, we distinguish food and non-food real consumption per adult equivalent. Larger coefficients of interest are found for non-food real consumption, perhaps as a result of non-homothetic preferences. We also replicate our main results excluding self-produced consumption. Such consumption is usually underestimated in household surveys. Given the possible exit out of subsistence agriculture into market-based activities in high-refugee areas compared with other areas, such a measurement error may introduce an upward bias in the estimated impact of the refugee presence on total consumption. In panel J of Table 4, our results are robust to the exclusion of self-produced consumption.

Robustness to the refugee index. Our results are robust to alternative definitions of the treatment variable. In particular we now generalize our refugee index to $\sum_{c=1}^{13} \frac{pop_c}{d_{v,c}^{\gamma}}$ with γ equal to 0.5, 2, or 3. We standardize the variable of interest in order to be able to compare the magnitude of the coefficients. Panels

[15]Such a coefficient in column 4 of Panel F is significantly different from zero at 95 percent level of confidence when using the Conley (1999) correction of the standard errors for spatial dependency. Following Cameron and Miller (2015), we also examine the distribution of bootstrapped values for our main results. We can conclude that our results are not sensitive to the inclusion of one or the other clusters. Indeed, paraphrasing Cameron and Miller (2015), the resulting histogram does not have a big "mass" that sits separately from the rest of the bootstrap distribution. We do not observe two distinct distributions, one for cases where one particular cluster is sampled and one for cases where it is not.

Table 5: Robustness to alternative refugee indices

Dep. var.	Log real consumption per adult equivalent					
	1991 - 2004			1991 - 2010		
	(1)	(2)	(3)	(4)	(5)	(6)
	Coef. RI	clustered SE village	clustered SE strata	Coef. RI	clustered SE village	clustered SE strata
A. $RI_{v,t}$ with $\gamma = 1$	0.076	(0.024)**	[0.020]***	0.193	(0.059)***	[0.059]***
B. $RI_{v,t}$ with $\gamma = 0.5$	0.642	(0.168)***	[0.169]***	1.363	(0.461)***	[0.415]***
C. $RI_{v,t}$ with $\gamma = 2$	0.033	(0.011)***	[0.011]***	0.027	(0.021)	[0.012]**
D. $RI_{v,t}$ with $\gamma = 3$	0.028	(0.011)**	[0.013]**	0.011	(0.014)	[0.005]*
E. $RI_{v,t}$, with refugees only from Rwanda	0.121	(0.032)***	[0.032]***	0.205	(0.059)***	[0.050]***
F. $RI_{v,t}$, with refugees only from Burundi	0.067	(0.017)***	[0.018]***	0.129	(0.043)***	[0.036]***
G. Minimum value from 13 regressions (dropping 1 refugee camp)	0.085	(0.022)***	[0.022]***	0.175	(0.049)***	[0.038]***
H. Maximum value from 13 regressions (dropping 1 refugee camp)	0.131	(0.034***	[0.037]***	0.239	(0.075)***	[0.082]***
I. $RI_{v,t}$ without log	0.004	(0.001)***	[0.001]***	0.011	(0.003)***	[0.002]***
J. Dummy for high-refugee area*D_t	0.355	(0.106)***	[0.051]***	0.400	(0.081)***	[0.017]***

Notes: Only the coefficient for Refugee Index (denoted RI) is reported. Most coefficients are standardized to ease comparison. No standardization is applied for panels I and J. Standard errors in parentheses are clustered at the initial village level. Standard errors in brackets are clustered at the strata level, using wild bootstrap method (Cameron et al. 2008). *, **, ***: significant at 10%, 5%, and 1%, respectively. The same specification with time, household and strata-specific time trends is estimated in both samples. All regressions are based on similar samples of 4,670 observations for 1991-2004 and 4,912 observations for 1991-2010.

A to D of Table 5 indicate that the larger γ is —that is, the sharper the decay function —, the smaller the coefficient of interest is. Such variation may point to non-linearities of the refugee index. Understanding the mechanisms behind such variation is certainly a path for further research. In panels E and F of Table 5, our results are also robust to restricting the construction of the refugee index to refugees from Rwanda or from Burundi. These refugees were indeed hosted in different refugee camps. We find that the impact of the refugees from Rwanda on the welfare of the hosting population is even stronger than that of those from Burundi. Economically, doubling the presence of refugees from Rwanda increases the welfare of the hosts by 12 percent by 2004 and 20 percent by 2010, even if refugees from Rwanda were forced to repatriate in 1996. The persistence of the welfare impact of hosting refugees is therefore further established. Our results are in a similar range when we exclude one refugee camp at a time, rejecting the risk that a single refugee camp is driving the results. The minimum and maximum values of the coefficient of interest are reported in panels G and H of Table 5. Furthermore, the logarithm transformation is not necessarily neutral. However, panel I of Table 5 confirms our main results, with a slightly different interpretation. An increase of about 100,000 refugees at 6.12 kilometers (the closest distance between the surveyed villages and any refugee camp) would give an increase in real consumption per adult equivalent by about 7 percent by 2004 and 17 percent by 2010. Finally, we also use an alternative treatment based on a dummy variable indicating whether the household belongs to the six villages most impacted by the presence of refugees. As indicated in panel J of Table 5, such an alternative treatment variable strongly increases the magnitude of the coefficients.

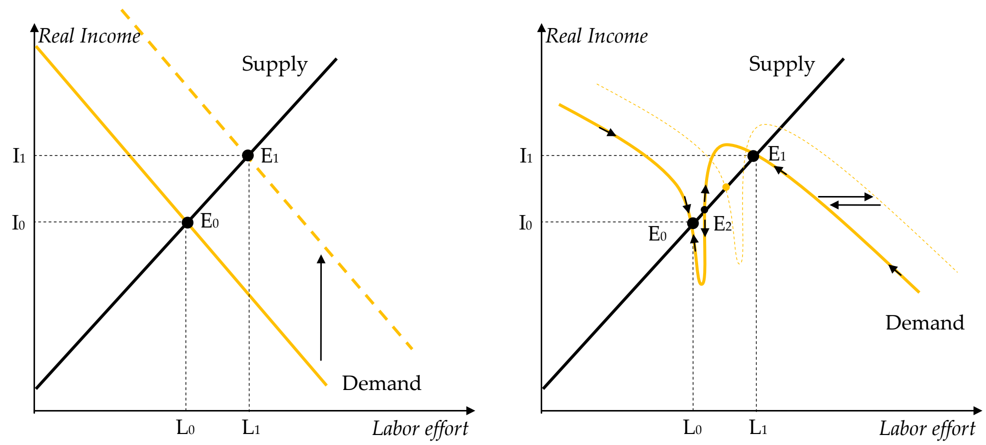
4 Investigating the Possible Channels of Transmission

4.1 Theoretical Framework

Our results so far document a sizeable increase in welfare (measured in terms of real consumption) for villages more exposed to refugees long after these refugees have returned to their home country. The presence of refugees had a positive and persistent impact on the hosting economy. The effect did not fade away over time. On the contrary, the impact became stronger between 2004 and 2010. Papers focusing on a shift in labor demand or supply resulting from an exogeneous increase in population cannot explain such a persistent impact (Maystadt and Verwimp, 2014; Ruiz and Vargas-Silva, 2015, 2016).

The first and most standard interpretation for this finding would be a shift in the unique equilibrium. To illustrate this, panel A of Figure 4 proposes a simple demand and supply framework. The horizontal axis measures a quantity that we can loosely refer to as labor effort, which combines both the quantity and the intensity of labor supplied. The supply of labor effort increases with labor income. This could be the result of workers' choosing to work more and have less leisure when the returns on labor increase. Alternatively, in a development context one may imagine that higher returns on labor allow workers

Figure 4: Shifting equilibrium versus multiple equilibria



Panel A
Shift of equilibrium

Panel B
Multiple equilibria

to feed themselves better and subsequently supply more labor (Strauss and Thomas, 1998, 2008). The demand for labor is sloping downward as marginal returns on labor decrease when more labor is supplied. There is a unique initial equilibrium in E_0 for (L_0, I_0). For a different level of income, such as (L_1, I_1), to persist after the departure of refugees, either the demand curve or the supply curve must permanently shift. Hence, the temporary refugee shock cannot be in itself an explanation for this permanent change. While one may imagine a shift in the supply of labor following, for instance, refugees' transmitting a different work ethic or skills to the local population, an upward shift in the demand for labor following an increase in productivity is more plausible. The issue is then to identify the factor (or set of factors) that underlies this shift in productivity / labor demand and leads to the new equilibrium in E_1.

There is another possible interpretation behind the change from E_0 with (L_0, I_0) to E_1 with (L_1, I_1). While the supply curve may continue to slope upward, the labor demand curve may not be monotonic.[16] Several possibilities can be envisioned to explain this. For instance, there might be a population threshold above which more productive interactions between workers may take place. These interactions are often referred to as agglomeration economies (Fujita and Thisse, 2002; Duranton and Puga, 2004). Alterna-

[16] One may also imagine situations where multiple equilibria would arise from non-monotonic labor supply curves. Although we view non-monotonic labor supply curves as less plausible than non-monotonic labor demand curves (which reflect non-convexities in production), they may arise from complicated tensions between the type of nutrition effects discussed further and increased demand for leisure. For our purpose, the exact source of multiple equilibria does not matter since they all imply the possibility of different equilibrium outcomes from the same 'fundamentals'. We can show instead that the (temporary) refugee shock led to a (permanent) change in local fundamentals.

tively, one may envision some non-monotonicity in the demand for products. Following Murphy et al. (1989), richer households may demand different goods produced under increasing returns to scale. In a similar vein, households below a given poverty threshold may be unable to save and face imperfect credit markets, preventing them from investing in more highly productive technologies (Azariadis and Drazen, 1990; Miguel and Roland, 2011). In panel B of Figure 4, we illustrate a case like this in which demand and supply intersect three times. The first intersection is in E_0. This is a stable equilibrium in the sense that following a small perturbation, the economy has a tendency to return to this equilibrium. There is a second stable equilibrium in E_1, which entails a higher level of labor effort and a higher income. There is also a third equilibrium in E_2. This equilibrium is unstable because any small perturbation away from it is self-reinforcing and will lead the economy to either E_0 or E_1.Threshold models in the spirit of Azariadis and Drazen (1990) typically lead to piecewise continuous labor demand curves and a low effort - low income equilibrium or a high effort - high income equilibrium. For instance, a high enough level of effort for all workers may allow them to make small savings which can enable a micro-credit scheme. In turn, farmers can buy tools that increase their productivity.

Initially the economy may have been in E_0. The arrival of the refugees arguably represented a large shock in the demand for labor to serve NGOs, the increased demand for food, etc. The labor demand curve may have shifted temporarily to the right, as represented by the dashed curve. The equilibrium E_0 then moves temporarily with labor demand. The key point is that when labor demand returns to its initial level following the return of the refugees, the temporary equilibrium is now in the "basin of attraction" of E_1. Hence, instead of reverting to E_0, the economy shifts to the higher equilibrium in E_1. For this to be possible, the temporary shock associated with the influx of refugees needs to be large enough, which is empirically plausible.

The above stylized theoretical framework indicates that finding a positive and persistent impact of refugees on the welfare of the hosting population is not enough to draw any conclusion on the existence of multiple equilibria. The existence of multiple equilibria, due to agglomeration economies, non-homothetic preferences, or the break-up of a poverty trap, should be assessed against an alternative hypothesis, a shift of equilibrium resulting from subsequent infrastructure investments and changes in local fundamentals that shifts the demand for labor. To that purpose, we gather information from the KHDS community questionnaires or from other secondary sources. We then assess how the presence of refugees affects those intermediary channels, using the following specification:

$$Y_{v,t} = \gamma_0 + \gamma_1 RI_{v,t} + \gamma_2 Rain_{v,t} + \gamma_v + \alpha_s * time + \gamma_t + \epsilon_{v,t} \tag{2}$$

The main difference with Equation (1) is that we assess the impact of the presence of refugees on outcomes, $Y_{v,t}$, defined at the village level, hence using village fixed effects, denoted γ_v. Standard errors are clustered either at the initial village or the strata levels. We use wild bootstrapping method in the later case (Cameron et al., 2008). Results are also robust to the use of Conley (1999) standard errors with a 90-kilometers threshold. We will also assess the importance of each hypothesised channel in explaining the persistence of the refugee impact by implementing a "horse race" exercise. At the cost of introducing endogeneity, we will introduce the variable $Y_{v,t}$ into the right-hand side of Equation (1) and assess how much our coefficient of interest may be affected by such addition.

4.2 Reduced Transport Costs as a Shifter of Equilibrium

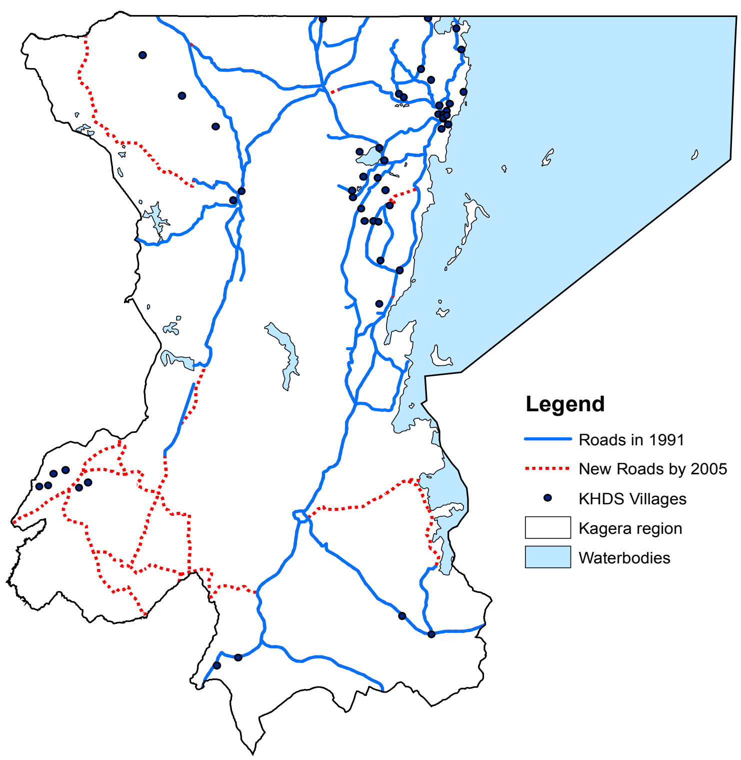
Following our fieldwork, one of the most plausible channels is investment in road infrastructure undertaken by UNHCR and the World Food Programme (WFP). Whitaker noted that "In Kagera region, more than 15 million dollars went towards the rehabilitation of main and feeder roads, airstrips, and telecommunications infrastructure," making "internal transportation cheaper and easier for host communities"(1999, 12). This might be very important in a region where the remoteness is an important determinant of the likelihood of growing out of poverty (De Weerdt, 2006). The literature has also provided important evidence on the ability of road infrastructure in particular to foster broad-based economic development (Jacoby, 2000; Jacoby and Minten, 2009; Khandker et al., 2009). Based on the above theoretical framework, improved road accessibility, associated with the inflows of refugees, would be supportive of a shift of equilibrium, as opposed to strong evidence for multiple equilibria. We measure road accessibility using the road networks in 1991 and in 2005 (see Figure 4). The data sources are provided in Appendix A. We can measure road accessibility as the shortest distance between each village and the road network. An alternative is to construct buffers around each village with 20-, 15-, 10- or 5-kilometer radius and measure the length of the road segments within each buffer. Descriptive statistics, given in Table 6, indicate strong

Table 6: Descriptive statistics for village-level variables (mean values)

	Refugee index no log	Rainfall average	Distance to road	Access within 20 km	Access within 10 km	Access within 5 km
1991						
High-refugee areas	0	139.4	38.0	0	0	0
Other areas	0	161.75	1.59	119.0	41.8	15.0
All	0	159.53	5.87	105.0	36.9	13.2
2004						
High-refugee areas	34,550	90.40	7.10	104.2	36.9	7.8
Other areas	6,040	144.83	3.13	108.0	36.4	14.2
All	8,880	139.40	3.59	107.6	36.5	13.5
2010						
High-refugee areas	34,550	105.28				
Other areas	6,040	117.03				
All	8,880	115.86				

Notes: Average monthly rainfall in millimeters during the growing periods of the last two years. Distance to roads or the lengths of roads are expressed in kilometers.

Figure 5: Road networks



Source: Road networks from DIVA-GIS *and the Tanzanian National Roads Agency.*
Note: KHDS = Kagera Health Development Survey

improvements in road accessibility in high-refugee areas.

As shown in Table 7, the presence of refugees has a positive and significant impact on road accessibility, measured in various ways. In columns 1 and 2 of panel A, we regress the length of roads within a buffer of 20 kilometers around each village on the presence of refugees, including or excluding time-varying village characteristics. Doubling the presence of refugees increases road accessibility by a factor of 4.5 to 5.4.[17]

This impact slightly decreases when the buffer is defined with a radius of 15 and 10 kilometers. Such a decrease reflects the lower ability to capture new road construction when the buffer is narrowly defined, because villages are not necessarily directly connected to the road networks. It decreases even further with a radius of 5 kilometers, up to the point where the coefficient becomes not statistically different from zero in column (8). The 5-kilometer radius seems to be too narrow to capture enough variation in road accessibility. The impact decreases on average by about one third when the roads that have been rehabilitated (independently from the presence of refugees) by the Tanzanian government are excluded from the road networks.

The road networks can also be used to identify six new road segments. In panels C and D, we replicate the previous regressions of panels A and B, replacing the village fixed effects with the road fixed effects. This alternative provides a better control for unobserved factors affecting the endogenous location of new roads. Basically, we compare the effect of the presence of refugees on road accessibility among villages sharing the same new road segment. Panels C and D provide slightly lower coefficients, but the impact of doubling the presence of refugees remains in a similar range. It is hardly deniable that the impact is economically large. But such an increase is coming from a particularly low level of road accessibility. A less sophisticated —but easier to interpret —approach is to introduce the closest distance to the road network. As indicated in panel E, doubling the presence of refugees decreases the distance to the closest road network in a range between 42 and 52 percent ($(2^{elasticity}) \times 100$). In high-refugee areas (where average distance to the road was about 38 kilometers in 1991), that is equivalent to moving the road closer by 16–20 kilometers.

All in all, the drastic decrease in transport costs mainly induced by massive transport investment by

[17] Given the lack of accuracy of the Taylor approximation for large values of quasi-elasticities, the value of 5.4 corresponds to an increase in road accessibility from a level A1 ($lnA1 = 2.4ln(RI)$) to a level A2 ($lnA2 = 2.4ln(2 * RI)$). Mathematically, applying basic rules for logarithmic transformations, one can show that $lnA2 = 2.4ln(2) + 2.4ln(RI) = 2.4ln(2) + ln(A1)$, which implies that $A2/A1 = exp(2.4ln2) = 2^{2.44} = 5.4$. The remaining interpretations of coefficients presented in Table 7 are computed in a similar way.

international organizations (WFP and UNHCR) is strongly associated with the persistent welfare improvement observed in high-refugee areas. While it is impossible to fully refute the notion that roads may be endogenous to economic development, the institutional context of our analysis suggests that these roads were built to serve refugee camps. Given UNHCR guidelines that require refugee camps to be well connected and given the large scale of the refugee flows in Tanzania, UNHCR and the Tanzanian Ministry of Home Affairs had to build roads to serve refugee camps.[18] The importance of decreased transport costs in explaining the persistent impact of refugees is confirmed when undertaking a "horse race" exercise. At the cost of exacerbating endogeneity issues, we augment Equation (1) with one of our proxies for road accessibility in Table 8. We do find that the impact of the presence of refugees disappears by 2010 and not by 2004, when controlling for road accessibility. Although potentially affected by strong endogeneity bias, road accessibility is positively correlated with real consumption per adult equivalent, with coefficients statistically slightly different from zero with standard errors clustered at the initial village level. Similar results are found when using alternative proxies for road accessibility in Table B.6 of Appendix B.

[18]A legitimate concern may be that refugee camps were located in easy-to-access areas to ease the provision of goods. For instance, the largest refugee camp, Benaco, took the name of an earlier Italian company that builds a road from Rusomo to Lusahunga between 1977 and 1985. At the time the refugees entered Kagera, the setup of the Benaco camp was reported to be eased by the presence of an Italian/Tanzanian road construction company, called Cogefar. After some works of port rehabilitation in the the islands of Zanzibar and Pemba from 1988 to 1992, Cogefar was then contracted in 1993 to build a road between Kobero (at the border with Rwanda) and Nyazkasanza (in Ngara district) in the region of Kagera ($http://baldi.diplomacy.edu/italy/Italians/ittz5.htm$). The contract of the company was immediately altered by the UNHCR to establish roads on the Benaco site (Tanzanian Affairs, 1994, $http://www.tzaffairs/1994/09/benaco-tanzanias-second-city/$). The presence of the company in Kagera certainly eased the establishment of new roads to provide food to refugee camps. That should be kept in mind while discussing the generalizable nature of our results (Section 5). However, it does not support the claim that refugee camps were located in areas with good road accessibility prior to the arrival of refugees. Regressing the presence of refugees on initial road accessibility reveals the opposite conditions. Refugee camps were likely to be located in poorly connected areas (Table B.5 of Appendix B). That is also the case when restricting the analysis to the two bordering districts (see panel B of Table B.5 of Appendix B). No significant difference is found when controlling for the distance to the closest border. Results are also robust to the exclusion of the Benaco camp (or any other camp, excluded separately) from the construction of the refugee index.

Table 7: Assessing the role of road accessibility

Panel A	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. var.	Road accessibility (log)							
Roads	All							
Accessibility within	20km	20km	15km	15km	10km	10km	5km	5km
Refugee Index	2.436	2.172	2.086	1.535	1.877	1.437	0.630	-0.055
	(0.286)***	(0.541)***	(0.382)***	(0.753)**	(0.343)***	(0.638)**	(0.356)*	(0.455)
	[0.298]***	[0.213]***	[0.399]***	[0.387]***	[0.377]***	[0.255]***	[0.272]**	[0.284]
Observations	102	102	102	102	102	102	102	102
R-squared	0.781	0.896	0.523	0.766	0.396	0.730	0.108	0.544
$Rain_{v,t}$	No	Yes	No	Yes	No	Yes	No	Yes
Strata time trends	No	Yes	No	Yes	No	Yes	No	Yes
Time fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Village fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B								
Dep. var.	Road accessibility (log), excluding road rehabilitated by the Tanzanian government							
Refugee Index	2.082	2.045	1.679	1.291	1.296	0.920	0.554	0.269
	(0.210)***	(0.375)***	(0.284)***	(0.549)**	(0.255)***	(0.436)**	(0.185)***	(0.274)
	[0.210]***	[0.136]***	[0.300]***	[0.306]***	[0.247]***	[0.137]***	[0.175]***	[0.274]
Observations	102	102	102	102	102	102	102	102
R-squared	0.798	0.900	0.530	0.744	0.389	0.703	0.301	0.640
Panel C								
Dep. var.	Road accessibility (log), using a new road fixed effect							
Refugee Index	2.336	2.077	1.985	1.610	1.699	1.414	0.420	-0.300
	(0.259)***	(0.461)***	(0.346)***	(0.611)**	(0.321)***	(0.559)**	(0.335)	(0.491)
	[0.291]***	[0.164]***	[0.385]***	[0.236]***	[0.394]***	[0.193]***	[0.255]*	[0.339]
$Rain_{v,t}$	No	Yes	No	Yes	No	Yes	No	Yes
Strata time trends	No	Yes	No	Yes	No	Yes	No	Yes
Time fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Road fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	102	102	102	102	102	102	102	102
R-squared	0.793	0.896	0.620	0.817	0.429	0.769	0.351	0.740
Panel D								
Dep. var.	Road accessibility (log), excluding road rehabilitated by the Tanzanian government and using a new road fixed effect							
Refugee Index	2.008	1.933	1.643	1.413	1.199	0.938	0.476	0.219
	(0.189)***	(0.335)***	(0.254)***	(0.447)***	(0.245)***	(0.445)**	(0.178)***	(0.322)
	[0.203]***	[0.120]***	[0.283]***	[0.128]***	[0.236]***	[0.130]***	[0.141]***	[0.362]
Observations	102	102	102	102	102	102	102	102
R-squared	0.856	0.912	0.679	0.796	0.497	0.744	0.459	0.754
Panel E								
Dep. var.	Distance to road network (log)							
	Incl. roads rehabilitated				Excl. roads rehabilitated			
Refugee Index	-1.250	-1.144	-1.067	-1.106	-1.247	-0.938	-1.135	-0.946
	(0.232)***	(0.379)***	(0.291)***	(0.701)	(0.203)***	(0.354)**	(0.240)***	(0.592)
	[0.231]***	[0.253]***	[0.229]***	[0.339]***	[0.237]***	[0.295]***	[0.193]***	[0.271]***
$Rain_{v,t}$	No	Yes	No	Yes	No	Yes	No	Yes
Strata time trends	No	Yes	No	Yes	No	Yes	No	Yes
Village fixed effects	Yes	Yes	No	No	Yes	Yes	No	No
New road fixed effects	No	No	Yes	Yes	No	No	Yes	Yes
Observations	102	102	102	102	102	102	102	102
R-squared	0.282	0.448	0.396	0.498	0.287	0.412	0.449	0.524

Notes: Standard errors in parentheses are clustered at the initial village level. Standard errors in brackets are clustered at the strata level, using wild bootstrap method (Cameron et al. 2008). *, **, ***: significant at 10%, 5%, and 1%, respectively. R-squared retrieved from regressions with standard errors clustered at the village level.

Table 8: "Horse Race" on the role of road accessibility

Dep. var.	Log real consumption per adult equivalent			
	Sample A: 1991 - 2004		Sample B: 1991 - 2010	
	(1)	(2)	(3)	(4)
Panel A				
Refugee Index	0.061	0.064	-0.085	-0.093
	(0.020)***	(0.020)***	(0.102)	(0.106)
	[0.020]***	[0.024]***	[0115]	[0.114]
Road Accessibility (20km)	0.060	0.065	0.123	0.125
	(0.027)**	(0.028)**	(0.040)***	(0.042)***
	[0.022]***	[0.023]***	[0.042]***	[0.044]***
Panel B				
Refugee Index	0.072	0.074	-0.051	-0.052
	(0.020)***	(0.020)***	(0.105)	(0.104)
	[0.019]***	[0.023]***	[0.128]	[0.124]
Road Accessibility (15km)	0.033	0.034	0.115	0.115
	(0.022)	(0.023)	(0.042)***	(0.042)***
	[0.023]	[0.023]	[0.051]**	[0.050]**
Panel C				
Refugee Index	0.071	0.072	0.067	0.068
	(0.020)***	(0.020)***	(0.089)	(0.086)
	[0.019]***	[0.022]***	[0.093]	[0.092]
Road Accessibility (10km)	0.042	0.042	0.065	0.065
	(0.019)**	(0.019)**	(0.035)*	(0.034)*
	[0.022]*	[0.022]*	[0.038]*	[0.036]*
Panel D				
Refugee Index	0.077	0.078	0.072	0.074
	(0.020)***	(0.020)***	(0.079)	(0.077)
	[0.021]***	[0.023]***	[0.082]	[0.082]
Road Accessibility (5km)	0.022	0.021	0.081	0.081
	(0.021)	(0.022)	(0.036)**	(0.035)**
	[0.026]	[0.028]	[0.045]*	[0.043]*
$Rain_{v,t}$	No	Yes	No	Yes
Time fixed effect	Yes	Yes	Yes	Yes
Strata time trends	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes

Notes: Only the coefficient for the Refugee Index is reported. Standard errors in parentheses are clustered at the initial village level. Standard errors in brackets are clustered at the strata level, using wild bootstrap method (Cameron et al. 2008). *, **, ***: significant at 10%, 5%, and 1%, respectively. The same regressions are estimated in both samples.

4.3 The impact of refugees on prices

The welfare-improving impact of road accessibility in high-refugee areas is further corroborated by the decreasing effect on good prices. We first use price indexes as dependent variables in equation (2). This is motivated by the idea that, although this type of evidence is only indirect, a better road infrastructure should first and foremost lower the price of imported goods and affect the price of locally produced goods.[19] More specifically, in Panels B to D of Table 9, we assess the impact of the refugee presence on the Paasche price index, based on 20 comparable goods, allowing us to distinguish between food (Panel C) and non-food (Panel D) consumption goods. Sensitivity analysis using other price indices (Laspeyres and Fisher ideal price indices) are provided in Tables B.7 and B.8 of Appendix B. The differences between the composition of these indices and that of the food and non-food indices are described in Appendix A. Consistently with these sensitivity analysis, Table 9 indicates that between 1991 and 2010, the presence of refugees had a decreasing and significant impact on consumption prices. The quasi-elasticity stands between 0.6 and 0.9. The decreasing impact is driven by the prices of food items within the consumption basket (Panel C). A negative association is also found by 2004 but not pronounced enough to be precisely estimated. The persistent impact on real consumption per adult equivalent is largely driven by such price effects. In sharp contrast with the results on real consumption, the presence of refugees had only a minor positive impact on nominal consumption per adult equivalent by 2004 and no impact by 2010 (Panel E). We can therefore conjecture that the welfare gains associated with the initial presence of refugees persist because of the decrease in consumption prices. The prominent role of decreased prices is supportive of the idea that a shift of equilibrium can be mainly explained by subsequent investment in road infrastructure in high-refugee areas. Improved road infrastructure is indeed expected to decrease the price of traded goods, in particular in remote rural areas like Kagera (Casaburi et al., 2013). We test that conjecture in more direct way in the next section.

4.4 Other Possible Channels

The drastic decrease in transport costs caused by increased road provision is not the only possible explanation for the persistent positive impact of refugees. Both our fieldwork and the above theoretical framework point to two sets of alternative explanations resulting in either the switch to a new equilibrium in a multiple-equilibria setting or a shift in the existing equilibrium.

[19] Lower shipping costs may increase the demand for goods for which the local economy has a comparative advantage on the export side. Cheaper shipping costs may also put some downward price pressure on local goods. They may help lower costs (and thus prices) if some key intermediate goods (e.g., fertilisers) become cheaper to source. In equilibrium, we then expect firm and worker location choices to be affected following these changes in prices. These are the key mechanics of the New Economic Geography (Fujita and Thisse, 2002).

Table 9: Impact on prices and nominal consumption (Summary)

	Sample A: 1991 - 2004		Sample B: 1991 - 2010	
	(1)	(2)	(3)	(4)
A. Log real consumption per adult equivalent	0.076	0.078	0.193	0.195
	(0.024)**	(0.026)**	(0.059)***	(0.057)***
	[0.020]***	[0.023]***	[0.059]***	[0.044]***
	N=4,670		N=4,912	
B. Log Paasche Price Index (20 items)	-0.040	-0.022	-0.858	-0.694
	(0.127)	(0.197)	(0.124)***	(0.113)***
	[0.087]	[0.115]	[0.163]***	[0.119]***
C. Log Paasche Price Index (Food)	-0.009	0.022	-0.890	-0.631
	(0.125)	(0.187)	(0.119)***	(0.136)***
	[0.091]	[0.099]	[0.177]***	[0.155]***
D. Log Paasche Price Index (Non-Food)	-0.031	-0.043	0.032	-0.063
	(0.014)**	(0.024)*	(0.036)	(0.073)
	[0.014]**	[0.023]*	[0.028]	[0.072]
E. Log nominal consumption per adult equivalent	0.054	0.041	0.001	-0.025
	(0.027)**	(0.028)*	(0.081)	(0.056)
	[0.022]**	[0.022]*	[0.107]	[0.044]
	N=4,208		N=4,428	
$Rain_{v,t}$	No	Yes	No	Yes
Time fixed effect	Yes	Yes	Yes	Yes
Strata time trends	Yes	Yes	Yes	Yes

Notes: Only the coefficient for the Refugee Index is reported. Standard errors in parentheses are clustered at the initial village level. Standard errors in brackets are clustered at the strata level, using wild bootstrap method (Cameron et al. 2008). *, **, ***: significant at 10%, 5%, and 1%, respectively. Panels A and E include household fixed effects. Panel B to D include village fixed effects.

Explanation supporting a shift in the existing equilibrium are related to other changes in local fundamentals. Based on our fieldwork observations, two possibilities appear as credible hypotheses. On the one hand, other public goods may have played a role. Interviews with local authorities suggest that tax revenues strongly increased due to a surge in activity around refugee camps when they were open. These revenues may have been invested in growth-enhancing sectors such as education or health services. The provision of local public goods could also improve through a more subtle channel. Local authorities reported better management skills and institutional efficiency after dealing with international organizations. In turn, these enhancements could have helped local authorities to improve their subsequent collaborations with non-governmental organizations. Based on the KHDS community questionnaires, we can proxy for the provision of local public goods using data measuring the distance to the closest health facility (health dispensary, hospital, health center) and to education provider (secondary school –there was already a primary school in each village in 1991), as well as the sum of social services or non-governmental organizations in the community. These data are available only for 1991 and 2004 and are further described in Appendix A.

On the other hand, the impact of refugees more than 10 years after most refugees left may be explained by the persistence of trade links between refugees and their former hosts. Interviews conducted with Red Cross officers during our fieldwork point to the fact that many refugees repatriated just beyond the border and continued to trade with the local population.[20] Such hypothesized trade channel would echo the facilitation of economic exchanges between displaced people (after their return) and the hosting communities in other contexts (Burchardi and Hassan, 2013). To explore further the plausible nature of that channel, we compute total exports and imports between Tanzania and the three neighboring countries over the five years prior to 1991, 2004, and 2010, respectively. We then interact these bilateral trade flows with the distance of the surveyed villages to the border of these countries. The data sources are further described in Appendix A. One of the drawbacks of such a proxy is the fact we largely overlook informal exchanges across borders.

Two other explanations are related to a switch to a new equilibrium in a multiple-equilibria setting. First, the existence of multiple equilibria is consistent with the importance of agglomeration economies that could potentially be generated by the concentration of population. The inflow of refugees was indeed followed by an inflow of economic migrants attracted by the opportunities associated with the refugee camps. This second form of migration, which follows humanitarian aid, is documented by Buscher and Vlassenroot (2009) in other contexts. Importantly, many of these economic migrants stayed after the refugees left. As a result of increased population, agglomeration economies working through denser and more efficient labor markets (labor pooling), stronger backward and forward linkages, and increased spillovers allowing innovations to spread (Fujita and Thisse, 2002; Combes et al., 2008; Duranton and Puga, 2004) could explain part of the persistent impact of refugees. Anecdotal evidence in other countries suggests that refugee inflows may strengthen the urbanization process in the regions of destination (de Montclos and Kagwanja, 2000; Buscher and Vlassenroot, 2009; Alix-Garcia et al., 2013). Agglom-

[20]Recent work questions the return of refugees just behind the border. In their survey of returned refugees from Tanzania in Burundi, Fransen et al. (2016) do find that 82% of the adult returnees either reside in the community where they were born or in a neighboring community, while over 90% reside in their province of birth.

eration economies may be measured by the total population reported by each village leader. These data are available only for 1991 and 2004. We also used population density, which is proxied by the ratio between the village population reported in the community questionnaires and the average distance between each household and the center of its community. The population data and the construction of a proxy of population density are further described in Appendix A. Second, there is a long tradition in development economics of relating the multiplicity of equilibria to the existence of a poverty trap (Azariadis and Drazen, 1990; Murphy et al., 1989). For instance, Miguel and Roland (2011) formalized such a possibility in the case of Vietnam. There is little doubt that the imperfect nature of credit markets in rural Kagera is likely to generate poverty traps (De Weerdt, 2006). Conjecturing that the presence of refugees and the associated welfare improvement allows for an escape from such a poverty trap is another matter. To explore that channel, we depart from Equation (2). We rather adapt Equation (1) investigating through a linear probability model the impact of the temporary inflows of refugees on poverty, defined as having a real consumption per capita lower than 253,530 Tzs. The description of the poverty line is given in Appendix A. However, that approach will only shed light on a change at the mean for the entire consumption distribution, while the non-linear estimation (with household fixed effects) draws inference based on the subsample of households, that change their poverty status. We therefore also implement quantile regressions. The breakup of the poverty trap should be consistent with a stronger impact at the lower part of the consumption distribution.

We do not find much evidence in support of the multiplicity of equilibria. First, the persistency of the impact of refugees cannot be explained by the existence of poverty traps. According to columns 1 and 3 of Table 10, a decrease in poverty is observed by 2004 and by 2010 (although not precisely estimated in the later case). By 2004 and 2010, poverty is reduced by about 38 percent and 69 percent, respectively. Implementing quantile regressions in columns 4 to 8, we confirm the positive impact along the consumption distribution but observe that the improvements in real consumption have not been concentrated in the lowest part of the consumption distribution, either by 2004 (panel A) or by 2010 (panel B). On the contrary, no statistical difference can be found across the lower and upper quantiles. Second, agglomeration economies do not seem to drive our results. Panel A of Table 11 indicates, at least in the most complete regression, that welfare improvements are not associated with stronger agglomeration economies in refugee-hosting areas.

Next, the role of improved accessibility in shifting the equilibrium may be counfounded by other changes in local fundamentals, either in the form of larger provision of public goods and strengthened trade links with neighboring countries. We do not find supportive evidence for these channels. Panels B and C of Table 11 reject the first alternative explanation. When strata-year fixed effects are introduced, the presence of refugees has no effect on the accessibility of health, education and social services.[21] Panel D of Table 11 also shows no strong impact of the refugee inflows on trade flows with neighboring countries. Finally, the "horse race" exercise also largely dismisses these alternative channels since adding these proxied channels on the right-hand side of Equation (1) does not alter significantly the impact of the presence of refugees on real consumption per adult equivalent. Detailed results are provided in Tables B.9 and B.10 of Appendix B.

We acknowledge that our exploration of alternative explanations may be limited by data availability and measurement errors. However, we do not find evidence that changes in the provision of local public goods, or in the role of agglomeration economies, or the enhanced trade with neighboring countries constitutes an alternative explanation for the persistent increase in real consumption in high-refugee areas compared with other areas.

[21] We can also reject a more subtle channel, i.e. the possible skill transferability between migrants and local hosts observed in other settings (Bazzi et al., 2016). Applying conventional and quantile regressions similar to the ones used in Tables 2 and 10 replacing the dependent variable with the average education of the household, does not provide strong evidence for that channel. On the contrary, while no impact is found by 2004, the presence of refugees is associated with a decrease in education in high refugee areas by 2010. No statistical differences are found between lower and upper quantiles.

Table 10: Impact on poverty and consumption distribution

Model	Linear probability model		Main results	Quantile regressions				
			mean	q10	q25	q50	q75	q90
Panel A	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. var.	Dummy for being poor by 2004		Log real consumption per adult equivalent, 1991 and 2004					
Refudee Index	0.005	-0.044	0.088	0.095	0.083	0.082	0.083	0.095
	(0.022)	(0.020)**	(0.019)***	(0.025)***	(0.016)***	(0.015)***	(0.014)***	(0.028)***
Observations	5,230	5,230	5,230	5,230	5,230	5,230	5,230	5,230
$Rain_{v,t}$	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Strata time trends	No	yes	Yes	Yes	Yes	Yes	Yes	Yes
t-statistic:					-0.012	-0.013	-0.012	0.000
Diff. from q10					(0.019)	(0.027)	(0.026)	(0.033)
t-statistic:						-0.000	0.000	0.012
Diff. from q25						(0.015)	(0.015)	(0.038)
Panel B								
Dep. var.	Dummy for being poor by 2010		Log real consumption per adult equivalent, 1991 and 2010					
Refugee Index	-0.025	-0.097	0.160	0.120***	0.170	0.196	0.170	0.120
	(0.054)	(0.093)	(0.051)***	(0.044)	(0.040)***	(0.056)***	(0.033)***	(0.032)***
Observations	6,616	6,616	5,788	5,788	5,788	5,788	5,788	5,788
t-statistic:					0.049	0.076	0.049	0.000
Diff. from q10					(0.036)	(0.058)	(0.047)	(0.048)
t-statistic:						0.026	0.000	-0.049
Diff. from q25						(0.044)	(0.034)	(0.045)

Notes: Robust standard errors clustered at the initial village level in parentheses. *, **, ***: significant at 10%, 5%, and 1%, respectively.

Table 11: Assessing the role of other channels

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	Population (log)		Population density (log)			
Refugee Index	0.315	-0.333	-0.018	0.055		
	(0.122)**	(0.244)	(0.030)	(0.090)		
	[0.094]***	[0.142]**	[0.013]	[0.047]		
Observations	100	100	100	100		
R-squared	0.131	0.399	0.037	0.226		
Panel B						
Dep. var.	Distance health dispensary (log)		Distance hospital (log)		Distance health center (log)	
Refugee Index	-0.426	0.012	-0.158	0.339	-0.200	0.404
	(0.250)*	(0.407)	(0.125)	(0.205)	(0.269)	(0.558)
	[0.063]***	[0.276]	[0.097]	[0.200]*	[0.172]	[0.319]
Observations	94	94	93	93	92	92
R-squared	0.165	0.290	0.048	0.395	0.047	0.293
Panel C						
Dep. var.	Distance school (log)		Number social services (log)		Number NGO (log)	
Refugee Index	-0.833	0.309	-0.154	0.254	-0.281	-0.406
	(0.325)**	(0.989)	(0.098)	(0.377)	(0.063)***	(0.264)
	[0.291]***	[0.929]	[0.105]	[0.351]	[0.087]***	[0.260]
Observations	101	101	102	102	102	102
R-squared	0.348	0.508	0.023	0.330	0.232	0.483
Panel D						
Dep. var.	Openness with Rwanda* Proximity to Rwanda		Openness with Burundi* Proximity to Burundi		Openness with Uganda* Proximity to Uganda	
Refugee Index	0.013	0.022	0.438	0.751	0.001	0.000
	(0.009)	(0.017)	(0.287)	(0.517)	(0.001)	(0.001)
	[0.007]*	[0.010]**	[0.229]*	[0.324]**	[0.001]	[0.001]
Observations	102	102	102	102	102	102
R-squared	0.228	0.393	0.259	0.447	0.015	0.151
$Rain_{v,t}$	No	Yes	No	Yes	No	Yes
Strata time trends	No	Yes	No	Yes	No	Yes
Time fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Village fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors in parentheses are clustered at the initial village level. Standard errors in brackets are clustered at the strata level, using wild bootstrap method (Cameron et al. 2008). *, **, ***: significant at 10%, 5%, and 1%, respectively. The same regressions are estimated in all panels. $Q_{v,t}$ includes not only include the monthly rainfall anomalies over the growing seasons of the last two years. R-squared retrieved from regressions with standard errors clustered at the village level.

5 Conclusions

Our results indicate that the refugee presence significantly increased real consumption per adult equivalent between 1991 and 2004 and between 1991 and 2010, although most refugees left between 1996 and 2000. We then investigate the possible channels of transmission of such persistence. The most important channel of transmission is a sizable decrease in transport costs following increased road building. We interpret these changes as a shift in equilibrium induced by the shock that represents the massive refugee inflows in the region of Kagera in the 1990s. We find no evidence supporting the notion that multiple equilibria may have been at play.

The findings undercut the view, which is commonly held today, that forced migrants systematically constitute a burden for hosting communities. On the contrary, our results suggest that a new paradigm is needed when dealing with a protracted refugee situation. In the short run, the priorities should certainly be to improve the ability of the local population to cope with changes in the price of final goods and factors. Then, progressively, humanitarian assistance should give way to long-term developmental efforts, capitalizing on the road investments made by international organizations. In a context similar to our case study in Tanzania, we can conjecture that local integration of the refugees into the local economy could have certainly acted as a multiplier of the welfare-improving effects of better road conditions. Our results also indicate that fostering regional integration with neighboring countries may be an interesting second-best option to consider when repatriation (or resettlement) is favored as a solution to a protracted refugee situation.

Finally, it is important to remain cautious about the generalizable nature of our results to other contexts. The positive path dependence emerging from the refugee inflows is not independent from the initial conditions prevailing at the time of arrival of the refugees. First, the fact that land availability is not a major constraint in the region of Kagera certainly eased the integration of refugees into the local economy. However, the region of Kagera was not necessarily an exception. Anecdotal evidence from Kenya and Uganda (Mabiso et al., 2014) also suggests positive outcomes (with potential redistribution effects) resulting from large refugee inflows. Second, there were no major historical grievances against refugees in northwestern Tanzania. In contrast, the security concerns were much higher when refugees from Rwanda (in particular the *genocidaires*) moved to eastern Democratic Republic of Congo where ethnic tensions constitute a strong historical legacy. Still, there is no reason to believe that the developmental benefits from road infrastructure could not be reaped in other rural economies. A question for further research is whether these benefits would have been so large without the dynamics initially induced by the establishment of refugee camps and the presence of a road construction company in the region. One limitation of our present analysis is that we are not able to qualify the optimal nature of the shift in equilibrium. Road investment has certainly been beneficial, but we cannot exclude the idea that a social planner could have possibly increased social welfare by building roads in other areas. The question of optimality of a new spatial equilibrium is a key question for further research (Jedwab et al., 2017) and would call for more research on the costs of new infrastructure and its maintenance.

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Separate Appendixes with Supplemental Material for:

The Development Push of Refugees: Evidence from Tanzania

March 8, 2018

Abstract

This document contains a set of appendixes with supplemental material.

Keywords: Refugees; Tanzania; Multiple equilibrium; Roads.

JEL Classification: I32; O18; Q54

Appendix A Data

Sample structure over time. Of the initial 915 households surveyed from 1991 to 1994, the field team managed to recontact 832 of them (see Beegle et al., 2006, for more details). Because many individuals had moved out from their original households (children in particular), the recontacted individuals from 1991 were part of 2,770 households interviewed for the 2004 KHDS. This number includes individuals who had moved out of their village of origin, the Kagera region, and even Tanzania since 1991. In 2010, a follow-up survey (wave 6) was administered to re-interview all respondents ever interviewed in the KHDS 1991–1994. That resulted in a sample of 3,314 households, originating from the 915 initial households. Despite almost 20 years' having elapsed since the first interviews, the field team achieved recontact with 92 % of the original households.

Sample definition. Several complications result from the multiplication of surveyed households over time and the links among the households.[22] The samples may be better defined in terms of a fictional household. In table A.1, we represent the way an original household with five members can multiply into four households in 2004 and six households in 2010. That example shows that the number of households originating from the same original household multiplies in 2004 because household members either create a new household (individuals 3 and 5 in 1991 create households with ID numbers 1000 and 1002) or join another non-surveyed household (member 4 in 1991 joins household ID 1001).

New households are usually named after a split-off household. In turn, children of these households can also create or join new households in 2010. Special cases exist if, for example, a household member joins a previously surveyed household, causing, the split-off households to be linked to two original households. In that case, we link the newly joined households to the original household of the majority of the household members, or we keep the original household of the household head.

The baseline analysis is based on 3,314 households, including households who had migrated outside of Kagera by 2004 and 2010. Due to missing consumption data, 207 households are excluded. The sample

[22] In the 2004 KHDS, in the migration section, the identification of a split-off household was made possible thanks to the following question (s9q1): "Is the current household living in the same homestead (same plot or house) as households interviewed 10 years ago?". In 2010, no question similar to s9q1 is available. We use the information related to RelR2 (Relation to the Head KHDS 2004) because we are interested in identifying the households that split-off between 2004 and 2010. Basically, we will define a split-off household in 2010 when a household head is different from its original household head in 2004. For consistency reasons, we also compute the same variable compared with the original household from 1991 to 1994. Nonetheless, when the analysis is performed on the period 1991–2004, the results are similar when using the more accurate question s9q1.

Table A.1: Illustration of household structure and links with original households

1991			2004		2010	
HH member	HH 1991	Status	HH 2004	Status	HH 2010	Status
1	1	Head	1	Head	1	Dead
2	1	Wife	1	Wife	1	Head
3	1	Child	1000	Head	1000	Head
			1000	Wife	1000	Wife
			1000	Child	1000	Child
			1000	Child	2900	Head
					2900	Child
					2900	Child
4	1	Child	1001	Wife	1001	
			1001	Head	1001	
			1001	Child	1001	
5	1	Child	1002	Head	1002	
			1002	Child	1002	
			1002	Child	3000	Wife
					3000	Head
					3000	Child
					3000	Child

Source: Authors' own construction.
Note : HH = Household. The numbers constitute fictive illustrations of possible household identification codes.

is reduced to 2,572 households when we exclude the migrants. That sample still includes those moving to a nearby village. Table A.2 provides further information about attrition and migration. Note that all our results are shown to be robust to the different samples, including those who have changed location by 2004 and 2010.

Consumption data are fully comparable for the years 1991, 2004, and 2010, because they use the same recall periods. The recall period for the consumption module in 1992, 1993, and 1994 was reduced from 12 to 6 months. That is an important issue given the sensitivity of consumption data to seasonality and the bias a different period may introduce (Beegle et al., 2012). The aggregated consumption data defined in 1991, 2004, and 2010 and provided by Economic Development Initiatives ($http : //www.edi - africa.com$) have been used for comparability reasons (recall periods, common definition of components). The consumption basket comprises 20 items. There are 15 food items (beef, chicken, chicken eggs, cooking bananas, cooking oil, dried beans, Fresh milk, groundnuts, onions,

Table A.2: Attrition and migration rates

Descriptive statistics for 2004	Attrition since 1991	Migration all (since 1991)	Migration outside of Kagera (since 1991)
Refugee-hosting areas	0.05	0.19	0.06
Other areas	0.1	0.32	0.11
All areas	0.09	0.31	0.11

Descriptive statistics for 2010	Attrition since 1991	Migration all (since 1991)	Migration outside of Kagera (since 1991)
Refugee-hosting areas	0.08	0.38	0.06
Other areas	0.13	0.46	0.19
All areas	0.12	0.46	0.18

raw cassava, rice, sorghum, sugar, sweet potato and tomatoes) and 5 non-food items (battery, charcoal, kerosene, linen, local Brew). We transform these consumption data in real terms (2010 Tanzanian shillings), using the Fisher ideal index described below.

The poverty status of each household is based on the consumption data described above as well as a poverty line similar to the one constructed by Beegle et al. (2011). In particular, these authors calibrated the poverty line on their sample of households who remained in Kagera to yield the same poverty rate as the 2000–2001 National Household Survey estimate for Kagera (29 percent). The same calibration can be performed on the non-migrant households of 2004 who were reinterviewed in 2010 to find a poverty line of 253,530 Tzs, expressed in 2010 values. We thank Kalle Hirvonen, who assented to share his code for that calibration exercise.

Price data have been aggregated by EDI ($http://www.edi-africa.com$). We apply the same method to construct three prices indices (Laspeyres, Paasche, and Fisher), distinguishing between food and non-food prices. The Laspeyres index compares the changes in prices assuming constant shares of expenditures, defined in 1991, between the 20 items composing the consumption basket. The Paasche index compares the change in prices assuming constant shares of expenditures, but defined in 2010, between the 20 items composing the consumption basket. The Fisher ideal index is a composite index of the two, potentially taking into account changes in the allocation within the consumption baskets. The Fisher ideal price index is indeed a combination of the square root of the sum of the deviation of price of each item compared with its baseline price, weighted by the budget share of each item in 1991 and 2010 (see $http://www.edi-africa.com$). In 2010, price data were reported by individuals, although previous rounds collected prices at the village level (proxying the price index for the migrants by regional and national inflation figures). Individual reporting prices have been found to provide major quality improvements (Gibson and Rozelle, 2005). A logarithm transformation is also applied when the price indices are used as dependent variable.

Rainfall data in total millimeters of rain per month from 1980 to 2010 are available from the Tanzanian Meteorological Agency based on more than 200 weather stations. Similar to Hirvonen (2016) and De Weerdt and Hirvonen (2016), an inverse distance weighting method is applied to link each household to the rainfall data. We thank Kalle Hirvonen, who assented to share his code for the inverse distance weighting method. Rainfall data are transformed into anomalies, that is, deviations from the long-term mean (1980–2010), divided by its long-run standard deviation. Anomalies during the growing periods should capture deviations from the normal conditions for agricultural production. The growing periods cover the months of March, April, May, October, November, and December).

Road accessibility are computed based on two road networks. First, data on road networks for the year 1991 are based on the DIVA-GIS ($www.diva-gis.org$) road network, excluding trails for comparability reasons with the 2005 data. The road network from 2005 comes from the Tanzanian National Roads Agency. For each, the Euclidian distance between each village and the closest road network is computed. We also compute the lengths of the roads, within buffers of different sizes (5, 10, 15, and 20 kilometers) around each village. Information on road rehabilitation is derived from Gachassin (2013).

Access to local public goods is based on the evaluation of the community leaders. The KHDS indeed provides community data based on a separate community survey addressed to the community leader. We define the following dependent variables: the distance needed to go to the closest health dispensary, the closest hospital, and the closest health center; the distance needed to go to the closest secondary school (all villages have a primary school, so that there is no variation in that dimension); the sum of social services or organizations operating in the community.[23]

Population in each village for each year is approximated by the village leader in the community survey of the KHDS database (unfortunately not in 2010). We also construct a proxy for population density.

[23]The related question is "Do any of the following social services or organizations (Daycare Centre, Tanzanian Red Cross, Partage Assistance, Bakwata, World Vision assistance, Roman Catholic Assistance, Others) exist in this community?"

Economic Development Initiative, the company that collected the data in the region of Kagera, has made available the distance between each household and the center of its community. Assuming that the village has a monocentric structure, we can estimate the area of each village for example using the average distance between the household and the community center. We assume that the area is proportional to the square of that average distance .

Trade-related flows are proxied as interaction terms between distance-to-the-border data and bilateral trade data, similar to Djemai (2009). We interact the Euclidian distance with time-varying bilateral trade data. Bilateral (or dyadic) trade data from the Correlates of War projects (Martin et al., 2008) are used to compute the total imports or exports from and to neighboring countries. These data are limited to 2006. We complete these data with bilateral trade data from ComTrade between 2007 and 2010. We compute total exports and imports for periods of five years prior to 1991, 2004, and 2010, respectively. We then use the real gross domestic product (GDP) data from the World Penn table (Heston et al., 2006) to compute a measure of trade openness with the three neighboring countries as the ratio of each country's total bilateral trade with Tanzania to its real GDP. We also compute the same ratio, using only total exports as a numerator. Using only exports provide similar results, though they are not reported.

Appendix B Supplementary Tables

Table B.3: The location of refugee camps in the Kagera region

Panel A	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.	Refugee index					
Sample	Region of Kagera (6 districts)					
$\log\left(\frac{C_{h,1991}}{P_{v,1991}}\right)$	-0.952		-0.986	-0.228		-0.316
	(0.766)		(0.773)	(0.175)		(0.221)
	[0.482]**		[0.502]**	[0.128]*		[0.143]**
1991–1993 Growth		0.008	-0.148		-0.111	-0.188
		(0.046)	(0.281)		(0.158)	(0.168)
		[0.141]	[0.213]		[0.125]	[0.122]
Distance to Rwanda				-5.098	-4.900	-5.715
				(3.448)	(3.804)	(3.567)
				[1.987]**	[1.974]**	[1.702]***
Distance to Burundi				-3.132	-3.522	-2.726
				(1.362)**	(3.037)	(1.111)**
				[0.799]***	[0.860]***	[0.619]***
Observations	51	51	51	51	51	51
R-squared	0.174	0.000	0.180	0.870	0.867	0.878
Panel B						
Dep. var.	Refugee index					
Sample	Districts of Ngara and Karagwe					
$\log\left(\frac{C_{h,1991}}{P_{v,1991}}\right)$	-1.999		-1.491	0.208		0.230
	(0.370)***		(0.349)***	(0.164)		(0.179)
	[0.192]***		[0.271]***	[0.117]		[0.113]
1991–1993 Growth		-2.004	-1.133		-0.011	0.052
		(0.320)***	(0.426)**		(0.113)	(0.144)
		[0.286]***	[0.393]***		[0.095]	[0.096]
Distance to Rwanda				-29.723	-29.404	-30.475
				(5.817)***	(4.915)***	(5.705)***
				[3.701]***	[2.713]***	[3.200]***
Distance to Burundi				-3.733	-3.151	-3.788
				(0.770)***	(0.876)***	(0.878)***
				[0.368]***	[0.528]	[0.466]***
Observations	11	11	11	11	11	11
R-squared	0.705	0.544	0.833	0.990	0.989	0.990

Notes: Standard errors in parentheses are clustered at the initial village level. Standard errors in brackets are clustered at the strata level, using wild bootstrap method (Cameron et al. 2008). *, **, ***: significant at 10%, 5%, and 1%, respectively. 1991-1993 Growth denotes the change in real consumption per adult equivalent between 1991 and 1993. Distances to borders are expressed in meters ($\times 10^6$). The same regressions are estimated in both panels. R-squared retrieved from regressions with standard errors clustered at the village level.

Table B.4: Placebo test (parallel trend assumption). Including future split-off households

Panel A	(1)	(2)	(3)	(4)	(5)
Dep. var.	Log real consumption per adult equivalent, 1991 and 1993				
Placebo $RI_{v,t}$	-0.055	-0.359	0.102	-0.145	-0.140
	(0.050)	(0.072)***	(0.103)	(0.144)	(0.142)
	[0.042]	[0.048]***	[0.116]	[0.187]	[0.186]
Household controls	No	No	No	No	Yes
$Rain_{v,t}$	No	Yes	No	Yes	Yes
Time fixed effect	Yes	Yes	Yes	Yes	Yes
Strata time trends	No	No	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	4,855	4,855	4,855	4,855	4,855
R-squared	0.987	0.988	0.989	0.989	0.990
Panel B					
Dep. var.	Log real consumption per adult equivalent, 1991 and 1993				
Placebo $RI_{v,t}$	-0.642	-0.649	-0.551	-0.584	-0.585
	(0.083)***	(0.086)***	(0.180)***	(0.179)***	(0.179)***
	[0.084]***	[0.088]***	[0.180]***	[0.184]***	[0.195]***
Placebo $RI_{v,t}$	0.068	0.060	0.063	0.054	0.055
$\times$ Initial cons$_{v,1991}$	(0.008)***	(0.010)***	(0.013)***	(0.013)***	(0.013)***
	[0.008]***	[0.011]***	[0.000]***	[0.013]***	[0.013]***
Observations	4,855	4,855	4,855	4,855	4,855
R-squared	0.989	0.989	0.989	0.989	0.990

Notes: Standard errors in parentheses are clustered at the initial village level. Standard errors in brackets are clustered at the strata level, using wild bootstrap method (Cameron et al. 2008). *, **, ***: significant at 10%, 5%, and 1%, respectively. The same regressions are estimated in both panels. R-squared retrieved from regressions with standard errors clustered at the village level.

Table B.5: The location of refugee camps in more accessible areas?

Panel A	Region of Kagera (6 districts									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. var.	Refugee index									
Accessibility within	20km	20km	15km	15km	10km	10km	5km	5km	Distance (log)	
Road Accessibility	-0.328	-0.197	-0.354	-0.202	-0.362	-0.164	-0.400	-0.136	0.236	0.081
(1991)	(0.017)***	(0.022)***	(0.021)***	(0.025)***	(0.048)***	(0.048)***	(0.071)***	(0.039)***	(0.045)***	(0.020)***
	[0.015]***	[0.025]***	[0.021]***	[0.028]***	[0.056]***	[0.043]***	[0.109]***	[0.046]***	[0.064]***	[0.029]***
Distance to		-2.902		-2.672		-2.667		-3.044		-3.069
Rwanda		(0.680)***		(0.772)***		(0.853)***		(1.023)***		(0.997)***
		[0.895]***		[1.021]***		[1.242]**		[1.504]**		[1.422]**
Distance to		-1.434		-1.772		-2.627		-3.248		-3.451
Burundi		(0.569)**		(0.639)***		(0.927)***		(0.841)***		(0.697)***
		[0.716]**		[0.778]**		[0.959]***		[0.917]***		[0.737]***
Observations	51	51	51	51	51	51	51	51	51	51
R-squared	0.894	0.961	0.878	0.953	0.780	0.922	0.625	0.897	0.538	0.898
Panel B	Districts of Ngara and Karagwe									
Dep. var.	Refugee index									
Road Accessibility	-0.304	0.004	-0.334	0.005	-0.315	0.003	-0.345	0.026	0.329	-0.002
	(0.027)***	(0.034)	(0.040)***	(0.031)	(0.066)***	(0.022)	(0.090)***	(0.035)	(0.078)***	(0.021)
	[0.023]***	[0.025]	[0.039]***	[0.023]	[0.054]***	[0.015]	[0.093]	[0.014]	[0.070]***	[0.012]
Distance to		-30.015		-30.139		-30.046		-33.473		-29.840
Rwanda		(8.726)***		(8.669)***		(8.577)***		(8.867)***		(7.549)***
		[5.877]***		[5.809]***		[5.613]***		[3.971]***		[4.935]***
Distance to		-3.181		-3.161		-3.118		-2.754		-3.135
Burundi		(0.766)***		(0.850)***		(1.029)**		(1.034)**		(0.912)***
		[0.324]***		[0.443]***		[0.650]***		[0.499]***		[0.593]***
Observations	11	11	11	11	11	11	11	11	11	11
R-squared	0.903	0.989	0.856	0.989	0.601	0.989	0.403	0.989	0.639	0.989

Notes: Standard errors in parentheses are clustered at the initial village level. Standard errors in brackets are clustered at the strata level, using wild bootstrap method (Cameron et al. 2008). *, **, ***: significant at 10%, 5%, and 1%, respectively. R-squared retrieved from regressions with standard errors clustered at the village level.

Table B.6: Horse Race: Alternative road accessibility

Dep. var.	Log real consumption per adult equivalent			
	Sample A: 1991 - 2004		Sample B: 1991 - 2010	
	(1)	(2)	(3)	(4)
Panel A				
Refugee Index	0.057	0.060	-0.069	-0.088
	(0.019)***	(0.020)***	(0.105)	(0.107)
	[0.021]***	[0.023]**	[0.134]	[0.130]
Alternative road (20km)	0.085	0.088	0.118	0.126
	(0.030)***	(0.031)***	(0.041)***	(0.043)***
	[0.020]***	[0.021]***	[0.048]**	[0.050]**
Panel B				
Refugee Index	0.071	0.073	-0.010	-0.015
	(0.019)***	(0.019)***	(0.103)	(0.101)
	[0.018]***	[0.021]***	[0.137]	[0.131]
Alternative road (15km)	0.053	0.054	0.098	0.100
	(0.024)**	(0.024)**	(0.040)**	(0.040)**
	[0.028]*	[0.027]**	[0.050]	[0.052]
Panel C				
Refugee Index	0.071	0.073	0.089	0.091
	(0.019)***	(0.019)***	(0.082)	(0.080)
	[0.018]***	[0.021]***	[0.092]	[0.092]
Alternative road (10km)	0.069	0.069	0.056	0.055
	(0.018)***	(0.018)***	(0.031)*	(0.030)*
	[0.018]***	[0.018]***	[0.037]	[0.028]*
Panel D				
Refugee Index	0.075	0.076	0.089	0.091
	(0.020)***	(0.019)***	(0.077)	(0.075)
	[0.020]***	[0.023]***	[0.086]	[0.085]
Alternative road (5km)	0.038	0.037	0.074	0.073
	(0.025)	(0.026)	(0.035)**	(0.034)**
	[0.029]	[0.030]	[0.047]	[0.046]
Panel E				
Refugee Index	0.072	0.073	0.104	0.106
	(0.019)***	(0.019)***	(0.086)	(0.084)
	[0.018]***	[0.021]***	[0.078]	[0.075]
Distance to road	-0.029	-0.030	-0.034	-0.033
	(0.026)	(0.025)	(0.023)	(0.022)
	[0.031]	[0.031]	[0.024]	[0.024]
$Rain_{v,t}$	No	Yes	No	Yes
Time fixed effect	Yes	Yes	Yes	Yes
Strata time trends	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes

Notes: Only the coefficient for the Refugee Index is reported. Standard errors in parentheses are clustered at the initial village level. Standard errors in brackets are clustered at the strata level, using wild bootstrap method (Cameron et al. 2008). *, **, ***: significant at 10%, 5%, and 1%, respectively. "Alternative road" exclude roads rehabilitated by the Tanzanian government from the computation of road accessibility.

Table B.7: Assessing the impact on prices, 1991-2004

Price index, 1991–2004	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Dep. var.	Log Laspeyres price index					
Food vs. non-food	20 items	20 items	Food	Food	Non-food	Non-food
Refugee Index	-0.016	-0.070	0.005	-0.050	-0.021	-0.020
	(0.149)	(0.226)	(0.147)	(0.217)	(0.013)	(0.023)
	[0.113]	[0.110]	[0.118]	[0.096]	[0.013]	[0.022]
$Rain_{v,t}$	No	Yes	No	Yes	No	Yes
Strata time trends	No	Yes	No	Yes	No	Yes
Time fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Village fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	102	102	102	102	102	102
R-squared	0.975	0.988	0.972	0.986	0.963	0.979
Panel B						
Dep. var.	Log Paasche price index					
Food vs. non-food	20 items	20 items	Food	Food	Non-food	Non-food
Refugee Index	-0.040	-0.022	-0.009	0.022	-0.031	-0.043
	(0.127)	(0.197)	(0.125)	(0.187)	(0.014)**	(0.024)*
	[0.087]	[0.115]	[0.091]	[0.099]	[0.014]**	[0.023]*
Observations	102	102	102	102	102	102
R-squared	0.980	0.991	0.978	0.989	0.968	0.983
Panel C						
Dep. var.	Log Fisher ideal price index					
Food vs. non-food	20 items	20 items	Food	Food	Non-food	Non-food
Refugee Index	-0.029	-0.047	-0.002	-0.012	-0.024	-0.026
	(0.138)	(0.211)	(0.135)	(0.202)	(0.014)*	(0.023)
	[0.100]	[0.113]	[0.104]	[0.097]	[0.014]*	[0.022]
Observations	102	102	102	102	102	102
R-squared	0.978	0.989	0.975	0.987	0.967	0.982

Notes: Standard errors in parentheses are clustered at the initial village level. Standard errors in brackets are clustered at the strata level, using wild bootstrap method (Cameron et al. 2008). *, **, ***: significant at 10%, 5%, and 1%, respectively. R-squared retrieved from regressions with standard errors clustered at the village level.

Table B.8: Assessing the impact on prices, 1991-2010

Price index, 1991–2010

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Dep. var.	Log Laspeyres price index					
Food vs. non-food	20 items	20 items	Food	Food	Non-food	Non-food
Refugee Index	-1.042	-0.964	-1.052	-0.895	0.011	-0.068
	(0.132)***	(0.116)***	(0.127)***	(0.134)***	(0.029)	(0.058)
	[0.179]***	[0.113]***	[0.191]***	[0.137]***	[0.023]	[0.058]
$Rain_{v,t}$	No	Yes	No	Yes	No	Yes
Strata time trends	No	Yes	No	Yes	No	Yes
Time fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Village fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	102	102	102	102	102	102
R-squared	0.992	0.996	0.989	0.995	0.979	0.983
Panel B						
Dep. var.	Log Paasche price index					
Food vs. non-food	20 items	20 items	Food	Food	Non-food	Non-food
Refugee Index	-0.858	-0.694	-0.890	-0.631	0.032	-0.063
	(0.124)***	(0.113)***	(0.119)***	(0.136)***	(0.036)	(0.073)
	[0.163]***	[0.119]***	[0.177]***	[0.155]***	[0.028]	[0.072]
Observations	102	102	102	102	102	102
R-squared	0.993	0.997	0.990	0.996	0.979	0.983
Panel C						
Dep. var.	Log Fisher ideal price index					
Food vs. non-food	20 items	20 items	Food	Food	Non-food	Non-food
Refugee Index	-0.950	-0.831	-0.971	-0.762	0.022	-0.064
	(0.128)***	(0.114)***	(0.123)***	(0.135)***	(0.032)	(0.065)
	[0.171]***	[0.118]***	[0.185]***	[0.141]***	[0.025]	[0.064]
Observations	102	102	102	102	102	102
R-squared	0.993	0.996	0.990	0.995	0.980	0.983

Notes: Standard errors in parentheses are clustered at the initial village level. Standard errors in brackets are clustered at the strata level, using wild bootstrap method (Cameron et al. 2008). *, **, ***: significant at 10%, 5%, and 1%, respectively. R-squared retrieved from regressions with standard errors clustered at the village level.

Table B.9: Horse Race (other channels), Part 1

Dep. var.	Log real consumption per adult equivalent			
	Sample A: 1991 - 2004		Sample B: 1991 - 2010	
	(1)	(2)	(3)	(4)
Panel A				
Refugee Index	0.092	0.092	0.205	0.206
	(0.029)***	(0.028)***	(0.062)***	(0.062)***
	[0.023]***	[0.023]***	[0.052]***	[0.047]***
Distance health Dispensary (log)	0.003	0.003	0.012	0.013
	(0.020)	(0.021)	(0.024)	(0.023)
	[0.016]	[0.018]	[0.022]	[0.021]
Panel B				
Refugee Index	0.084	0.087	0.192	0.196
	(0.026)***	(0.025)***	(0.057)***	(0.057)***
	[0.021]***	[0.022]***	[0.041]***	[0.035]***
Distance Hospital (log)	0.009	0.003	0.001	-0.000
	(0.033)	(0.034)	(0.024)	(0.025)
	[0.014]	[0.019]	[0.020]	[0.022]
Panel C				
Refugee Index	0.090	0.095	0.193	0.195
	(0.026)***	(0.026)***	(0.060)***	(0.059)***
	[0.019]***	[0.022]***	[0.047]***	[0.041]***
Distance health center (log)	-0.057	-0.059	0.000	-0.000
	(0.020)***	(0.021)***	(0.021)	(0.021)
	[0.019]***	[0.019]***	[0.014]	[0.014]
Panel D				
Refugee Index	0.088	0.089	0.192	0.194
	(0.026)***	(0.025)***	(0.057)***	(0.056)***
	[0.021]***	[0.024]***	[0.043]***	[0.039]***
Distance school (log)	0.027	0.026	0.007	0.006
	(0.031)	(0.033)	(0.028)	(0.029)
	[0.024]	[0.028]	[0.022]	[0.023]
Panel E				
Refugee Index	0.086	0.088	0.138	0.142
	(0.026)***	(0.025)***	(0.093)	(0.094)
	[0.022]***	[0.023]***	[0.096]	[0.093]
Number NGO (log)	-0.005	-0.005	0.314	0.300
	(0.034)	(0.033)	(0.226)	(0.235)
	[0.030]	[0.030]	[0.222]	[0.222]
Panel F				
Refugee Index	0.087	0.088	0.193	0.195
	(0.026)***	(0.025)***	(0.063)***	(0.062)***
	[0.021]***	[0.023]***	[0.050]***	[0.045]***
Number social services (log)	-0.014	-0.014	0.002	-0.000
	(0.046)	(0.047)	(0.066)	(0.065)
	[0.034]	[0.035]	[0.072]	[0.073]

Notes: Only the coefficient for $RI_{v,t}$ is reported. Standard errors in parentheses are clustered at the initial village level. Standard errors in brackets are clustered at the strata level, using wild bootstrap method (Cameron et al. 2008). *, **, ***: significant at 10%, 5%, and 1%, respectively. The same regressions are estimated in both samples.

Table B.10: Horse Race (other channels), Part 2

Dep. var.	Log real consumption per adult equivalent			
	Sample A: 1991 - 2004		Sample B: 1991 - 2010	
	(1)	(2)	(3)	(4)
Panel G				
Refugee Index	0.063	0.071	0.143	0.144
	(0.025)**	(0.025)***	(0.088)	(0.091)
	[0.018]***	[0.021]***	[0.088]	[0.084]*
Population (log)	0.112	0.132	0.081	0.081
	(0.056)*	(0.054)**	(0.034)**	(0.034)**
	[0.048]*	[0.045]**	[0.036]**	[0.037]**
Panel H				
Refugee Index	0.065	0.069	0.128	0.130
	(0.025)**	(0.025)***	(0.094)	(0.092)
	[0.018]***	[0.021]***	[0.116]	[0.106]
Population density (log)	-0.027	-0.027	-0.005	-0.006
	(0.021)	(0.021)	(0.016)	(0.018)
	[0.011]	[0.011]	[0.023]	[0.026]
Panel I				
Refugee Index	0.078	0.081	0.134	0.134
	(0.025)***	(0.025)***	(0.066)**	(0.066)**
	[0.021]***	[0.024]***	[0.066]**	[0.058]**
Openness with Rwanda	2.829	3.028	-0.479	-0.477
*Proximity to Rwanda	(1.245)**	(1.259)**	(0.118)***	(0.119)***
	[0.293]***	[0.399]***	[0.096]***	[0.099]***
Panel J				
Refugee Index	0.077	0.080	0.131	0.131
	(0.025)***	(0.025)***	(0.069)*	(0.069)*
	[0.021]***	[0.024]***	[0.070]*	[0.063]**
Openness with Burundi	0.085	0.092	-0.382	-0.383
*Proximity to Burundi	(0.041)**	(0.042)**	(0.106)***	(0.109)***
	[0.006]***	[0.012]***	[0.101]***	[0.097]***
Panel K				
Refugee Index	0.086	0.088	0.215	0.218
	(0.026)***	(0.025)***	(0.052)***	(0.051)***
	[0.021]***	[0.022]***	[0.035]***	[0.029]***
Openness with Uganda	-0.005	0.002	-0.844	-0.851
*Proximity to Uganda	(0.602)	(0.598)	(0.155)***	(0.155)***
	[0.065]	[0.061]	[0.125]***	[0.121]***
$Rain_{v,t}$	No	Yes	No	Yes
Time fixed effect	Yes	Yes	Yes	Yes
Strata time trends	Yes	Yes	Yes	Yes
Household fixed effects	Yes	Yes	Yes	Yes

Notes: Only the coefficient for $RI_{v,t}$ is reported. Standard errors in parentheses are clustered at the initial village level. Standard errors in brackets are clustered at the strata level, using wild bootstrap method (Cameron et al. 2008). *, **, ***: significant at 10%, 5%, and 1%, respectively. The same regressions are estimated in both samples.