Agricultural Returns and Conflict: Quasi-Experimental Evidence from a Policy Intervention Programme in Rwanda*

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Abstract

The 1994 genocide in Rwanda led to massive waves of population displacement. We use these movements of peoples as a quasi-natural experiment with which to measure the cost of civil conflict at a microeconomic level. We argue that forced displacement might result in human capital depreciation for the displaced. In order to control for potential skill spill-overs between returnees and stayers, we exploit a resettlement and land redistribution policy in post-conflict Rwanda which grouped returnees together into new resettlements. We use a difference-in-differences methodology to assess the asset effect of the programme on returnees' agricultural output, and hence achieve a more appopriate measure of the human capital cost of conflict. Evidence is suggestive that displacement resulted in a loss in human capital. However, higher returns to on-farm labour are found among the returnees in non-programme areas. Whereas the policy is found to have raised migrants' agricultural production by increasing access to land, evidence of lower positive skill spill-overs in policy areas suggests the existence of a 'ghetto effect'.

Introduction

An impressive body of literature has recently emerged on the topic of civil conflicts, their causes, and the linkage between peace and a country's socioeconomic performance. This research suggests the existence of a positive correlation between economic under-performance and the likelihood of civil strife (Collier, 1999), although the existence of a causal link is yet to be proven. Conflicts are costly, both in terms of physical and human capital. However, these costs are difficult to measure, and few studies have so far attempted to provide such cost estimates. Abadie and Gardeazabal (2003) propose a measure of the aggregate economic cost of the conflict the Basque Country's[1]; however, few studies have proposed to

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[1] Other studies on the economics of conflict Among studies using macroeconomic data, Bates (2000) uses a cross-sectional data on 46 African countries over the 1970-1995 to investigate the relationship between ethnicity and the occurrence of violence and protest. His results suggest that high levels of ethnic diversity in Africa partly account for the strong incidence of violent conflicts in the region. Uvin (1999) provides detailled description and comparison of the Burundian and Rwandan conflicts, whereas Ngaruko and Nkurunziza (2000) focus on the Burundian conflict.

measure the costs asscociated to civil conflicts at the microeconomic cost.[2] However, the impact of civil strife on agricultural productivity at the household level in LDCs has not been tackled in the literature, and this study is an attempt to fill this gap.

Providing an economic assessment of a post-conflict situation appears particularly relevant to measuring the likelihood of conflict resurgence. Using cross-sectional comparison, Collier (2003) finds supportive evidence of a 'conflict trap', whereby low aggregate levels of physical as well as human capital correlate to the likelihood of conflict resurgence. Some studies by Deininger (2003), Bigombe *et al.* (2000, also quoted in Ali, 2000), and Verwimp (2005) suggest that poverty in general, and the lack of economic prospects in particular, increase the likelihood of conflict.

In this study, we use household survey data to measure the human capital impact of conflict on subsistence agricultural households in rural Rwanda. We also assess a post-conflict resettlement and land redistribution policy that targeted returnees in post-war Rwanda. We argue that providing an assessment of the post-conflict economic status of agricultural subsistence households is particularly desirable in the Rwandan context. as they have access to credit, live mostly on auto-consumption or rely on informal markets to buy consumption goods and labour, and so have virtually no impact on consumption and credit figures, which are normally used in aggregate applied analyses of aggregated wealth. However, as subsistence households account for over 90 percent of the Rwandan population, long term macroeconomic performance is tied to their activities.

Rwanda has experienced sustained waves of ethnic violence since 1959, with a peak in the intensity of killings in 1994. These episodes of civil strife lead to massive population displacements, either within Rwandan borders, or to the border countries. As the Rwandan conflict has lasted for over thirty years, some of the displaced were left in camps for long periods of time. Often, only the children of refugees, born in camps, ever returned to Rwanda. Conflict-induced waves of internal displacement after 1994 resulted in a land and housing crisis in some areas, which threatened to trigger even more violent outbursts. The population density in Rwanda is one of the highest in the world, and, combined with the collapse of infrastructure resulting from the conflict, these return migrations put more strain on the demand for land and housing.

Choosing to use conflict-induced displacement to proxy the incidence of conflict at the household level is debatable, and is the motivation to this choice is three-fold. First, we argue that conflict-induced displacement is likely to be costly in terms of human capital (or level of agricultural know-how), and hence have a direct effect on a household's agricultural output upon return. Indeed, periods spent in camps may have some impact on the level of agricultural skills. No assumption is made at this stage as to whether returnees have incurred a positive or a negative shock in their level of human capital.[3] Moreover, it is conceivable that displacement status is associated to an exposure to a particularly high level of violence during the conflict. Such experience is likely to have, through sociological or psychological channels, an effect on a person's future socioeconomic outcomes. Moreover, displacement is likely to result in the "atomization" of the household, hence potentially reducing intergenerational agricultural skill transfers. A second motivation to using displacement as a proxy of conflict incidence is that, whereas most micro-level measures of the incidence of conflict are hard to come by in typical household survey data, migration is often reported, hence providing a straight-forward proxy for conflict incidence.[4] Finally, we argue that

[2] Contributions to the microeconomics of conflict are so far to be mostly found in the realm of theoretical economics (cf. Caselli and Coleman, 2002, and Bhavnani and Backer, 2000. In the empirical microeconomic litterature, Blattman (2006) provides an estimate of the long-term costs of military participation in Norhern Uganda at the individual level.

[3] Note that by skill depreciation does not necessarily imply that the displaced, while economically idle, are bound to lose their skills according to a certain depreciation. Indeed, it could also be that agricultural skills are (partly) soil or altitude dependent. Hence, upon return, when they do not return to their residence of origin, the change in the type of land could lower their observed level of know-how. However, this 'loss' of know-how might have been paired with gains of a different set of skills. In that sense the direction of the change in human capital as a result of displacement is, *a priori*, ambiguous.

[4] Postulating that the human capital cost of conflict as measured in this study can be retrieved by comparing those who migrated as a result of the conflict to those who stayed presupposes that those who stayed incurred a lower cost of conflict in terms of agricultural know-how. Should this assumption be invalid, then this study would, by understating the effect,

conflict-induced displacement in Rwanda consists of an exogenous shock, as the militia operated along a "war paths" within Rwanda[5], and all non-perpetrators within those villages were targeted. However, some other (related) selection issues arise from such assumption, which are addressed later in this study.

As mentioned above, this study aims at testing the hypothesis that returnees within agricultural subsistence households in Rwanda have, upon return, lower returns to agricultural inputs than their stayer counterparts, all else equal. However, the only data source available on Rwandan households containing enough information to address this question was collected in 2000/01. This presents a limitation to our analysis insofar as most returnees resettled in Rwanda between 1995 and 1998. Hence it is likely that, assuming that skill spill-over mechanisms were at play between returnees and stayers after the returnees resettled in a permanent residence, then using data from a 2000/01 household survey would not allow us to capture the real microeconomic human capital cost associated to the episodes of conflict-induced displacement in Rwanda. In order to retrieve a better measure of this cost, we exploit a resettlement policy, the imidugudu policy, put into place in 1997 in Rwanda, which consisted of grouping the returnees into isolated housing facilities on the border of more traditional settlements. We expect that, by reducing the integration of the returnee population living in policy areas with the rest of the community, the policy also prevented skill spill-over mechanisms to operate between stayers and returnees in policy areas.[6] Time differentials in the spatial implementation of the policy are exploited using a difference-in-differences method of estimation, comparing the outcomes of stayers and returnees in policy and non-policy areas, to retrieve a more accurate measure of the cost of displacement, and of the impact of the policy on the returnees.

The imidugudu, or villagisation, policy was implemented by the government to reduce land-related tensions through re-settlement and land allocation for post-1994 returnees. The traditional pattern of settlement in rural Rwanda is relatively dispersed, households having their house side-by-side with their field, away from the nearest house. Hence villages are often spread out over large distances. The chosen pattern of re-settlement was to agglomerate new housing facilities (or imidugudu) in relatively small areas on the border of existing Rwandan villages. Other forms of re-settlement were forbidden within programme villages, and all returnees to these areas were moved into imidugudu. They were then allocated parcels of land within larger fields, not necessarily positioned alongside their house.[7] At the time of data collection, not all villages benefited from the programme, hence dividing the population of returnees into a control and a treatment groups. As described above, we exploit this quasi-natural experiment to identify the impact of conflict-induced migration on agricultural productivity at the microeconomic level.

Land plays a critical role within Rwandan society, insofar as most households' survival depends on it. The legislation defining and enforcing access to land in Rwanda is complex and has undergone numerous changes over past decades.[8] However, in the late nineties, the custom of 'right to use' land prevailed in most villages of Rwanda, whereby anyone who wished was free to use any available parcel of land, as long as one cultivated it. As land became scarcer, the opportunities for returnees to rely on customary law to find a parcel of land declined. Although purchase of land in Rwanda was possible, the market for land is unregulated and illegal.

The impact of the villagisation policy on returnees' economic performance is two-fold. By re-allocating

provide more consistent estimates of the cost of conflict on agricultural human capital.

[5] Ali (2000) outlines the idea that ethnic wars differ from other types of wars in that they are rarely driven by economic motives (Sambanis, 2000, also quoted in Ali, 2000). This goes to provide some credential to the one of our main working assumptions, which is that, during the Rwandan genocide, the militia did not select the villages they attacked on the basis of their economic performance. However, that economic discontent was among the factors driving individuals into perpetrating the genocide in Rwanda (Verwimp, 2005) is still perfectly compatible with this assumption.

[6] The idea of skill-spillovers being influenced by patterns of settlement is also linked to the literature on technical change and skill spill-overs in rural areas (cf. Besley and Case, 1993; Foster and Rosenzweig, 2000). (Besley

[7] These 'fields' could either be some former forest areas or grazing fields converted into arable land. Not much however is know about the practical terms of the policy implementation.

[8] Explaining and summarising all these changes is not the aim of this study. For a comprehensive report on the topic, see Andre (1998).

parcels of arable land to returnees in programme areas, the policy had an 'asset effect' on returnees' agricultural production. Indeed, returnees to non-policy areas appropriated the land not occupied by the stayers. Isolated returning households were unlikely to convert forest or grazing land into arable land themselves, since there are large sunk costs associated to such endeavour. Hence there was no guarantee for returnees in non-policy areas to find any available land. In contrast, as the government arranged for returnees living in imidugudu to be allocated some converted land, they are, at least initially, likely to produce more on average than their counterparts in non-policy areas, other things equal. However, that does not take into account potential differentials in the quality of land cultivated within policy and non-policy areas, and by stayers and returnees. To the best of our knowledge, no data on the quality of land is available for Rwanda that could be exploited in this framework. Moreover, we have no reason to believe that the displaced in non-policy areas were allocated some higher quality land relative to their counterparts in policy areas. The second effect on the policy on returnees is, as mentioned earlier, that the pattern of resettlement might have lowered the amount of skill spill-overs across stayers and returnees. This is referred to as the human capital effect of the policy.

In order to separate input and human capital effects, we use an agricultural household input-output framework. We rely on a basic agricultural household model (Singh, Squire & Strauss 1986) to identify the effect of the policy on the treated 'other things equal'. Relying on the assumption that skill-spillovers across stayers and returnees are lower in policy areas than in the rest of Rwanda, we exploit time differentials in the implementation of the policy across areas in a difference-in-differences analysis to measure the human capital impact of migration. In this framework, the impact of migration on agricultural productivity is measured by interacting all production inputs with policy and migration status dummies. Quantile treatment effects are also measured, so as to capture potential heterogeneous treatment effects at different points of the agricultural output distribution. The endogenous nature of some of the production inputs is dealt with using instrumental variable estimation. We use the Chernozhukov and Hansen (2005a, 2005b) method to compute IV estimation in the quantile regressions.

The first section gives background historical information on Rwanda, the villagisation policy. The second section describes a general agricultural household model, and a theoretical discussion on its potential application in the study of Rwandan households. The data and the variables of interest are introduced in a third section. The empirical identification strategy is outlined in a fourth section. A fifth section presents the econometric estimation results.

1 The Rwandan case

1.1 The conflict

Rwanda is one of the poorest countries in sub-Saharan Africa, with a GNI of $230 in 2003 (World Bank). In 2001, Rwanda ranked 152 out of the 162 countries for which the United Nations computed a Human Development Index (HDI), with a life expectancy at birth of 39.9 years, and an adult literacy rate of 65.8 percent. Rwanda has experienced sustained waves of ethnic violence since independence in 1959, culminating in 1994, when the extremist Hutu militia, with the help of the national military forces, committed a genocide against the Tutsi ethnic minority, and moderate Hutus.[9] These successive episodes of civil strife lead to massive population displacements, and much of the population was stranded in refugee camps, either within Rwandan borders, or in the border countries of Burundi, the Democratic Republic of Congo (DRC), Tanzania, and Uganda.[10] After the genocide, the Rwandan Patriotic Front (RPF), consisting principally of Tutsis who had been exiled in Uganda in the late 1950s or early 1960s, formed a transitional government. Fearing retaliation from the new government, most of the genocide

[9] For an outline of the Rwandan political and historical background, as well as a detailed account of those episodes of violence (comparatively analysed to the Burundian context), see Uvin (1999).

[10] Some estimations suggest that half of the population was thereby displaced (Bucagu 2000).

perpetrators, together with some Hutus concerned that the RPF would indiscriminately strike against their ethnic group, fled into refugee camps on the DRC border with Rwanda. In 1996 the RPF, still in power in Rwanda, lead an assault on these camps, encouraging Tutsi and Hutu refugees to return to Rwanda. The genocide perpetrators seized the opportunity to return to Rwanda in the mass of refugees. Upon return, they committed further killings and persecutions in the Northwest of the country (particularly in the prefecture of Ruhengeri). In turn, these assaults caused the national army to deploy in these areas, leading to renewed displacements. This resulted in further deterioration of the housing and land access crisis

Given these waves of displacement, two types of returnees are commonly defined: the Old Case Load (OCL) – refugees who fled before 1994 – and those who fled as a result of the 1994 genocide and the 1996 insurrection, the New Case Load (NCL). Finding reliable information on the number of OCL refugees is virtually impossible, since no precise records were held. A working-paper by the Rwandan National Office for Population (ONAPO) reports 300,000 (Bucagu, 2000). The same source estimates that over one million pre-1994 refugees returned to Rwanda after 1994, including children and grand-children of the 1959 refugees.

1.2 The Policy

In 1993, the Arusha Agreements specified that migrants returning to Rwanda after 10 or more years of exile were not entitled to claim their property back. Whereas this regulation left no room for legal claims over land on the OCL refugees' part, it also offered no solution to the waves of migration triggered by the 1994 genocide. Hence, as genocide survivors and perpetrators came back to Rwanda, they often found their land and houses had been occupied. Moreover, most OCL refugees became homeless and land-less, living in plastic shelters on the roads of Rwanda. This created tensions, and in 1996, the government became concerned that a second wave of ethnic violence would emerge.

In 1997, the government implemented a land redistribution and housing relocation programme, the villagisation policy, or imidugudu policy. The initial coverage of the policy was not total, and, in 2001, at time of survey collection, both programme and non-programme areas were observed (cf. Table 5). Although it was expected that, in the longer run, the programme would be extended to the whole of the Rwandan population, it initially only offered a solution to genocide survivors and OCL refugees who resettled within the pilot areas of the programme after 1994. The programme was partly motivated by security issues, as the government believed the size of the new settlements would deter rebel groups from attacking. Returnees who benefited from the programme were not generally assigned to their area of origin. There is no evidence that any sort of selection took place in the sorting of returnees across imidugudu and non-policy villages (RISD, 1999). As a check, we dedicate part of our results section to testing for selection of villages into the policy on the basis of observables village characteristics.

This policy triggered a controversy over the respect of human rights in rural Rwanda. Human Rights Watch questions the legitimacy of the villagisation policy, arguing that most displacements induced by the implementation of the policy were made against civilians' wishes, and even, in some cases, with the use of force (Moussalli, 2001). However, some papers presented in 1999 during the Rwandan Initiative for Sustainable Development argue that the implementation of the villagisation policy was vital to Rwandan socioeconomic welfare (Palmer, 1999). As mentioned in the introductory section above, this policy aimed to group returnees in agglomerated settlements within existing villages, endowing them with parcels of land situated outside the settlement. These parcels of land consisted in most cases of former forest areas, or grazing land, converted into arable land, although no authoritative documentation is available on this topic. Whether the quality of the land allocated to imidugudu inhabitants is comparable to that of stayers is an issue of concern, and is addressed later in the study.

The expected socioeconomic outcome of the policy is two-fold. Firstly, the policy is expected to raise migrants' agricultural output through an asset effect, that is, other things equal, by increasing their access to land. Secondly, by grouping imidugudu inhabitants' houses and parcels of land together, the government hoped to encourage a positive productivity effect, through agglomeration externalities. On the one hand, this pattern of settlement is expected to facilitate the use of more mechanised production techniques, and to improve productivity through scale economies. A more clustered pattern of settlement, when coupled with access to public goods, could be productivity-enhancing both by facilitating access to this infrastructure, and by making the use of tools more profitable when shared by several households.[11] Eventually the authorities hoped to encourage the formation of labour markets within these enlarged versions of traditional villages, by promoting hired labour on larger farms or non-agricultural activities rather than family workers on small plots.[12]

Some of the potential drawbacks of the villagisation policy as it was implemented in Rwanda are the following. Increasing the distance between producers' house and their land parcels could lower agricultural productivity, as farmers might be reluctant to cultivate areas that could not be watched closely, and are thus vulnerable to theft or sabotage (Andre, 1998).[13] Secondly, as in the early years of policy only the post-1994 returnees benefited from the programme, grouping this part of the population in agglomerated settlements on the border of existing villages could create a 'ghetto effect', hence offsetting any positive skill spill-over effect across groups.[14] The majority of imidugudu settlers are OCL returnees, and hence likely to be mainly Tutsis. Should ethnic tensions be ongoing, this could condition their integration with stayers (mainly Hutus), and security may become more precarious. Moreover, skill spill-overs across returnees and stayers may not occur at all. The threat of renewed ethnic tensions could in turn harm agricultural production, by inducing households to under-cultivate their land.[15]

2 The data

2.1 General Description

The data used in this analysis is the 2000-01 National Rwandan Household Living Conditions Survey (HLCS), which surveyed over 6400 households across all twelve Rwandan prefectures. The questionnaire consists of 12 sections, some of which are at the individual level, and some at the household level, providing a comprehensive set of variables regarding employment, education, migration and agricultural production and consumption. However, the questionnaire contains no record of ethnicity.

Both rural and urban households were surveyed in all prefectures: 5271 households were visited in rural areas, and 1149 in urban areas. However, given that we are interested in agricultural productivity at the household level, we exclude both urban households and rural households holding a non-agricultural

[11] Such provision of public service could be the access to an agricultural extension services (AES). An agricultural extension service is a cooperative in charge of improving cultivation techniques, mainly by providing seeds and tools, or by helping farmers with parcel management. Those infrastructures do not have full coverage in Rwanda, but are located both in policy and non-policy areas. We do control for such infrastructure by adding a dummy for access to AES within the village in all regressions. In the subsequent analysis, the associated coefficients are only reported where they pass the test of individual significance at conventional levels.

[12] This method of agglomeration was also supposed to induce more skill spill-overs across OCL, NCL, and stayers, which would in turn help reduce economic inequalities within and across groups. However, that skill spill-overs are expected to occur across stayers and returnees presupposes that stayers would also be relocated within imidugudu. That was not the case in the early years of the policy, although the government aim at extending the policy to the whole population in the future.

[13] We control for distance to field in all subsequent regressions, but fail to reject the null of individual insignificance for that covariate (coefficient not reported).

[14] Our discussion of the potential 'ghetto effects' induced by the imidugudu settlements is, in spirit, very similar to that proposed by Cutler and Glaeser (1997) on the potential channels through which segregation might affect the socioeconomic outcomes of Blacks in the US .

[15] Indeed, when introducing uncertainty on yield, or unenforced property rights, in a simple producer programme, one can easily derive that agents would invest less in their production and save more than in a world without risk, other things equal.

business. This reduces our sample, after removing outliers[16], to some 4900 households, spread out evenly[17] across the 11 rural Rwandan prefectures. The survey design is clustered at the village level, and 440 rural villages were visited across 11 prefectures.[18]

The sampling procedure was designed by the World Bank, and the corresponding report (Scott, 1997) indicates that households were selected in the following way. First, the survey collectors visited each primary sampling unit (*i.e.* enumeration area, or village), and undertook a global census of the population in each of these units.[19] The households surveyed were then randomly drawn from these lists. This survey is the most comprehensive on Rwanda to date containing detailed information on agricultural production at the household level.

2.2 Variables of Interest

2.2.1 The agricultural production variable

Rwandan households are mostly multiple-crop producers, which is consistent with the fact that their on-farm activity consists of subsistence agriculture. Hence, restricting the analysis to only one type of crop would yield selection issues. The issue of how to measure household agricultural production is much discussed in the literature. Generally, even if the quantities produced are observed for each type of crop, estimating the total value of output is made difficult by the absence of a price index for each commodity at a reasonably disaggregate level. In the data set, an estimation by the household of the value of its annual harvest of each crop is reported.[20] As is often the case in this type of survey, prices are not observed at the village level. Relying on the prices declared by households in survey data is not without risk, as measurement error is reputedly high.[21] However, as it is the most straightforward way to approximate a multi-crop agricultural production variable, we choose to employ this strategy. Hence, we need to address the issue of what level of aggregation should be used to build a price for each crop. Averaging values across larger number of households allows for lower levels of error, if one believes the measurement error component to be a well-behaved white noise, and therefore to cancel out when summed up over 'large' numbers of observations.[22] Deaton (1997) argues that prices are likely to be correlated at the village level, due to neighbourhood effects, such as homogeneity in land quality. Hence, choosing the village level as the unit of aggregation would seem a reasonable compromise. However, comparing households' price estimation at the village level, striking disparities are observed in commodity prices within a village, which seems unlikely to reflect the real level of prices.[23] Comparing values within prefectures reduces the dispersion and, where there are few observations within a village, potentially reduces the incidence of measurement error.[24] Hence we opt for a prefecture level of aggregation to build the prices used in measuring agricultural output. We use the median value of the reported price (for each crop) at the

[16] This concerns all observations that declared aberrant values for prices of crops, produced quantities with respect to the available size of land declared, and amounts transferred or received. In total, we trim out around 20 households.

[17] Indeed, each prefecture represents between 8.08% and 9.56% of the sample.

[18] The entity we refer to as "village" is the enumeration area of the data, which refers to actual villages in rural areas.

[19] There is however no indication as to whether migrants living in precarious shelters are included in the sample or not.

[20] The issue of a common unit of measure is also a challenge. In this survey, the price reported was *per* unit produced of the corresponding crop, and each household could select from a range of units of measure. We however find that there seems to be a consensus in the unit used by crop type, as the amount of variation in the choice of unit within-crop is very low. Hence we choose to ignore this issue and consider that all households within a same municipality refer to the same unit of measure.

[21] We must note, though trivially, that this argument is based on pure conjecture, as an inherent feature of measurement error is that its extent cannot, in most cases, be assessed.

[22] In the case of rare crops, the number of price observations is very low; hence we base our analysis on the 30 most produced crops only.

[23] Potential heterogeneity in the quality of agricultural commodities across prefectures is not addressed.

[24] For instance, for the most produced crop, sorghum, the standard deviation is reduced by 10% when using prefecture level relative to village level aggregation.

muncipality level.[25]

More sophisticated approaches have been suggested to indirectly estimate prices on agricultural markets. For instance, shadow price estimation, which relies on a profit maximisation framework to induce the real price level in equilibrium. Where wages are observed, the nominative price of the commodity is then straightforwardly derived by solving a constrained maximisation problem. However, this technique is more appropriate in a single-crop framework. Moreover the outside option for the representative household member's labour supply decision is virtually non-existent in most villages and prefectures, as a very low proportion of households are observed in paid employment. A direct implication being that wages are typically unobserved at a village or even prefecture level, rendering the identification of nominative crop prices at a sufficiently disaggregate level unlikely.

2.2.2 Policy variable

The information contained within the data on the villagisation policy essentially consists of an entry in the communal level questionnaire: *"Has the commune built imidugudu since 1994, and, if yes, how many, and which were the two main sources of financing?"*. However, the information contained about the number of imidugudu in the village is often not exploitable, as 40% of the answers were 'do not know'. Moreover, the number of houses each of these imidugudu contains is unobserved. The information enclosed on which body funded the construction work is not detailed enough to allow precise analysis, and is ignored in the subsequent analysis.[26] Consequently, we rely on a single village-level dummy variable for the presence of imidugudu to assess the policy at the household level (Table 1). The impact of this lack of precision on our estimates is ambiguous. One the one hand, it could underestimate the wealth effect of the policy on returnees, since among the post-1994 returnees not included in the programme, most are homeless, and few could retrieve their former land and house (Andre, 1998, and RISD, 1999). Although we do not observe any homeless households in the data, it seems reasonable to assume nonetheless that this fraction of returnees living in policy areas, but not included in the programme, are on average worse off than those benefiting from the programme.[27] Moreover, given the lack of formal property rights in Rwanda, those who found themselves homeless upon return are likely to have found a housing solution within months. However, that they have access to arable land is less obvious. Under this assumption, the asset effect of the policy is likely to be downward biased. On the other hand, the impact of this imprecise measure on the human capital impact of the policy could be that it underestimates the potential negative 'ghetto effect' of the imidugudu policy on returnees' productivity. The difference-in-differences framework we use allows us to estimate the treatment effect that corresponds to an 'intent-to-treat' effect, as all post-1994 returnees living in villages where imidugudu were built could expect to be included in the programme.[28]

2.2.3 Conflict-induced migration variable

One section of the survey has information, for all household members over 15, on migration status, place of previous residence, the date of arrival in the current residence, the period of time spent in the previous residence and occupation while there, and the main reason for migrating to the current residence.[29]

[25] As outlined by Capeau and Dercon (2006), using the mean value of the reported price would tend to give too much weight to outliers. Opting for the median hence seems a more attractive option. Moreover, this allows us to partly overcome the issue of heterogeneity in the unit of measure, as these observations are likely to be treated as outliers (cf. 4^{th} footnote above).

[26] The reported answers to this question where (percentage quoted as first main source/percentage quoted as second main source) : voluntary contributions (64%/32%), association of nationals (5%/12%), government (1%/7%), NGOs/International organisations (30%/49%).

[27] A descriptive analysis of access to land by migration and policy status tends to confirm this assumption in terms of access to land (Kondylis, 2005).

[28] This is, in spirit, similar to the identification strategy used by Edin *et al.* (2003) and Field (2002).

[29] Migration status refers here to the answer to the question: "Have you ever lived in another residence for more than one month?".

Only information related to the most recent migration was surveyed. Moreover, only the date of return and the reason for return are surveyed. No information was recorded with regard to out-migrations, *i.e.* potentially conflict-induced displacements. One implication of this is that we do not observe the returnees' municipality of residence before displacement occurred. The clustered pattern of departures in 1994 and in the 1970s suggests that most migrations that occurred after 1994 correspond to returns from conflict-induced exiles (Figure 1).[30] As we can identify the date of out-migration, we could theoretically discriminate between OCL and NCL refugees. However, most OCL refugees who returned to Rwanda after 1994 were the children or grandchildren of those who fled ethnic persecutions in the late fifties and early sixties. Hence, it is not clear whether the years spent abroad are precise enough to infer what type of returnees each household member belongs to. Around 60.7% of migrants who left in 1994 spent between 0 and 5 years in their previous residence, which would indicate they belong to the post 1994 genocide refugees. The frequency of out-migration by year across policy and non-policy areas (Figure 1) provides results in line with historical facts, as it shows two peaks in the distribution, one in 1994, and one around 1975. The peak around the mid-seventies suggests that the post-1994 OCL returnees were mainly children of those who fled Rwanda in 1959, and who were often born in exile. Hence it simply indicates that the mode of the second generation year of birth is 1975. The high frequency of out-migration in 1994 is in line with the idea that, as the genocide started, massive and sudden displacements of population occurred. The fact that the distributions for the individuals living in policy and non-policy areas nearly overlap indicates that the OCL and NCL are in similar proportions across migrants living in imidugudu, and those living in non-policy areas. Hence, if there was selectivity in the access to shelter and to land within imidugudu, then returnees were not sorted according to their out-migration date.

Refugee camps were put together to accommodate people fleeing the waves of inter-ethnic violence between 1959 and 1994. In a given camp, refugees were usually all from the same wave of conflict-induced migration. We find that the pattern of returns in time is smooth within the same group of migrants, OCL or NCL, and across policy and non-policy areas, which tends to corroborate this assumption (see Kondylis, 2005, for the reported results). Kondylis (2005) also finds that the patterns of returns in time is analogous across policy and non-policy areas. We further investigate the issue of (self-) selection into migration and policy areas on the basis of observable characteristics further in the next sub-section.

3 Estimation Strategy

3.1 Selection Issues

A main concern in assessing the impact of the villagisation programme on the population of returnees in Rwanda is that certain households may have self-selected into migration or programme areas on the basis of their characteristics. Indeed, such sorting would make returnees living in non-policy areas and stayers in all areas unsuitable to serve as control groups. It is therefore of interest to investigate the distribution of observable exogenous characteristics across groups, so as to check for patterns of selection into policy areas on the basis of observables.

3.1.1 Selection into migration

Given the nature of the Rwandan conflict, ethnicity is obviously an element at stake in the selection process whereby specific households were targeted by the militia, and hence fled Rwanda. However, we cannot account for the role of ethnicity in this study, and assume that migration occurred randomly.[31]

[30] The date of departure is inputted as the reported date of arrival in current residence *minus* the time (in years) spent in the previous residence.

[31] Indeed, ethnicity is not included in the data set, and hence cannot be controlled for, and neither can any form of ethnic ideology.

Especially, we contend that the nature of the conflict did not leave much room for selection into displacement, as, when a militia group attacked a given village, the villagers' options were to flee or to be killed. Obviously this implies that selection might have been at play between death and migration, but this is not an issue we can empirically address with the existing data. Moreover, we argue that, as genocide perpetrators targeted all Tutsis and moderate Hutus, they did so regardless of their socioeconomic background. This case is supported by Uvin (1999), who outlines how the victims of the genocide just happened to be *"(...) in the wrong place at the wrong time (...)"*. Nevertheless Uvin acknowledges that among other motives, economic opportunism may have driven the perpetrators into committing killings. However, he argues that this is unlikely to have been a main source of motivation or played a large role in the selection of the victims. Hence, no particular selection into migration should have occurred otherwise than through ethnic and ideological background. Nevertheless, should ethnicity and ideological belief be systematically related to some form of unobserved heterogeneity which affects expected output, then our estimates would be inappropriate.

Nevertheless, we propose, we propose, as a check, to investigate the potential determinants of individual selection into migration. We are constrained to limit our analysis to selection on the basis of observable characteristics, as we do not have access to a longitudinal data source. Our test consists of empirically examining potential determinants of migration on aggregate and across policy status in a regression framework. Table 3 presents the results of logistic regression models at the household level, taking migration status as the dependent variable with a 1 if the individual is a returnee and 0 otherwise.[32] The results on the overall sample (column 1) suggest that households with younger heads, in partnership, with fewer children, and with a larger proportion of members born abroad are more likely to be post-1994 returnees. These results seem to be in line with the idea that healthier (and hence younger) households were more likely to escape the persecutions. These characteristics should be simultaneously included in subsequent estimation, as they may also affect agricultural output. Comparing the estimated coefficients across policy and non-policy areas in columns 2 and 3 highlights which characteristics might explain any sorting of returnees across policy and non-policy areas. The average age of the household is negative and significant in non-policy areas, and insignificant in non-policy areas. However, we cannot reject the null hypothesis of parameter constancy across samples at the 5% level. Similar observations are made for the coefficients on the proportion of children within the household and the dummy for whether the head is in a partnership. However, the coefficients on the proportion of household members born in Rwanda is significant in both samples, and parameter constancy cannot be accepted. Moreover, this coefficient increases when restricting the sample to policy areas, suggesting that the proportion of OCL migrants is higher in these areas. A dummy for OCL status is included in all regression models in an attempt to control for this potential source of systematic heterogeneity.[33]

Despite the existence of some disparities in the coefficients across samples, the general lack of statistical significance bolsters the use of returnees in non-policy areas as the control group. However, the fact that different types of returnees are present in unequal proportions across areas is of concern, as it suggests the possible existence of unobserved heterogeneity across returnees in policy and non-policy areas.

3.1.2 Selection into return

By identifying returnees as those who fled in or before 1994, and returned after 1994, we make sure that the majority of moves will be conflict, and not job related, although we cannot identify labour market related moves in the data. However, and despite the fact that labour markets do not operate efficiently in

[32] Standard errors are clustered at village level, and prefecture fixed-effects are included. The same regression model is estimated applying different restrictions to the sample: in column 1, on the whole sample; in column 2, on the sample restricted to policy areas; in column 3, on the sample restricted to non-policy areas.

[33] By taking the average effect over all types of returnees, we avoid the question of sorting of those displaced returning into their residence prior conflict, and the question of heterogeneity across NCL and OCL. This is, in spirit, similar to the identification strategy used by Cutler and Glaeser (1997) to measure the average effect of racial segregation on individual time use and other socioeconomic outcome variables in the US.

Rwanda, some noise may persist. In a standard New Economics of Labour Migration (NELM) framework, the individual migration decision is made at the household level, and determined by the implicit on-farm wage compared to the expected outside option wage. However, this presupposes three things. First, that migrants, while stranded in camps, manage to get enough information on the situation in Rwanda to form expectations of their earnings and that labour markets are reasonably active in urban areas, although a non-zero level of urban unemployment would only impact on the migration decision by lowering the expected outside option wage. Second, that agents are free to move, which is not the case in Rwanda, where rural to urban migrations are highly controlled and regulated by the authorities. Indeed, moving to an urban area without holding a job offer *ex ante* is prohibited (Bucagu, 2000). Hence, labour market related migration in Rwanda is small and mainly restricted to public service jobs or highly qualified positions, which are the only ones advertised at a national level. Hence, we consider return migrations as exogenous in the Rwandan case. However, as we only observe those conflict-induced migrants who returned to Rwanda, we have no means of testing this assumption empirically.

3.1.3 Selection into the programme

That villages may have been selected into the programme on the basis either of village-level or villagers' characteristics is of concern. We therefore estimate logistic regression models of policy status at the village level, conditioning on household characteristics, and restricting the sample to stayers and pre-1997 returnees.[34] The results are presented in Table 4, columns 2 and 3. The positive and significant coefficient on the proportion of children in the sample of stayers suggest that the programme particularly targeted areas with high stayers' fertility. As the fertility decision might be positively correlated to output, this may reflect that high-yield areas are more likely to be included into the programme. However, we do not observe pre-policy levels of output, and thus cannot test this assumption. Should this be verified, our estimates of the policy effect on output would be upward biased. Should fertility and output be negatively correlated, then, using the same reasoning, our estimates of the policy effect on output would be downward biased.

The positive and significant coefficient on the dummy for a female household head in columns 1 and 3 suggests that areas with high proportions of pre-1997 returning widows were more likely to be included in the programme.[35] The impact on our measure of the impact of villagisation on agricultural productivity is unclear, and we control for widow status in all subsequent estimations. Any systematic heterogeneity across policy and non-policy areas should be captured by the policy dummy and its interaction terms, hence leaving the difference-in-differences estimates uncontaminated.

That the dummy for OCL returnees has a positive and significant coefficient in the whole sample and in the sample of stayers and pre-1997 returnees suggests that villages with a high proportion of OCL returnees were more likely to be included in the imidugudu programme. Hence a dummy for OCL returnees is included in all subsequent regression models.[36]

3.1.4 Selection of returnees into programme areas

Another issue of concern is that returning households may have self-selected into migration and programme villages. We compute sample means of households' characteristics (Table 2), and we estimate logit regression models of policy status at the village level, restricting the sample respectively to post-1997 returnees, and to all returnees (Table 4, columns 4 and 5).

[34] An ideal test on this selection issue would be to include some pre-policy land allocation (and quality) indicators at the village level. However, we do not hold such information. Moreover, as the policy potentially had a direct impact on land allocation for all groups, controlling for land allocation in 2001 would be inappropriate.

[35] The correlation coefficient between having a woman head and having a widowed head is 0.78.

[36] Splitting the sample between OCL and NCL was also attempted, although a formal test lead us to fail to reject the Null of parameter constancy across samples. The dummy for OCL is only reported in the subsequent regression tables when its explanatory power is individually significant.

Heads of households within the sample of returnees are on average 2 years older in programme areas than in the rest of Rwanda (Table 2). Moreover, age of the head bears a positive and significant coefficient in the sample of all returnees (Table 4, col. 5), suggesting that being 1 year older increases the probability for returnees to resettle in policy areas by 3.6%, all else equal. We control for age in all subsequent regressions. However, there is little difference in the average age of all household members. Finding a similar proportion of children across groups (Table 2, 4^{th} row) is reassuring, as the adult-to-children ratio can then be used as a measure of the incidence of the conflict. However household size may have been part of the determinants of any self-selection process. Although agricultural output and fertility may be linked, any effect is assumed to be negligible over the one-year horizon covered by the data, and household size and the proportion of children are controlled for in all subsequent regression models. However, should programme status have influenced the household fertility decision, our estimates would be inappropriate.[37]

Looking at the average number of years spent in previous residence by returnees (Table 2, 5^{th} row), it can be seen that heads living in programme areas spent on average more time away than their non-policy area counterparts. This is consistent with the fact that the dummy for OCL is significant in the village level selection equations (Table 4, col. 1 & 3). Similarly, significant disparities in the proportion of adults born in border countries are found across policy and non-policy areas in the sample of returnees. The proportion of married heads of household within the sample of returnees is roughly equivalent across programme areas, both for returnees and stayers.

The amount of schooling received by children is likely to be endogenous with respect to agricultural production, but less likely to have been at stake in the selection process of returnees' into programme areas. Hence this characteristic is not included in the model. However, the educational attainment of those involved in the production process (producers and land holders) over the year is less likely to be endogenous to output, as the average age for on-farm producers is 21.3 years, with a minimum value of 14, and the average age for holders is 43 years. However, as conflict and displacement might have delayed schooling, adult educational attainment is likely to be endogenously determined by migration status. Difference-in-differences estimation controlling for educational attainment should, by differencing across returnees in policy areas and returnees in non-policy areas, capture any effect arising from that in the returnee dummy and its interactions with the educational attainment variables.

We find no disparities in the proportion of the population non-employed in the previous residence across groups of returnees. This is reassuring in terms of potential self-selection, either on the basis of observable or unobservable characteristics. That employment status while in exile be comparable across returnees living in policy and non-policy areas is critical. Indeed, should sub-groups have experienced differential non-employment rates then skill-depreciation, economic, and social pressure associated with conflict induced migration would be likely to differ widely across groups.

We also observe that post-1997 returnees who returned to a village where water supply was available before 1994 are 18.4% less likely to benefit from the policy than those who returned to villages without water supply before 1994. However sample means of the provision water supply in the 2001 sample suggest that these differences did not persist, and hence should not influence output.

3.2 Conceptual framework

Our strategy to measure returns to agricultural inputs is inspired by the literature on agricultural household models. Those models are used in development microeconomics as an alternative to the standard consumer and producer models, especially in subsistence economies. For instance, postulating that individuals maximise utility with respect to labour supply and consumption is not appealing in a context where formal labour markets are quasi-inexistent, and where households are mainly subsistence agri-

[37] This does not take into account the possibility of agricultural yield being autocorrelated overtime, which would also introduce endogeneity in the regression model. The data however does not allow to control for this.

cultural households, *i.e.* simultaneously producing and consuming. Instead, modelling microeconomic behaviour in this framework amounts to solving a household utility maximisation problem simultaneously with its profit maximisation programme.

Consider a multiple-crop environment, and let households maximise their utility as follows:

$$\begin{cases} Max\ U = U(X_1, X_2, ..., X_n, X_L) \\ \\ s.t.\ Y = \sum_{i=1}^{L} p_i X_i \end{cases} \tag{1}$$

with n consumption goods X_i (own and others), X_L denoting leisure, and $\{p_i\}_{i=1}^{L}$ the corresponding prices. Total income is given by:

$$Y = p_L T + \sum_{l=1}^{M} q_l Q_l - \sum_{i=1}^{I} q_i V_i - p_L L + E \tag{2}$$

where T is the household's total time endowment; Q_l is output of each crop produced $l = \{1, \ldots, M\}$, with corresponding prices $\{q_l\}_{l=1}^{M}$; V_i non-labour variable production inputs, $i = \{1, ..., I\}$, and corresponding prices $\{qi\}_{i=1}^{I}$; L labour demand; E is exogenous income.

Levels of know-how at the household level are assumed to be captured by an implicit multi-crop agricultural production function, given by:

$$G(Q_1, \ldots, Q_M, V, L, K_1, \ldots, K_O) = 0 \tag{3}$$

where $\{K_s\}_{s=1}^{O}$ are the fixed production inputs. In this framework, testing for different levels of know-how across groups can be done by allowing $G(.)$ to vary across groups. This is done empirically by interacting all terms in $G(.)$ with policy and migration status dummies and using them as covariates in a reduced form equation of household agricultural production, controlling for observable exogenous household characteristics. The condition for identification described below relies mainly on the standard assumptions of the agricultural household model theory when there is excess labour.

The standard model (Singh *et al.*, 1986) assumes that fixed inputs are land, physical capital such as tools, family labour, and any other inputs which can be considered inelastic to the level of output over the period of analysis. The data exhibits values that are in line with this assumption. For example the sample proportion of households who declared having bought a parcel of land during the year is only 4%, and the total purchase of capital inputs over the year represents only 21% of the overall mechanical tool ownership. Moreover, tools ownership consists of 'incompressible' inputs for 99.2% of the sample, and their consumption is assumed to be fairly inelastic to agricultural output.[38] We do not control for the value of physical capital, *i.e.* tools, due to measurement issues.[39] Nevertheless, the sample means suggest no significant difference across groups (values not reported). We also do not control for exogenous household income, that is, off-farm income, as this is potentially endogenous to agricultural output over the period of analysis.

Variable inputs are sensitive to changes in the level of production over the sample period. Such inputs are mainly hired labour and purchase of seeds. Estimation of the model will require dealing with the endogeneity issue arising from these variable inputs. Annual expenditure on hired labour in the data set has a median value of 0, and a mean of US$3 concentrated on 25% of the sample. The median annual value for the part of the sample with non-zero consumption is US$4. Hence, in the absence of a valid

[38] So called incompressible inputs are as follows: hoe, machete, hatchet, sickle, pick, spade, rake.

[39] We can retrieve the value of each tool owned by the household, and hence the value of the total physical capital owned. However, the prices are noisy and do not take into account capital depreciation, hence potentially contaminating our estimates with measurement error.

instrument hired labour is not taken into account in this analysis.[40] Seeds however are included in the specification, as over 50% of surveyed households reported having bought some over the period. The IV strategy is pursued in the next subsection.

Households are assumed to be price-takers in the market for all goods (own and other), as well as in the on-farm and off-farm labour markets. In formula, the model we hope to estimate is:

$$\begin{cases} Q = \alpha_1 + \beta_1' X_1 + \delta_1' V + \gamma_1'(T - L) + \lambda' K + \varepsilon_1 \\ V = \alpha_2 + \beta_2' X_2 + \delta_2' Z + \mu Q + \varepsilon_2 \end{cases} \tag{4}$$

with: $Q = \sum_{l=1}^{M} q_l Q_l$, multiple-crop household agricultural output, X_1 are exogenous household characteristics, V the only variable input considered, seeds, $(T-L)$ family on-farm labour supply, K other fixed input (land), X_2 some exogenous variables, and Z is a vector of instruments. Subject to identification, interacting all independent variables with dummies for migration status, policy status, and an interaction term would then capture any differentials in productivity, other things equal, across returnees and stayers in policy and non-policy areas.

3.3 Instrumental Variable strategy

3.3.1 Exclusion Restriction

The instrumental variable used to estimate (**??**) is the level of seed consumption aggregated at the village level. The use of the aggregate level of a variable as an instrument at the individual level is discussed in the literature, and can be justified on the grounds that there may be some spill-over effects from village seed consumption to household level demand for seeds (Moffitt, 1996, and Currie and Cole, 1993), although it should be uncorrelated with the individual error term. In the case of household seed consumption, it is likely that some neighbourhood effects operate at the village level. If the consumption of seeds in a village is low, supply is also likely to be low, insofar as seed sellers would not find it profitable to visit the local market. Similarly, a high demand at the village level would imply better chances of finding seeds on the market.[41] Nevertheless, the aggregate demand for seeds is unlikely to be correlated with the individual error term, as there is arguably no reason why the unexplained component of individual performance should impact on village level seed consumption, unless some natural disaster occurs within the village.

Finding identification through aggregate consumption at the village level requires that a reasonable number of households consumed a non-zero quantity of seeds over the period. In the sample, out of 434 villages, 52 have missing data on seed consumption, and are therefore excluded from the subsequent IV estimations.[42] The potential selection bias arising from the exclusion of some villages on the basis of an observable characteristic is not dealt with in this study, although robustness checks are carried out to check for parameter constancy across restricted and non-restricted samples which suggests that this does not significantly affect our results (results not reported).

[40] Moreover, when included as a covariate in the regression model, the null of no explanatory power for the individual test of parameter significance cannot be rejected. Although this does not imply that results are immune to omitted variable bias, it justifies the exclusion of the covariate from the system in the absence of a suitable instrument.

[41] Note that the whether the market for seeds is in excess demand or supply is also likely to affect the price at the village level. However, under the assumption that, within a same village, all farmers face the same price for seeds, this only reinforces our identification strategy.

[42] This amounts to dropping 546 households. For comparison purpose, we also drop these households from the OLS and QR regressions of the full input-output specification.

3.3.2 IV on the quantiles

In order to capture heterogeneous treatment effects across different ranges of the distribution of agricultural output, we use a quantile method of estimation at the three quartiles of the distribution. In order to allow seeds to be endogenous to output, as previously exposed in the least squares estimation, we use a novel method of instrumental quantile regression estimation, IVQR, proposed by Chernozhukov and Hansen (2005*a*), generalised to accommodate one endogenous variable and its three interactions with returnee status, policy status, and the returnee-policy interaction.[43] By imposing some restrictions on changes in the ranking of the dependent variable across values of the endogenous regressors, they derive a condition for validity of the instrumentation in quantile regression (QR), and hence identification of the quantile treatment effect (QTE) without relying on any functional form assumption. Their method of estimation is particularly appealing since it postulates rank similarity, a weaker version of rank invariance.

Empirically, inverse quantile estimation technique can be used to retrieve an appropriate measure of the coefficients on the endogeneous covariates in each quantile of interest, as Chernozhukov and Hansen (2005*a*) demonstrate. This is done as follows. First, we estimate the 2-stage quantile regression estimates using OLS to get the predicted values of the first-stage. We then use the regular method of quantile estimation, replacing the endogeneous variables with the first-stage predictions, and then use the 99% confidence intervals of the coefficients thus computed on the endogenous covariates as grid search regions to search iteratively for the combination of coefficients on the endogenous regressors that satisfies the least absolute value criterion, and thus retrieve the QTE that satisfies this criterion on the interval.

3.4 Identification

Evaluating the impact of the imidugudu policy on post-1994 migrants would ideally require the use of a two-period cross-sectional data or, even better, a longitudinal data, with one wave of observations before the policy implementation, and one after. Unfortunately, such data is not available for Rwanda. However, we can exploit the spatial differentials in the patterns of policy implementation to measure the differentials in returns to inputs across returnees and stayers, and across policy and non-policy areas. This will give us a quantitative measure of both the average impact of conflict migration on the stock of human capital at the household level, and of the policy effect on the migrants' population. For a difference-in-differences approach to be valid requires that observations be independent, non-identically distributed (Wooldridge, 2002*b*) across treatment and control groups, and across policy and non-policy areas.

Selection across treatment and control groups, *i.e.* across migrants and stayers, is resolved by assuming that conflict-induced migration is exogenous, and hence determined independently from all observable and unobservable household characteristics. However, the issue of self-selection into the programme, whereby returning households could decide whether to move to a policy area or not, needs to be addressed. Although some evidence that no selection occurred on the basis of patterns of migration across years is presented in section 5, this evidence is not robust to potential sorting on the basis of unobservable characteristics. Should such selection be at present, failing to control for it would cause the estimates of the policy effect on the treated to be biased. However, as shown by Rubin (1977), conditioning the outcome variable on the characteristics on which the self-selection process is based would ensure an unconfounded measure of the programme effect on the treated.[44] We now outline the difference-in-differences estimation relied upon in this paper to capture the effect of the policy on the treated returnees, and hence arguably retrieve a measure of the cost of conflict at a microeconomic level.

Let P_i be the policy status variable, where P_i takes the value 1 if household i is included in the

[43] As mentioned by Chernozhukov and Hansen (2005), empirical studies using this method of IVQR are Chernozhukov and Hansen (2004, 2006)
, Hausman and Sidak (2004) , and Januszewski (2004). The following discussion is largely inspired by these works.

[44] Most of the derivations given here build on Wooldridge (2002*a*) and Firpo (2004) .

programme, and 0 if not. Let $Q_i^m(P_i) = Q_i^m(1)$ be the agricultural output of household i when benefiting from the programme, and $Q_i^m(P_i) = Q_i^m(0)$ the potential outcome for the same household if not benefiting from the programme, for any migration status $m \in \{r; s\}$, where r denotes returnees, and s stayers. Households' potential agricultural output depends on a set of observable characteristics X_i.[45]

Using the same notation as in section 4, and allowing for the implicit production function to vary across returnees and stayers, and also across policy regimes, $Q_i^m(1)$ and $Q_i^m(0)$ can be written as:

$$\begin{cases} Q_i^m(1) = G_1^m(X_i) \\ Q_i^m(0) = G_0^m(X_i), \forall m \in \{r; s\} \end{cases} \tag{5}$$

Where $G_{P_i}^m(.)$ is the agricultural production function of household i with migration status m. Assume that returning households self-select into the programme on the basis of a cost-benefit analysis of their potential gain from the programme. Given their utility function $U(.)$, and $C_j(.)$ the incurred cost from choosing 'policy status' j, $\forall j \in \{0; 1\}$, returning households choose their policy status based on their potential agricultural output expectation, as follows:[46]

$$P_i = \mathbb{1}\{E[U(Q_i^r(1)) - U(Q_i^r(0))|X_i] - (C_1(X_i) - C_0(X_i)) \geq 0\} \tag{6}$$

Thus potential agricultural earnings solely depend on X_i. It then follows that, controlling for X_i, the choice of benefiting from the policy will be independent of the household potential earnings. The characteristics X_i to be included in the specification are discussed in section 6.2.

Variation in the implicit agricultural production function $G_j^m(.)$ is allowed for by interacting dummies for migration status, policy status, and an interaction dummy between migration and policy status, with household characteristics and production inputs when estimating the reduced form.[47]

Hence, the 'migration status impact' on agricultural output for all policy status j, $\forall j \in \{0; 1\}$ is:

$$E(\Delta Q_j^m|X) - E(\Delta Q_j^s|X)$$

and the 'pure policy impact' is, other things equal, then the difference of the differences in output between returnees and stayers across policy and non-policy areas:

$$E(\Delta Q_1^m - \Delta Q_1^s|X) - E(\Delta Q_0^m - \Delta Q_0^s|X)$$

Denoting a set of covariates $X = \{X_1, ..., X_N\}$, Γ a corresponding set of true regression coefficients, and a white noise error term u, and ignoring subscripts and superscripts, let:

$$Q = \Gamma' X + u$$

Differentiating totally and taking expectations:

$$E(\Delta Q) = \Gamma' E(\Delta X)$$

Holding all covariates constant but one, say X_j, and denoting β_j its true regression coefficient :

[45] Here only potential agricultural yield is taken into account as a determinant of household decision to enter the programme, hence excluding potential income from selling labour. Although one could argue that potential off-farm income could play a role in the self-selection process, it is here ignored on the basis that labour markets are generally inactive in Rwanda (cf. discussion in section 3).

[46] The indicator function $\mathbb{1}(F)$ is equal to 1 if F is true, and 0 otherwise.

[47] In subsequent econometric analysis, both OLS and the instrumental variable method of estimation (instrumenting for Seeds and all its interaction terms) are presented.

$$E(\Delta Q|\{X_k\}_{k\neq j}) = E(\Delta X_j)\beta_j$$

Introducing subscripts and superscripts into this expression and subtracting across migration status we get the partial expected migration status effect:

$$E(\Delta Q_1^m - \Delta Q_0^s | X_2^m, ..., X_N^m, X_2^s, ..., X_N^s) = E(\Delta X_1)(\beta_j^m - \beta_j^s), \forall j \in \{0, 1\}$$

Differencing this expression across policy sub-samples, we get the expected pure policy effect:

$$E[(\Delta Q_1^m - \Delta Q_1^s) - (\Delta Q_0^m - \Delta Q_0^s)|X_2^m, ..., X_N^m, X_2^s, ..., X_N^s] = E(\Delta X_1)[(\beta_1^m - \beta_1^s) - (\beta_0^n - \beta_0^n)]$$

and testing whether $[(\beta_1^m - \beta_1^s) - (\beta_0^n - \beta_0^n)]$ is significantly different from zero is equivalent to testing whether the policy had an impact, controlling for the migration status effect.

Hence the estimated reduced form consists of an agricultural output equation, with the yearly monetary value of the household production on the left-hand side[48], and on the right-hand side some exogenous household characteristics and level of consumption of production factors. In the unrestricted specification, all covariates are interacted with dummies for migration status and policy status, and an interaction between policy and migration status, in order to capture any heterogeneity in returns to production inputs and household characteristics across displacement and policy status. The estimated reduced form, for output Q and variable input V, can be written:

$$\begin{cases} Q = \alpha_1 + [\beta' X_1 + V\delta + \gamma'(T - L) + \lambda' K + \partial' E] \\ \quad + \sum_{t\in\{m,p,mp\}}[\beta_t' X_1 + V\delta_t + \gamma_t'(T - L) + \lambda_t' K + \partial_t' E]D^t + \varepsilon_1 \\ V = \alpha_2 + \beta_2' X_2 + Z\delta_2 + [\sum_{t\in\{m,p,mp\}} ZD^t \delta_{2,t}] + \mu Q + \varepsilon_2 \\ VD^s = \alpha_{2,s} + \beta_{2,s}' X_2 + Z\delta_{2,s} + [\sum_{t\in\{m,p,mp\}} ZD^t \delta_{2,s,t}] + \mu_{2,s} Q + \varepsilon_2,\ \forall s \in \{m, p, mp\} \end{cases} \tag{7}$$

where the$\{D^i\}_i$ denote dummy variables, where the superscript i describes $\{m, p, mp\}$, *i.e.* respectively dummies for migrant status, policy status, and a migration and policy interaction.

Testing the assumption that conflict-induced displacement had some effect on output is done as follows. Finding no discrepancies in returns to inputs across groups suggests no effect. In contrast, should this difference-in-differences framework isolate significant differences in returns, it would then also give the sign of the shock in human capital incurred.

A positive shock would be characterised by some positive sign on the migration interactions (the D^m interactions in the first equation of (7)). In the case where skill spill-overs occur from returnees to stayers in non-programme areas, then some negative coefficient would be observed among the policy interactions (D^p), and some positive on the policy-migration interactions (D^{mp}) to offset the latter. Should this positive spill-over be total, then some coefficient on the migration interactions would be insignificant. Should a positive sign be observed on some of the policy-migration interactions only, then the assumption of a positive policy effect would be chosen over that of a human capital effect for migration.

Conversely, a negative shock on human capital as a result of conflict-induced migration would yield negative or zero coefficients on the migration interactions (D^m interactions in (7)). Should positive spill-overs occur between returnees and stayers in the non-policy areas[49], we would find no effect on the policy

[48] All regression models reported are log-linear specification, to allow for more intuitive interpretation of the coefficients.

[49] If so, the argument that negative social pressures were applied as a result of migrants returning to the villages would be the most convincing.

interactions (D^p), and some negative coefficients on the policy-migration interactions (D^{mp}). In contrast with the case of a positive shock, this specification would not necessarily separate a negative human capital effect from a potential land quality effect, should the policy-migration interactions only bear some negative and significant sign. Indeed, in the absence of a land quality index, it is unclear whether the interaction terms between the surface of cultivated land and the policy and migration dummies would entirely capture a land quality effect. Similarly, differentials in the returns to seeds across groups may be capturing either discrepancies in the level of know-how, either systematic disparities in the quality of land.

4 Results

4.1 Descriptive analysis

This subsection presents some simple descriptive statistics of the variable of interest, land and agricultural production.

4.1.1 Access to land

The villagisation policy was intended to address the housing and land crisis exacerbated by the massive return of the old case load and genocide refugees. Hence it seems natural that the policy should target these groups. However, and as the Rwandan Initiative for Sustainable Development (RISD, 1999) points out concerning the selection procedure in the access to the programme:

> "(...) there were no systematic procedures set to ensure a uniform and fair selection."

From Table 5, three things can be inferred. First, on average, both stayers and returnees in policy areas are significantly better endowed in land than those in non-policy areas, whether looking at per adult or per household sample mean land holdings. Second, in villages that did not benefit from the policy, differences in land allocation across returnees and stayers are insignificant (as are land holdings in policy areas). Finally, returnees' land in policy areas is broken down into fewer parcels than it is for returnees and stayers in non-policy areas, as well as for stayers in policy areas (Table 5, rows 1-4). This is in line with the expected effect of the policy, given the aims presented earlier.

This suggests that the land allocation policy had a positive impact on migrants' land endowments, and induced less unequal patterns of land distribution. Again this is not surprising, if (part of) the policy was effective in meeting its aims. However, observed household characteristics are not accounted for in this approach, and neither are unobservables variables such as land quality.[50]

4.1.2 Agricultural Productivity

The main concern of this study is to measure whether there are differentials in agricultural productivity across migration groups, and across policy regimes. The mean of annual agricultural production *per* adult estimated in this study is 11 810 Rwandan Francs (approximately US$36), and the median value is 16 900 Rwandan Francs (or US$51).

Kernel density estimates of the distribution of output by migration and policy are presented in Figure. Few disparities exist between stayers in policy and non-policy areas. If anything, stayers in policy areas

[50] The value of the parcel can be observed in the data for owned properties only, but these values are very noisy, and therefore cannot be relied on to control for quality in regressions. For instance, not all households formally own a property right title, and this is due to the complicated legislation on land property rights in Rwanda. This is however not directly the subject matter of this study. For more reference on the subject, refer to Andre (1998). Moreover, huge variations are observed within villages. This is not very surprising since the Rwandan market for land consists of a black market, and hence the values declared by households can be considered notional measures rather than accurate ones.

seem to be doing better than those in non-programme villages, but dispersion is similar across areas. However, on the right-hand side of Figure 2, there are obvious differences in the distribution of agricultural production between post-1994 migrants in policy and non-policy areas. Indeed, migrants living in villages where imidugudu were built experience higher levels of output per adult than those living in other places. One potential (partial) explanation to these differentials are the variations in land allocation patterns by migration and policy status, as outlined above.

4.2 Econometric Analysis

Tables 6 to 9 report the coefficients on the main variables of interest in a series of specification, using the natural log of agricultural output by adult at the household level as the dependent variable. In all regressions, we control for a number of household characteristics, although all corresponding coefficients are not reported for parsimony's sake.[51]

4.2.1 Raw difference-in-differences estimates

Table 6 presents regression outputs of the difference-in-differences estimation of the natural log of agricultural output per adult (first row), and per adult, per hectare (second row), where agricultural inputs are not controlled for.[52] Columns (1) and (5) report the results from the OLS estimation. In order to capture any heterogeneity in the treatment effect, we report the results of quantile estimations at the quartiles of the distribution (0.25, 0.5, and 0.75 quantiles) in columns (2)-(4) and (6)-(8). The use of quantile regression to measure quantile treatment effect (QTE) is still under discussion in theoretical econometrics, and it is unclear whether estimates computed using simple quantile regression technique achieve the best measure of the QTE. Firpo (2004) suggests the use of a semi-parametric quantile regression technique, with involves computing propensity score estimates, and using them in the quantile estimation to yield estimates with standard large sample properties. However, we use conditional quantile estimation methods, or least absolute value (LAV) model, to compute QTE as the debate regarding the properties of QTE estimators is still on. Regression models (1)-(4) are run without controls, whereas controls are introduced in (5)-(8).[53]

Considering agricultural output per adult, the results presented in the first row of Table 6, columns (1), (3), and (4) suggest that returnees are better off than stayers by 20.4% on average, by 23.7% in the second quantile, and 37.7% in the upper quantile.[54] However, there are no significant differences

[51] Those controls include prefecture fixed effects, age of the head of the household and its square, the average age of the household, dummies for the head being a woman, a widow, and other marital status, a dummy for having domestics, the size of the household, average number of years spent in school by the on-farm 'producers', number of years spent in school by the 'holder' of the household's agricultural production unit, the proportion of adult household members employed in off-farm job, a dummy for renting some land, total exogenous revenue per adult, (average) distance between the house and the cultivated field(s), a dummy for renting any parcel of land, a dummy for letting any parcel of land, a dummy for owning cattle, the annual output from cattle products, value of the parcel cultivated per hectare (when owned, zero otherwise), a dummy for receiving any money transfer, a dummy for sending any money transfer, the total value of the money transfers made, the number of years since arrival, the proportion of household members who originated from a different prefecture, the proportion of household members who were unemployed in their previous place of residence (conditional on being a migrant), the proportion of household members born in a border country to Rwanda, a dummy variable indicating that the village had saw more arrivals since 1994, and a dummy variable indicating that the village had saw more departures since 1994. Most of these controls are interacted with policy or migration status or both, except for prefecture fixed-effects, the village-level variables, age, marital status, and migration-related controls. All standard errors are clustered at the village level.

[52] An index containing variable names and description is presented in Table 10.

[53] The included controls are the following: size of the household, years since head lives in this residence, years spent in previous residence (when migrated after 1994), the proportion of members who where living in a different prefecture before they migrated, age of the head and its square, the average age within the household, a dummy for whether the head is a woman, a dummy for whether the head is a widow, a dummy for having domestic(s).

[54] All effects implied by estimated coefficients on dummy variables in this study are computed as suggested by Halvorsen & Palmquist (1980).

between returnees and stayers in the first quantile of the distribution. That the coefficient estimates on the migration status dummy are heterogeneous and increasing across quantiles is interesting, although no intuition can be inferred at this stage as to the mechanism behind these disparities.

The coefficient estimates on the policy regime dummy are positive and significant only in the first two quantiles of the distribution, implying a positive effect on output of 16.9% and 16.1% in the first and second quartiles respectively (Table 6, upper panel, col. 6 and 7). This suggests that stayers producing in the lower part of the distribution are better off in policy areas.

The coefficients on the interaction dummy between migration and policy (Table 6, 4^{th} row) suggest that, although the policy increased post-1994 returnees' output by on average 24.7% relative to that of returnees in non-policy areas, this effect is heterogeneous across quartiles. Hence the effect of the policy on returnees in the first quartile of the distribution is very large, implying an increase in input of up to 49.8% (col.2 and 6), whereas the effect on returnees producing in the second quantile relative to returnees in non-policy areas is smaller with 21.2% (col. 3 and 7). The policy appears to have no significant impact on the returning households producing in the upper quartile. All the results presented above are robust to the inclusion of household characteristics, as shown in columns (5)-(8).

We also compute the average treatment effect on the treated using propensity score matching (Table 7), in order to relax the linear functional form of the covariates in Table 6.[55] The results, using agricultural output per adult member suggest that the policy had a positive and significant effect on returnees, whereas the measured effect on stayers is insignificant. These results are similar to those presented in Table 6, col. (5), last row. Indeed, although the reported point estimate of the ATT (Table 7, upper panel) is larger than the suggested difference-in-differences coefficient, the size of the standard errors do not allow to tell those coefficients apart.

However, when looking at the estimates of the same specifications using output per head normalised to the amount of land cultivated, the results change drastically (Table 6, lower panel). The coefficient estimates on migration status are insignificant in most specifications (row 5), apart from the QR on the first two quartiles when controlling for household characteristics (col. 6, 7) which remain positive and significant, implying a higher output for returnees by 18.5% and 20.8% respectively. Yield per adult and per hectare estimations produce different estimated effects of the policy dummy from the output per head specification. The policy now has a negative and significant effect in all specifications ranging from -14.2% to -67.4%. This suggests that policy areas are less productive *per* hectare, both in the case of returnees and stayers.

The coefficients on the policy-migration interaction term are now insignificant in most specifications using production by head normalised by land holding (Table 6, row 8). The positive effect estimated for the first quartile is not robust to the inclusion of controls (col. 2 and 8).

Hence some of the results using output normalised by land suggest the existence of a negative productivity gap in programme areas, both for stayers and returnees. Therefore, it is now of interest to estimate a more structural specification, controlling for all production inputs in an attempt to isolate any differential returns, and thus the channels through which these productivity differentials operate. Indeed, although it seems that normalising production *per* hectare brought massive differences in the measure of the policy effect, using production per hectare as dependent variable imposes that returns to land are equal to one across groups, and estimating a more flexible form would be desirable.

4.2.2 Policy effect on the use of agricultural inputs

4.2.3 Assessing the impact of migration on returnees' agricultural returns

In this subsection we use OLS, 2SLS, QR and IVQR estimation techniques to isolate difference in agricultural returns across groups. This is done by using a difference-in-differences specification and by

[55] The propensity score used in Table 7 is computed using the same set of controls as used in columns 5-8 in table 6.

interacting the policy and migration status dummy variables with all production inputs. The set of controls used in this model are otherwise the same as those included in the raw difference estimation model. In formula, we estimate the equation (7) given above.

Regression outputs for the OLS and IV estimations are presented in Table 8. That the estimated coefficients on the (non interacted) dummies for migration, policy and their interaction are all insignificant in Table 8, rows 1-3, is reassuring, as it suggests that the input-output specification captures much of the heterogeneity in output observed in Table 6. Among the production inputs however productivity differentials across groups are found for labour and seeds. The interaction term between hours worked and the dummy for migration status suggests that returns to labour are between 47% and 57.9% higher for returnees than they are for stayers, other things equal. Moreover, the coefficient on the difference-in-differences interaction term is significant for labour supply in the IV estimation, and offsets the positive effect of migration. That these effects are equal in absolute terms tends to suggest the existence of a productivity penalty as a result of migration, compensated in programme areas by positive skill spill-overs from stayers to returnees. These findings are in stark contrast with those obtained on the restricted specification (*i.e.* where production inputs were excluded), suggesting that the positive impact of migration status in policy areas suggested in Table 6 was an asset effect.

The difference-in-differences interaction term on seed consumption is negative and significant, suggesting that returns to seeds are on average 69.7% to 77.1% lower for returnees in policy areas than for the rest of the sample, other things equal. Possible explanations for this effect could be that migrants may use seeds efficiently in policy areas, which might result from having a lower stock of human capital and not benefiting from positive skill spill-overs from the stayers. This would imply that the villagisation policy induced a 'ghetto effect' on the returnees. Second, it may be that the land allocated to returnees living in imidugudu is inferior, and therefore less fertile. Finally, it may be that our instrument is too weak to entirely remove the unobserved effects. This would imply that this negative coefficient captures the mechanism whereby returnees who need to buy seeds are those who did not produce enough in the previous period to save seeds for cropping, *i.e.* the relatively 'low ability' farmers. This would imply that some selective sorting into programme areas occurred, based on unobserved ability.[56]

Estimating the difference-in-differences coefficients of the reduced form suggests heterogeneity in returns to factors of production across migration status, and policy status. However, Figure 2 and the simple difference-in-differences estimation at the quartiles suggest that taking the mean as the reference point may not capture the adequately summarise the data, as disparities in output across groups appear to vary across ranges of the distribution.

The results of difference-in-differences estimation at three quartiles[57] (0.25, 0.5, 0.75) are presented in Table 9, col.1-3, using the same specification as in the difference-in-differences model above. Results using IV on the quantiles are reported in Table 9, columns 4-6.

The QR estimation yields insignificant coefficients estimates for the dummies for migration, policy, and their interaction, whereas the coefficient on the dummy for migration is negative, large, and significant at the 10% level when the regression model is estimated by IVQR at the median. The implied effect is that, at the median, returnees' production is 60.7% lower than that of stayers, other things equal. Moreover, that the dummy is significant suggests that the specification used fails to isolate the channels through which migration impacts on agricultural production at the median. This tends to support the assumption that unobserved factors are at play in determining the differential in productivity across stayers and returnees (*e.g.* sociological pressures, land quality).

The QR and IVQR regressions capture some heterogeneity in returns to inputs across groups. In the QR, the coefficients on family on-farm labour supply interacted with the migration dummy are significant and positive in the second and third quartiles, implying returns for those living in policy areas respectively

[56] None of the coefficients on the dummy for the agricultural extension service within the cell and its interaction terms are significant in the OLS and 2SLS estimation of the difference-in-differences specification (coefficients not reported).

[57] The reported standard errors were obtained by estimating each equation separately, to allow for consistency across results from QR and IVQR. Hence the covariance matrix is unsuitable to testing restrictions on coefficients across quantiles.

32.4% and 64.3% higher than in the rest of Rwanda. However, the difference-in-differences estimate of returns to labour is negative and significant at the median, implying that returns to labour are 27% lower for returnees in programme areas relative to the rest of the returnee population. In contrast with the mean effect measured above, not only does this suggest that the policy prevented any positive skill spill-overs operating from stayers to returnees, but also induced a negative productivity shock on returnees' on-farm labour at the median.

The returns to labour obtained through IVQR estimation (Table 9, rows 4-7) differ from the QR ones. Indeed, they suggest the existence of a positive productivity shock on labour as a result of migration in the first and second quantiles, implying returns respectively 61.8% and 66.7% higher for returnees relative to stayers. The difference-in-differences term is insignificant in the IVQR estimation.

The returns to seeds are also found to be heterogeneous across quantiles and groups (rows 8-11). Moreover, the results differ across QR and IVQR methods of estimation. Indeed, whereas the difference-in-differences coefficient is found to be negative and significant in all quantiles using QR, it is insignificant in all quartiles using IVQR. The interaction between seed consumption and the policy status dummy is significant and positive, though small, in the upper quantile for the QR. The IVQR estimation shows a negative impact of the policy on the return to seeds at the median, implying a 80.3% lower productivity. This effect is of particular interest, as it corroborates the idea of a negative productivity differential in policy areas for all groups, as suggested by the ATE and QTE estimation of the policy on output per hectare. This could be due the villages with land of lesser quality being systematically selected into the programme.

The estimated returns to land do not vary across groups or across estimation method.

The impact of having access to an agricultural extension service varies across quantiles, groups, and estimation methods. The QR estimation suggests that access to such infrastructures increase output by 10% in the first quartile for stayers and returnees in all areas, and an increase of 40.6% for returnees in programme area at the median. However, the IVQR estimation does not yield any significant impact. Overall, the effects reported in the QR estimation are large. Finding no evidence that having access to such service increase output in the highest quantile is not surprising, insofar as relatively wealthier farmers are probably not in need of this type of help as much as those producing in the lower ranges of the output distribution.

5 Conclusion

Using the first household survey data collected after the1994 genocide in Rwanda, this chapter attempts to isolate the potential effect of a conflict-induced migration on the stock of human capital, and to assess the impact of a related policy intervention in post-war Rwanda on post-1994 returnees' economic outcomes. We use simple difference and difference-in-differences regression models, relying on OLS and quantile regression to estimate both the average and quartile treatment effects of displacement on agricultural returns. As seed consumption is likely to be a variable production input, and, therefore, endogeneous to the level of agricultural output, we need to find a valid instrument to retrieve unbiased and consistent estimates of the corresponding coefficients. We use the level of seed consumption at the village level as an exclusion restriction. We find that some of the negative correlation between seed consumption and unobserved effects is removed through this instrumental variable method of estimation, using both 2SLS and instrumental variable quantile regression. A productivity-enhancing effect of the policy was tested against a pure asset effect. In order to capture some potential heterogeneity in agricultural returns across groups, and across different ranges of the distribution of agricultural output, we use a method of instrumental variable quantile regression.

The empirical evidence suggests that the villagisation programme in Rwanda on average increased returnees' output essentially by increasing access to land for this group. We isolated differentials in returns to labour, implying that returnees are more productive than stayers on average and at the median.

However, the 2SLS estimates suggest the existence of a negative productivity shock on returns to on-farm labour in for returnees in policy areas. This would tend to lend some support to the idea that returnees are, on average, more 'motivated', although the pattern of resettlement within imidugudu tends to generate negative social pressures that tend to offset this positive productivity gap. Moreover, the coefficients on seed consumption and its interactions with returnee, policy, and returnee-policy dummies computed in the OLS, 2SLS and QR estimations suggest that returnees in imidugudu experience lower returns to seeds relative to their non-policy counterparts. One interpretation may be that conflict displacement generated a negative shock on the stock of agricultural know-how, which is compensated, in non-policy areas, by positive skill spill-overs from stayers to returnees, but not in policy areas. In that sense, we refer to a 'ghetto' effect of the policy on returnees. However, we cannot rule out the possibility that these differentials in returns to seeds across migration status and policy areas are the results of the land allocated to returnees being of lower quality in policy areas. For instance, the IVQR estimation results suggest that both returnees and stayers producing at the median of the distribution experience lower returns to seeds in policy areas, which could be due to the land being of lesser quality in these parts. We find no evidence of an effect of the provision of agricultural extension services on agricultural output.

Overall, our findings tend to corroborate the notion that conflict-induced displacement affected returnees' stock of human capital in Rwanda. Assessing the effect that the imidugudu policy had on agricultural output and returns, we find that, although the policy induced returnees to produce, on average, more relative to returnees in non-policy villages, that this is mainly due to the fact that imidugudu settlers were allocated more land. Indeed, not only do we find that the policy was not conducive to higher productivity *per* hectare, but also that agricultural returns where, on average and at the quartiles of the distribution, lower for returnees in policy areas relative to their counterparts in non-policy areas. These results suggest that there is room for policy intervention in Rwanda, insofar as, to promote durable peace in Rwanda, these sources of economic inequalities across groups should be addressed.

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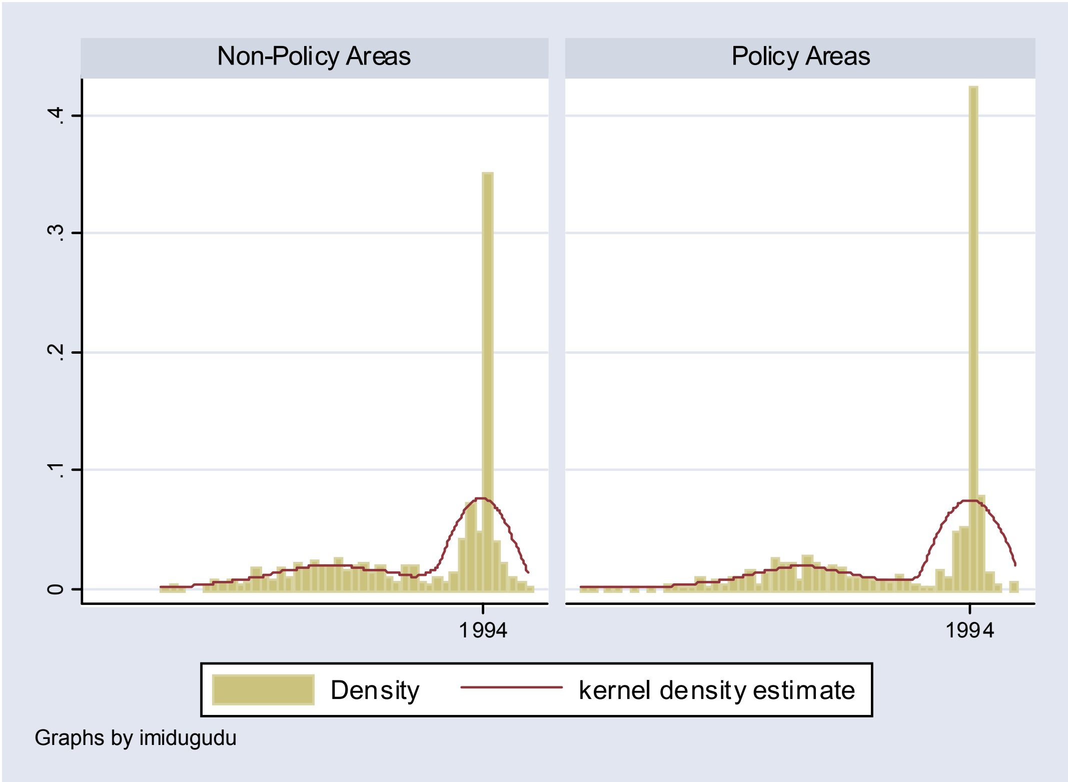

Figure 1: Histogram and kernel density estimate of year post-1994 returnees left the country.

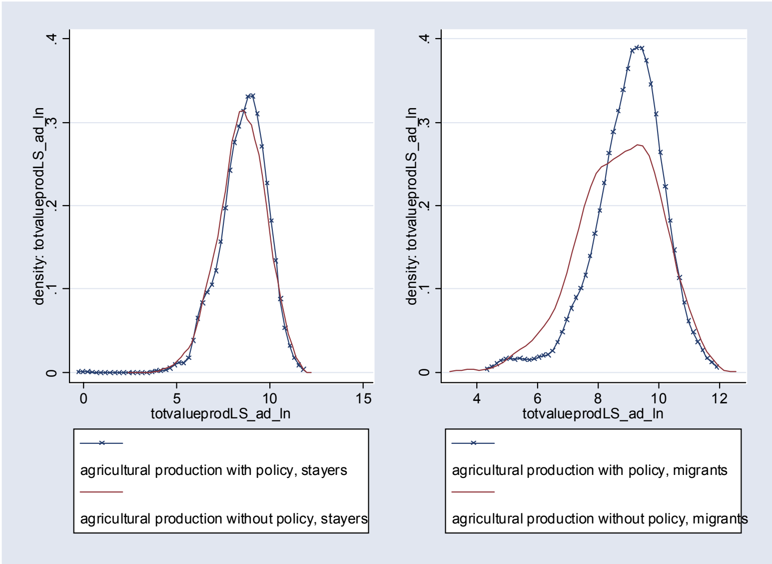

Figure 2: Kernel density estimates of the natural log of agricultural yield per adult member, by migration status, and policy regime.

Table 1: Policy coverage across Rwandan prefectures.

	Communes without Policy		*Communes with Policy*	
	Freq.	Percent	Freq.	Percent
Butare	30	10.03	10	7.41
Byumba	32	10.70	8	5.93
Cyangugu	32	10.70	7	5.19
Gikongoro	37	12.37	3	2.22
Gisenyi	35	11.71	5	3.70
Gitarama	26	8.70	14	10.37
Kibungo	4	1.34	36	26.67
Kibuye	35	11.71	5	3.70
Kigali Ngali	23	7.69	16	11.85
Ruhengeri	33	7.36	18	13.33
Umutara	23	7.69	13	9.63
Total	299	100.00	135	100.00

Table 2: Sample Means by migration and policy status

	Returnees			Stayers			
Variable	No Policy Mean (SD)	Policy Mean (SD)	diff. t-stat	No Policy Mean (SD)	Policy Mean (SD)	diff. t-stat	DID t-sat.
Age head	41.78 (14.95)	43.65 (14.50)	-2.23	45.61 (15.41)	44.42 (14.64)	2.12	-3.04
Avg. age	21.63 (9.55)	22.35 (9.31)	-1.34	23.83 (11.72)	22.72 (10.09)	2.78	-2.74
Size household	4.89 (2.22)	5.22 (2.41)	-2.50	4.92 (2.23)	4.90 (2.11)	0.35	-2.32
Prop. children in HH	0.37 (0.01)	0.36 (0.01)	0.41	0.38 (0.00)	0.40 (0.01)	-1.94	1.42
Yrs head spent in previous residence	7.42 (12.30)	9.81 (14.33)	-3.13	1.92 (6.65)	3.07 (8.31)	-3.82	-1.51
Avg. yrs spent in previous residence	9.65 (10.03)	9.52 (10.69)	0.22				
Prop. of adults born in a border country	0.09 (0.01)	0.14 (0.01)	-3.65	0.00 (0.00)	0.01 (0.00)	-1.53	-3.36
Woman head	0.26 (0.02)	0.28 (0.02)	-0.88	0.33 (0.01)	0.39 (0.02)	-3.40	1.29
Widow head	0.21 (0.02)	0.25 (0.02)	-1.60	0.29 (0.01)	0.33 (0.02)	-2.58	0.24
Married head	0.42 (0.02)	0.45 (0.02)	-1.21	0.45 (0.01)	0.42 (0.02)	1.98	-2.11
Partnership head	0.29 (0.02)	0.22 (0.02)	2.51	0.17 (0.01)	0.15 (0.01)	1.39	1.51
Prop. members with any educ.	0.54 (0.02)	0.57 (0.02)	-1.19	0.58 (0.01)	0.57 (0.01)	0.62	-1.33
Prop. of literate adults	0.52 (0.01)	0.51 (0.01)	0.73	0.47 (0.01)	0.52 (0.01)	-3.49	2.60
Prop. unemployed in previous residence	0.45 (0.02)	0.48 (0.02)	-1.30	0.00 (0.00)	0.00 (0.00)	0.29	-1.32

Table 3: Logistic regression at the household level of post-94 returnee status on household characteristics.

Dependent: Returnee	All	No Policy	Policy
Age of the head	-0.002	-0.002	0.002
	(0.003)	(0.003)	(0.007)
Avg. age in household	-0.003**	-0.004**	0.002
	(0.001)	(0.002)	(0.003)
Woman head	-0.034	-0.017	-0.056
	(0.032)	(0.027)	(0.089)
Widow head	0.063	0.042	0.071
	(0.044)	(0.042)	(0.112)
Size of household	-0.002	-0.004	0.005
	(0.005)	(0.005)	(0.010)
Prop. women in household	0.015	0.030	-0.047
	(0.033)	(0.031)	(0.085)
Prop. children in household	-0.181***	-0.116***	-0.318***
	(0.042)	(0.039)	(0.113)
Married head	0.055	0.051	0.035
	(0.043)	(0.042)	(0.111)
Head living in partnership	0.103**	0.078*	0.137
	(0.043)	(0.040)	(0.117)
Divorced head	0.063	0.045	0.016
	(0.068)	(0.073)	(0.164)
Separated head	0.079	0.067	0.034
	(0.053)	(0.050)	(0.136)
Prop. members born within Rwanda	-1.320***	-0.916***	-2.443***
	(0.172)	(0.163)	(0.384)
Prop. orphans within household	0.019	0.002	0.054
	(0.022)	(0.021)	(0.056)
Yrs head spent in previous residence	-0.001	-0.001	0.003
	(0.002)	(0.001)	(0.004)
Observations	4907	3402	1505

Notes: Regression run at the household level. The marginal effects of the logistic estimation are reported. Dependent: dummy for conflict-induced migration status at the household level. Standard errors clustered at the village level in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4: Logistic regression of the effect of household and commune characteristics on policy status.

Dependent: Imidugudu	Whole sample	Stayers	Stayers & returnees before 97	Returnees after 97 only	All Returnees
Prop. children	0.363	0.841**	0.321	-0.104	-0.310
	(0.436)	(0.387)	(0.428)	(0.261)	(0.253)
Age HHH	0.020	-0.006	0.016	0.025	0.036*
	(0.029)	(0.027)	(0.029)	(0.021)	(0.020)
Woman HHH	0.419**	0.206	0.358**	0.068	0.101
	(0.174)	(0.129)	(0.171)	(0.119)	(0.100)
Avg. age within HH	-0.008	-0.004	-0.008	0.012	0.006
	(0.012)	(0.010)	(0.011)	(0.008)	(0.008)
Prop. with any	-0.205	-0.160	-0.138	0.055	-0.090
education	(0.223)	(0.164)	(0.220)	(0.142)	(0.128)
Returnee	-0.191	–	-0.272	–	–
	(0.323)		(0.330)		
OCL refugees	0.513**	–	0.552**	0.113	0.144
	(0.250)		(0.262)	(0.111)	(0.097)
Prop. returnees	0.021	–	0.020	–	–
living within the	(0.231)		(0.224)		
village					
More arrivals in	-0.080	-0.092	-0.090	-0.097	-0.136
village after 94	(0.125)	(0.130)	(0.123)	(0.184)	(0.161)
More departures	-0.073	-0.073	-0.082	-0.074	-0.197
from village aft 94	(0.125)	(0.129)	(0.122)	(0.182)	(0.159)
No moves to/from	-0.087	-0.072	-0.090	-0.139	-0.119
village aft 94	(0.133)	(0.138)	(0.130)	(0.205)	(0.173)
Owns cattle	0.021	0.043	0.004	-0.095	-0.101
	(0.142)	(0.120)	(0.141)	(0.109)	(0.102)
School in village	-0.053	-0.060	-0.052	0.019	0.040
before 94	(0.064)	(0.066)	(0.062)	(0.110)	(0.090)
Health centre in	0.036	0.054	0.035	0.157	0.082
village before 94	(0.107)	(0.116)	(0.105)	(0.212)	(0.145)
Road through village	0.093	0.116	0.099	0.157	0.142
before 94	(0.071)	(0.073)	(0.070)	(0.116)	(0.105)
Water in village	-0.035	-0.034	-0.026	-0.204**	-0.080
before 94	(0.054)	(0.055)	(0.053)	(0.087)	(0.069)
Observations	434	406	426	235	304

Notes: All regressions are estimated at the household level, restricting the sample to returnees and stayers in columns 3 and 4 respectively (prefecture fixed-effects are included - the marginal effects of the logistic estimation are reported). Standard errors in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Sample means of the difference-in-differences in land tenure across migration status and across policy regime (all areas are in hectares).

		Post 1994 Returnees		Stayers		
		Average	*Std Dev*	*Average*	*Std Dev*	*|t|*
Number of parcels per household	*All*	2.93	2.01	2.97	1.74	0.54
	No Imidugudu	3.08	2.28	2.95	1.69	1.19
	Imidugudu	2.76	1.62	3.05	1.86	4.07
	Diff-in-diff	2.85	–	1.55	–	3.21
Area per household	*All*	0.82	0.85	0.69	0.94	5.09
	No Imidugudu	0.68	0.78	0.65	0.94	0.57
	Imidugudu	0.99	0.89	0.78	0.96	5.44
	Diff-in-diff	6.43	–	3.38	–	3.12
Area per adult member	*All*	0.34	0.40	0.29	0.39	4.12
	No Imidugudu	0.30	0.40	0.27	0.38	1.20
	Imidugudu	0.39	0.40	0.33	0.42	3.23
	Diff-in-diff	3.92	–	3.73	–	1.15

Table 6: Simple difference-in-differences specification.

Variables	OLS (1)	Quant. 0.25 (2)	Quant. 0.50 (3)	Quant. 0.75 (4)	OLS (5)	Quant. 0.25 (6)	Quant. 0.50 (7)	Quant. 0.75 (8)
Dependent: Natural log of Agricultural Yield per Adult								
Returnee	0.186*	0.115	0.213**	0.320**	0.217*	0.114	0.304**	0.299**
	(0.088)	(0.087)	(0.063)	(0.064)	(0.098)	(0.085)	(0.081)	(0.084)
Imidugudu	0.081	0.156*	0.149**	0.049	0.076	0.189**	0.154**	0.022
	(0.079)	(0.077)	(0.056)	(0.056)	(0.078)	(0.062)	(0.060)	(0.062)
Returnee *	0.221†	0.404**	0.192†	-0.012	0.254*	0.371**	0.205†	0.068
Imidugudu	(0.122)	(0.138)	(0.100)	(0.101)	(0.122)	(0.112)	(0.107)	(0.112)
Other controls					✓	✓	✓	✓
Dependent: Natural log of Agricultural Yield per adult, per hectare of land								
Returnee	0.016	0.113	0.111	-0.120	0.023	0.170†	0.189*	-0.130
	(0.106)	(0.084)	(0.073)	(0.095)	(0.121)	(0.097)	(0.083)	(0.117)
Imidugudu	-0.267*	-0.184*	-0.259**	-0.515**	-0.258*	-0.133†	-0.214**	-0.492**
	(0.130)	(0.074)	(0.065)	(0.084)	(0.130)	(0.073)	(0.061)	(0.085)
Returnee *	-0.064	0.239†	-0.087	-0.145	-0.047	0.184	-0.159	-0.164
Imidugudu	(0.167)	(0.134)	(0.117)	(0.151)	(0.166)	(0.131)	(0.110)	(0.153)
Other controls					✓	✓	✓	✓

Notes: Columns (1) to (4) report the impact of returnee status, policy status, and an interaction measured by OLS and quantile regressions without any controls. Columns (5) to (8) present the results of similar regression when some household characteristics are included. Controls included are described in Section 6.2, and include household and village characteristics, as well as prefecture fixed-effects. Standard errors clustered at the village level in parentheses; † significant at 10%; * significant at 5%; ** significant at 1%.

Table 7: ATT of the policy effect on households' agricultural production, by migration status.

Method of Matching	ATT	t-value	#Treated	#Controls
Post-1994 Returnees				
Nearest neighbour *	0.307	2.946	570	332
Kernel	0.346	4.638	570	672
Radius *	0.380	5.278	568	672
Stayers				
Nearest Neighbour *	0.103	1.414	935	759
Kernel	0.073	1.613	935	2725
Radius *	0.074	1.550	935	2725

Notes: Regressions run at the household level, Dependent variable: ln(annual household agricultural production per adult member) - bootstrapped standard errors) - propensity score estimate computed on basis of exogenous household characteristics (cf. list of controls in Section 6.2) - * : numbers of controls and treated refer to actual matches.

Table 8: OLS and 2SLS estimation results of the difference-in-differences sepcification.

Variable	**OLS Coefficient** (Std. Err.)	**IV Coefficient** (Std. Err.)
Returnee	-0.379	-0.537
	(0.294)	(0.340)
Imidugudu	0.040	-0.082
	(0.276)	(0.307)
Returnee_imidugudu	-0.019	0.268
	(0.433)	(0.490)
On-farm LS	0.034**	0.038**
	(0.008)	(0.008)
On-farm LS _returnee	0.016*	0.022*
	(0.008)	(0.009)
On-farm LS _imid	0.008	0.009
	(0.006)	(0.006)
On-farm LS _ returnee _imid	-0.016	-0.022†
	(0.011)	(0.012)
Seeds bought	0.035**	0.089**
	(0.004)	(0.020)
Seeds bought_ returnee	0.001	0.019
	(0.004)	(0.017)
Seeds bought_imid	0.007†	0.029
	(0.004)	(0.027)
Seeds bought_ returnee _imid	-0.027**	-0.062†
	(0.007)	(0.036)
Land	1.017**	0.811**
	(0.114)	(0.135)
Land_ returnee	0.140	-0.020
	(0.174)	(0.203)
Land_imid	-0.129	-0.097
	(0.147)	(0.167)
Land_ returnee_imid	0.017	0.142
	(0.272)	(0.313)
Observations	4364	4364

Notes: All regressions are run at the household level. Column 1 reports the OLS coefficients of agricultural output on inputs and some household characteristics interacted with dummies for returnee, policy, and returnee in policy area status (cf. list in Section 6.2); column 2 reports the 2SLS coefficients of the same specification, instrumenting for seeds. Standard errors clustered at the village level are reported in brackets. Significance levels: † : 10%, * : 5%, ** : 1%.

Table 9: QR and IVQR Estimation results of the difference-in-differences specification.

Variable	**QR**			**IVQR**		
	Quant. 0.25	**Quant. 0.50**	**Quant. 0.75**	**Quant. 0.25**	**Quant. 0.50**	**Quant. 0.75**
Returnee	-0.333	-0.391	-0.276	-0.64	-0.933†	-0.499
	(0.404)	(0.305)	(0.382)	(0.636)	(0.518)	(0.530)
Imidugudu	0.316	-0.052	-0.163	-0.031	0.179	-0.210
	(0.369)	(0.297)	(0.259)	(0.490)	(0.336)	(0.344)
Returnee_imidugudu	0.265	0.550	-0.119	0.86	0.282	0.019
	(0.591)	(0.473)	(0.496)	(0.973)	(0.797)	(0.796)
On-farm LS	0.030**	0.037**	0.028**	0.034**	0.042**	0.029**
	(0.009)	(0.007)	(0.009)	(0.009)	(0.009)	(0.009)
On-farm LS_returnee	0.014	0.012†	0.018†	0.021†	0.028*	0.015
	(0.010)	(0.007)	(0.010)	(0.013)	(0.011)	(0.01)
On-farm LS_imid	0.009	0.008	0.007	0.007	0.002	0.004
	(0.008)	(0.007)	(0.007)	(0.009)	(0.008)	(0.010)
On-farm LS_returnee_imid	-0.021	-0.022†	-0.018	-0.015	-0.015	-0.005
	(0.014)	(0.011)	(0.013)	(0.022)	(0.016)	(0.015)
Seeds bought	0.044**	0.038**	0.033**	0.073**	0.066**	0.059†
	(0.006)	(0.005)	(0.003)	(0.028)	(0.025)	(0.031)
Seeds bought_returnee	0.003	0.001	-0.001	0.018	0.106	0.064
	(0.007)	(0.005)	(0.004)	(0.072)	(0.114)	(0.109)
Seeds bought_imid	-0.002	0.010	0.008*	0.068	-0.053*	0.016
	(0.008)	(0.007)	(0.004)	(0.139)	(0.027)	(0.116)
Seeds bought_returnee_imid	-0.030*	-0.030**	-0.021*	-0.096	0.160	0.070
	(0.012)	(0.012)	(0.009)	(0.242)	(0.345)	(0.451)
Land	1.083**	0.939**	0.815**	1.075**	0.981**	0.832**
	(0.162)	(0.155)	(0.104)	(0.191)	(0.18)	(0.144)
Land_returnee	0.019	0.273	0.393	-0.140	0.063	-0.058
	(0.206)	(0.195)	(0.245)	(0.363)	(0.249)	(0.254)
Land_imid	-0.226	0.005	-0.095	-0.265	0.007	-0.028
	(0.167)	(0.209)	(0.118)	(0.236)	(0.236)	(0.153)
Land_returnee_imid	0.344	0.070	0.088	0.428	-0.197	0.06
	(0.312)	(0.349)	(0.321)	(0.594)	(0.392)	(0.461)
AES	0.065†	0.095	0.082	0.000	0.044	-0.020
	(0.056)	(0.053)	(0.053)	(0.066)	(0.064)	(0.063)
AES_returnee	0.019	-0.105	-0.021	0.065	0.044	-0.020
	(0.143)	(0.125)	(0.142)	(0.066)	(0.064)	(0.063)
AES_imid	-0.130	-0.038	0.005	-0.125	-0.033	-0.02
	(0.131)	(0.103)	(0.098)	(0. 185)	(0.119)	(0.191)

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… table 9 continued

Variable	QR			IVQR		
	Quant. 0.25	**Quant. 0.50**	**Quant. 0.75**	**Quant. 0.25**	**Quant. 0.50**	**Quant. 0.75**
AES_returnee_imid	0.352	0.341[†]	0.296	0.339	0.263	0.311
	(0.220)	(0.189)	(0.206)	(0.263)	(0.418)	(0.284)

Notes: All regressions are run at the household level. Columns 1-3 reports the quantile regression coefficients of agricultural output on inputs and some household characteristics interacted with dummies for returnee, policy, and returnee in policy area status (cf. list in Section 6.2) at the 0.25, 0.50, and 0.75 quartiles; columns 2 reports the IV quantile regression coefficients of the same specifications, instrumenting for seeds. Analytical standard errors are reported in brackets. Significance levels: † : 10%, * : 5%, ** : 1%.

Table 10: Index of variables and abbreviations

Variables	Definitions
Returnee	Dummy for returning after 1994
Imidugudu	Dummy for policy status
Returnee_imidugudu	Interaction term of dummies for migration and policy status
X_returnee	Indicates that variable X is interacted with the dummy for returnee status
X_imid	Indicates that X is interacted with the dummy for policy status
X_returnee_imid	Indicates that X is interacted with both dummies for returnee and policy status
On-farm LS	Annual weekly average per adult of on-farm family labour supply
Seeds bought	Amount of seeds bought over the year in 1000 Rwandan francs (approx. US dollars 3)
Land	Amount of land cultivated by the household, normalised by the number of adults in the household
AES	Dummy for access to an agricultural extension service in the commune. This service is an infrastructure through which farmers can buy/rent/benefit from tools, seeds, advice on agricultural techniques