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

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Abstract
In 1997 Rwanda introduced a re-settlement policy for refugees displaced during previous conflicts. We exploit geographic variation in the speed of implementation of this policy to investigate the impact of conflict-induced displacement and the re-settlement policy on household agricultural output and on skill spill-over mechanisms between returnees and stayers. We find that returns to on-farm labour are higher for returnees relative to stayers, although the evidence suggests that the policy contributed little additional effect to this differential. More speculatively, these differentials suggest that, upon return from conflict-induced exile, returnees are more motivated to increase their economic performance.

Keywords: Microeconomic cost of conflict, migrations, land redistribution, instrumental variable quantile regressions
JEL Classifications: C4, O12, Q12, Q15, R15, R23
data: 2000-01 EICV - World Bank & Office of Statistics in Kigali

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Introduction

Rwanda has experienced sustained waves of ethnic violence since 1959, which has resulted in massive population displacements that have strained the supply for land and housing. In 1997, after the latest wave of violence, the Rwandan government implemented a re-settlement and land redistribution policy, the villagization or imidugudu policy, for displaced returnees. This paper studies the effect of conflict-induced displacement and of the villagization policy on the output of subsistence agricultural households in post-conflict rural Rwanda. The villagization policy randomly targeted returnees, and we use geographic variation in the speed of implementation of the policy to estimate the impact of the villagization policy on returning household agricultural output and on skill spill-over mechanisms between returnees and stayers. As the imidugudu settlements were built on the border of existing villages, we observe both stayers and returnees living in policy and non-policy areas. Since conflict-induced displacement in Rwanda presents an exogenous shock, the population of stayers can serve as a control group to account for potential systematic heterogeneity across policy and non-policy areas in a difference-in-differences analysis.

The motivation for using conflict-induced displacement to proxy the incidence of conflict at the household level is three-fold: conflict-induced displacement is likely to be costly in terms of human capital, displacement in Rwanda is an exogenous shock at the household level, and, more pragmatically, displacement is more easily observable in the data. First, conflict-induced displacement is likely to be costly in terms of human capital and have a direct effect on a household's agricultural output upon return. This effect is likely to operate through different channels. For instance, displacement status may be associated with an exposure to a particularly high level of violence during the conflict and its resulting sociological or psychological effects. Furthermore, 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 net positive or a negative shock to their level of human capital, as while they might have lost some skills, they might have gained new ones. Displacement is also likely to result in the "atomization" of the household, potentially reducing intergenerational agricultural skill transfers. Finally, as displaced households are spatially "re-shuffled" across Rwanda, specific agricultural skills developed in their original place of residence may not be transferable to their new place of residence. One impediment to the transfer is the drastic difference in soil type, elevation, and the slope of the cultivated land across and within Rwandan villages, and cultivating a specific parcel of land is likely to require specific agricultural knowledge on the farmers' part. Second, we argue that conflict-induced displacement in Rwanda consists of an exogenous shock, as the militia operated along "war paths" within Rwanda[1]. This method of operation likely

[1] 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; for specific qualitative evidence on the Rwandan case, see also Straus, 2004, and Uvin,1999). This goes to provide some credential to one of our main working assumptions, which is that, during

led to the displacement of both non-perpetrators, who were driven from their homes, and perpetrators, who joined the militia. Hence, by using conflict-induced displacement, we do not distinguish between non-perpetrators and perpetrators. Although this limits our study to measuring the effect of conflict "averaged" across those two groups, it allows us to overcome any potential selection issue related to the decision of the population of Hutus who were on the war path to join the militia or to be persecuted.[2] The third, and more pragmatic, argument to motivate using displacement as a measure 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.

This study contributes to the literature on the effects of civil conflicts in that it offers a measure of the cost at the household level and to the literature on the sources of civil conflicts by assessing the existence of systematic economic disparities. The costs of conflicts, both in terms of physical and human capital, are difficult to measure, and few studies have so far attempted to provide such cost estimates at the microeconomic level.[3] Blattman (2006) provides an estimate of the long-term costs of military participation in Northern Uganda at the individual level. However, this study's focus on the impact of civil strife on agricultural productivity at the household level in LDCs has not been tackled in the literature. An impressive body of literature has recently emerged on the topic of the causes of civil conflicts, and the linkage between peace and a country's socioeconomic performance. Miguel *et al.* (2004) find a positive correlation between economic under-performance and the likelihood of civil strife, using rain-shocks as an instrument to establish causality.[4] 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 microeconomic studies (Deininger, 2003; Bigombe *et al.,* 2000; Verwimp, 2005) suggest a causal link between poverty in general, and the lack of economic prospects in particular, and the likelihood of conflict. Based on this strand of literature, this study's economic analysis of a post-conflict situation and re-settlement policy becomes relevant to the assessment of the likelihood of conflict resurgence.

This paper is structured as follows: The first section presents background historical information on Rwanda and the villagization policy, the data and the variables of interest are described in a second

the Rwandan genocide, the militia did not select the villages they attacked on the basis of their economic performance. However, that land scarcity may have been among the factors driving individuals to perpetrate the genocide in Rwanda (Verwimp, 2005) is still perfectly compatible with this assumption.

[2] Straus (2004) provides evidence from qualitative interviews in post-war Rwanda that the perpetrators main motivation is "in-group, intra-Hutu coercion and intimidation".

[3] Cost of conflict has also been estimated at a macroeconomic level. For instance, Abadie and Gardeazabal (2003) propose a measure of the aggregate economic cost of the conflict in the Basque Country.

[4] Some theoretical contributions also explore the causes of conflict at the microeconomic level (Caselli and Coleman, 2002; Bhavnani and Backer, 2000).

section, selection issues are dealt with in a third section, a fourth section presents the regression analysis results, and a fifth section concludes.

1 Background

Rwanda has experienced sustained waves of ethnic violence since independence in 1959.[5] These successive episodes of civil strife led to massive population displacements. Much of the population was stranded in refugee camps either within the Rwandan borders or in the border countries of Burundi, the Democratic Republic of Congo (DRC), Tanzania, and Uganda.[6] After the 1994 genocide, the government encouraged all Rwandans located in refugee camps abroad to return to Rwanda. Given these different waves of displacement, two types of returnees are commonly defined: the Old Case Load (OCL) – refugees who fled before 1994 – and the New Case Load (NCL) – refugees who fled as a result of the 1994 genocide and the 1996 insurrection. The Rwandan National Office for Population (ONAPO) reports that 300,000 people fled as a result of the post-decolonization war (Bucagu, 2000). The same source estimates that over one million pre-1994 refugees returned to Rwanda after 1994, including children and grandchildren of the 1959 refugees.

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. This regulation left no room for legal claims over land on the OCL refugees' part and offered no solution to the waves of displacement triggered by the 1994 genocide. As genocide survivors and perpetrators came back to Rwanda, they often found their land and houses occupied. As a consequence, most OCL refugees became homeless and landless, 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, called the villagization policy or imidugudu policy. This policy grouped returnees in agglomerated settlements within existing villages, endowing them with parcels of land situated outside the settlement. The main expected benefits of the programme were the following. First, the efficient pattern of re-settlement was hoped to reduce land-related tensions through re-settlement and land allocation for post-1994 returnees. Second, it was expected that security would be enhanced by promoting more densely populated settlements over the traditional Rwandan villages.[7] Third, it was hoped that, thus re-settled, the re-

[5] 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).

[6] Some estimations suggest that half of the population was thereby displaced, either temporarily or for longer periods of time (Bucagu, 2000).

[7] It was hoped that militia men, normally operating in small units, would hence be deterred from attacking larger groups of villagers.

turnees would have recourse to more mechanised means of production, which would enhance productivity at the farm-level through agglomeration externalities. Finally, a more clustered pattern of settlement when coupled with access to public goods, such as an agricultural extension services (AES)[8], 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.

The initial coverage of the policy was not universal, and, in 2000/01, both programme and non-programme areas were observed (Table 1). Although it was expected that, in the longer run, the programme would be extended to the whole of Rwanda, it initially only offered a solution to NCL and OCL refugees who re-settled within the pilot areas of the programme after 1994. Although there is no evidence that any sort of selection took place in the sorting of returnees across imidugudu and non-policy villages (RISD[9], 1999), we dedicate a section to testing for any systematic correlation between (observable) household and village characteristics and policy status. The pilot implementation of the programme partly targeted the less densely populated prefectures. That voluntarily re-settlement of returnees within those areas would occur was not obvious, due the war trauma endured and the fear of higher insecurity in these areas. For instance, Ruhengeri, strategically located on the border with the DRC, was highly targeted by the militias during the conflict and a scene of intense displacement. Thus, it was relatively less densely populated and richer in land in 1996. Similarly, Gitarama, Kibungo, Kigali rural, and Umutara were, as a result of the conflict, less densely populated and thus more suitable to accommodate the imidugudu villages.

Some of the potential drawbacks of the villagization policy as it was implemented in Rwanda include increasing the distance to the farm land, not integrating the settlements into existing villages, and restricting the policy to post-1994 returnees. 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).[10] Second, by reducing the integration of the returnee population living in policy areas with the rest of the community, the policy might prevent skill spill-over mechanisms to operate between stayers and returnees in policy

[8] 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. Although we cannot be sure that those infrastructures were randomly distributed, we find no appreciable differences in observed village characteristics across villages with access to an AES and those without (not reported). We control for such infrastructure by adding a dummy for access to AES within the village in subsequent regression analysis.

[9] RISD stands for Rwandan Initiative for Sustainable Development.

[10] 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).

areas[11], and/or yield negative sociological pressures, hence creating a 'ghetto effect'.[12]. In addition, 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.[13]

2 Data

The study uses two data sources: the 2000/01 National Rwandan EICV, which surveyed over 6400 households across all twelve Rwandan prefectures, and the Community Survey, which was carried out in the same villages and at the same time as the EICV, and contains pre- and post-1994 village-level characteristics. The EICV questionnaire consists of twelve sections, some at the individual level and some at the household level, providing a comprehensive set of variables regarding employment, education, migration, agricultural production, and consumption. The data contain 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. Given that we are interested in agricultural productivity at the household level, we exclude both urban households and rural households who do not engage in agriculture. This reduces our sample, after removing outliers[14], to some 4900 households, spread out evenly[15] across the eleven rural Rwandan prefectures. The sampling procedure designed by the World Bank (Scott, 1997) encompassed 440 rural villages across the prefectures.[16] The survey collectors undertook a population census of the population in each enumeration area (or village)[17]; and then the households surveyed were randomly drawn from these lists. Since most returnees re-settled in Rwanda between 1995 and 1998, the date of the data collection (2000/01) presents a limitation on the assessment of the human capital cost of displacement. However, we argue that, if anything, our estimates are a conservative measure of the cost

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

[12] 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 .

[13] 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.

[14] 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 less than 30 households.

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

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

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

of conflict.[18]

2.1 Agricultural output

Because of the predominance of subsistence agriculture, Rwandan households are mostly multiple-crop producers. Hence, restricting the analysis to only one type of crop would yield selection issues. The 2000/01 EICV surveyed the quantity and value of the annual harvest for each crop produced by the household.[19] We do not observe prices at the village or national level.[20] Relying on the prices declared by households in survey data is not without risk, as they might be mis-reported.[21] Hence, we choose to aggregate the reported prices at the prefecture level.[22] Indeed, this should allow for lower levels of error, should the measurement error component be well-behaved white noise. We use the median value of the reported price (for each crop) at the municipality level.[23]

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). Figure 1 presents (unconditional) kernel density estimates of the distribution of agricultural household output by displacement and policy status. Few disparities exist between stayers in policy and non-policy areas. If anything, stayers in policy areas 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 1, there are obvious differences in the distribution of agricultural production between post-1994 migrants in policy and non-policy areas. Migrants living in villages where imidugudu were built experience higher levels of output per adult

[18] Indeed, should displacement be associated to lower levels of human capital, then positive skill spill-overs mechanisms from stayers to returnees from 1998 and 2001 would lead us to estimate a less negative value of the treatment. Similarly, should displacement translate into higher levels of human capital, then positive skill spill-overs from returnees to stayers would also bias our measure of the treatment effect towards zero.

[19] 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.

[20] Shadow price estimation, which relies on a profit maximisation framework to induce the real price level in equilibrium, would ideally be used to retrieve the value of the household production. However, 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 is 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.

[21] 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.

[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. Comparing values within prefectures reduces the dispersion and, where there are few observations within a village, potentially reduces the incidence of measurement error. For instance, for the most produced crop, sorghum, the standard deviation is reduced by 10% when using prefecture level relative to village level aggregation.

[23] 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.

than those living in other places. Moreover, most of the variation is located at the lower tail of the distribution of output, suggesting that the policy had a particularly strong effect on those returnees producing relatively low levels of output. This suggests that measuring the effect of the policy at the quantiles of the distribution of output, rather the mean, in the subsequent regression analysis might allow for more appropriate values of the treatment effect to be computed.

2.2 Policy variable

The main question in the Community Survey on the villagization policy is: *"Has the commune built imidugudu since 1994, and, if yes, how many, and which were the two main sources of financing?"*. The information about the number of imidugudu in the village is often not exploitable as 40% of the answers were 'do not know', the number of houses contained in each of these imidugudu is unobserved, and the information on which body funded the construction work is not detailed enough to allow precise analysis.[24] 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 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.[25]

2.3 Conflict-induced displacement

The survey has information 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 the occupation while there, and the main reason for migrating to the current residence for all household members over 15.[26] That only information related to the most recent migration was surveyed presents a limitation in precisely identifying different waves of post-1994 migration. 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 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 2).[27] 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

[24] 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%).

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

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

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

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 returnees spent between 0 and 5 years in their previous residence, which would indicate they belong to the NCL refugees. The frequency of out-migration by year across policy and non-policy areas (Figure 2) 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 were often born in exile, where the mode of the year of birth was 1975. The high frequency of out-migration in 1994 corroborates that, as the genocide started, massive and sudden displacements of population occurred.[28]

3 Estimation Strategy

3.1 Difference-in-differences analysis

In this study, we exploit geographic variation in the speed of implementation of the villagization policy to estimate the effect of displacement and of the policy on the displaced, as well as differentials in returns to inputs across groups, using a difference-in-differences analysis. Let P be the policy status variable, where P takes the value 1 if a household is included in the programme, and 0 if not. Let $Q^m(P) = Q^m(1)$ be the agricultural output of a given household living in a policy area, and $Q^m(P) = Q^m(0)$ its level of output had it not benefited from the programme, for a given a displacement status $m \in \{returnee; stayer\}$. A household potential agricultural yield is also conditional on a set of observable characteristics, X. Allowing for the (implicit) production function, $G_P^m(.)$, to vary across returnees and stayers and also across policy regimes, $Q^m(1)$ and $Q^m(0)$ can be written as:

$$\begin{cases} Q^m(1) = G_1^m(X) \\ Q^m(0) = G_0^m(X), \forall m \in \{returnee; stayer\}. \end{cases} \tag{1}$$

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 assuming that there is no cost associated with choosing policy status P, a returning household chooses its policy status solely based on its expected agricultural output, as follows:[29]

$$P = \mathbb{1}\{E[U(Q^{returnee}(1)) - U(Q^{returnee}(0))|X] \geq 0\}. \tag{2}$$

[28] Kondylis (2007) also finds that the patterns of returns in time is analogous across policy and non-policy areas.
[29] The indicator function $\mathbb{1}(F)$ is equal to 1 if F is true, and 0 otherwise.

It then follows that, controlling for X, the choice of benefiting from the policy will be independent of the household potential earnings. We allow for variations in the implicit agricultural production function across groups by interacting dummies for displacement status, policy status, and an interaction dummy between displacement and policy status, with production inputs. The impact of displacement on agricultural output for a given policy status P is:

$$E(\Delta Q_P^{returnee}|X) - E(\Delta Q_P^{stayer}|X).$$

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

$$E(\Delta Q_1^{returnee} - \Delta Q_1^{stayer}|X) - E(\Delta Q_0^{returnee} - \Delta Q_0^{stayer}|X).$$

The corresponding (true) linear regression model can be written (where Γ is a vector of regression coefficients, and a white noise error term u):

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

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 associated true regression coefficient :

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

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

$$\begin{aligned} & E(\Delta Q_P^{returnee} - \Delta Q_P^{stayer}|X_2^{returnee}, ..., X_N^{returnee}, X_2^{stayer}, ..., X_N^{stayer}) \\ = \quad & E(\Delta X_j)(\beta_j^{returnee} - \beta_j^{stayer}), \forall P \in \{0, 1\}. \end{aligned}$$

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

$$\begin{aligned} & E[(\Delta Q_1^{returnee} - \Delta Q_1^{stayer}) - (\Delta Q_0^{returnee} - \Delta Q_0^{stayer})|X_2^{returnee}, ..., X_N^{returnee}, X_2^{stayer}, ..., X_N^{stayer}] \\ = \quad & E(\Delta X_1)[(\beta_1^{returnee} - \beta_1^{stayer}) - (\beta_0^{returnee} - \beta_0^{stayer})]. \end{aligned}$$

Testing whether $[(\beta_1^{returnee} - \beta_1^{stayer}) - (\beta_0^{returnee} - \beta_0^{stayer})]$ is significantly different from zero is then equivalent to testing whether the policy had an impact, controlling for the displacement status effect. Implementing such difference-in-differences analysis requires that observations be independent, non-identically distributed (Wooldridge 2002, chap. 6) across treatment and control groups, and across policy and non-policy areas. In this study, three sources of concern arise in assessing the impact of the villagization programme on the population of returnees in Rwanda using such design. First, certain households might have self-selected into displacement. Second, the government might have systematically chosen the villages where the imidugudu were built based on some village-level characteristics. Third, among those households who returned to Rwanda after the genocide, some might have self-selected into programmes areas. In this section, we exploit the large array of household and village characteristics available in the EICV to investigate their distribution across groups, so as to identify any potential systematic correlation between observable household and village characteristics, and displacement and policy status.

3.2 Selection into displacement

Given the nature of the Rwandan conflict, ethnicity obviously played a large part in the selection process whereby specific households were targeted by the militia and fled Rwanda. However, we cannot account for the role of ethnicity in this study. Moreover, it seems reasonable to assume that ethnicity is uncorrelated to a person's agricultural output, all else equal. We further 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 displacement, but this is not an issue we can empirically address with the existing data. Particularly, 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"*. Although Uvin acknowledges that among other motives, economic opportunism may have driven the perpetrators into committing killings, he argues that this is unlikely to have been a main source of motivation or have played a large role in the selection of the victims. Straus (2004), using data from qualitative interviews carried out in post-war Rwanda also finds that economic motives were not prevalent in the killing decision. 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.

As a check, we empirically examine potential determinants of displacement on aggregate and across

policy status in a regression framework.[30] We are limited in our analysis to selection on the basis of observable characteristics, as we do not have access to a longitudinal data source. Table 2 presents the results of logistic regression models at the household level, where returnee status is the dependent variable, taking the value 1 if the head of the household is a returnee, and 0 otherwise. We estimate the same regression model applying diverse sample restrictions: column 1 presents results on the whole sample; in column 2, the sample is restricted to policy areas; and in column 3, the sample excludes all policy areas. The results on the overall sample (col. 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 younger (and most likely healthier) households were more likely to escape the persecutions. 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 parameter constancy across samples at conventional levels. 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. Nevertheless, 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 unobserved heterogeneity.[31]

3.3 Selection into the programme

The next concern is the selection of prefectures and villages into the programme. From Table 1, it is obvious that some prefectures were more likely to be selected into the programme than others. As we still observe a number of treated and control villages within each prefecture, controlling for prefecture fixed-effect in subsequent regression analysis should control for any unobserved heterogeneity across prefectures that might otherwise contaminate our estimates. That villages may have been selected into the programme on the basis either of village-level or villagers' characteristics is of concern. As a check,

[30] As the urban areas are not considered in this analysis, we run the risk that the sample of migrants we base our analysis on is a selected sample. A descriptive analysis of the observed characteristics across urban and rural groups (results not reported) suggest that those living in the urban areas are, on average, more educated than their rural counterparts. However this is also the case for stayers. We control for education in all subsequent regressions.

[31] Splitting the sample between OCL and NCL was also attempted, although the null of parameter constancy across samples was rejected at conventional levels. Moreover, taking the average effect over all types of returnees helps avoid the question of sorting of those displaced returning into their residence prior conflict, and of potential 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.

we 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.[32] The results are presented in Table 3, columns 2 and 3. The positive and significant coefficient on the proportion of children in the sample of stayers suggests 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 the case, the difference-in-differences should absorb this source of heterogeneity, allowing a consistent estimate of the policy effect on returnees to be measured. The positive and significant coefficients on the dummy for female head of the household in columns 1 and 3 of Table 3 suggest that areas with high proportions of pre-1997 returning widows were more likely to be included in the programme.[33] The impact on our measure of the impact of villagization on agricultural productivity is unclear, and we control for widow status in all subsequent estimations. The coefficient on the dummy for OCL returnees is positive and significant coefficient in the whole sample and in the sample of stayers and pre-1997 returnees. This suggests that villages with a high proportion of OCL returnees were more likely to be included in the imidugudu programme. As mentioned above, we include a dummy for OCL returnees in all subsequent regression models.

3.4 Selection of returnees into programme areas

There is very little evidence that those returnees who benefited from the imidugudu programme were selected by the programme officials on any basis (RISD, 1999) However, we address the concern that returning households may have self-selected into displacement and programme villages. 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 3, cols. 4, 5). The coefficient on the age of the head bears a positive and significant coefficient in the sample of all returnees (Table 3, col. 5), suggesting that being 1 year older increases the probability for returnees to re-settle 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. The coefficient on the dummy for OCL is significant in the village level selection equations (Table 3, cols. 1, 3). 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-

[32] 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.

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

groups have experienced differential non-employment rates then skill-depreciation and economic and social pressure associated with conflict induced displacement 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 should not influence output. Overall, those results suggest that there was no selection into displacement, of villages into the conflict, or of returnees into programme areas on the basis of observable characteristics.

4 Regression Analysis

4.1 Input consumption

In this subsection, we use regression analysis to outline the patterns of agricultural input consumption at the household level by displacement and policy status. In doing so, we intend to check that there are no systematic differences in input consumption across groups. Indeed, should there be any strong correlation between input consumption and the treatment variables, it would suggest that displacement and policy status are determinants of input consumption and, therefore, the identification of the treatment effects in subsequent regression analysis would be affected.

Table 4 presents OLS estimates of the effect of displacement, policy, and their interaction on the consumption of agricultural inputs (land, seeds bought, on-farm labour, and adult on-farm participation rate) at the household level. Column (1) presents the unconditional treatment effects, whereas column (2) presents the results conditional on exogenous household characteristics: household head's age and its square, household members' average age, household head's sex, a dummy for widower head, household size, number of adults in the household, average educational attainment for members involved in farm production and for members in charge of the production (years of education, excluding repeated years), average distance to the household's cultivated parcels, and OCL status. We further control for prefecture fixed-effects in column (3).

The results for land allocation are included in the first panel of the table. When no additional controls are included (col. 1), we find that living in a policy area is associated with a significant and positive effect, and that the effect is larger for returnees than for stayers by a factor of 2.5. Being a returnee in non-policy areas, however, does not appear to have any significant impact on access to land. These effects are relatively small, corresponding to an increase in land endowment of a third of a standard deviation for returnees in policy areas. The low R-squared (0.01) associated with this specification indicates that policy interacted with displacement status explains very little of the sample variation. These results are

not significantly affected by the inclusion of household controls (col. 2); however, introducing prefecture fixed-effects (col. 3) washes out all of the effects associated with displacement and villagization. Overall, this suggests that spatial differences and the sorting of policy areas across prefectures account for most of the difference in land allocation across groups. This result has the implication that land-rich prefectures were more likely to be included in the re-settlement programme.

The second panel of Table 4 reports the results for seed consumption. The estimates of the unconditional effects suggest that returnee status and policy status have a significant and positive effect on seed consumption. However, those effects are relatively small, differences on the order of 9 to 31 percent of a standard deviation, and the associated R-squared is very low. The inclusion of additional household characteristics does not significantly affect the coefficients (col. 2), although the coefficient on the policy dummy is now barely significant. Adding prefecture fixed-effects (col. 3) renders the coefficient associated with the returnee status dummy insignificant, but the effect of living in a programme area does not vary. Overall, these results suggest that access to seeds is enhanced in policy areas, although, given the nature of the market for seeds, causality cannot be established on the basis of these results.

Finally, the last two panels of Table 4 contain the results for the average on-farm weekly participation (in hours) *per* adult member and for the on-farm adult participation rate. Comparing the R-squared across the specifications, it is clear that household characteristics account for most of the variations in on-farm participation. Displacement status is associated with adults working fewer hours, in a given week, on the household farm in columns (1) and (2). In contrast, migrants in policy areas are found, in columns (1) and (2), to allocate more hours to on-farm production, completely off-setting the negative effect associated with displacement status. The inclusion of prefecture fixed-effects (col. 3) washes out both effects. Similarly, for on-farm participation rate, whereas a significant and negative effect of displacement is found in column (1) and (2), suggesting that the fall in hours for migrants is due to lower on-farm participation, this effect washes out entirely with the inclusion of prefecture fixed-effects in column (3). Taken together, the main determinants of on-farm labour allocation appear to be household characteristics and, to a lesser extent, local labour markets. The policy and displacement status do not seem to have any appreciable effect once those sources of heterogeneity are accounted for.

Overall, once household characteristics and regional disparities are taken into account, these results indicate no strong systematic differences in inputs consumption across groups. The results also reassure us that simultaneously controlling for input consumption, displacement and policy status is not likely to generate a source of endogeneity in our specifications.

4.2 Agricultural output

4.2.1 OLS and QR estimates

In the first part of this section, we present the result of regressions of households' agricultural production on returnee status, policy status, and a returnee and policy interaction. Our aim is to measure the effect on returnees' agricultural output of conflict-induced displacement and of the villagization programme. As outlined above, because the pilot of the policy was non-randomly distributed across Rwandan prefectures, we try to achieve an appropriate measure of this parameter by additionally controlling for a large set of observable household and unobservable prefecture characteristics. In the second part of the section, we control for production inputs and interact them with all three treatment dummies to measure potential variations in returns to inputs across groups. Moreover, controlling for production inputs is likely to purge any further source of bias from our estimates of the effect of displacement and policy. We report OLS and Quantile Regressions (QR) estimates to account for potential heterogeneity in treatment effects across ranges of the output distribution.[34]

First, we regress agricultural output and yield on dummies for returnee status, policy status, and their interaction, along with household characteristics and prefecture fixed-effects.

$$Q = \alpha_1 + \beta_1 X + \gamma_{11} \operatorname{Re} turnee + \gamma_{12} \operatorname{Im} idugudu + \gamma_{13} \operatorname{Re} turnee * \operatorname{Im} idugudu + \eta_j + \varepsilon_1 \qquad (3)$$

The upper panel of Table 5 reports the effect of displacement and of the villagization policy, and their interaction, on agricultural output at the household level. Columns (1), (3), and (5) are computed on the sample of returnees only, whereas columns (2), (4), and (6) present estimates obtained on the whole sample. The estimates of the unconditional effects are reported in columns (1) and (2), while the estimates conditional on exogenous household characteristics and on prefectures are reported in columns (3) and (4) and columns (5) and (6) respectively.

The measured unconditional effect of displacement is large, positive, and significant, implying an increase in output of 20 percent for returnees relative to stayers in non-policy areas (col. 2, row 1). The unconditional effect of the policy on stayers is insignificant, but the coefficient on the displacement-policy interaction is large, positive and significant and associated with an increase of 30 to 38 percent (cols. 1, 2, row 3). Columns (3) and (4) show that controlling for household characteristics does not significantly affect the point estimates. This corroborates the idea that households were not selected into displacement, nor into policy areas, on the basis of observable characteristics. Columns (5) and (6) show

[34] The use of Quantile Regression estimation in a difference-in-differences analysis is still debated in the literature. Firpo (2004) suggests the use of a semi-parametric quantile regression technique. However, we use the least absolute value model of conditional quantile estimation (Koenker and Bassett, 1978).

that controlling for prefecture fixed-effects removes all significant effect associated to returnee and policy status. The result suggests that the effects measured in columns (1)-(4) were entirely attributable to unobserved heterogeneity across prefectures. Furthermore, it indicates that returnees were more likely to re-settle in prefectures where their expected agricultural output was higher and the pilot imidugudu villages were not randomly scattered across Rwanda but were particularly targeted in places where output was higher.

The results in the lower panel of Table 5 are obtained by replacing agricultural output by yield (output *per* hectare) on the left-hand side. This allows us to understand to what extent disparities in land use might be driving our results. The results present a stark contrast with those obtained in the upper panel of the table. Indeed, the coefficients on returnee and the policy-returnee are now insignificant, whereas the coefficient on policy is now large, negative, and significant, implying a fall in output of 26 percent for returnees and stayers living in policy areas. As was the case for output, with the inclusion of prefecture fixed-effects (cols. 5, 6), the point estimate falls to zero and becomes insignificant. These findings are in line with the results on the determinants of input consumption shown in Table 4. Overall, they suggest that, in order to retrieve an appropriate measure of the effect of displacement and villagization, inputs and prefecture fixed-effects need to be controlled for.

We now regress agricultural output on dummies for displacement, policy, displacement-policy interaction, agricultural inputs, and their interactions with the three treatment dummies, plus observed household and unobserved prefecture characteristics. The estimates are computed using OLS and QR. In formulas,

$$\begin{aligned} Q \quad = \quad & \alpha_2 + X\beta_2' + \gamma_{21}\,\mathrm{Re}\,turnee + \gamma_{22}\,\mathrm{Im}\,idugudu + \gamma_{23}\,\mathrm{Re}\,turnee.1_n'.\,\mathrm{Im}\,idugudu \\ & + Inputs.\delta_{21}' + (1_n.\,\mathrm{Re}\,turnee')'.Inputs.\delta_{22}' + (1_n.\,\mathrm{Im}\,idugudu')' * Inputs.\delta_{23}' \\ & + (\mathrm{Re}\,turnee.\,\mathrm{Im}\,idugudu').Inputs.\delta_{24}' + \eta_k + \varepsilon_2, \end{aligned} \tag{4}$$

where *Inputs* is the matrix of all 3 production inputs: Labour, Seeds, Land, and a dummy for access to an AES in the village; and η_k is the prefecture fixed-effect.

Column (1) of Table 5 reports the results of an OLS regression of output on the three treatment dummies, inputs and their interaction with the three treatment dummies, and the household controls enumerated above.[35] Column (2) shows results from the same specification when prefecture fixed-effects are added. Columns (3) to (5) present the results of QR respectively at the three quartiles (0.25, 0.50, 0.75), on the same specification as shown in column (2).

[35] The square of each input consumption variable is also included, to allow for decreasing marginal returns.

Looking at the coefficients in column (1) associated with inputs (not interacted), we find that they are all positive and significant, in line with what we would expect. Including prefecture fixed-effects (col. 2) does not have an appreciable effect on the point estimates of return to inputs and their interactions.

The interactions between inputs and the treatment dummies provide some insight on the effect of displacement and villagization on returnees' stock of human capital. In column (1), the coefficient on the interaction between own-farm labour and returnee is positive and significant, implying a increase in returns to labour for returnees in all areas by 40 percent. That returnees experience higher returns to labour might suggest that they are more motivated to achieve better livelihoods relative to stayers, all else equal. Moving to the columns (3) to (5), the coefficient on the interaction between labour and the returnee-policy dummy is now negative and significant at the 10 percent level in the first quartile, totally offsetting the increase in returns to labour associated with returnee status (col. 3). Together these results indicates that living in imidugudu might be associated with a form of discouragement for those producing at the lower quartile. However, this coefficient is barely significant and fails to pass the test of individual parameter significance at the conventional levels in all other specifications presented in Table 5.

In column (1), we also find that the coefficient on the interaction between seed consumption and the returnee-policy dummy is negative and significant, associated with returns to seeds 70 percent lower for returnees in policy areas relative to all other groups.[36] The coefficient on the seed-policy interaction is positive and significant (at the 1 percent level) at the median of the distribution (col. 4), and the result is found to be robust to the exclusion of prefecture fixed-effects (results not reported). However, it does not offset the negative differential in returns to seeds for returnees in policy areas, which is still large and now associated with a 55 percent decrease in returns relative to stayers and returnees in non-policy areas. That returns to seeds are found to be lower exclusively for returnees living in imidugudu suggests two things. First, displacement might have lowered returnees' level of agricultural know-how. Indeed, planting seeds involves relatively more know-how than other agricultural tasks, and it might be that returns to seeds capture a differential in the level of agricultural skills across groups. That those lower returns appear to be significant only in policy areas suggest that returnees living in non-policy areas could benefit from positive skill spill-overs from the population of stayers. In contrast, those returnees living in imidugudu seem not to have benefited from such positive externalities, suggesting that the policy might have had a form of "ghetto effect" on the population of returnees.

The coefficient on the land-returnee-policy interaction is positive and significant in the first two quartiles of the distribution (cols. 3, 4), associated with an increase in returns respectively by 43 and

[36] That the coefficient on the interaction between seed consumption and the dummy for returnee is positive and significant at the 10 percent level is more likely to be a case of data mining, as it is not robust to slight variations in the specification (col. 3). For instance, the effect disappears with the exclusion of prefecture fixed effects (results not reported).

48 percent relative to stayers in all areas and to returnees in non-policy areas. As the data does not contain any soil quality index at the household level and the policy effect is identified using village-level variations, we cannot test whether our findings of lower returns to seeds for returnees in policy areas are the result of systematic variations in soil quality across groups.[37] However, this result tends to indicate that, if anything, the land allocated to returnees in imidugudu villages producing in the first two quartiles of the distribution is of relatively higher quality. This suggests that the estimated lower returns to seed truly are the result of variations in the stock of know-how across groups.

Although the coefficient on the dummy for access to an AES is not significant in columns (1) and (2), the coefficient on its interaction with the returnee-policy dummy is positive and significant, implying an increase in output by on average 50 percent for returnees living in imidugudu and having access to an AES in their village. However, very little can be inferred from the data as to the quality of those AES. It might be that this effect is entirely attributable to a systematic difference in the quality of the service provided to the farmers through those AES across policy and non-policy areas. In columns (3) to (5), the dummy for access to an AES is now positive and significant in the first quartile, associated with an increase in output by 13 percent. Moreover, the access to an AES-returnee-policy interaction is now positive and significant only at the median of the distribution. Whereas these results tend to nuance the conclusion drawn from the OLS regression that villagization enhanced the benefit of an access to an AES infrastructure for returnees, it demonstrates that access to such service is production-enhancing for all groups and particularly for those producing low levels of output.

In column (1), the coefficients on all three dummies are now insignificant, suggesting that controlling for inputs absorbs all the heterogeneity picked up by the treatment dummies in the upper panel of Table 5, columns (1)-(4). In column (2) (and also (3) to (5)[38]), the coefficient on the dummy for returnee is, however, now negative and significant at the 10 percent level. This suggests that, once differences in input consumption are controlled for, along with unobserved prefecture heterogeneity, returnees' unobserved heterogeneity is associated with 22 to 32 percent lower output. This large effect indicates that conflict-induced displacement is associated with a loss in output which was not affected by the pattern of re-settlement. Moreover, that it does not translate into a difference in returns to production inputs suggests that this effect does not result from a loss of human capital, but from sociological pressures.

[37] Even in the case where we could identify the policy treatment at the household level, including village fixed-effects to our specification would most likely not solve this issue, as within-village variations in soil quality are known to be quite large.

[38] The negative effect associated with returnees' unobserved heterogeneity is significant at the five percent level in the first quartile.

4.2.2 2SLS and IVQR estimates

This subsection presents the effect of displacement, the villagization policy, and the effect of the policy on the returnees conditional on household characteristics, input consumption, and prefecture fixed-effects, using instrumental variable estimation (IV-2SLS and IVQR) to control for potential sources of endogeneity bias in the input-output specification. Whereas the consumptions of on-farm labour participation and land are considered as "fixed" inputs in the agricultural household literature (Singh *et al.*, 1986), *i.e.* fairly inelastic to changes in output over a given period, seed consumption is a flexible input. For instance, higher ability farmers might be more/less able to save seeds for the next season relative to the lower ability farmers. Market seed consumption and output would then be negatively/positively correlated, and our estimates of returns to seeds downward/upward biased. We are concerned that the coefficient on seeds (and other covariates) as estimated in columns (1) to (5) in Table 5 is inconsistently estimated. In order to achieve a more appropriate measure of this parameter, we use instrumental variable estimation, choosing the level of seed consumption aggregated at the village level for our exclusion restriction.[39] 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.[40] 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 village level seed consumption, unless some natural disaster should occur within the village.

In practice, we estimate (4), using the predicted values for seeds as obtained from regressing seed consumption on the aggregate level of seed consumption at the village level, along with all the controls included in (4). In formulas, [41]

[39] Using distance to market would also be a candidate to serve as IV here. However, almost all villages have a market, and, since distance to market was surveyed at the village level in kilometers, most have a zero value, which does not allow us to identify any strong correlation with seed consumption.

[40] 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.

[41] The F-statistic from the first-stage estimation (not reported) is 23.16, which suggests that the instrument is valid (Staiger and Stock, 1994).

$$Seeds = \alpha_3 + \rho_3 Seeds_{.jk} + W\Gamma_3' + \eta_k' + \varepsilon_3, \tag{5}$$

where W is the set of controls also included in the second stage and $Seeds_{.jk}$ the seed consumption at the village level.

Columns (6) and (7) in Table 5 present the 2SLS estimates of the effect of displacement and policy on agricultural output and returns, when seed consumption (and its interactions with the treatment dummies) is instrumented using (5). The results are very much in line with those obtained with OLS (cols. 1, 2), except for the coefficients on seeds. Indeed, the coefficient on seed consumption (not interacted) has increased by 50 percent, although the precision of the estimates does not allow us to tell them apart. The coefficient on the interaction between seed consumption and the returnee-policy dummy is now imprecisely estimated and non-significantly different from zero. Rather interestingly, these results suggest that the unobserved effect removed from our estimates through the use of IV on seeds –which could, for instance, relate to farmer ability– is negatively correlated to seed consumption.

Columns (8) to (10) report the results obtained by estimating the same specification, but using the method of instrumental variable quantile regression (IVQR) suggested by Chernozhukov and Hansen (2005a)[42] generalised to accommodate one endogenous variable and its three interactions with returnee status, policy status, and the returnee-policy interaction.[43]. The results are also very much in line with those obtained using QR (cols. 3-5) and 2SLS (cols. 6, 7); however, some differences are worth noting. Particularly, the coefficient on seed consumption has increased at the median (col. 9), suggesting that the IV removed some negative correlation between the unobserved effect and seed consumption, but it has decreased in the first and last quartile (cols. 8, 10), relative to the QR estimation (col. 3, 5). This indicates that the sign of the correlation between the unobserved effect and seed consumption switches across quartiles of the distribution of yield. The coefficient on the seed-policy interaction estimated at the median is now negative and significant at the 10 percent level, a rather large change from the QR estimation, where the coefficient was positive and significant at the 1 percent level. The interaction between seeds and the returnee-policy dummy are insignificant in all quartiles, in line with the 2SLS results. The interaction between land and the returnee-policy dummy is positive and significant at the 10 percent level in the first quartile of the distribution.

[42] The standard errors are computed, as suggested by Chernozhukov and Hansen (2005a) using Powell's method (1986).

[43] Other empirical studies that use this method of IVQR are Chernozhukov and Hansen (2001, 2005b), Hausman and Sidak (2004) , and Januszewski (2004).

5 Summary and Conclusion

Using the first large household survey data collected after the 1994 genocide in Rwanda, this study attempts to measure the effect of conflict-induced displacement and of a re-settlement policy in post-war Rwanda. Conflict-induced displacement is used as an exogenous measure of the incidence of conflict to measure the impact of the 1994 Rwandan genocide on subsistence household agricultural output. The imidugudu post-conflict re-settlement and land redistribution policy randomly targeted returnees in post-war Rwanda and is exploited as a source of exogenous variations in the pattern of re-settlement to measure the extent to which differences in patterns of re-settlement can account for differences in output and in skill spill-over mechanisms between returnees and stayers. We employ difference-in-differences, using stayers as a control group to account for potential systematic heterogeneity across policy and non-policy areas. We estimate both the average and quartile treatment effects of displacement and villagization on agricultural output and returns, controlling for production inputs along with a large array of observed household and unobserved prefecture characteristics. As seed consumption is likely to be endogenous to the level of agricultural output, 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 IVQR.

First, this study provides several insights on the effects of displacement in post-war Rwanda. We contribute to the understanding of the spatial sorting of conflict-induced migrants upon return, as we find that returnees are more likely to re-settle in the "more productive" areas of Rwanda. We also find that returns to on-farm labour are consistently found to be significantly higher for returnees in all specifications and across ranges of the distribution of output. This result is consistently observed in all proposed regression models, on average as well as in all three quartiles of the distribution of output. One speculative interpretation is that returnees are more motivated in achieving better economic performance, relative to stayers. Controlling for prefecture heterogeneity and input consumption, the unobserved heterogeneity associated with conflict-induced displacement is found to have a negative and significant effect on agricultural output in all areas. As this effect does not translate into differentials in returns to input, it tends to suggest that it is the result of negative sociological pressures, as opposed to a human capital effect. However this effect is not robust to the exclusion of prefecture fixed-effect, and only significant at the 10 percent level.

Second, this paper contributes to the assessment of the imidugudu policy. Although the unconditional estimate of the effect of villagization on output and access to inputs is positive and significant, this effect goes to zero with the inclusion of prefecture fixed-effects. This suggests that, although villagization in Rwanda might have had, during its pilot years, a positive effect on agricultural output, this positive effect

is entirely attributable to regional differences, and, therefore, we contend that extending the programme nationwide would, most likely, not prove production-enhancing. OLS and QR estimates imply that returnees in policy areas experience, at all quartiles of the distribution, lower returns to seeds than all other groups. It suggests that displacement had a negative impact on returnees' stock of agricultural know-how. However, this appears to have been counterbalanced, in non-imidugudu areas, by positive skill spill-overs from stayers to returnees. This tends to corroborate the idea that villagization reduced skill spill-over flows from stayers to returnees relative to non-programme areas, creating a form of a "ghetto effect". However, those results are not confirmed when seed consumption is instrumented for. Finally, most specifications suggest that the programme increased, on average, the positive effect of the provision of an agricultural extension service on output. This might indicate that villagization increased, by grouping parcels of land together, the use of more mechanised production techniques. Nevertheless, our design does not allow us to reject that the agricultural extension services provided in imidugudu villages might be of higher quality relative to the non-policy areas.

Overall, this study sheds some light on the effects of conflict-induced displacement and of the villagization policy on household agricultural output in rural Rwanda. We find that returnees experience higher returns to on-farm labour relative to stayers, possibly implying a higher level of motivation to increase their economic performance than their stayer counterparts. Moreover, we find that the effect of villagization, as it was implemented in post-war Rwanda, had no appreciable effect on returnees' agricultural output, suggesting that extending the programme to the whole of Rwanda is unlikely to prove production-enhancing.

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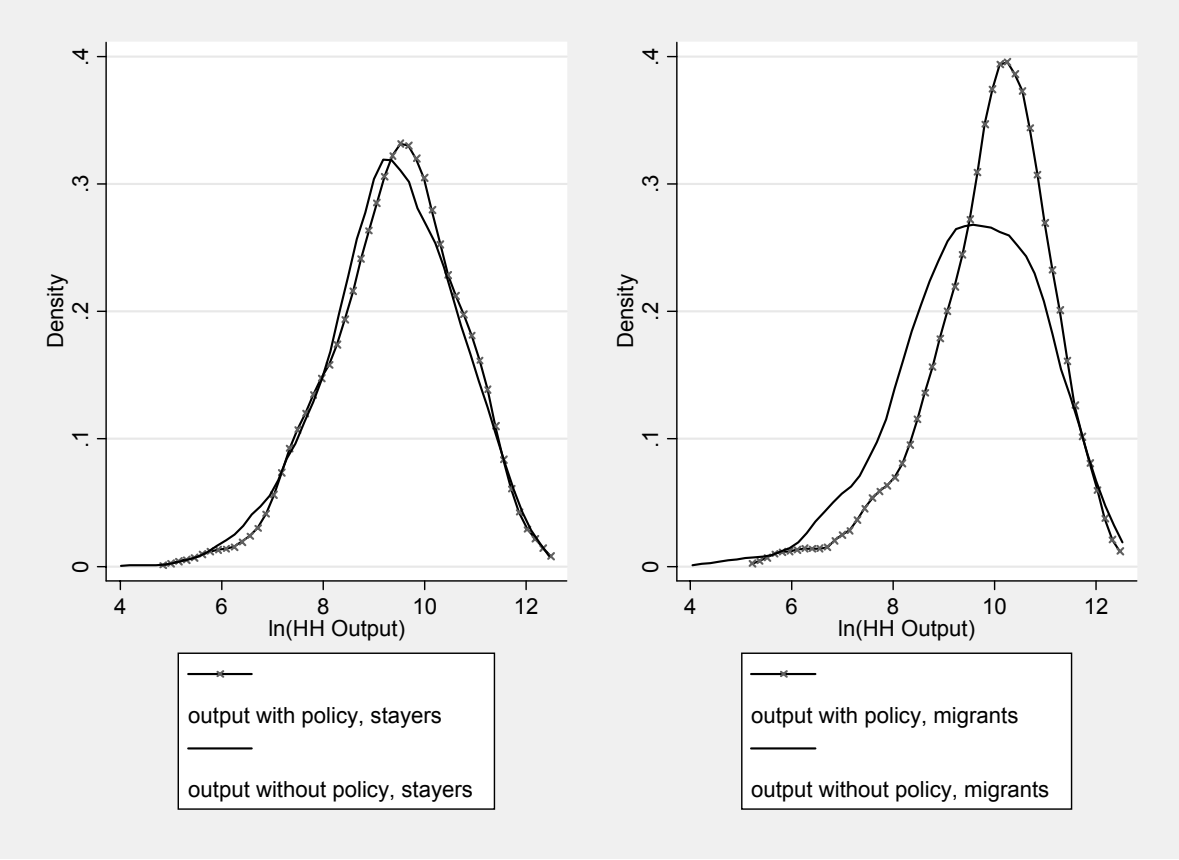

Figure 1: Kernel density estimates of the natural log of household agricultural output, by migration status, and policy regime.

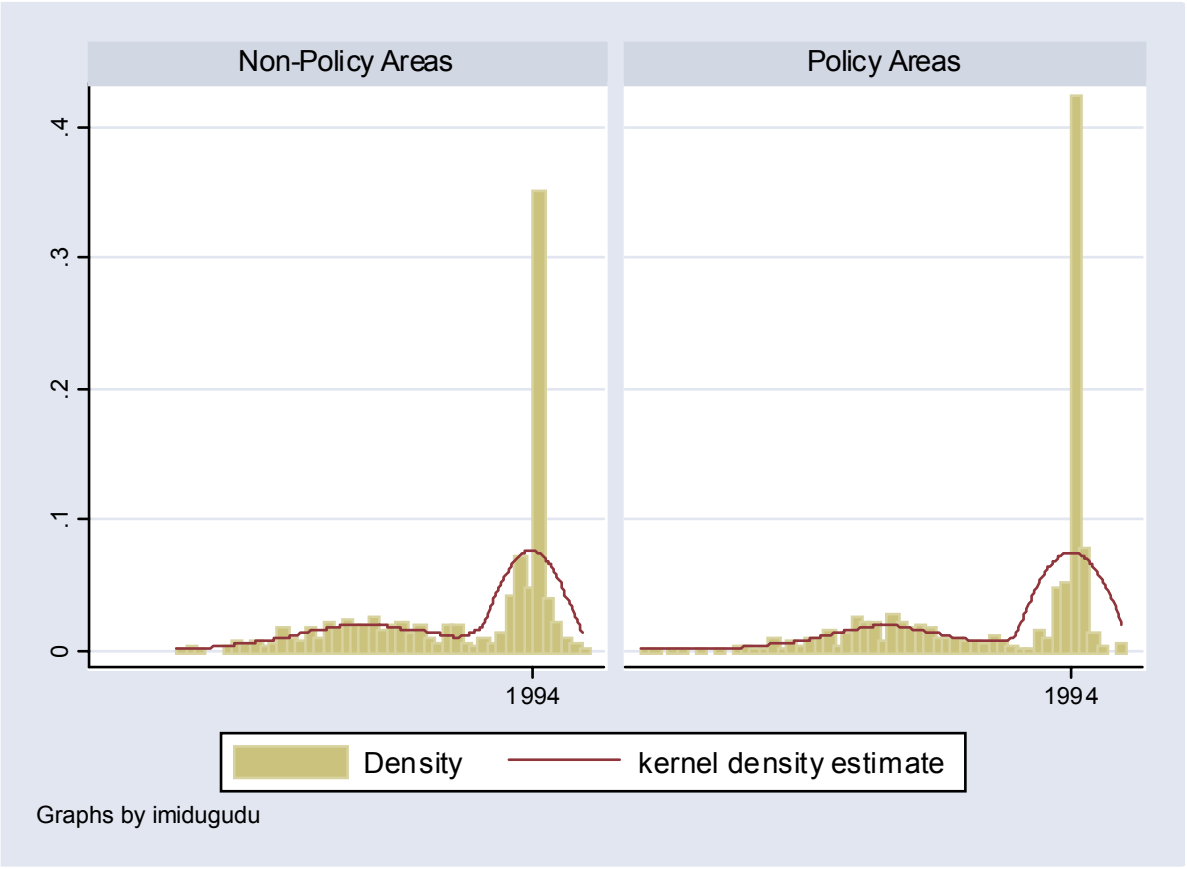

Figure 2: Histogram and kernel density estimate of year post-1994 returnees left the country.

Table 1: Policy coverage and proportion of returnees across Rwandan prefectures.

	Communes without Policy			***Communes with Policy***		
	Freq.	Percent	Percent of Returnees	Freq.	Percent	Percent of Returnees
Butare	30	10.03	32.66	10	7.41	21.62
Byumba	32	10.70	34.60	8	5.93	57.30
Cyangugu	32	10.70	7.27	7	5.19	20.73
Gikongoro	37	12.37	14.12	3	2.22	45.45
Gisenyi	35	11.71	2.75	5	3.70	1.75
Gitarama	26	8.70	7.00	14	10.37	9.76
Kibungo	4	1.34	7.78	36	26.67	74.55
Kibuye	35	11.71	9.27	5	3.70	13.79
Kigali Ngali	23	7.69	10.61	16	11.85	8.28
Ruhengeri	33	7.36	14.45	18	13.33	14.55
Umutara	23	7.69	71.20	13	9.63	73.57
Total	299	100.00	Mean: 54.29	135	100.00	Mean: 45.71

Table 2: Logistic regression at the household level of post-94 returnee status on household characteristics.

Dependent: Returnee	(1) All	(2) No Policy	(3) 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 displacement status at the household level. Standard errors clustered at the village level in parentheses; * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3: Logistic regression of the effect of household and commune characteristics on policy status.

Dependent: Imidugudu	(1) Whole sample	(2) Stayers	(3) Stayers & returnees before 97	(4) Returnees after 97 only	(5) 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 education	-0.205	-0.160	-0.138	0.055	-0.090
	(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 living within the village	0.021	--	0.020	--	--
	(0.231)		(0.224)		
More arrivals in village after 94	-0.080	-0.092	-0.090	-0.097	-0.136
	(0.125)	(0.130)	(0.123)	(0.184)	(0.161)
More departures from village aft 94	-0.073	-0.073	-0.082	-0.074	-0.197
	(0.125)	(0.129)	(0.122)	(0.182)	(0.159)
No moves to/from village aft 94	-0.087	-0.072	-0.090	-0.139	-0.119
	(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 before 94	-0.053	-0.060	-0.052	0.019	0.040
	(0.064)	(0.066)	(0.062)	(0.110)	(0.090)
Health centre in village before 94	0.036	0.054	0.035	0.157	0.082
	(0.107)	(0.116)	(0.105)	(0.212)	(0.145)
Road through village before 94	0.093	0.116	0.099	0.157	0.142
	(0.071)	(0.073)	(0.070)	(0.116)	(0.105)
Water in village before 94	-0.035	-0.034	-0.026	-0.204**	-0.080
	(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 4: Input Consumption: the effects of migration and villagization.

	(1)	*(2)*	*(3)*
	Land	Land	Land
Returnee	0.026	0.068	-0.019
	(0.046)	(0.061)	(0.059)
Imidugudu	0.122*	0.132+	0.048
	(0.050)	(0.073)	(0.069)
Returnee*imidugudu	0.185*	0.146	0.067
	(0.080)	(0.097)	(0.089)
Other controls		✓	✓
Prefecture Fixed-Effects			✓
Observations	4907	4907	4907
R-squared	0.01	0.05	0.10
Mean (SD)	0.720 (0.921)	--	--
	Seeds	Seeds	Seeds
Returnee	2.925**	2.196*	1.027
	(0.780)	(1.096)	(1.110)
Imidugudu	1.170*	1.625+	1.640+
	(0.580)	(0.933)	(0.869)
Returnee*imidugudu	-2.008	-2.541	-1.870
	(1.269)	(1.581)	(1.712)
Other controls		✓	✓
Prefecture Fixed-Effects			✓
Observations	4907	4907	4907
R-squared	0.01	0.04	0.05
Mean (SD)	3.598 (13.180)	--	--
	On-farm Labour	On-farm Labour	On-farm Labour
Returnee	-1.449**	-1.126*	-0.741
	(0.416)	(0.554)	(0.544)
Imidugudu	-0.879*	-0.370	-0.536
	(0.424)	(0.589)	(0.587)
Returnee*imidugudu	1.925**	1.721*	0.879
	(0.706)	(0.789)	(0.774)
Other controls		✓	✓
Prefecture Fixed-Effects			✓
Observations	4907	4907	4907
R-squared	0.00	0.23	0.25
Mean (SD)	24.312 (11.373)	--	--
	On-farm Particip. Rate	On-farm Particip. Rate	On-farm Particip. Rate
Returnee	-0.031**	-0.019+	-0.011
	(0.009)	(0.012)	(0.013)
Imidugudu	-0.002	0.007	0.008
	(0.008)	(0.010)	(0.010)
Returnee*imidugudu	-0.011	0.003	-0.008
	(0.019)	(0.018)	(0.018)
Other controls		✓	✓
Prefecture Fixed-Effects			✓
Observations	4907	4907	4907
R-squared	0.01	0.16	0.17
Mean (SE)	0.88 (0.003)	--	--

Notes: OLS estimates of the effect returnee status, policy status, and their interaction, on the consumption of agricultural inputs. The first panel presents the results for land (measured in hectares), the second, annual seed consumption (in 1000 of Rwandan francs), the third, on-farm labour (weekly average per adult member), and the fourth, adult on-farm participation rate at the household level. Column (1) presents the unconditional treatment effects (no additional controls), whereas column (2) presents the results conditional on arguably exogenous household characteristics: household head's age and its square, household members' average age, household head's sex, a dummy for widower head, household size, number of adults in the household, average educational attainment for members involved in farm production and for members in charge of the production (years of education, excluding repeated years), average distance to the household's cultivated parcels, OCL status ; we further control for prefecture fixed-effects in column (3). Standard errors (in parentheses) are clustered at the village level; † significant at 10%; * significant at 5%; ** significant at 1%.

Table 5: Simple difference-in-differences specification.

Variables	*Returnees Only*	*Whole Sample*	*Returnees Only*	*Whole Sample*	*Returnees Only*	*Whole Sample*
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent:	**Natural**	**log of**	**Agricultural**	**Output**		
Returnee	--	0.197*	--	0.235*	--	-0.091
		(0.094)		(0.107)		(0.095)
Imidugudu	0.377**	0.073	0.315**	0.088	0.047	0.056
	(0.120)	(0.079)	(0.119)	(0.078)	(0.131)	(0.077)
Returnee *	--	0.303*	--	0.231†	--	0.008
Imidugudu		(0.126)		(0.123)		(0.123)
HH controls			✓	✓	✓	✓
Prefect. FE					✓	✓
Sample Size	1247	4907	1247	4907	1247	4907
R-squared	0.02	0.02	0.09	0.09	0.20	0.16
Dependent:	**Natural**	**log of**	**Agricultural**	**Yield**		
Returnee	--	0.016	--	0.040	--	-0.037
		(0.106)		(0.124)		(0.111)
Imidugudu	-0.331*	-0.265*	-0.344*	-0.260*	-0.129	-0.076
	(0.144)	(0.130)	(0.144)	(0.130)	(0.167)	(0.125)
Returnee *	--	-0.066	--	-0.065	--	-0.008
Imidugudu		(0.167)		(0.166)		(0.153)
HH controls			✓	✓	✓	✓
Prefect. FE					✓	✓
Sample Size	1247	4907	1247	4907	1247	4907
R-squared	0.01	0.01	0.03	0.01	0.17	0.15

Notes: Columns (1), (3), and (5) present the estimates of the villagization policy effect on returnees only; columns (2), (4), and (6) present the impact of returnee status, policy status, and an interaction on the whole sample. Columns (1) and (2) present the unconditional effects; household controls are included in columns (3)-(6), and include: household head's age and its square, household members' average age, household head's sex, a dummy for widower head, household size, number of adults in the household, average educational attainment for members involved in farm production and for members in charge of the production (years of education, excluding repeated years), average distance to the household's cultivated parcels, OCL status ; prefecture fixed-effects are introduced in columns (5) and (6). All regression specifications are estimated using OLS, and standard errors (in parentheses) are clustered at the village level; † significant at 10%; * significant at 5%; ** significant at 1%.

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

Variable			**QR**					**IVQR**		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**
	OLS	**OLS**	**Quant. 0.25**	**Quant. 0.50**	**Quant. 0.75**	**2SLS**	**2SLS**	**Quant. 0.25**	**Quant. 0.50**	**Quant. 0.75**
Returnee	-0.055	-0.276†	-0.390*	-0.247†	-0.283†	-0.294	-0.368†	-0.434†	-0.506*	-0.301
	(0.165)	(0.152)	(0.166)	(0.134)	(0.153)	(0.201)	(0.193)	(0.229)	(0.246)	(0.243)
Imidugudu	0.011	-0.055	-0.136	0.012	-0.103	0.054	-0.053	-0.089	0.100	-0.096
	(0.140)	(0.143)	(0.148)	(0.115)	(0.132)	(0.158)	(0.155)	(0.201)	(0.143)	(0.149)
Returnee*Imidugudu	0.216	0.077	0.407	0.010	0.015	0.303	0.139	0.331	0.040	-0.016
	(0.233)	(0.233)	(0.260)	(0.208)	(0.241)	(0.274)	(0.265)	(0.322)	(0.382)	(0.381)
Labour	0.037**	0.036**	0.040**	0.036**	0.025**	0.041**	0.041**	0.050**	0.040**	0.029**
	(0.008)	(0.007)	(0.009)	(0.007)	(0.008)	(0.008)	(0.008)	(0.011)	(0.008)	(0.009)
Labour*Returnee	0.015*	0.012†	0.015†	0.015*	0.014†	0.026**	0.017*	0.019†	0.020*	0.017†
	(0.007)	(0.007)	(0.008)	(0.007)	(0.008)	(0.009)	(0.009)	(0.011)	(0.010)	(0.009)
Labour*Imidugudu	0.007	0.007	0.013†	0.004	0.006	0.005	0.007	0.010	0.003	0.006
	(0.006)	(0.006)	(0.008)	(0.006)	(0.007)	(0.006)	(0.006)	(0.008)	(0.007)	(0.006)
Labour*Returnee	-0.015	-0.012	-0.026†	-0.012	-0.010	-0.019	-0.017	-0.019	-0.010	-0.011
*Imidugudu	(0.011)	(0.010)	(0.013)	(0.011)	(0.012)	(0.012)	(0.012)	(0.016)	(0.014)	(0.013)
Seeds	0.041**	0.040**	0.047**	0.042**	0.037**	0.061**	0.050*	0.043†	0.049*	0.031*
	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.022)	(0.020)	(0.023)	(0.024)	(0.014)
Seeds*Returnee	0.003	0.003	0.008†	0.005	-0.000	0.029	0.008	0.008	0.076	-0.001
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.018)	(0.019)	(0.030)	(0.064)	(0.029)
Seeds*Imidugudu	0.007	0.007†	0.003	0.013**	0.005	-0.008	-0.002	-0.000	-0.040†	0.006
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.021)	(0.018)	(0.034)	(0.0.24)	(0.023)
Seeds*Returnee	-0.029**	-0.029**	-0.029**	-0.036**	-0.018**	-0.022	-0.023	-0.029	0.017	-0.019
*Imidugudu	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.029)	(0.026)	(0.042)	(0.116)	(0.070)
Land	0.437**	0.463**	0.525**	0.481**	0.420**	0.403**	0.422**	0.475**	0.415**	0.433**
	(0.049)	(0.042)	(0.050)	(0.041)	(0.044)	(0.056)	(0.048)	(0.069)	(0.056)	(0.052)
Land*Returnee	-0.037	-0.057	-0.082	-0.063	0.048	-0.141	-0.091	-0.151	-0.127	-0.039
	(0.099)	(0.095)	(0.094)	(0.068)	(0.068)	(0.106)	(0.105)	(0.117)	(0.116)	(0.111)

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... table 6 continued

Variable	(1) OLS	(2) OLS	QR (3) Quant. 0.25	QR (4) Quant. 0.50	QR (5) Quant. 0.75	(6) 2SLS	(7) 2SLS	IVQR (8) Quant. 0.25	IVQR (9) Quant. 0.50	IVQR (10) Quant. 0.75
Land*Imidugudu	-0.009	-0.017	-0.026	-0.024	-0.003	0.036	-0.011	-0.036	0.020	-0.022
	(0.050)	(0.046)	(0.061)	(0.049)	(0.055)	(0.063)	(0.064)	(0.092)	(0.076)	(0.047)
Land*Returnee	0.168	0.161	0.226†	0.220*	0.053	0.190	0.184	0.255†	0.149	0.156
*Imidugudu	(0.123)	(0.116)	(0.126)	(0.099)	(0.102)	(0.133)	(0.131)	(0.154)	(0.169)	(0.145)
AES	0.038	0.025	0.126*	0.055	-0.018	0.030	0.026	0.107	0.068	-0.010
	(0.082)	(0.076)	(0.063)	(0.050)	(0.058)	(0.082)	(0.076)	(0.075)	(0.056)	(0.060)
AES*Returnee	-0.016	0.010	0.048	-0.015	-0.046	-0.015	-0.012	0.058	0.063	-0.111
	(0.146)	(0.131)	(0.147)	(0.117)	(0.135)	(0.156)	(0.136)	(0.194)	(0.143)	(0.147)
AES*Imidugudu	-0.224	-0.090	-0.183	-0.091	-0.010	-0.225	-0.070	-0.135	-0.097	0.011
	(0.150)	(0.137)	(0.122)	(0.096)	(0.112)	(0.151)	(0.136)	(0.140)	(0.108)	(0.103)
AES*Returnee	0.503*	0.405*	0.355	0.318†	0.343	0.525*	0.406†	0.301	0.239	0.377†
*Imidugudu	(0.214)	(0.205)	(0.232)	(0.185)	(0.214)	(0.227)	(0.209)	(0.265)	(0.207)	(0.199)
Prefecture FE	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Observations	4907	4907	4907	4907	4907	4907	4907	4907	4907	4907

Notes: All columns of the table present the estimates of a regression of output on dummies for returnee, villagization and their interaction, production inputs and their interactions with the 3 treatment dummies. Household controls (cf. Notes to Table 5) are included in all specifications. Prefecture fixed-effects are included in columns (2), and (7)-(10). Columns (1), (2), report the OLS estimates, whereas columns (3) to (5) report the estimates from QR at the 0.25, 0.50, and 0.75 quantiles of agricultural output. Columns (6) and (7) presents the results of IV-2SLS estimation of the same specification, instrumenting for seed consumption; columns (8)-(10) reports the IVQR estimates at the 0.25, 0.50, and 0.75 quantiles of agricultural output, instrumenting for seed consumption. Standard errors are clustered at the prefecture level in the OLS and IV-2SLS estimations, boostrapped in the QR estimation, and computed using Powell's (1986) method in the IVQR estimation. Significance levels: † : 10%, * : 5%, ** : 1%.

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