The Intergenerational Effects of Refugee Camps: Haiti, 1937–2009

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June 30, 2022

Abstract

With so many refugees in the world settling into camps, it is important to understand the camps' effects on the refugees' descendants. I examine the short- and long-run effects of refugee camps by looking at camps established after the 1937 massacre of Haitians living in the Dominican Republic. In the short-run, refugee camps changed land-use and increased tax revenues. In the long-run, 70 years later, the differences in land-use persist, wealth converges, and the descendants have higher social capital.

*I appreciate helpful comments from Leticia Abad, Jennifer Alix-Garcia, Kara Dimitruk, Brian Leonard, Joshua Mask, Noel Maurer, Colette Salemi, Emily Sellars, Marianne Wannamaker and participants at the Eastern Economic Association Conference.

Today, hundreds of thousands of people are displaced annually, with 86% of them moving as refugees to developing countries (UNHCR 2022). Since these destinations have limited resources, the refugees frequently do not integrate with the population and settle in camps. Recent studies of refugee camps have found that they tend to positively affect the local economy (Alix-Garcia et al. 2018, Maystadt and Verwimp 2014), though they do impose some costs (Anti and Salemi 2021). Yet there are two important questions that this literature has not been able to answer. First, do the camps have large effects without international aid? Camps provide easy sites to coordinate international aid, and many of the positive effects of refugee camps can be traced to the NGO personnel and donations that flow to these areas. Second, what are the intergenerational effects of refugee camps? Most of the work is limited to observing refugee camps within the first 15 years of operation. While this gives us a good sense of the short- and medium-run economic impact, they can not answer whether refugee camps continue to impact the next generation.

In this paper, I address both questions by looking at the long-run effects of refugee camps established in Haiti in 1938. In October 1937, the Dominican Republic massacred thousands of Haitians living in its borders. The massacre was sudden (occurring over a few days) and unexpected, and it lead to a mass exodus from the country. Although they did not receive international aid, many of the refugees settled near the border in government-sanctioned camps. Using historical and modern data, I can observe the refugee camps' short-run (12 years) and long-run (70 years) impacts on the surrounding area.

First, I examine the camps' short-run effects on land-use. During this period, idle public land was available to rent for tenants willing to improve it. Using both difference-in-differences and synthetic difference-in-differences strategies, I show that the refugee camps caused requests to rent public land to increase 8–10 times in surrounding areas. Since most of these requests were for small agricultural properties, it looks like the camps caused an increase in subsistence farming, probably from refugees starting new lives in Haiti.

Next, I examine camps' long-run effects in 2009. Seventy years later, areas within 20 km of a refugee camp still have three times as many government rental properties. These areas show no discrepancies in wealth (as proxied by livestock) and they have equal access to amenities. Interestingly, the areas around camps in 2009 had higher levels of social cooperation. To address the endogeneity of camp placement, I construct an instrumental variable from the population shifts in the Dominican Republic between 1935 and 1950. The IV analysis confirms the OLS results.

Haiti's camps are useful to study because of the unique characteristics relative to modern camps.

Many of the positive and negative effects found around refugee camps come from international aid. For example, NGOs create jobs in and around camps and increase demand for local goods, plus recipients of in-kind donations can sell them to non-refugees (Alix-Garcia et al. 2018, Maystadt and Verwimp 2014). On the other hand, these donations and high-income competition might create local price shocks that decrease real incomes for nearby residents (Alix-Garcia and Saah 2009, Anti and Salemi 2021). The camps in Haiti show that even without aid, camps have local economic impacts. Similarly, many countries today restrict refugees to camps, prohibiting them from integrating into the larger economy. In Haiti, on the other hand, there were no mobility restrictions. While refugee camps might create local effects just because the institutions prevent economic activity from spreading wider, Haiti's camps show that they affect the economy even when refugees are free to move. This suggests that the placement of refugee camps affects future outcomes through path dependence, like cities that today exist around former portage cites (Bleakley and Lin 2012). Finally, the refugees in Haiti were co-ethnic and low-skill. A consistent finding across countries is that refugees that are co-ethnic and similar in skill to the receiving population are most likely to have large negative effects (Bohnet et al. 2021, Calderón-Mejía and Ibáñez 2016, Depetris-Chauvin and Santos 2018).[1]

1 Migrant Labor and the Trujillo Massacre

During the early 1900s, tens of thousands of Haitians migrated to other countries to work on sugar plantations. Though the actual number of migrants was impossible to document, about 100,000 went to the Dominican Republic each year (State Department 1924), and between 10,000 and 25,000 traveled to Cuba (Haiti Bureau du representant fiscal 1926 p. 96). These countries offered workers wages three to seven times higher than they could get in Haiti (Haiti Bureau du representant fiscal 1926 p. 97), and some companies were even willing to pay the travel costs and a bond to insure the migrant's return (State Department 1924). At its peak, the labor flows to Cuba and the DR were equivalent to about 20% of Haiti's prime-age (25-55) male workforce. Work opportunities decreased in the 1930s as Cuba decided to stop migrant labor from Haiti, but this shock was partially absorbed by the large demand for labor in the Dominican Republic (Smith 2009 p. 30).

Then the opportunities in the DR ended. In October 1937, without warning, the Dominican Republic's President Rafael Trujillo sanctioned the slaughter of Haitians living in the DR. The exact

[1] See also Maystadt and Verwimp (2014) for the effect of refugees with similar skills but are not co-ethnic.

Table 1. Distribution of Haitians in Dominican Republic by Province, 1935 and 1950

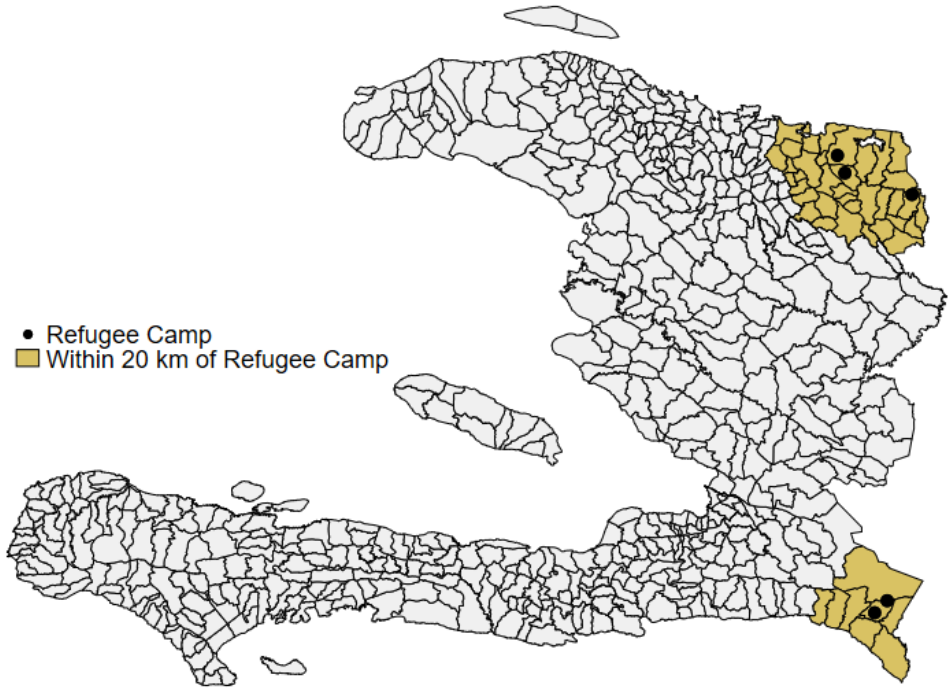
Relative to Haiti	Province	1935	1950	Change	% Change
Border	Barahona	7,327	1,658	-5,669	-77%
	Independencia	1,491	648	-843	-57%
	Libertador	2,444	1	-2,443	-100%
	Montecristi	1,372	2	-1,370	-100%
	San Rafael	3,442	4	-3,438	-100%
Near Border	Bahoruco	9,647	2,989	-6,658	-69%
	Benefactor	1,785	20	-1,765	-99%
	Puerto Plata	2,313	226	-2,087	-90%
	Santiago	1,255	14	-1,241	-99%
East	All Interior Provinces	21,584	13,210	-8,374	-39%
Total		52,660	18,772	-33,888	-64%

number of deaths is unknown, and estimates vary widely; however, the most reasonable estimates count between 12,000 (Vega 1988) and 15,000 (Heinl et al. 1996 p. 482; Smith 2009 p. 31) deaths over a few days. The massacre came as a complete surprise. Prior to the Trjuillo massacre, the Dominican Republic and Haiti experienced border issues, but they settled it diplomatically (Roorda 1996). Just months before the massacre, the DR began limiting Haitian migration (Smith 2009 p. 30), but since at the same time Cuba was deporting thousands of Haitians (McCleod 1998), the policies were not unusual. Although some have attributed the massacre to economic and racial causes, the consensus is best expressed by Turits (2003), "What caused Trujillo to order the 1937 massacre will probably remain forever obscure" (p. 179).[2]

The massacre triggered an exodus from the DR. It was the refugees entering the country that first notified the Haitian government that the massacre had occurred, and later the government spent $40,000 ($726,000 in 2019) to repatriate citizens living in the DR (Haiti Bureau du representant fiscal 1938, p. 75). In one instance, refugees leaving on one of these buses were approached by sugar company recruiters offering higher wages for them to stay, but only three of the 2,000 passengers stayed (Vega 1988). Even Dominicans of Haitian descent abandoned land and livestock to avoid the risk of death (Turits 2003, Palmer 1976). This exodus significantly reduced the DR's Haitian population. In 1935, the Dominican census counted almost 53,000 Haitians in the country,

[2]See also Heinl et al. 1996 p. 482 and Smith 2009 pp. 30–31 .

Figure 1. Refugee camps and treatment areas



but the 1950 Census found fewer than 19,000. The loss occurred throughout the country, but, as seen in Table 1, the border areas lost 90–100% of their Haitian populations.

As the refugees entered Haiti, they settled in government-sanctioned camps, but the camps' success was unclear. Although the government was unprepared to receive the thousands of refugees (Haiti Bureau du representant fiscal 1938, p 89), it created five refugee camps (see Figure 1) and committed $250,000 ($4.5 million in 2019) for transferring public land to private holdings and for providing public works in the camps and surrounding areas.[3] After the first four months, President Vincent was satisfied with the camp's progress in giving refugees homes and 3-4 ha of land with a third of a hectare already planted with subsistence crops (Vincent 1938 pp. 219–225). According to Vincent, the government had distributed seeds and agricultural tools, established agricultural extension stations, and built roads to connect camps to markets. He further outlined plans to build chapels, schools, and pharmacies, as well as a plan for establishing social cohesion. But, Pierre-Charles (1965 pp. 111–112) claims the government only gave 4,400 ha of land to 1,425 families and never established a sustainable, long-term plan. The camps were soon neglected, leaving behind refugees in undesirable territory. The refugees, however, stayed and could be found there decades later (Derby and Turits 1993).

[3]Le Moniteur, 14 March 1938

While there are no definitive estimates of the number of refugees who settled in or around the camps, the range of estimates suggests a significant population shock to the area. The best estimate for the number of refugees in the camps is Pierre-Charles' (1965, p. 112), who says the government settled 6,000 people. But his report suggests there were thousands more who went unserved. One estimate comes from subtracting the number of deaths (12,000–15,000) from the 34,000 missing Haitians in Table 1. This implies there were 19,000–22,000 refugees. If we assume they stayed in the border districts, this would constitute about 5–6% of the population. Another estimate looks at the excess men in districts close to refugee camps in 1950. This approach, deatailed in Appendix Table A1, finds this region had 8% more men than expected.

While the political consequences of the massacre have been examined, there has been no examination of the economic consequences. The political analysis is understandable because the massacre inspired a popular resistance across the political spectrum (Smith 2009, pp. 33–36) and it reflected the tensions in US foreign policy (Roorda 1996). But the massacre reshaped the Haitian labor market, so it is surprising that there is little evidence of the contemporaneous economic effects. Since the massacre both decreased the demand for Haitian labor and increased the supply of labor in Haiti, a natural starting place would be to examine the effect on wages. Unfortunately, one of the reasons no one has examined the economic effects is that there are no widely available wage data for this period. But even in the absence of data, one would expect to encounter stories about the massacre creating hardships in the labor market. Yet these stories are missing. I help clarify this puzzle in the empirical work.

2 Data

I use three data sets to look at the contemporaneous and long-run effects of refugee camps. But since the data sets are at different administrative levels, it is important to understand how Haiti's political administrations are organized. The most granular unit I can analyze is the section, of which there were 571 in 2009 (displayed in Figure 1). This is the unit of observation in the long-run outcomes. Multiple sections make up a district (*commune* in French). The district is the level at which I observe contemporaneous land requests. In 1934, there were 107 districts.

Land Rental Data

For the contemporaneous effects, I have collected data from Haiti's land rental program. In 1928, under the U.S. marine occupation, the Haitian government reformed a long-existing government land rental program. The reforms corrected many distortions that had crept into the program and made available for rent a reported 915,000 ha (3,700,000 acres) located throughout the country (Millspaugh 1929 p. 561). A few years later, in 1934, the Haitian government started a homesteading program, where tenants who had paid rent for three years could convert their government-owned rentals into privately-owned farms.

I collected data on the universe of properties started under the program from 1928 to 1950. The program's legislation required the government to publish a notification in its official gazette, *Le Moniteur,* any time someone started a farm on uncultivated land or requested an unoccupied urban property. Each notification contains descriptive information about the requested property, listing the plot's location in one of Haiti's 107 administrative districts (*communes*) and when the property was requested and approved. Across all property types, there were 8,874 properties requested and approved over this period.

While the program was not designed for the refugees, there clearly was a shift in the program when the refugees arrived. Before the massacre, the average annual number of requests was 200 across the whole country. After the massacre, there was an immediate increase in requests, going from 174 in 1937 (the massacre ocurred in October 1937) to 678 in 1938. Requests stayed high in the entire post-massacre period, averaging 500 per year. The empirical work estimates whether the refugees caused this increase.

Agricultural Census 2009

To understand the intergenerational effects of the refugee camps, I use data from Haiti's 2009 Agricultural Census. The Agricultural Census created two data sets useful for this investigation. First, it reported section-level aggregates of key agricultural variables. The census tabulates the share of properties that were rented from the state and that were homesteads. These variables are important for examining the persistence of the land rental program (described above) and whether tenants took the program's next step and converted the rental into a homestead. The census also reports the average number of cows, goats, and pigs per parcel, which I use as a proxy for wealth. While these are not the only livestock in the data, they are the only ones where MARNDR tracks market prices.

While creating the Agricultural Census, the Ministry of Agriculture (MARNDR) also collected a Community Survey of resources available in the sections. The resources include the presence of state services (e.g. post office and court), the education resources available (e.g. elementary, secondary, and technical schools), and other amenities (e.g. sanitation facilities, gas station, and recreation center). For each resources, the survey indicates whether it is available or not without any further details. MARNDR also asked community leaders to list the top sources of conflict in their section as well as to rate the community's level of cooperation in eight different scenarios. For the analysis, I use a factor analysis to create four factors: state presence, educational opportunity, other amenities, and cooperation (see Appendix Table A6 for more details).

3 Short-Run Effects

Before exploring the intergenerational effects of the refugee camps, I establish the camps had short-run impacts. While it is widely known refugees arrived, we have yet to document contemporaneous economic effects from the arrival. This is puzzling given how the massacre cut off a significant source of labor demand and pushed refugees into the country. If the refugee camps did not immediately affect the Haitian economy, then it would be hard to believe there are long-run effects. But I argue that the refugee camps caused an increase in land rental requests.

3.1 Empirical Strategy

To identify the effect of the refugee camps, I follow the standard in the literature and define treatment as distance to the closest camp. Several other papers show that the effects of refugee camps vary by distance but are usually confined to 20 km (Alix-Garcia et al. 2018, Anti and Salemi 2021, Salemi 2021). Thus, my primary treatment variable is whether the district is within 20 km of a refugee camp (see Figure 1). To test the robustness of this definition, I also have a second treatment variable which allows treatment to vary by distance in 10 km rings (e.g. the administrative unit is less than 10 km from a camp, 10-20 km, etc.).

I estimate a difference-in-differences model. The data are aggregated into six-month periods,[4] and the following regression estimated:

$$Req_{it} = \delta_i + \delta_t + \beta D_i \times Post_t + \varepsilon_{it} \tag{1}$$

[4]One trouble with aggregating the requests by district is that there are many districts with months where no land requests were made. Summing the requests across months reduces this problem, but even after adding all of the districts' requests in six-month periods, about one-third of the observations are zero.

where Req_{it} is the number of requests per 1,000 inhabitants in district i in the six-month period t, and since there is no staggered treatment I can use two-way fixed effects, where δ_i is a district fixed effect and δ_t is a period fixed effect. The D_i indicates that district i is within 20 km of a refugee camp, and $Post_t$ indicates that the period is after the October 1937 massacre. Hence, β is the parameter of interest. Appendix Table A2 shows the results are robust to different specifications of the dependent variable. To account for serial correlation, standard errors are clustered at the district level.

Under the second treatment definition, I allow treatment to vary by distance and estimate the following regression:

$$Req_{it} = \delta_i + \delta_t + \sum_d \beta_d(\tilde{D}_{id} \times Post_t) + \varepsilon_{it} \tag{2}$$

In this regression, the treatment $\tilde{D}_{id}$ is a binary variable for whether district i is in ring d. I define five concentric rings—[0,10) km, [10,20) km, [20,30) km, [30,40) km, and [40-50) km—which produces five β_ds. Again, standard errors are clustered at the district level.

Since the difference-in-differences analysis relies on the parallel trends assumption, I check the robustness of the results by using a synthetic difference-in-differences analysis. This approach relaxes the parallel trends assumption by re-weighting control units and periods to match the pre-treatment trends (Arkhangelsky et al. 2021) and is implemented in Stata using the sdid package from Pailañir and Clarke (2022).

3.2 Results

I report the difference-in-differences results in Table 2. Panel A shows that requests for all properties increased by 0.5 per 1,000 inhabitants in sections within 20 km of a refugee camp. The effect is statistically significant at the 5% level and represents an eight-fold increase over the pre-massacre average. When we allow the treatment effect to vary by distance (Panel B), we see that the magnitude of the effect is roughly equal for sections within 10 km and sections that are between 10 and 20 km from a camp, though only the coefficient for the shorter radius is statistically significant at the 1% level. The treatment effect for sections greater than 30 km away quickly falls to zero, with no statistically significant results. The deteriorating effect is consistent with the literature for the effects of refugee camps and strengthens the case that the refugee camps had a causal effect.

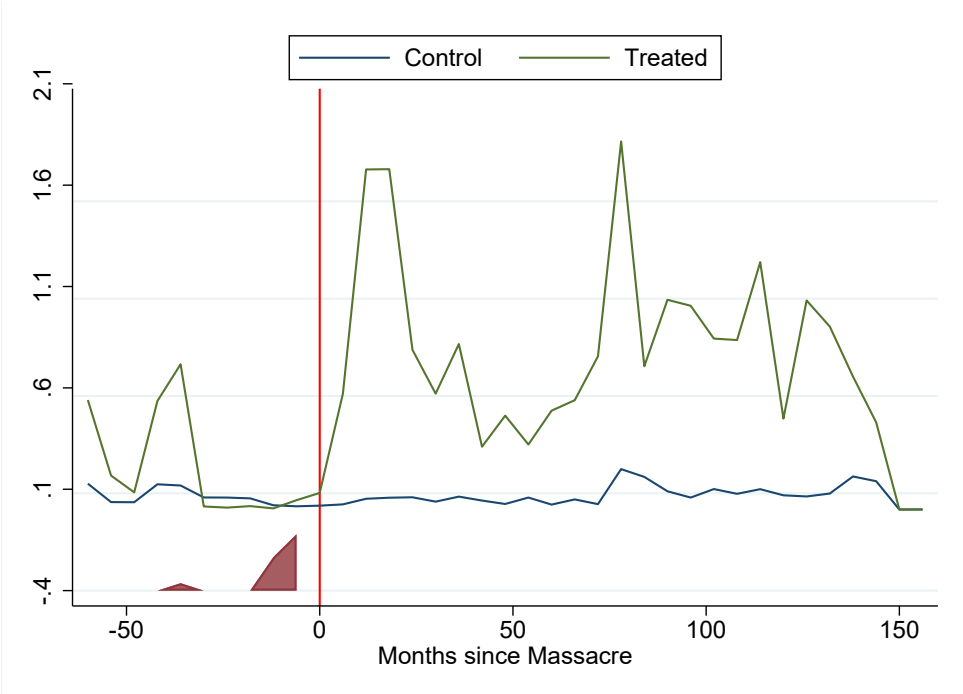
Since the state offered two types of properties for rent—agricultural and urban—I look at how camps affect requests by type. Table 2 shows that demand for agricultural properties account for about 75% of the increase and urban properties account for the other 25%.

Table 2. Difference-in-differences estimation of the effect of refugee camps on land rental requests

	All Properties	Agricultural	Urban
Panel A			
[0,20) km X Post Massacre	0.49**	0.38*	0.11**
	[0.22]	[0.21]	[0.047]
Panel B			
[0,10) km X Post Massacre	0.45***	0.29***	0.16**
	[0.16]	[0.095]	[0.074]
[10,20) km X Post Massacre	0.56	0.50	0.059**
	[0.46]	[0.47]	[0.026]
[20,30) km X Post Massacre	0.11	0.061	0.050*
	[0.073]	[0.063]	[0.030]
[30,40) km X Post Massacre	0.028	0.028	0.00046
	[0.049]	[0.048]	[0.0036]
[40-50) km X Post Massacre	-0.069	-0.043	-0.027
	[0.049]	[0.035]	[0.018]
Pre-Massacre Sample Mean	0.062	0.048	0.014

Notes: All regressions have 3,811 observations. All regressions contain district and time fixed effects.

Figure 2. Synthetic difference-in-differences analysis of the effect of refugee camps on land requests



Notes: The shaded area below the lines show the weights used to average pre-treatment time periods. Graph created in Stata using the sdid package from Pailañir and Clarke (2022).

Figure 2 shows the synthetic difference-in-differences results. In the 24 months leading to the massacre, trends in the treatment districts and the synthetic control are similar and receive the most weight when averaging pre-treatment time periods. But after the massacre there was an immediate and significant jump in requests in the treatment districts. The ATT is 0.61, which is statistically significant and even larger than the results found in Table 2.

The large shift in land-use could explain why the historical record speaks so little about the effect of closing the Dominican border. While the change in land-use could have come from locals starting farms to hire the newly available labor like Bangladeshi who settled near Rohingya refugee camps because of their economic opportunity (Dampha et al. 2022), the farms were too small to support that hypothesis. The average size of the rental plots was 2.2 ha, about enough land for a single family to cultivate. Since the requests were for idle land, there could not have been many coffee or fruit trees. Thus, the most likely output was subsistence crops for household consumption. This gave refugees the opportunity to provide for themselves while minimizing the effect on the labor and goods market. Similar effects have been observed in Tanzania (Maystadt and Verwimp 2014).

3.3 Other Short-Run Effects

Through their effect on the land rental program, the refugee camps also affected government services and tax revenues. These effects have been documented in Palsson (2021b), so here I summarize the findings and their relevancy to the discussion of refugee camps.

First, the refugee camps caused a significant increase in delays for properties. By stimulating demand for rental land, the refugee camps overwhelmed the rental program. Struggling to process the requests, the program went from approving properties in 10 months to taking 40 months. These delays were not isolated to the refugee camp areas; the shock affected the entire country.

Second, the refugee camps' increased tax revenues. Since the government leased the land, it collected rent from the tenants. By 1942, four years after the refugees arrived, the government was collecting 50% more rental revenue from the areas with refugee camps relative to the areas without. This gap persisted through 1949, the end of the study period.

The results are important for the broader discussion on how refugees affect their host communities. Economists have focused on the effect of refugees on wages, with mixed results (Card 1990, Carrington and de Lima 1996, Calderón-Mejía and Ibáñez 2016, Bohnet et al. 2021). But they have also explored the effects of refugees on government services. For example, since law enforce-

ment services and the social safety net did not not grow with refugee influxes, crime increased in Colombia and Turkey after refugees from Venezuela and Syria arrived (Knight and Tribin 2020, Akbulut-Yuksel et al. 2022). But this might not be as bad of a problem for richer countries. When a different set of Haitian refugees arrived in Florida after the 2010 Haitian earthquake, schools were able to accommodate them without taking resources away from their classmates (Figlio and Özek 2019). Likewise, Evans and Fitzgerald (2017) have calculated that refugees are a net-gain to US tax revenues. But the US has greater economic opportunity and more robust fiscal capacity than the typical host country. Haiti provides encouraging evidence that even low-income, low-capacity states can see fiscal benefits from refugees.

But Haiti's struggles to respond to the refugee influx may have hurt the refugees' long-run welfare. In other situations, refugees' economic outcomes have been significantly influenced by the initial conditions of settlement. For example, refugees arriving in the U.S. in months with higher unemployment rates have lower wages and labor force participation five years later (Mask 2020); refugees placed in U.S. states with more generous welfare policies got jobs with higher wages (LoPalo 2019); and refugees arriving in Uganda when land was available had higher incomes and consumption (Zhu et al. 2018). Since the government of Haiti responded so poorly to the refugee crisis, we might worry that this had persistent negative effects. We now examine those effects.

4 Intergenerational Effects

4.1 Empirical Strategy

Although the camps stopped being refugee camps and transformed into regular settlements, there is clear evidence that the refugees' descendants stayed in the area (Derby and Turits 1993). To examine the intergenerational effects, I compare outcomes in 2009 for sections close to camps (within 20 km) to those farther away from camps. The main objective is to ask whether we observe a difference for sections located next to refugee camps. For example, the refugees arrived without any livestock. If their descendants never overcame that deficit, then sections near refugee camps may today still have less livestock than comparable sections around the country. I use the following regression to compare the long-run outcomes:

$$y_i = \beta D_i + \Gamma X_i + \varepsilon_i \tag{3}$$

where y_i is the outcome of interesting in 2009 for section i, D_i is the treatment variable for the section being within 20 km of a refugee camp, and X_i are additional controls. I control for the section's population in 2009 and the section's longitude to account for the distance from the border. The coefficient of interest is β, which describes the difference between sections close to camps and those farther away conditional on the controls.

Since the placement of refugee camps may be endogenous, I also use an instrumental variables strategy for whether a section was within 20 km of a refugee camp. My instrumental variable is a receptivity index similar to the instruments used in Depetris-Chauvin and Santos (2018) and Calderón-Mejía and Ibáñez (2016). The index for section i is defined as

$$R_i = \ln\left(\sum_{c \in C} \frac{\Delta HaitianPopulation1950_c}{Distance_{ic}}\right) \tag{4}$$

where $\Delta HaitianPopulation1950_c$ is the change in the Haitian population of Dominican *comunero* c between 1935 and 1950 (see Table 1 for examples) and $Distance_{ic}$ is the distance from Haitian section i to Dominican *comunero* c. Since the regression already controls for longitude, this is not just a proxy for distance to the Dominican Republic. The index captures the additional effect of proximity to refugee flows.

The instrumental variable strategy rests on the identifying assumptions that this distance-weighted sum of population changes in 1950 (1) predicts which sections were close to camps but (2) does not affect any contemporary outcomes other than through its effects on the refugee camps. The first identifying assumption is satisfied: the F-statistic of the first-stage regression is 21.97, and the coefficient on R_i is 0.51 with a standard error of 0.12, which is significant at the 1% level. While it is impossible to verify the second identifying assumption, it seems reasonable to assume that population changes 70 years earlier in another country would only affect the 2009 outcomes through the refugee camps.

4.2 Results

First, I examine whether the land rental program persisted across generations. Table 3 shows that the children and grandchildren of the initial renters still participate in the program at higher rates. Both OLS and IV analyses show the share of farms that were rented from the state was 4 percentage points higher for sections within 20 km of a refugee camp. Since the average section has 2% of farms rented from the state, the share of state rentals was three times higher in these sections.

Table 3. Refugee camps and long-run outcomes

	State Rentals	Homesteads	Farm Size	Land Conflict	Livestock Value	Amenities	Literacy Rate	Cooperation
A. OLS								
[0,20) km	0.043***	0.0031	0.51***	-0.18**	0.15*	-0.033	-0.048*	0.39**
	[0.013]	[0.0023]	[0.068]	[0.082]	[0.093]	[0.034]	[0.025]	[0.16]
Dependent Variable Mean	0.022	0.002	-0.44	0.72	10.5	0.41	0.45	3.65
B. 2SLS								
[0,20) km	0.051	0.013	1.62***	1.33**	3.75***	-0.33*	-0.21*	0.35
	[0.039]	[0.0094]	[0.48]	[0.52]	[1.01]	[0.18]	[0.12]	[0.66]
Hausman P-value	1	0.62	0.069	0.0037	0.00011	0.45	0.6	1

Notes: All regressions have 564 observations and control for total population and longitude. Robust standard errors in brackets.

Persistent participation in the state rental program is interesting, but it is also puzzling. The rental program was the first step for a homesteading program. Tenants who made timely payments for three years could apply to convert the state-owned rental into a privately-owned homestead for a fee of 1 gourde per hectare (about 10% of the average annual rent). With so much time between the refugees' arrival and the 2009 agricultural census, the puzzle is why renting would persist when the tenants could convert to homesteads.

One hypothesis is that the descendants are more likely to use the rental program because they know they can convert to homesteads. Table 3 looks at the share of farms that are homesteads. While the difference is large relative to the average (0.003 vs 0.002), the difference for sections within 20 km is not statistically significant. In Appendix Table A4, when we narrow the radius to 10 km, the magnitude grows to 0.008 and is significant at the 10% level. So the refugee camps are associated with an increase in the presence of homesteads. But the share of homesteads is strikingly small compared to the prevalence of land rentals.

Since rentals persisted to the present, I also look at average farm size. Palsson (2021a) showed that the average farm size in Haiti is small because of inheritance institutions that subdivide plots over generations, but that land rentals were larger on average because they were started de novo instead of encumbered with these institutions. With persistent rentals, we might expect to see larger farms in these communities. Table 3 shows that this is the case. The average farm in communities close to refugee camps was 67% (0.51 log points) larger than in farther away communities. Farm size might be even larger according to the IV results, though the Hausman test is not significant at the 5% level. These larger farms represent a significant increase in potential income for these agricultural families.

The final land outcome I examine is the prevalence of conflict over land. The sudden influx of refugees could have increased conflict over scarce land. But, under the rental program, property rights were clearer than in the traditional land institutions, requiring significant state capacity to define (Palsson 2021b). Since previous research has shown that clearly defining initial property rights reduces future conflict (Libecap and Lueck 2011), the land rental program may have reduced conflict over land. In the Community Survey, each community listed the top three sources of conflict. Table 3 reports results from a regression where the dependent variable is a dummy for whether land disputes were included in this list of contentions. The OLS results show that the communities near refugee camps were 25% (18 percentage points) less likely to consider land a major source of conflict, a result significant at the 5% level. When using the IV, however, the sign

flips and these communities are significantly more likely to fight over property. While the point estimate of the coefficient is unreasonable (1.33), the confidence interval is wide enough to include reasonable effects. While this is the only outcome where the IV results differ in sign from the OLS results, it may indicate that the placement of camps was endogenous to camp conflict and the IV resolved this endogeneity issue.

Next I look at intergenerational wealth. Since the refugees arrived penniless (Haiti Bureau du representant fiscal 1938, p 89) and had abandoned their livestock in the DR (Turits 2003, Palmer 1976), we might expect these disadvantages to persist to the descendants. But when we proxy for wealth using livestock, Table 3 shows that any differences disappeared. Evaluating livestock holdings at market prices, these sections are 15% wealthier than the control group, a result that is significant at the 10% level. The IV results show a much larger wealth advantage for the descendants close to refugee camps, suggesting that there is some measurement error in the wealth proxy. This convergence is consistent with evidence that disadvantages can disappear over time. For example, chiefdoms in Sierra Leone that had experienced violence during the civil war from 1991 to 2002 showed no measurable socioeconomic disadvantages 2-3 years after the war's end (Bellows and Miguel 2009).

Did the descendants benefit from the plans for the original refugee camps? As mentioned above, President Vincent planned to use the camps to provide amenities that could help the refugees get established. When I measure amenities using the factor analysis described in Appendix A.3, I find no meaningful difference in amenities for communities closest to refugee camps. The IV analysis shows a larger discrepancy in amenities, but the Hausman test indicates that we should stick with the more efficient OLS estimate. On the one hand, the refugee camps did not create extra amenities. On the other hand, these communities do not appear to be disadvantaged. So maybe the camps worked for creating amenities. We can examine whether the amenities were sufficient by looking at whether they benefitted the descendants. In the amenity factor analysis, 4 of the 13 variables used are related to education, and in the years following the massacre, schooling was one of the main outcomes tracked for the refugee camps (Republic of Haiti 1939). But when we look at the descendants' literacy rates, we see that they are four percentage points lower than the average section. This difference is only significant at the 10% level, but it is not favorable evidence for the amenities.

The most encouraging result is that communities close to refugee camps exhibited much higher levels of cooperative behavior. The dependent variable is a factor estimated from eight assessments

of cooperation within the section (see Appendix Table A6), and the OLS and IV analyses show very higher levels of cooperative behavior around refugee camps. This is consistent with results from both Uganda and Sierra Leone, which show that victims of violent trauma can overcome the tragedy and exhibit greater pro-social behavior (Blattman 2009, Bellows and Miguel 2009). These results, however, are unique because they show high levels of social capital 70 years after the trauma. This suggests that the resilience and bonding responses to trauma have intergenerational effects. The intergenerational effects are consistent with what we see in the American South today. Historically, lynchings discouraged Black voters from voting (Jones et al. 2017), and today US counties with higher levels of historical lynching have lower Black voter participation (Williams 2021). The mechanism for persistence appears to be the intergenerational transmission of values: children learn voting is important from their parents, but Black parents could not vote, so the values could not be modeled. Fortunately, in Haiti, the intergenerational transmission was on a positive path.

Overall, refugee camps seem not to have any negative intergenerational effects on the areas closest to them. Wealth and other proxies for well-being are approximately the same and maybe better. This long-run convergence was probably helped by the quick provision of land to the refugees. This would be consistent with several other studies that have shown that giving land to refugees is beneficial to short- and long-run outcomes (Alix-Garcia et al. 2018, Zhu et al. 2018, Bharadwaj and Mirza 2019). The one gap to note is that we would have expected to see more homesteads in these areas. That there is still such a low share indicates that there are ways the lives of the refugees could be improved, and we might even see these areas surpass their peers.

5 Conclusion

Previous studies on refugee camps are limited in telling us their intergenerational effects. In this paper, I have looked at refugee camps formed in 1938, allowing me to observe both the short-term and long-term effects of refugee camps. I find that in the first 15 years refugee camps changed land-use and increased tax revenues. Then, 70 years later, I find persistent effects from the change in land-use, no differences in wealth, and higher levels of social cohesion. Since these camps did not receive international aid, they are evidence that refugee camps can have positive economic effects even in the absence of aid.

It is also important to highlight that this study does *not* argue that the refugees and their descendants were helped by the massacre. There have been several studies arguing that forced

displacement leads to better outcomes for the displaced. For example, displacement increased incomes for Japanese immigrants forced into internment camps (Arellano-Bover 2021), for Louisiana families who lost their homes in Hurricane Katrina (Deryugina et al. 2018), and for the descendants of Icelandic families displaced by lava (Nakamura et al. 2021). But in this study, I do not have a counterfactual for the refugees' lives if the massacre had never happened. Even comparing refugees in Haiti to the ethnic Haitians who stayed in the Dominican Republic would be difficult because displacement can disrupt development in the origin country (Testa 2020).

One thing to keep in mind when comparing this situation to other refugee camps is the nature of the refugees. Not only were the refugees co-ethnic, the massacre that forced them from the Dominican Republic was anti-Haitian. The refugees received popular support in Haiti and calls for greater government action (Smith 2009, pp. 33–36). While these cries for help were ultimately unanswered (Pierre-Charles 1965 pp. 111–112), the shared identity may have helped with integrating the refugees. For example, it is likely that many of the refugees already knew about the land rental program (Casey 2012 p. 86), which played an important role in helping the camps. The refugees probably understood how the government functioned and what resources were available better than refugees in other contexts.

Another caveat of the study is that these refugee camps had no mobility restrictions. Many countries create camps as a safeguard for refugees, but they also prevent the refugees from leaving them. Since Haiti's camps did not have mobility restrictions, refugees had an exit option. Since many of the refugees stayed in the camps for decades (Derby and Turits 1993), the camps had to provide at least as good an opportunity as the outside option. Thus, it might not be surprising to see such positive long-run effects. In camps with mobility restrictions, on the other hand, the positive effects might be limited.

Table A1. Triple difference estimate of refugee effect and population

	Coefficient	Standard Error
Male	-0.043	[0.028]
Male X Age 25 or older in 1950	-0.10***	[0.028]
Within 20 km of camp X Male	0.026	[0.034]
Within 20 km of camp X Age 25 or older in 1950	0.052	[0.076]
Within 20 km of camp X Male X Age 25 or older in 1950	0.079*	[0.041]

Notes: Regression contains district and age-bin fixed effects. Standard errors are clustered at the district level.

A Appendix

A.1 Estimates of Refugee Shock

The 1950 Census was the first official census in Haiti. There are no individual-level microdata available, but the census did report district-level cross-tabulations on the age and sex distribution. Although the limited data cannot reveal the absolute population effects, we can get a sense of the relative effects using a triple-difference strategy with the 1950 census data.

Discerning the refugee effect exploits three sources of variation. The first uses the five refugee camps established by the government to coordinate aid: as seen in the appendix, the districts most affected by the population shock were within 20 km of a refugee camp. Second, because refugees were agricultural workers in the Dominican Republic and therefore more likely to be males, within each district I compare population differences between men and women. Finally, between males and females in each district I compare population differences for potential refugees, identified by age. The census reported ages in five year bins, and I define a potential refugee as someone who was at least 25 years old in 1950 (ages 13-18 at the time of the massacre). Because refugee camps should not affect population differences between boys and girls born after the massacre, the young serve as a control group for what we expect to see in the older groups. The analysis clusters standard errors at the district level.

Table A1 reports the triple difference analysis and finds an effect of refugees on population. Throughout the country the male population 25 or older is 10 log points below women in the same age group relative to what we expect given gender ratios among the young. The coefficient's magnitude is consistent with the large migration flows to the Dominican Republic. But the triple

Table A2. Robustness of land request results to different specifications of the outcome variable

	IHS Requests/cap (OLS)	Requests/cap (OLS)	IHS Requests (OLS)	Requests (Poisson)
[0,20) km X Post Massacre	0.284**	0.491**	0.872***	1.023**
	[0.116]	[0.215]	[0.280]	[0.423]

difference coefficient reveals that many of the missing men can be found in the districts near refugee camps. In districts within 20 km of a refugee camp, the population of older men is 8 log points higher than differences in population across gender in the younger group and in districts farther away from refugee camps. Note the coefficient is only significant at the 10% level, but it is the only data-based estimate of the refugee population shock.[5]

A.2 Comparing Synthetic Control to Event Study

The synthetic control analysis presented in Section 3 provides convincing evidence that the refugees and the capacity expansion had significant effects on tax revenues. But a weakness of the synthetic control analysis is that researchers have some degrees of freedom in selecting the synthetic control. To assuage concerns about researcher bias, I also estimate a difference-in-difference event study and compare the estimated treatment effects to the synthetic control. To test for refugee effects on tax receipts, I run the following regression

$$\ln T_{it} = \delta_i + \delta_t + \beta_t(Refugee_i \times \delta_t) + \varepsilon_{it} \tag{5}$$

where T_{it} is, for tax precinct i in year t, the tax receipts from land rentals (though in other situations below, the dependent variable will be taxes in other categories). The regression includes fixed effects for the year (δ_i) and the precinct (δ_t). The $Refugee_i$ variable is an indicator for whether the tax precinct hosted a refugee camp, and the β_t gives, for year t, the difference in receipts between refugee and non-refugee tax precincts. Because there are only 10 precincts, I obtain confidence intervals for β_t using the wild bootstrap-t methods described in Cameron et al. (2008) and implemented in Stata by Judson Caskey.

[5]It should also be acknowledged that the 10% significance level is for a two-tailed test. If we used a higher powered one-tail test—which is reasonable since we expect the refugees to increase the population in the treated districts—then the coefficient is significant at the 5% level.

Table A3. Treatment effect estimates for synthetic control and difference-in-differences analysis

	Land Rental Receipts		Property Transfer Receipts	
	SC	ES	SC	ES
1938	0.204	0.217	0.094	0.107
	[0.00]	[0.495]	[0.27]	[0.51]
1939	0.202	0.145	0.018	0.0235
	[0.02]	[0.020]	[0.83]	[0.66]
1940	0.235	0.227	-0.173	-0.129
	[0.14]	[0.00]	[0.38]	[0.52]
1941	0.232	0.221	-0.304	-0.27
	[0.06]	[0.00]	[0.02]	[0.10]
1942	0.465	0.448	0.034	0.0689
	[0.00]	[0.010]	[0.83]	[0.67]
1943	0.499	0.497	0.301	0.335
	[0.00]	[0.00]	[0.03]	[0.01]
1944	0.489	0.423	0.119	0.16
	[0.00]	[0.00]	[0.44]	[0.17]
1945	0.531	0.447	-0.013	0.0139
	[0.02]	[0.00]	[0.95]	[0.93]
1946	0.513	0.43	0.099	0.129
	[0.02]	[0.01]	[0.69]	[0.31]
1947	0.471	0.389	0.129	0.159
	[0.08]	[0.05]	[0.55]	[0.285]
1948	0.527	0.416	0.152	0.198
	[0.11]	[0.06]	[0.45]	[0.29]
1949	0.49	0.387	0.103	0.137
	[0.17]	[0.14]	[0.38]	[0.24]

Notes: P-values in brackets. The column headers indicate the approach used to estimate the treatment effects—SC means synthetic control and ES means event study.

Table A4. Refugee camps and long-run outcomes

	State Rentals	Homesteads	Farm Size	Land Conflict	Cows	Goats
[0,10) km	0.056**	0.0080*	0.71***	-0.20*	0.21	-1.01***
	[0.024]	[0.0046]	[0.077]	[0.12]	[0.17]	[0.20]
[10,20) km	0.038***	-0.000066	0.65***	-0.16	0.042	-0.87***
	[0.013]	[0.00075]	[0.10]	[0.11]	[0.11]	[0.23]
[20,30) km	0.0087	-0.00057	0.62***	-0.1	-0.20**	-0.94***
	[0.010]	[0.00060]	[0.083]	[0.10]	[0.091]	[0.17]
[30,40) km	0.01	0.000063	0.41***	0.055	-0.20***	-0.98***
	[0.010]	[0.0011]	[0.083]	[0.080]	[0.070]	[0.19]
[40-50) km	0.00096	0.0034	0.17	0.034	0.047	-0.60***
	[0.0071]	[0.0032]	[0.11]	[0.077]	[0.089]	[0.20]
	Pigs	Livestock Value	State	Education	Amenities	Cooperation
[0,10) km	-0.48***	0.19	0.095	0.072	-0.098	0.33
	[0.10]	[0.14]	[0.077]	[0.045]	[0.063]	[0.23]
[10,20) km	-0.38***	0.065	0.096	-0.074	-0.098**	0.56***
	[0.090]	[0.12]	[0.071]	[0.047]	[0.049]	[0.21]
[20,30) km	-0.28***	-0.19	0.041	0.044	-0.12***	0.22
	[0.094]	[0.13]	[0.062]	[0.059]	[0.046]	[0.19]
[30,40) km	-0.42***	-0.23**	0.00029	-0.042	-0.064	0.047
	[0.10]	[0.11]	[0.050]	[0.043]	[0.057]	[0.18]
[40-50) km	-0.16	0.15	0.054	-0.0005	-0.025	0.14
	[0.12]	[0.11]	[0.059]	[0.049]	[0.057]	[0.16]

Notes: All regressions have 139 observations. All regressions contain district and time fixed effects.

Table A5. Refugee camps and long-run outcomes–sections within 40 km of a camp

	State Rentals	Homesteads	Farm Size	Land Conflict
[0,20) km	0.060***	0.002	-0.15*	-0.16
	[0.018]	[0.0013]	[0.084]	[0.12]
Dependent Variable Mean	0.02	0.002	0.73	0.72
	Literacy Rate	Amenities	Livestock Value	Cooperation
[0,20) km	-0.084*	0.04	0.052	0.22
	[0.045]	[0.059]	[0.13]	[0.23]
Dependent Variable Mean			10.19	3.65

Notes: Sample is restricted to just the sections that are within 40 km of a refugee camp. All regressions have 99 observations and control for total population and longitude. Robust standard errors in brackets.

A.3 Factor Analysis

The analysis in Section 4 includes four regressions where a latent factor is the dependent variable. In this section, I provide more details on how the factors were constructed.

The variables used to estimate the latent factors all come from the 2008 Community Survey, as described in the Data section. Table A6 shows the variables behind each factor and the means for sections within 20 km of a refugee camp and sections outside of that radius. For the first three factors (state presence, education services, and other amenities) all variables are binary, where a value of one indicates the amenity is present. Since the variables are all binary, the factor model is estimated from a tetrachoric correlation matrix. For the cooperation factor, the variables are scored on a Likert scale, where 1 is "practically non-existent", 2 is "weak", 3 is "average", 4 is "high", and 5 is "very high." Since the variables are all ordinal, the factor model is estimated from a polychoric correlation matrix. The factor loadings are given in the third column.

While the factor analysis provides a convenient way to reduce the number of variables, it lacks transparency. So in the fourth and fifth columns of Table A6 I report the regression coefficient and standard error from a regression of the variable on the treatment of being within 20 km of a refugee camp. All regressions include controls for population and longitude. All four factors have individual variables that are statistically different for the treated sections.

Table A6. Details for factor analysis

	Within 20 km	Outside 20 km	Factor Loadings	Regression Coefficient	Standard Error
A. State Presence					
Civil Registry Office	0.16	0.10	0.99	0.082	0.059
Post Office	0	0.04	0.65	-0.041***	0.014
Court	0.16	0.09	0.98	0.085	0.059
B. Education Services					
Literacy Center	0.16	0.25	0.40	-0.18***	0.065
Primary School	0.98	0.97	0.92	0.034	0.026
Secondary School	0.48	0.52	0.76	0.02	0.082
Professional/Technical School	0.14	0.17	0.97	-0.0027	0.058
C. Other Amenities					
Health/Sanitation Center	0.39	0.51	0.70	-0.058	0.084
Pharmacy	0.20	0.25	0.76	-0.022	0.069
Recreational Facilities	0.36	0.11	0.48	0.27***	0.076
Internet/Cybercafe	0.02	0.19	0.87	-0.13***	0.038
Telephone Lines	0.18	0.17	0.72	-0.026	0.066
Gas Station	0.02	0.09	0.80	-0.057**	0.029
D. Cooperation					
In agricultural activities	2.4	2.6	0.35	-0.12	0.21
In times of severe weather	2.5	2.6	0.53	0.11	0.21
Management of roads/infrastructure	2.9	2.5	0.42	0.50***	0.17
In the case of social/family tragedies	2.7	2.3	0.45	0.43*	0.23
Management of natural resources	2.7	2.3	0.32	0.44***	0.15
In cultural/religious events	2.9	2.7	0.48	0.35*	0.19
In finances	2.7	2.5	0.31	0.22	0.17
In the case of security issues	2.7	2.3	0.32	0.19	0.19

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