The Forces of Path Dependence:

Haiti's Refugee Camps, 1937–2009

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March 6, 2023

Abstract

Refugee camps are sudden, spontaneous population centers that can persist for years. Their persistence provide an opportunity to learn about the forces of path dependence. I argue that residents stay because the camps create local amenities. I examine this question using refugee camps established in Haiti after a 1937 massacre in the Dominican Republic. Despite the residents' freedom to migrate, the camps evolved into persistent settlements where the refugees' descendants resided 70 years later. I show that these camps gave residents access to land with incomplete rights and to social networks that help with informal insurance. While residents 70 years later have slightly lower levels of literacy, they are not significantly disadvantaged on other margins. I interpret these results as evidence of path dependence driven by amenities rather than local productivity advantages.

*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). These population movements have been used to study shocks to labor supply (Card 1990, Calderón-Mejía and Ibáñez 2016, Bohnet et al. 2021), housing demand (Depetris-Chauvin and Santos 2018), education (Figlio and Özek 2019, Green and Iversen 2022), crime (Knight and Tribin 2020, Akbulut-Yuksel et al. 2022), and deforestation (Salemi 2021). But many of these displaced people do not have a chance to integrate into local economies. Instead, they are settled in camps. These camps often start from nothing and develop their own economies, which can then influence the native economy (Alix-Garcia et al. 2018, Maystadt and Verwimp 2014). Since camps start from arbitrary origins and persist to the future, refugee settlements give us an opportunity to study the mechanisms of path dependence.

One of the open questions about path dependence is why people do not respond to bad situations by migrating. Since path dependence can create a misallocation of resources, it lowers welfare for those affected. This gap in welfare is usually shown empirically by comparing two groups of people living in areas that were affected differently by the historical choice (Dell 2010, Libecap and Lueck 2011, Acemoglu et al. 2012, Banerjee and Iyer 2005, Valencia Caicedo 2019). But if these areas are true counterfactuals, then why would the treated group not migrate to the control area and take advantage of the higher welfare? Allen and Donaldson (2022) explain that people stay and create path dependence when the agglomeration forces (e.g. the productivity and amenities that make the origin attractive) exceed the dispersion forces (e.g. the characteristics that make destinations attractive). In this paper, I argue that refugee camps shed light on these forces.

I address this question by looking at 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 led to a mass exodus from the country. Many of these refugees settled in camps created by the government of Haiti. Key for this question, in contrast to typical refugee settlements where refugees are prohibited from leaving and integrating with the rest of the economy, these refugees were free to leave. Even though the government stopped supporting the camps, the settlements retained a significant population. Over seventy years later, the settlements had evolved into villages populated by the refugees' descendants. Examining why these settlements persisted can help us understand path dependence.

I argue that these settlements persisted despite the occupants' free choice to migrate because the camps created amenities the residents valued. The first is an amenity common to developing countries: access to land with incomplete rights. Incomplete property rights dampen migration

because they create a wedge between the value of staying versus leaving and they prevent owners from using the land to finance their migration (Chernina et al. 2014, De Janvry et al. 2015, Adamopoulos et al. 2022, Rao et al. 2021). Thus, incomplete property rights may be a general explanation for why path dependence persists. The second amenity is more general: access to social networks. While social networks provide informal insurance (Udry 1994, Townsend 1994), they also limit migration (Munshi and Rosenzweig 2016, Morten 2019). Path dependence may persist because social networks create a coordination problem: if the entire network moves together, they can achieve higher welfare, but if one member moves unilaterally, he will be harmed.

I look for evidence of incomplete property rights around refugee camps using both short-run and long-run data. 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. Furthermore, since the properties were owned by the government, the residents would not have complete rights over the land. Then, I examine how this land use persists to 2009. Seventy years later, the share of properties rented from the government for areas within 20 km of a refugee camp is three to four times higher than comparable areas. The sections' farms are also larger and there is less conflict over land. These results support the hypothesis that people who stayed in the area had access to land with incomplete rights and that leaving it was risky.

Next, I look for evidence that these areas had access to social networks. Using data from 2009, I show that the sections close to camps had higher levels of social capital. Using a factor analysis that combines eight measures of social cooperation, I show that social capital in the treated sections is 11% higher. This is in part driven by factors associated with informal insurance: these sections were 28% more likely to express high levels of community support during family tragedies and 13% more likely to have high levels of support in times of financial troubles. While I cannot conclude that the refugee camps caused the increase in social capital, previous work has shown that survivors of similar violent trauma show increased levels of pro-social behavior (Blattman 2009, Bellows and Miguel 2009), so it is possible there is a causal link. Even in the absence of causality, the results show that these areas had access to social networks that might have limited their migration and, therefore, allowed the camps to persist.

Having established two amenities near the camps, I look at their possible consequences. A simple theory of amenities shows that the amenities' value will be reflected in compensating differentials. Thus, I look for margins on which these areas might be harmed by remaining near the defunct camps. First, I look at one of the most studied outcomes for displaced people: education. A long-standing theory hypothesizes that since refugees have been displaced from their physical goods, they will shift away from physical investments and invest more in human capital because it stays with them (Brenner and Kiefer 1981). This theory has been supported with evidence from several different displaced populations showing the displaced people and their descendants acquiring more education than the average resident (Bauer et al. 2013, Becker et al. 2020, Ayesh 2022). Using data from Haiti in 2009, I show that this result does not hold in Haiti. The literacy rate in sections near the camps was 10% lower than the rest of the country. While the difference is not statistically significant, it is notable that it is so opposed to the literature. A possible contributor to the gap is that treatment sections are 72% less likely to have an adult literacy center, suggesting that a compensating differential could be the lack of educational amenities.

I explore other outcomes where we might expect compensating differentials. These areas show no discrepancies in wealth (as proxied by livestock holdings), but there are mixed results for other amenities. They have slightly higher access to state institutions like courts and registry offices, possibly because of the history of the state establishing the settlements. But they have slightly less access to health services and internet. Overall, since the treatment and control areas differ on so many margins, it is hard to evaluate whether the treated residents are better or worse off.

This study contributes to our understanding of path dependence. First, I highlight mechanisms for why path dependence persists despite the option to migrate. Indeed, this failure to migrate is of general theoretical interest, since macroeconomic models of the effect of population shocks require migration frictions for persistent effects (Peters 2022). Second, this focus on local amenities changes how we interpret studies of path dependence. Many such studies show that path dependence leads to negative outcomes for affected groups. But these negative outcomes need to be weighed against the local amenities that keep residents there. While the best outcome is that the residents get both the good outcome and the local amenities, the existence of a gap suggests there is something worth staying for.

Haiti's camps also contribute to our understanding of the economics of refugee camps. Some work has examined which policies are effective at integrating refugees into labor markets (Foged et al. 2022), but since so many refugees are confined to camps, we need a better understanding

of what happens there. This study provides a few contributions. First, many of the positive and negative effects found around refugee camps come from international aid (Alix-Garcia et al. 2018, Maystadt and Verwimp 2014, Alix-Garcia and Saah 2009, Anti and Salemi 2021). The camps in Haiti show that even without aid, camps have local economic impacts. Second, many studies of refugees show that the initial conditions of settlement—such as the type of assistance given, the health of the labor market, or the number of business owners in the refugee network—are important for refugee welfare (Mask 2020, LoPalo 2019, Zhu et al. 2018, Dagnelie et al. 2019). This study shows that those initial conditions—such as the choice to give refugees land—can persist long into the future. Finally, Haiti shows that camps affect the economy even when refugees are free to move. This suggests that the placement of refugee camps can have long-run effects even after the camps stop coordinating aid. This persistence in the face of change is of general interest and is comparable to how cities exist today around former portage cites (Bleakley and Lin 2012).

This paper also contributes to the history of Haiti. The massacre is one of the most culturally significant events of the 20th century. While the political consequences of the massacre have been examined, there has been almost 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). One study has shown that the refugee camps increased tax revenues (Palsson 2023), similar to other work that shows the fiscal benefits of refugees (Evans and Fitzgerald 2017). But the massacre was a large shock to the Haitian labor market as thousands fled the Dominican Republic, and such large shocks often have negative effects when the refugees are co-ethnic and similar in skill to the receiving population (Bohnet et al. 2021, Calderón-Mejía and Ibáñez 2016, Depetris-Chauvin and Santos 2018).[1] While it is surprising that there is little evidence of hardships in the labor market, I help clarify this puzzle in the empirical work by showing that the wide availability of idle state-owned land helped absorb the free labor.

1 Haitians in the Dominican Republic and the 1937 Massacre

During the early 1900s, tens of thousands of Haitians lived outside of Haiti, primarily for two reasons. First, many 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

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

(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).

The second reason many Haitians were outside of Haiti was because many lived in the Dominican frontier. The border between Haiti and the Dominican Republic was outside of state influence, leading to a bicultural frontier (Turits 2003). Haitians and Dominicans freely interacted on both sides of the border, with thousands of Haitians settling on the Dominican side. Their children were ethnically Haitian, but, through *jus soli,* were legally Dominican. Defining the border became a priority over time. During the American occupations of Haiti and the Dominican Republic, the border was better defined and monitored so trade could be taxed. In the 1930s, President Rafael Trujillo of the Dominican Republic made strengthening the border a policy priority. Part of his strategy was to establish a Dominican presence on his side of the border by giving land to farmers to create agricultural colonies, though he ironically gave much of the land to farmers who were ethnically Haitian. He also sought a diplomatic solution with Haiti, coming to a border agreement with Haiti's President, Stenio Vincent. The peaceful settlement earned the two presidents 14 nominations for the 1936 Nobel Peace Prize.

Then the border conflict reached a disturbing end. In October 1937, without warning, Trujillo sanctioned the slaughter of Haitians living in the DR along the Northern border while sparing those who worked on sugar plantations. The exact 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 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]

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

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%

Notes: Data come from the 1935 and 1950 Dominican Republic censuses. The row "All Interior Provinces" includes the aggregate of all provinces not named in the table. For a visual representation of the data, see Appendix Figure A3.

Despite the international attention the massacre brought to Trujillo, he continued to push Haitians out of the frontier in 1938. The most contended part of the border was in the South, near the Dominican Republic's Pedernales. In the Spring of 1938, Trujillo began pushing thousands of Haitians out of the region and across the border in an event called *el desalojo*, or the eviction. While this action was much less bloody than the 1937 massacre, it still led to hundreds of deaths (Turits 2003 p. 169).

The massacre triggered an exodus from the DR. Table 1 shows that in 1935 the Dominican census counted almost 53,000 Haitians in the country, but the 1950 Census found fewer than 19,000. 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). Even though Haitians on sugar plantations were spared from the massacre, the plantations lost many workers. 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 accepted (Vega 1988). Even Dominicans of Haitian descent abandoned land and livestock to avoid the risk of death (Turits 2003, Palmer 1976). While the loss occurred throughout the country, Table 1 and Appendix Figure

A3 show the border areas lost 90–100% of their Haitian populations.

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, detailed in Appendix Table A1, finds this region had 8% more men than expected.

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 camps' 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).

2 Theoretical Framework

A lot of research has examined how the population movements of forced displacement affect the economy. Displaced people are usually studied for their effects on the labor market because they are large, exogenous shocks to local markets (Card 1990, Carrington and de Lima 1996, Calderón-Mejía and Ibáñez 2016, Bohnet et al. 2021). They even can affect productivity through their effects on agglomeration (Peters 2022). This makes sense when they are allowed to integrate into the

[3]Le Moniteur, 14 March 1938

economy, but in many developing countries, displaced people are settled in camps and are unable to work outside of them. Thus, rather than shocking pre-existing population centers, the camps are new, exogenous population centers. Since understanding how population centers evolve is central to theories of economic growth, understanding whether and why refugee camps persist can inform our understanding of spatial economics and path dependence.

The Haitian refugee camps are interesting for this question. Since the refugees were ethnically Haitian, they were free to integrate into the Haitian economy and were, therefore, not restricted to the camps like in other countries. Yet, we know a core population remained around the camps, and that the refugees' descendants were there (Derby and Turits 1993). The free mobility combined with the persistent population center makes this an interesting case for path dependence.

Theories of path dependence point to where we should look for the mechanisms of persistence. Allen and Donaldson (2022) use a model of agglomeration economies and migration to show that populations persist when agglomeration forces are greater than dispersion forces. One of the most important agglomeration forces is the effect of increasing returns to scale on local economic activity. The increasing returns to scale can come from larger labor markets, lower transportation costs, and knowledge spillovers (Greenstone et al. 2010), and there is good empirical evidence that this matters for path dependence (Bleakley and Lin 2012). Yet these mechanisms do not fit refugee camps because the camps are not centers of industrial production with active labor markets. Instead, I focus on the other agglomeration force: local amenities. In theories of spatial economics, these amenities do not increase productivity, but the workers want them enough to accept lower wages or higher costs of living (Greenstone et al. 2010, Diamond 2016). I argue that two important amenities provided by refugee camps were access to property with incomplete rights and access to social networks.

The first amenity provided by refugee settlements was access to land with incomplete rights. Not only are these settlements defined by their ceding land to displaced people, many governments make land a center of relief policy (Bharadwaj and Mirza 2019, Alix-Garcia et al. 2018, Zhu et al. 2018). Indeed, the impact of refugee settlements on land use is so pronounced that one of the main opposition to them is fear that they will cause deforestation, though a recent study of African refugee camps shows this concern might be exaggerated (Salemi 2021). But if this land is granted with incomplete property rights—such as the inability to transfer or mortgage it—then this might tie people to the area (Chernina et al. 2014, De Janvry et al. 2015, Adamopoulos et al. 2022, Rao et al. 2021).

The second amenity is access to social networks. Social networks serve an important role in developing countries by providing informal insurance for families in incomplete markets (Udry 1994, Townsend 1994). But they also limit migration because migration reduces access to the insurance (Munshi and Rosenzweig 2016) and because access to the insurance reduces the need to migrate (Morten 2019). Thus, social networks might explain persistent population centers. Social networks could be especially important in refugee settlements. Evidence from both Uganda and Sierra Leone shows that victims of violent trauma exhibit greater pro-social behavior (Blattman 2009, Bellows and Miguel 2009). If these pro-social behaviors create stronger social networks, then participants may be hesitant to leave. Furthermore, the values in a network can persist over generations. For example, in India, parents guide their kids' career choices to maintain access to social networks (Munshi and Rosenzweig 2006). In the US, the trauma from historical lynchings has led to 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 lynchings prevented Black ancestors from voting (Jones et al. 2017), so the values could not be modeled.

These theories have testable implications. First, if access to land is a significant factor to persistence, then we should observe that the settlements transformed land use in the surrounding areas. Furthermore, to contribute to persistence, we should see the changes in land use to continue to the present. In the empirical section, I test for these implications using historical data on land use before and after the massacre and modern land use data. Next, if social networks are a factor, then we should see high levels of social capital around the refugee settlements. I test for this using survey data on cooperation in Haitian communities.

The theory that amenities cause persistence has a more general prediction: amenities will be reflected in compensating differentials. In a typical labor market, this is interpreted as workers accepting lower real wages in exchange for access to the amenities (Greenstone et al. 2010, Diamond 2016). While wages are difficult to test in developing countries generally and in this context specifically, we can look at evidence for compensating differentials in other amenities or measures of well-being. One measure that is important for the descendants of displaced people is education. There is a long-standing theory that displaced people prioritize investments in human capital because it is an asset that cannot be taken away from them (Brenner and Kiefer 1981). This theory has been supported with evidence from Germany (Bauer et al. 2013), Poland (Becker et al. 2020), and Pakistan (Ayesh 2022). Hence, it is the first place I look. I also look for difference in

wealth, using data on livestock holdings, and in other amenities.

3 Data

To look at the forces of path dependence for these refugee camps, I use three data sets. 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; note, only 564 had sufficient data). 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 historical 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 (Palsson and Porter 2023).

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 occurred 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 persistent effects of the refugee camps, I use data from Haiti's 2009 Agricultural Census collected by the Ministry of Agriculture (MARNDR). 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 the share 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, 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., health clinics, gas station, and recreation center). For each resource, 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, such as whether they help in times of financial duress.

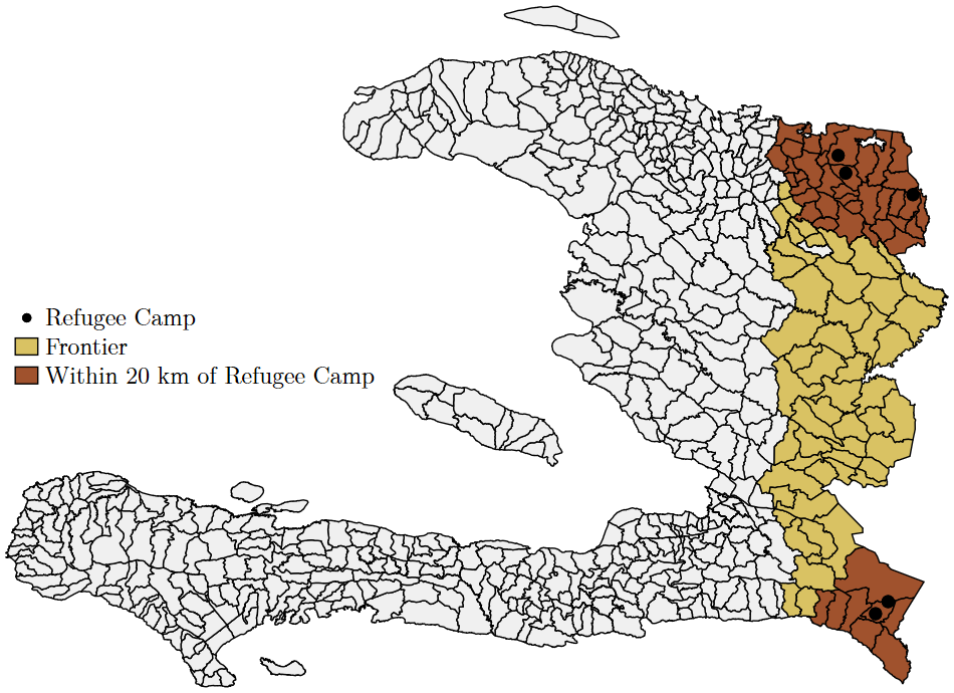
4 Empirical Framework

Given we know the refugee camps created persistent population centers, the empirical work is meant to investigate why they persisted. There are two hypotheses to test: (1) camps gave refugees access to property with incomplete rights and (2) they facilitated the formation of social networks. In this section, I outline how I test these hypotheses. But first I investigate the definition of treatment and address concerns with endogeneity.

4.1 Refugee Camp Placement

Throughout the empirical work, I define treatment as distance to the closest camp. This definition is standard in the literature. Several other papers show that the effects of refugee camps vary by

Figure 1. Refugee camps and treatment areas



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.).

Since treatment is determined by proximity to a camp, it is important to understand how the government selected sites in case the selection process is correlated with factors related to the dependent variable. A series of balance tests in Appendix A.2 shows that the treatment and control districts look similar on observables. The difficulty, however, is whether the settlement process included factors that are unobservable to me. Unfortunately, there is no authoritative account of the settlement. The law that established the settlements did not name the sites nor explain how they would be selected. We can, however, deduce some influential factors.

First, it is clear that camps were placed near the Dominican border near the points of conflict. The three camps in the North were located across the border from the massacre, and the two camps in the South were across from Trujillo's spring 1938 campaign *el desalojo* (the eviction), where Haitians were forced across the border (Turits 2003, p. 169). Placing camps near points of conflict is common practice in selecting sites for refugee camps since it puts camps where the

refugees are entering the country (Salemi 2021).

Second, the camps had to be placed somewhere with sufficient idle government land. The law establishing the settlements stated that the land would come from the government's holdings. But this requirement did not restrict which district hosted the camps because the government was the country's largest land owner, holding about 915,000 hectares around the country (Millspaugh 1929 p. 561). Indeed, one of the biggest problems facing the government was that it owned too much idle land and needed to put it into production, but historical property rights and scattered ownership created transaction costs that made it hard to use (Palsson 2021). This problem is seen in the discussion of the camps in President Vincent's memoirs (Vincent 1938). He focused less on the refugees' plight and more on how the camps provided a good model of how to move idle government land into production.

While government land might not have restricted which districts hosted camps, it likely influenced where within the district the camp was located. For the land to be idle over a century after independence, there must have been few competing claims to the land. Thus, the land could have been less valuable than the typical Haitian farm. Maybe the land was lower quality, though since it was idle it might have been better than farms that had been overworked for a century. Or it could have been that the land was farther away from amenities such as natural water sources or roads. Indeed, Pierre-Charles (1965 pp. 111–112) claims the camps were placed in undesirable locations.

The placement of camps is important to consider when claiming causality. Note that the most important factors affecting camp placement were either time invariant (proximity to the border) or fixed in 1937 (availability of government land). Thus, a difference-in-differences strategy could claim causality by controlling for the placement of camps using location fixed effects and relying on the exogenous timing of the massacre. I use this strategy when looking at short-run effects on land use, as described below. The long-run effects, however, require a stronger assumption for claiming a causal effect. Fortunately, in the long-run analysis, the effect that I am interested in is that there are still communities around the refugee camps, and the analysis focuses on describing these communities. Thus, I do not need to claim causality. I do explore an instrumental variable strategy in Appendix Table A7 that is common to other papers on displaced people, but I also show that the instrument is not suitable in this context and is likely questionable in other work.

This is also a good place to address inference. Since the unit of observation and treatment are both defined by geography, there is a concern that spatial correlations will bias the inference. Standard inference assumes that the residuals are independently and identically distributed, but

when geography influences the treatment and outcome, this assumption is likely to be violated. This concern is especially salient when considering persistence studies, since many might be misinterpreting spatial correlations as treatment effects (Kelly 2020). Appendix Table A4 tests for spatial correlation in the residual using Moran's statistic. In 20 out of 28 outcomes, the residuals display strong spatial correlation. To correct for this correlation in the inference, I use Conley standard errors as implemented by Hsiang (2010). These standard errors account for spatial correlations under a given radius. They are sensitive, however, to the radius chosen, and there is no best practice for selecting a radius. Since the treatment is defined by a 20 km radius from the camps, I am already assuming that there is a spatial correlation within 20 km. Thus, I use 20 km when calculating the Conley standard errors, but Appendix Table A4 reports standard errors for 10 and 30 km to show the sensitivity of the standard errors to the radius.

4.2 Short-Run Effects on Land Use

One mechanism to explain the persistent camps is access to land with incomplete property rights. The first step to establishing this mechanism is to show that the refugees caused a contemporary change in land use around the camps. To examine this effect, I estimate a difference-in-differences model. The data are aggregated into six-month periods,[4] and the following regression estimated:

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

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 A3 shows the results are robust to different specifications of the dependent variable.

To test the sensitivity of the results to the 20 km radius, I allow treatment to vary by distance and estimate the following regression:

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

[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.

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.

I interpret the β in Equation 1 and the β_ds in Equation 2 as causal effects. Identification comes from eliminating the time-invariant features of camp placement through the location fixed effects (δ_i) and assuming the timing of the massacre was exogenous to trends in land use in Haiti. Since the massacre was unexpected and committed by another country, the timing can be seen as exogenous.

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). Treatment is still defined as whether the district is within 20 km of a camp. The units outside of that radius become donor districts, and the estimator finds a set of weights for pre-treatment trends that minimizes the difference between the weighted average of the donors and the average of the treated group. The donor weights are given in Appendix Figure A2, which shows that only 7 of the donor districts receive zero weight and the rest receive positive weights. The time-period weights are reported with the results in Figure 2. This estimator is done in Stata using the sdid package from Pailañir and Clarke (2022).

4.3 Long-Run Characteristics

The long-run analysis explores both hypotheses as well as other features of the communities surrounding the refugee camps. The two hypotheses are people chose to stay around refugee camps because of amenities, specifically access to land with incomplete property rights and strong social networks. While I will not make causal claims, I can look at whether the descriptive evidence for these communities matches the hypotheses. In the long-run analysis, I also explore descriptive data on the welfare of these communities. Since these amenities keep populations tied to the area, theory predicts we should see a compensating differential somewhere else; that is, these communities might be worse off in another outcome.

Across all outcomes, the long-run analysis is similar but with different dependent variables. For the long-run, the unit of analysis is the section (the administrative level below the district in the previous analysis), and there are 564 sections in the data. I estimate the following regression:

$$Y_s = \beta_1 + \beta D_s + \Gamma X_s + \varepsilon_s \tag{3}$$

where Y_s is the outcome of interest in section s, D_s is a binary variable for whether section s is within 20 km of a camp, and X_s are section-level controls (section's population and the centroid's longitude). All outcomes are measured in 2009.

Since the long-run analysis makes identifying a causal effect harder, I use two different samples. With cross-sectional data, I can no longer control for time-invariant factors using section fixed effects. Thus, to claim a causal effect, I have to assume that had the massacre and refugee camps never existed, outcomes in 2009 for the sections near refugee camps would have evolved similarly to those far away from the camps. One potential challenge to this assumption is that these sections are located on the Haitian-Dominican frontier, which exposes them to different economic opportunities. While this is partially accounted for by controlling for the centroid's longitude (which proxies for distance from the Dominican Republic), there might still be concerns that this does not adequately account for the differences in the frontier. Thus, I use two samples: the full sample of 564 sections and a frontier sample of 95 sections, shown in Figure 1. The convenient part of the refugee camp placement is that there are two distinct regions on the frontier with camps and a region with no camps.

5 Why Camps Persist

5.1 Land and Incomplete Property Rights

First, I explore the hypothesis that the long-run inhabitants of refugee camps had access to land with incomplete rights. I do this in two parts. First, I show the effects on land use when the refugees first arrived. Then I show that the land use persists to the present.

Table 2 reports the difference-in-differences analysis on short-run land use. 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 1% level and represents an eight-fold increase over the pre-massacre average. When the treatment effect varies by distance (Panel B), 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. The treatment effect for sections greater than 30 km away quickly falls to zero and even becomes negative. 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.

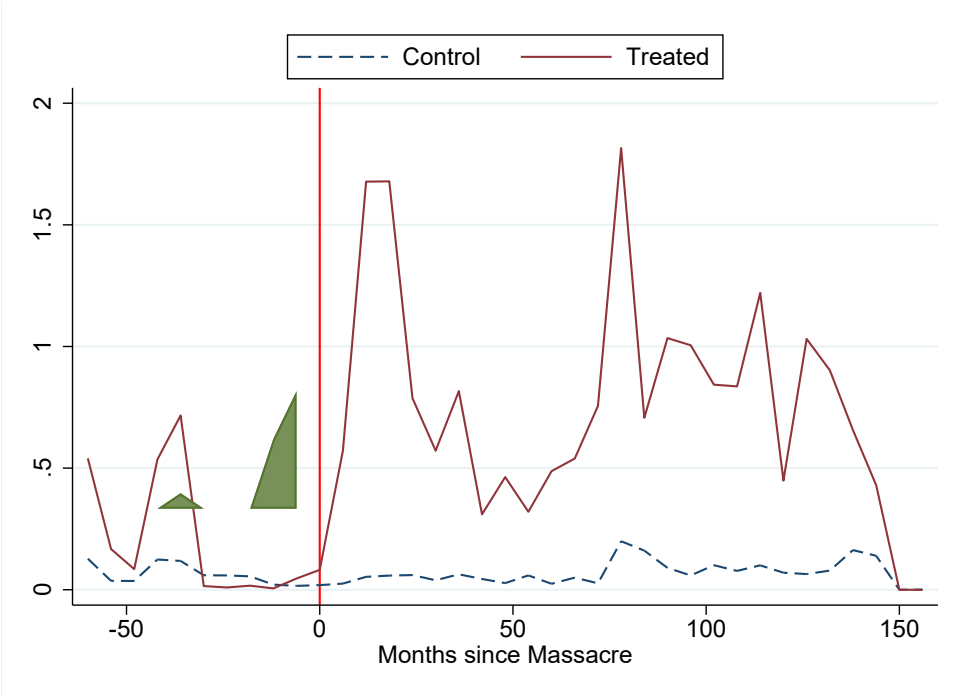
Since the state offered two types of properties for rent—agricultural and urban—I look at how camps affect requests by type. Table 2 shows that demand for agricultural properties account for

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***
	[0.14]	[0.13]	[0.034]
Panel B			
[0,10) km X Post Massacre	0.45***	0.29**	0.16***
	[0.16]	[0.13]	[0.055]
[10,20) km X Post Massacre	0.56**	0.50**	0.059***
	[0.24]	[0.24]	[0.022]
[20,30) km X Post Massacre	0.11*	0.061	0.050*
	[0.066]	[0.049]	[0.029]
[30,40) km X Post Massacre	0.028	0.028	0.00046
	[0.037]	[0.036]	[0.0066]
[40-50) km X Post Massacre	-0.069**	-0.043*	-0.027**
	[0.031]	[0.024]	[0.012]
Pre-Massacre Sample Mean	0.062	0.048	0.014

Notes: The dependent variable is the number of rental properties requested in the district per 1,000 residents per time period. The treatment variable is the district's distance to the closest refugee camp. All regressions have 3,811 observations. All regressions contain district and time fixed effects. Conley standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1

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



Notes: The dependent variable is the number of rental properties requested in the district per 1,000 residents per time period. The treated group is districts within 20 km of a refugee camp while the synthetic counterfactual is constructed from districts more than 20 km away. The weights for donor districts are reported in Appendix Figure A2. The shaded area below the lines shows the weights used to average pre-treatment time periods. Graph created in Stata using the sdid package from Pailañir and Clarke (2022).

about 75% of the increase and urban properties account for the other 25%.

Figure 2 shows the synthetic difference-in-differences results. Along with the donor weights given in Appendix Figure A2, the synthetic difference-in-differences also weights time periods, and the weights are displayed in Figure 2 as shaded areas in the pre-period. Since the height of the shaded area represents the weight placed on the time period, we can see that the analysis puts most of the weight on the 24 months leading to the massacre, but also puts some weight on a period about four years earlier where the treated areas saw a spike in requests. The benefit of the synthetic difference-in-differences approach is that it can account for the early deviation while also putting more weight on the more recent period when the trends are more similar. The results in Figure 2 show that even accounting for the synthetic counterfactual, the treated districts saw a significant increase in requests following the massacre. The treatment effect 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

Table 3. Refugee camps and land use in 2009

	Dep Var Mean	Full Sample (N=564)	Frontier Sample (N=95)
State Rentals	0.022	0.043***	0.058***
		[0.0096]	[0.018]
log(Farm Size)	-0.44	0.51**	0.029
		[0.22]	[0.11]
Land Conflict	0.72	-0.18	-0.34***
		[0.11]	[0.11]
Homesteads	0.002	0.0031***	-0.00025
		[0.0011]	[0.0028]

Notes: Each cell is a separate regression where the dependent variable is listed in the first column. State rentals indicates the share of farm properties in the district that are rented from the state; Farm Size is the size of the average farm in hectares; Land Conflict is whether the district listed disputes over property as one of the top three problems in the district; Homesteads is the share of farm properties in the district that are homesteads. The cell reports the coefficient on a dummy variable for whether the section is within 20 km of a refugee settlement. All regressions control for total population and longitude. Conley standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1

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, which is just in the range of what Foster and Rosenzweig (2022) calculate to be small enough for a single family to cultivate but not big enough to justify the transaction costs with hiring non-family labor. 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).

Next, I look at how this land use persisted to the present. Table 3 estimates Equation 3 for the outcomes related to land use. The first outcome of interest is the share of farms in the section that are rented from the state. The increase in land use seen in Table 2 was through a government-run rental program. Since the rental program still existed in 2009, it is reasonable to ask whether those rentals persisted. Table 3 shows that in the typical section, 2% of farms were state rentals. But in the sections within 20 km of the refugee settlements, the share of state rentals was 4 percentage points higher, and the difference is significant at the 1% level. When I restrict the sample to the

frontier sections, the difference is about 6 percentage points and is still significant at the 1% level. The refugee shock led to persistent participation in the rental program.

Renting land from the government gives the residents access to secure land with incomplete property rights. Because of a historical tradition of subdividing land amongst heirs, Haiti's farms have two big problems. First, they are small. Palsson (2021) shows that rental properties gave farmers access to larger plots of land that had not been subject to subdivision, and the results in Table 3 confirm this. Farms around refugee camps were much larger than farms in typical sections, though they were only slightly larger than farms on the frontier. Another problem is that nearly the whole country claims land but there are few formal distinctions on who owns what (Palsson 2021). Thus, it is not surprising that one of the biggest sources of conflict listed in the community survey was conflict over land, with 72% of sections including it as one of their top three problems. But Table 3 shows that sections near refugee camps were 18 percentage points less likely to include it in their top three. This difference is not significant at conventional levels in the full sample, but in the frontier sample the magnitude increases to 34 percentage points and the difference is significant at the 1% level. This reduction in conflict is likely a result of the demand for rental properties seen in Table 2 leading to an increase in property that had been surveyed and titled by the state (Palsson 2023). Thus, these farms were secure, but the farmers did not own them, so the rights were incomplete.

The renters could have gained more complete property rights if they had converted the rentals to homesteads. In 1934, the government passed a homesteading program aimed at putting state rentals in private hands, hoping to create an incentive to invest on the land. But the program was historically ineffective (Palsson and Porter 2023), and in the present it is nearly impossible to access (de Soto 2000). Since rentals were popular in these areas for so long, we might expect them to be prime candidates for homesteading. In the full sample, these sections are more than twice as likely to have homesteads, but the difference goes away when the sample is restricted to the frontier. The residents had access to land, but the government was not helping them take advantage of the path to more complete property rights.

These properties with incomplete rights are a plausible mechanism for why the population centers persisted. Since the properties were owned by the state, the farmers could not sell, mortgage, or transfer them. The state protected their right to cultivate them, but if a farmer migrated, he lost any investment on the plot without even being able to use it to finance migration. In many other contexts, incomplete property rights have constrained migration (Chernina et al. 2014, De Janvry

et al. 2015, Adamopoulos et al. 2022, Rao et al. 2021).

5.2 Social Capital

The second hypothesis for the persistent population is that social networks keep people in the area to maintain informal insurance networks. To test this hypothesis, I look at indicators of social cooperation in 2009 around the refugee camps.

Table 4 estimates Equation 3 for eight components of social cooperation. The survey asked the section leaders to rate how well the community participated in different social activities. The dependent variable is a dummy for whether the section rated cooperation "average," "high," or "very high," which for most of the variables split the sample in half. In the full sample, the results show that while there was no significant difference for three activities, there were large, statistically significant, differences for the other five. Two of these five are activities strongly related to informal insurance: helping with finances or in times of tragedy. Since it is hard to evaluate eight separate regressions, Table 4 also reports estimates of Equation 3 for a latent factor estimated from the eight questions (see Appendix Table A6 for factor loadings). In the full sample, social cooperation is 10% higher around the refugee camps, which is statistically significant at the 5% level.

In the frontier sample, most of the differences decrease in magnitude and in statistical significance, but most are still positive. One reason for the shrinking differences is that frontier sections probably face the same problem. In a century of urbanization, there has to be something tying people to the frontier. Since these people are free to move, those who stay will be selected similarly to those who stay in other frontier sections. What is notable is that the sections around refugee camps were relatively recent settlements and they have similar (or higher) levels of social capital as the other frontier sections.

The evidence shows that sections around refugee camps had higher levels of social capital than most of the country. While I cannot directly link the levels of social capital with the decision to stay, other research has shown that social networks significantly reduce migration (Munshi and Rosenzweig 2016, Morten 2019). And this could be a general phenomenon since communities that experience shared trauma frequently see an increase in social capital (Blattman 2009, Bellows and Miguel 2009).

Table 4. Refugee camps and social capital in 2009

	Dep Var Mean	Full Sample (N=564)	Frontier Sample (N=95)
High level of community participation in:			
Agricultural activities	0.60	-0.086	-0.045
		[0.075]	[0.17]
Severe weather conditions	0.56	0.036	0.079
		[0.071]	[0.14]
Maintaining roads and infrastructure	0.55	0.25***	0.079
		[0.087]	[0.10]
Community or family tragedies	0.49	0.14**	0.18
		[0.059]	[0.13]
Protecting natural resources	0.45	0.16***	0.046
		[0.058]	[0.10]
Religious and cultural events	0.62	0.18**	0.11
		[0.077]	[0.12]
Finances	0.54	0.069**	0.12
		[0.030]	[0.080]
Security problems	0.49	0.083	0.21**
		[0.072]	[0.080]
Cooperation Factor Variable	3.65	0.39**	0.26
		[0.18]	[0.28]

Notes: Each cell is a separate regression where the dependent variable is listed in the first column. The dependent variable is a dummy variable that equals one if the district rated cooperation in that issue as "average," "high," or "very high." The Cooperation Factor Variable is a factor variable made from all eight measures of social cooperation (see Appendix Table A6 for factor weights). The cell reports the coefficient on a dummy variable for whether the section is within 20 km of a refugee settlement. All regressions control for total population and longitude. Conley standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table 5. Refugee camps and education in 2009

	Dependent Variable Mean	Within 20 km (1)	(2)	(3)
Literacy Rate	0.45	-0.048	-0.046	-0.042
		[0.039]	[0.037]	[0.037]
Has Primary School	0.97	0.034	0.034**	0.021
		[0.021]	[0.014]	[0.023]
Has Secondary School	0.52	0.020	0.036	0.061
		[0.084]	[0.080]	[0.13]
Has Technical School	0.17	-0.0027	0.0028	0.077
		[0.055]	[0.048]	[0.058]
Has Adult Literacy Center	0.25	-0.18*	-0.18*	-0.34***
		[0.10]	[0.10]	[0.097]
Control for 1950 Literacy			X	X
Frontier				X
N		564	554	95

Notes: Each cell is a separate regression and reports the coefficient on a dummy variable for whether the section is within 20 km of a refugee settlement. Literacy Rate refers to the section's literacy rate calculated in the 2009 agricultural census; each of the "Has" variables is a dummy for whether the section has at least one of those educational institutions. The regressions in column (1) include controls for log population in 2009 and the section's longitude. Regressions in (2) contain the same controls as (1) plus a control for the literacy rate in 1950 (see text for calculation). Regressions in (3) restrict to districts frontier. Conley standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1

6 Evidence for Compensating Differentials

I now turn to the consequences of these persistent populations. If amenities tie the population to these areas, then the value of these amenities should be reflected in compensating differentials (Greenstone et al. 2010). For example, many people tend to stay in areas where they grew up, but when natural disasters force them out of their homes they move to areas that provide more opportunity and higher incomes (Deryugina et al. 2018, Nakamura et al. 2021). This suggests that the area where they grew up provided some amenity value. In this section, I look for evidence that the amenities that kept people in the area led to a trade-off on other margins.

First, I look at education outcomes. A consistent result across many countries is that refugees

and their descendants tend to invest more in human capital (Bauer et al. 2013, Becker et al. 2020, Ayesh 2022). In fact, in the years following the massacre, schooling was one of the main outcomes tracked for the refugee camps (Republic of Haiti 1939). If refugees and their descendants place a higher value on education, we should observe higher literacy rates in sections near the settlements. But, as Table 5 shows, sections around refugee camps have literacy rates 4.8 points (10%) lower than sections outside the radius. While the result is not statistically significant, when contrasted to the strong prior that this should be positive, it is still a remarkable result. But this could be a result of pre-existing differences across sections where refugees happen to land in sections with much lower literacy rates, and today they would be even lower had the refugees not arrived. To correct for pre-existing differences, the regression in column (2) controls for literacy rates in 1950. The difference persists, and when I limit the sample to just frontier sections, the gap stays constant.

Since these lower literacy rates contrast with the rest of the literature, I explore why literacy rates were lower. I focus on educational opportunities since this is where a developing country is most likely to differ from countries in Western Europe. The rest of Table 5 tests for differences in education opportunities. There is no difference in access to primary schools, which is not surprising since 97% of sections have one. These sections were also no less likely to have a secondary school or technical school. The only educational opportunity that was different was that these sections were 18 percentage points less likely to have an adult literacy center, statistically significant at the 10% level. While one might think this lack of literacy centers is because the sections are on the frontier, when the sample is restricted to the frontier the gap expands to 34 percentage points and becomes significant at the 1% level.

While we do not have evidence for how effective these literacy centers are, these results suggest they might play an important role in Haitian education. Primary schools are available in 97% of sections, yet the average literacy rate is only 45%. Thus, Haiti's primary schools might not contribute much to literacy. Indeed, the literacy rate for men who had completed five years of school in 1954 was 92%, but by 1996 it had fallen to 73% (Le Nestour et al. 2021). These literacy centers could be filling an important gap in Haiti's education system.

Next, I examine wealth. While I do not have measures of income, I can look at livestock 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 6 shows that these sections are not noticeably disadvantaged. Evaluating livestock holdings at

Table 6. Refugee camps, livestock wealth, and other amenities in 2009

	Dep Var Mean	Full Sample (N=564)	Frontier Sample (N=95)
log(Livestock Value)	10.5	0.15	-0.27
		[0.24]	[0.19]
Section has a:			
Post Office	0.04	-0.041***	-0.027
		[0.015]	[0.021]
Civil Registry Office	0.10	0.082	0.17*
		[0.069]	[0.097]
Peace Court	0.09	0.085	0.16**
		[0.057]	[0.079]
Health Clinic	0.49	-0.058	-0.12*
		[0.066]	[0.066]
Pharmacy	0.25	-0.022	-0.049
		[0.050]	[0.088]
Sports Facilities	0.13	0.27***	0.23**
		[0.091]	[0.097]
Internet Cafe	0.17	-0.13**	-0.067***
		[0.053]	[0.019]
Landline Telephone	0.17	-0.026	0.096
		[0.082]	[0.085]
Gas Station	0.08	-0.057	0.015
		[0.040]	[0.046]

Notes: Each cell is a separate regression where the dependent variable is listed in the first column. Livestock Value is the value of all cows, goats, and pigs in the section; each of the "Has" variables is a dummy for whether the section has that amenity. The cell reports the coefficient on a dummy variable for whether the section is within 20 km of a refugee settlement. All regressions control for total population and longitude. Conley standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1

market prices, these sections are 15% wealthier than the control group, though the difference is not significant. The sign flips when the sample is restricted to the frontier sections, but it is again not statistically significant. These noisy results suggest the residents are not sacrificing significant livestock wealth to stay in the communities. Of course, this is only one source of wealth, and there might be other margins where they are significantly behind. I will note, however, that land is another source of wealth, and Table 3 shows that these communities have as large or larger farms than most sections.

Another margin where these communities could be lacking is in other amenities. Table 6 looks at the availability of nine other amenities. The first three are all related to the presence of the state. Relative to the rest of the country, these sections are significantly less likely to have a post office, but relative to the frontier the difference is not as large. They are more likely to have a civil registry or a peace court, though the difference is only significant in the frontier sample and only at the 10% level. I do not want to overinterpret these results, but the slightly larger state presence here could be a result of the government's heavy involvement in the settlements' creation. The other six amenities are supplied by a mix of state and private entities. These sections might have less access to health care, as indicated by lower chances of having a health clinic or a pharmacy. They also have less information communication technology, with noisy results on having landline telephones but significantly less access to internet cafes. And they have slightly less access to gas stations. While the access to most amenities is weakly negative, there is one amenity that is far more likely to be available around refugee camps than anywhere else in the country: sports facilities. I am not sure why sports facilities are more likely to be in these sections, but it could be a result of non-profits trying to help the refugees and their descendants, or from higher social capital coordinating public goods.

In summary, the evidence for compensating differentials is weak. It looks like the populations in these areas were able to settle around the camps and receive the local amenities without sacrificing much on other margins. The most significant sacrifice might be access to education, though this assumes that refugees would have wanted much higher levels of literacy rather than roughly the same level as comparable populations. There could, of course, be other margins where these communities are disadvantaged. But given the benefits with little perceived costs, it makes sense that these populations would persist.

7 Conclusion

The forces of path dependence mean that historical events can continue to influence economic outcomes decades or centuries later. But for those forces to have power, something must keep populations from migrating. In this paper, I look at why refugee camps formed in 1938 are still population centers 70 years later. I argue people stay to retain access to amenities.

These amenities influence how we interpret studies of path dependence. While many studies show that path dependence leads to lower welfare for some individuals, these gaps exist because those living under the influence of historical events value something about the status quo. Instead of migrating, they choose to take advantage of local amenities. Clearly it would be better if they could retain their amenities while closing the observed gaps. But if we focus on the gaps and ignore the amenities, we may overstate the welfare consequences of path dependence.

Beyond the contribution to our understanding of path dependence, the focus on amenities is important to the study of refugee camps. An understanding of amenities is important for designing and addressing refugee camps. Camps are helpful for coordinating aid, but they also help coordinate social networks. Any attempt to close or relocate camps could disrupt those networks and cause significant harm. Furthermore, since giving refugees land is a common relief practice (Bharadwaj and Mirza 2019, Alix-Garcia et al. 2018, Zhu et al. 2018), policymakers need to consider that that these in-kind contributions might anchor refugees to the land.

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.

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 settling 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 that in the long-run these areas fare well. In camps with mobility restrictions, on the other hand, the positive effects might be limited. Thus, for refugee camp policy, policymakers should carefully consider context before generalizing the results.

There are a few paths forward for this research. First, some research has shown that refugees increase FDI to their country of origin (Mayda et al. 2022) and that when refugees return they bring home higher levels of human capital (Bahar et al. 2022). But these studies look at refugees who move from low-income to high-income countries. In Haiti, and in most of the world, refugees setttle in poor countries, and we might not expect these same results to hold. Second, for understanding the economic history of Haiti, an interesting path forward for research would be to understand more about the economic consequences of the massacre in the Dominican Republic. Some work has shown that displacement can disrupt development in the origin country (Testa 2020), and there is some evidence that Dominican food prices increased after the massacre (Turits 2003 p. 178). Since so much research focuses on the arrival of refugees, it would be worth pursuing the effects of refugees leaving.

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 distance to the nearest refugee camp. Second, because refugees were agricultural workers in the Dominican Republic and therefore more likely to be males, and because the majority of massacre victims were women and children (Derby 1994 p. 525), 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 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 Balance Test of Treatment and Control Districts

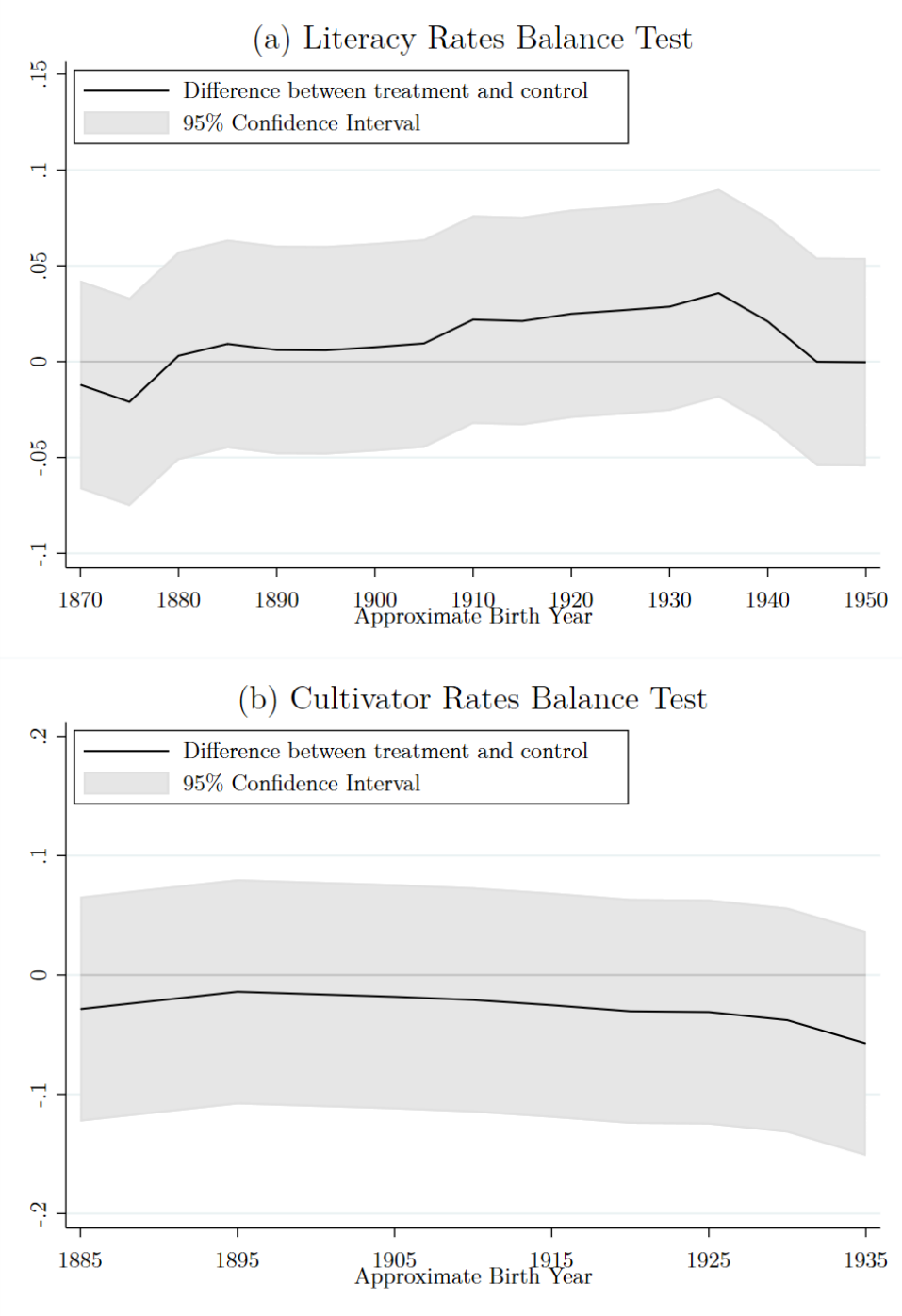
When looking for causal effects, it is important to compare the treatment and control group to see if covariates balance. Unfortunately, this is difficult to do in this context because the data needed from this period in Haiti are practically non-existent. The first place to look for such data would be in the census, but Haiti's first census was in 1950. Since this was 13 years after the massacre, the data are contaminated by the effects of the refugee camps and must be used with caution. For example, since the refugees are counted in these populations, we cannot test for balance in population or population density before the massacre.

Understanding that the comparisons are complicated by the refugees in the census, I look at balance tests for outcomes that should be less affected by the counting refugees. Since most of the refugees lived in these regions already but on the Dominican side of the border, they should be similar to the Haitian-side populations in areas like literacy and occupations. In across Appendix Figure A1, I test for balance on these two outcomes across cohorts and then discuss possible biases in the next paragraph. Testing across cohorts shows not just the balance for that age group, but it also reveals whether the treatment and control group were experiencing similar trends. Appendix Figure A1a shows the balance test for literacy rates across cohorts. Across all cohorts, there are no statistically significant differences between literacy rates. Appendix Figure A1b shows the share of employed who were cultivators. Again, there are no statistically significant differences for any cohort.

Since the 1950 census includes the refugees, it is important to consider how this might bias the balance test. Suppose we thought that before the refugees arrived, these districts were not balanced. For example, suppose the treatment areas were more literate than control areas and the difference was statistically significant. For these balance tests to show no statistical difference between the two groups, the literacy rate of the refugees would have to be low enough for this small group (8% of the population) to dilute the difference. And it would have to work across all cohorts.

[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.

Figure A1. Literacy and Occupational trends across cohorts in 1950 census



Notes: Data come from 1950 census. The treatment group is districts within 20 km of a refugee camp and the control group is all other districts.

Table A2. Balance test of covariates available before 1937

	Observations	Within 20km	Outside 20km	Diff
log(Public Land Revenue)	90	7.1	7.1	0.005
				[0.405]
log(Other Rural Revenue)	92	6.5	8.2	-1.72***
				[0.558]
log(Distance to Closest Port)	107	2.9	3.0	-0.089
				[0.225]
log(District Area)	107	4.9	5.4	-0.52**
				[0.203]
Ruggedness Index	107	12.6	20.3	-7.66***
				[2.027]

Notes: Public Land Revenue is the average revenue from public land rentals collected in the district from 1925 to 1931. Other Rural Revenue indicates the average of total non-rental revenue collected over the same period. The Distance to Closest Port is the distance to the closest port of entry listed in Haiti Bureau du representant fiscal (1937). District area is the area of the administrative unit in km^2. Ruggedness Index measures the ruggedness of the district.

In reality, the refugee and non-refugee populations in these regions were probably so similar that including the refugees has no material effect on the balance tests.

Outside of the census, there are two sources of data that I can explore. The only district-level data available before the massacre are rural tax revenues from 1925 to 1931, and even then there are only reports for about 92 of the 107 districts. These reports show how much revenue the district collected for public land rentals, vital statistics fees, and other sources where the local collectors had stewardship. Since the public land revenues come from the public land rentals that I explore in the empirical work, I look for balance here first. Appendix Table A2 reports the average log-revenues from public lands collected in districts within and outside of 20 km of a refugee camp, noting that two of the districts outside of the radius did not report land rental receipts. The difference between the two groups is not statistically significant. The table also reports the difference in average log-revenues for all other rural receipts. Here there is a statistically significant difference where districts within 20 km collected less.

Another district-level variable that was not reported but that I can proxy is market access. One concern could be that the districts near camps had better access to markets because of their proximity to the border. I look at market access using the 17 ports of entry listed in a 1937 fiscal report (Haiti Bureau du representant fiscal 1937) and calculate the distance to the district's nearest port of entry. The balance test for market access is reported in Appendix Table A2. Using this measure of market access, there is no significant difference between the two groups.

Table A3. Robustness of land request results to different specifications of the outcome variable

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

Notes: The dependent variable used in the main text is Requests/capita, reported in the first column. The second column takes the inverse hyperbolic sine (IHS) of requests/capita, and the third just takes the IHS of requests. The final column uses a Poisson count model with the number of requests as the dependent variable. All regressions use panel data with 3,811 observations. Standard errors are clustered by district.

Finally, I can look at time-invariant characteristics of the district. One concern could be that camps were placed where more land was available. While I do not have measures of land availability, the district's area should be a good proxy. Appendix Table A2 shows that districts near camps had smaller area, so they likely had less land available. Finally, since Haiti is a mountainous country, I look at ruggedness between the two groups. Districts near camps are less rugged, and the difference is statistically significant. Since these features are time-invariant, the differences are accounted in the short-run analysis using district fixed effects. But they may cause some concerns with the long-run analysis.

A.3 Robustness Checks

In this section, I perform a series of robustness tests for the analyses in the paper.

First, in Table A3 I look at how the short-run effects on land use vary across different specifications. In the first column, I include the main results, where the dependent variable is the number of requests per 1,000 inhabitants in 1950. The second column looks at the inverse hyperbolic sine (IHS) of requests per capita. The third column shows the IHS of total requests (not scaled for population). Finally, the fourth column uses a Poisson regression where the dependent variable is the number of requests. Every specification shows large, significant effects.

Table A4 explores the issues with spatial correlation in the long-run results. The first column lists the coefficients in the main results, and the second column reports the Moran's I statistic. In 20 out of 28 outcomes, the residuals display strong spatial correlation. Throughout the text, I use Conley standard errors with a 20 km cutoff. But since there is no general rule for determining cutoffs, I also report the standard errors for 10 km and 30 km.

Table A5 takes the long-run analysis but removes sections with populations greater than 50,000.

Table A4. Testing for spatial correlation of residuals and exploring different specifications of standard errors

	Coef	Moran	Robust	Conley, 10	Conley, 20	Conley, 30
State Rentals	0.043	7.33	0.013	0.021	0.025	0.020
log(Farm Size)	0.51	39.2	0.068	0.11	0.16	0.19
Land Conflict	-0.18	8.05	0.082	0.11	0.11	0.085
Homesteads	0.0031	-0.79	0.0023	0.0020	0.0015	0.0017
Cooperation Factor Variable	-0.086	1.01	0.085	0.097	0.075	0.075
Agricultural activities	0.036	2.29	0.085	0.072	0.071	0.064
Severe weather conditions	0.25	2.65	0.076	0.060	0.087	0.065
Maintaining roads and infra.	0.14	3.42	0.084	0.086	0.059	0.054
Community/family tragedies	0.16	2.00	0.085	0.068	0.058	0.062
Protecting natural resources	0.18	0.85	0.076	0.079	0.077	0.065
Religious/cultural events	0.069	1.92	0.083	0.075	0.030	0.047
Finances	0.083	3.41	0.083	0.093	0.072	0.057
Security problems	0.39	1.28	0.16	0.20	0.18	0.18
log(Livestock Value)	0.15	29.4	0.093	0.14	0.15	0.18
Civil Registry Office	-0.041	0.07	0.014	0.015	0.015	0.011
Post Office	0.082	-0.49	0.059	0.059	0.069	0.048
Peace Court	0.085	-0.28	0.059	0.056	0.057	0.043
Health Clinic	-0.058	4.22	0.084	0.092	0.066	0.045
Pharmacy	-0.022	2.71	0.069	0.072	0.050	0.045
Sports Facilities	0.27	3.96	0.076	0.093	0.091	0.084
Internet Cafe	-0.13	6.83	0.038	0.043	0.053	0.055
Landline Telephone	-0.026	12.0	0.066	0.080	0.082	0.070
Gas Station	-0.057	7.66	0.029	0.030	0.040	0.046
Share Literate	-0.048	8.34	0.025	0.040	0.039	0.038
Primary School	0.034	1.6	0.026	0.027	0.021	0.018
Secondary School	0.02	9.37	0.082	0.070	0.084	0.083
Technical School	-0.0027	3.19	0.058	0.074	0.055	0.061
Adult Literacy Center	-0.18	12.0	0.065	0.076	0.10	0.069

Notes: The Coef column reports the coefficient for the treatment of being within 20 km of a refugee camp. The Moran column reports Moran's I, a test statistic for whether there is spatial correlation in the residuals. The Robust column reports robust standard errors without accounting for spatial correlation. Since Conley standard errors are sensitive to the cutoff distance, each of the Conley columns report the Conley standard error using the column's cutoff distance (10, 20, or 30).

Table A5. Robustness of estimates to removing sections with populations above 50,000

	Full	Frontier		Full	Frontier
State Rentals	0.045*	0.061**	Civil Registry Office	0.089	0.19*
	[0.025]	[0.025]		[0.069]	[0.10]
log(Farm Size)	0.50***	0.037	Post Office	-0.040**	-0.024
	[0.14]	[0.091]		[0.016]	[0.021]
Land Conflict	-0.17	-0.31***	Peace Court	0.092	0.18**
	[0.12]	[0.11]		[0.057]	[0.082]
Homesteads	0.0032*	-0.000068	Health Clinic	-0.043	-0.098
	[0.0017]	[0.0028]		[0.084]	[0.078]
Cooperation Factor Variable	-0.082	-0.00028	Pharmacy	-0.0061	-0.035
	[0.075]	[0.17]		[0.063]	[0.093]
Agricultural activities	0.025	0.052	Sports Facilities	0.26***	0.20**
	[0.068]	[0.13]		[0.082]	[0.085]
Severe weather conditions	0.26***	0.08	Internet Cafe	-0.12**	-0.063**
	[0.099]	[0.11]		[0.048]	[0.030]
Maintaining roads and infra.	0.14**	0.16	Landline Telephone	-0.018	0.11
	[0.054]	[0.13]		[0.082]	[0.092]
Community/family tragedies	0.17**	0.072	Gas Station	-0.076***	-0.029*
	[0.068]	[0.11]		[0.029]	[0.015]
Protecting natural resources	0.17**	0.086	Share Literate	-0.047	-0.032
	[0.073]	[0.11]		[0.038]	[0.041]
Religious/cultural events	0.085*	0.16	Primary School	0.033	-0.0024
	[0.048]	[0.11]		[0.021]	[0.012]
Finances	0.099	0.24***	Secondary School	0.014	0.066
	[0.074]	[0.085]		[0.085]	[0.14]
Security problems	0.40**	0.27	Technical School	0.011	0.11*
	[0.19]	[0.30]		[0.051]	[0.063]
log(Livestock Value)	0.17	-0.27	Adult Literacy Center	-0.17	-0.23
	[0.15]	[0.16]		[0.10]	[0.15]

Notes: All specifications use Conley standard errors and control for population and longitude. The Full sample is all sections in Haiti with populations below 50,000 (N=536), and the Frontier sample is the frontier sections with populations below 50,000 (N=93). *** p<0.01, ** p<0.05, * p<0.1

Table A6. Details for factor analysis

	Within 20 km	Outside 20 km	Factor Loadings
In agricultural activities	2.4	2.6	0.35
In times of severe weather	2.5	2.6	0.53
Management of roads/infrastructure	2.9	2.5	0.42
In the case of social/family tragedies	2.7	2.3	0.45
Management of natural resources	2.7	2.3	0.32
In cultural/religious events	2.9	2.7	0.48
In finances	2.7	2.5	0.31
In the case of security issues	2.7	2.3	0.32

Notes: The variables reflect the section's level of cooperation in the eight outcomes. 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." The factor model is estimated from a polychoric correlation matrix.

A.4 Synthetic Difference-in-differences weights

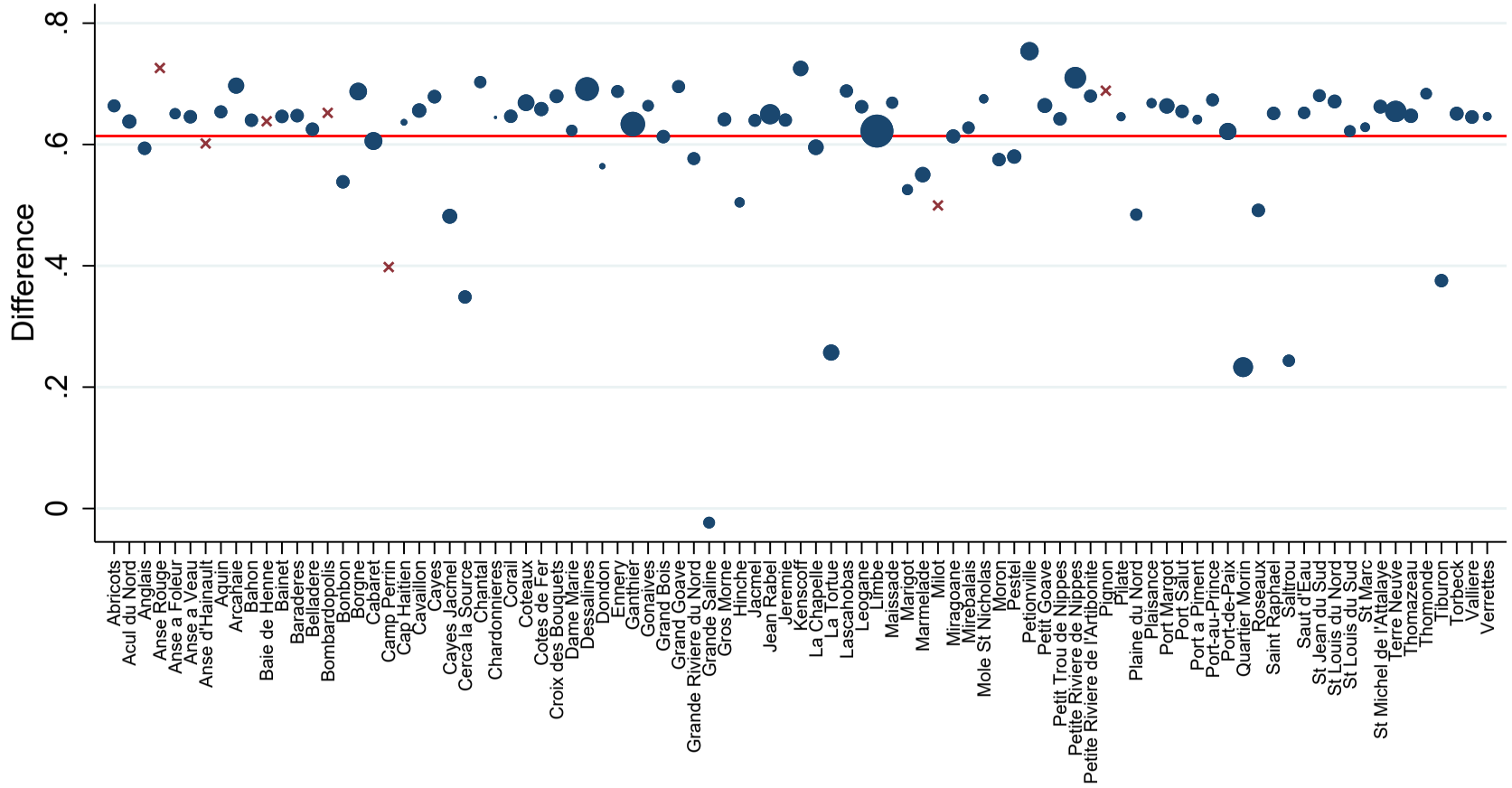
The synthetic difference-in-differences analysis creates counterfactual outcomes using two sets of weights. In the words of Arkhangelsky et al. (2021) "The use of weights in the SDID estimator effectively makes the two-way fixed effect regression 'local,' in that it emphasizes (puts more weight on) units that on average are similar in terms of their past to the target (treated) units, and it emphasizes periods that are on average similar to the target (treated) periods." The weights for the periods are reported in the main text in Figure 2. The weights for units are reported here in Figure A2. The weights show that all donors except 7 receive non-zero weight.

A.5 Factor Analysis

The analysis in Table 4 includes a latent factor as 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. 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.

Figure A2. Weights on donor districts used in synthetic difference-in-differences analysis.



Notes: The size of the dot is proportional to the weight put on the district. An "x" indicates the district received zero weight. The y-axis measures the difference between that district and the treated districts, and the weighted average of all differences is the horizontal red line. Analysis done in Stata using the sdid package from Pailañir and Clarke (2022).

A.6 IV Estimates

The placement of refugee camps creates problems for interpreting the long-run correlations as causal. Even if we assume the only factors influencing their placement were things like proximity to the border or the availability of state land, these factors can also be related to confounders that affect the outcomes of interest. We can control for proximity to the border, but there are no records of the availability of state land, so we still would have an omitted variable. But there could be other factors affecting camp placement that we have not discussed that could also create endogeneity problems. In this appendix, we explore an instrumental variable strategy used in other papers on displaced people. Our exploration finds that this particular instrument may be weaker than previously thought due to spatial correlation.

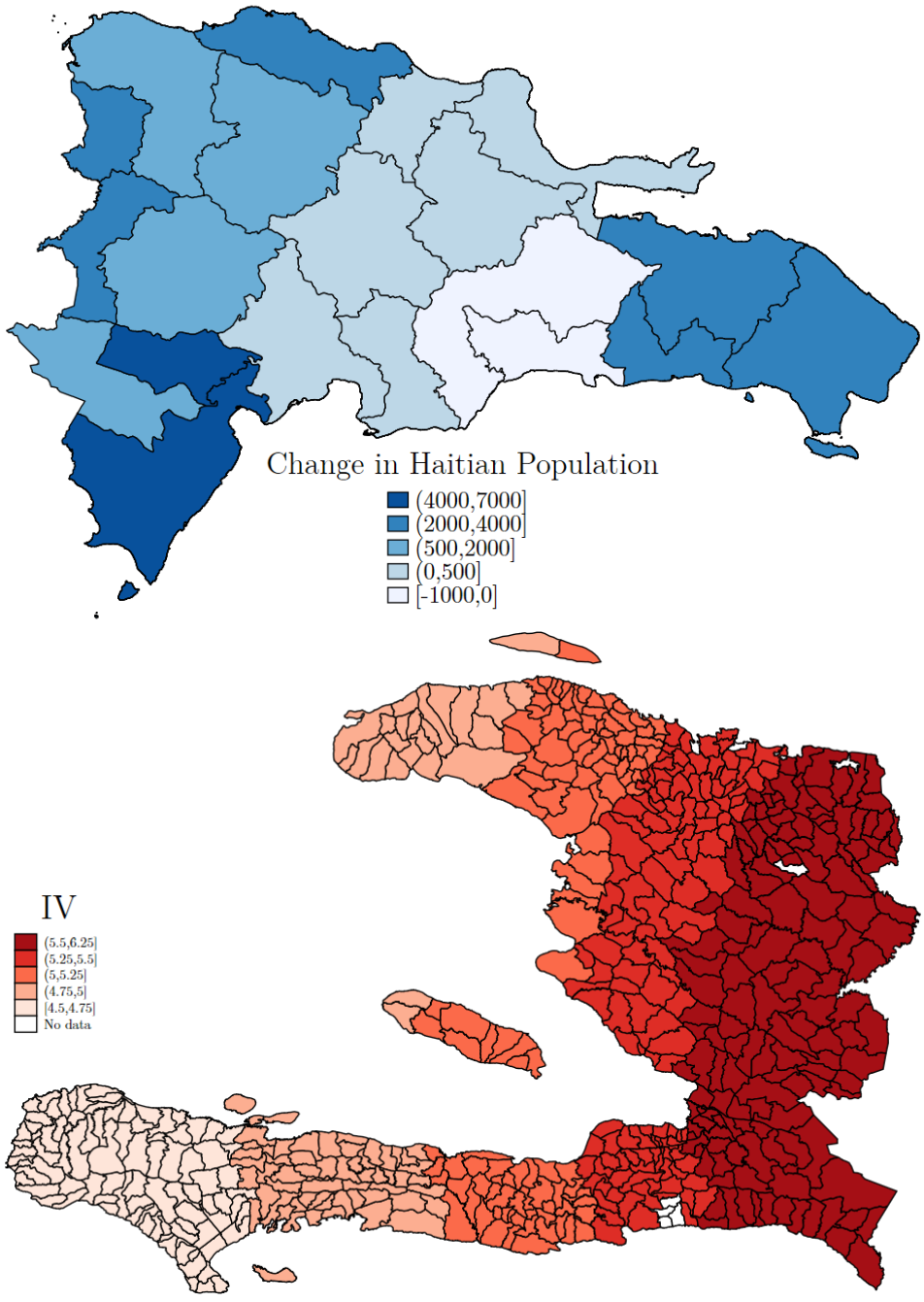
These papers instrument for the presence of displaced people using a receptivity index. This index is developed in Depetris-Chauvin and Santos (2018) and Calderón-Mejía and Ibáñez (2016). The index for section i is defined as

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

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. The index works similar to a gravity model of trade: Haitian sections that are closer to Dominican *communeros* with greater outflows are more likely to receive the refugees.

The key concepts behind the instrument are displayed in Figure A3, which also helps to show its weakness. Panel A3(a) shows the spatial distribution of outflows from Dominican *communeros*. The largest outflows are along the Western frontier, where the massacre and evictions took place, and in the East, where the largest sugar plantations (and, therefore, Haitian migrant labor) were located. Panel A3(b) shows the spatial distribution of the receptivity instrument. The receptivity index is highest along the Eastern border and gets weaker moving West. This map highlights the instrument's weakness. While this index might work when violence is randomly distributed throughout the country, it is not convincing when the violence and displacement emanate from a single origin. The instrument effectively proxies the section's distance from the Dominican border. Since the regressions control for distance from the border using the section's longitude, the instrument's remaining variation comes from slight differences in how close the section was to the DR's

Figure A3. Spatial distribution of IV



Haitian population centers. Despite the weakness, I proceed with the analysis, understanding that we need to be careful about drawing too strong of conclusions.

The instrumental variable strategy rests on the identifying assumptions that this receptivity index 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. It is not clear that either condition is satisfied. At first glance, the first identifying assumption is satisfied: if we do not account for spatial correlation, the F-statistic of the first-stage regression is 33.27. But the spatial correlation is clearly the dominating feature of this instrument. Once that is accounted for, the F-statistic is 2.28, which means this is a weak instrument. For the second identifying assumption, while it is impossible to verify, the strongest case for it is that population changes in another country 70 years earlier would only affect the 2009 outcomes through the refugee camps. The case against it is that the index is a proxy for distance to the Dominican Republic, and that distance is still important for 2009 outcomes regardless of the refugee camps.

Despite all of the flaws, I compare the IV results to the OLS results in Table A7. All specifications use Conley standard errors and use the full Haiti sample. There are no strong patterns to discern. Sometimes the results are similar across specifications, sometimes the magnitude gets really large (a typical result in IV estimates), and in a few cases the sign of the estimate flips. Many of the differences are likely due to the increased bias from the weak instrument. The main takeaway is a warning that failing to account for spatial correlation in the instrument can create significant bias in the estimates.

Table A7. Comparison of OLS and IV estimates

	OLS	IV		OLS	IV
State Rentals	0.043	0.051	Civil Registry Office	0.082	-0.075
	[0.025]	[0.070]		[0.069]	[0.27]
log(Farm Size)	0.0031	0.013	Post Office	-0.041	-0.03
	[0.0015]	[0.0085]		[0.015]	[0.13]
Land Conflict	0.51	1.62	Peace Court	0.085	-0.034
	[0.16]	[1.17]		[0.057]	[0.22]
Homesteads	-0.18	1.33	Health Clinic	-0.058	-0.18
	[0.11]	[1.22]		[0.066]	[0.52]
Cooperation Factor Variable	0.39	0.35	Pharmacy	-0.022	-0.34
	[0.18]	[0.83]		[0.050]	[0.48]
Agricultural activities	-0.086	0.32	Sports Facilities	0.27	0.29
	[0.075]	[0.49]		[0.091]	[0.31]
Severe weather conditions	0.036	0.55	Internet Cafe	-0.13	-0.58
	[0.071]	[0.52]		[0.053]	[0.43]
Maintaining roads and infra.	0.25	0.65	Landline Telephone	-0.026	-0.65
	[0.087]	[0.40]		[0.082]	[0.64]
Community/family tragedies	0.14	-0.6	Gas Station	-0.057	-0.18
	[0.059]	[0.63]		[0.040]	[0.23]
Protecting natural resources	0.16	0.16	Share Literate	-0.048	-0.21
	[0.058]	[0.46]		[0.039]	[0.26]
Religious/cultural events	0.18	0.024	Primary School	0.034	0.017
	[0.077]	[0.42]		[0.021]	[0.098]
Finances	0.069	-0.038	Secondary School	0.02	-0.13
	[0.030]	[0.46]		[0.084]	[0.66]
Security problems	0.083	-0.3	Technical School	-0.0027	-0.52
	[0.072]	[0.69]		[0.055]	[0.52]
log(Livestock Value)	0.15	3.75	Adult Literacy Center	-0.18	0.091
	[0.15]	[2.82]		[0.10]	[0.56]

Notes: All specifications use Conley standard errors and control for population and longitude. The sample for all regressions is all sections in Haiti (N=564). Stars indicating statistical significance have been omitted for ease of comparison.

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