Education and Human Capital Externalities: Evidence from Colonial Benin*

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November 12, 2013

Abstract

We use a unique dataset on students from the first regional schools in colonial Benin to investigate the effect of education on living standards, occupation and political participation. Because of the near-random selection of the school location and the first student cohorts, we can estimate the effect of education by comparing the treated to the untreated living in the same village, as well as those living in villages where no schools were set up. We find a significant positive treatment effect of education for the first generation of students, as well as their descendants – they have higher living standards, are less likely to be farmers, and are more likely to be politically active. We find large village-level externalities – descendants of the uneducated in villages with schools do better than those in control villages. We also find extended family externalities – nephews and nieces directly benefit from their uncle's education – and we show that this represents a "family-tax," as educated uncles transfer resources to the extended family.

JEL codes: N37, O15, J27

*We would like to thank Alberto Alesina, Marcella Alsan, Joseph Altonji, Chris Blattman, Brandon Miller de la Cuesta, Janet Currie, Thomas Fujiwara, Paula Giuliano, Claudia Goldin, Ahmed Mushfiq Mobarak, Christian Moser, Nathan Nunn, Nancy Qian, Jim Robinson, Mark Rosenzweig, Cyrus Samii, Andrei Shleifer, Sotima Tchantikpo, Chris Udry, Sarah Weltman, and Yang-Yang Zhou; seminar participants at Harvard University, LSE, Princeton University, University of Warwick, and Yale University; BREAD Conference on Development Economics and the NBER political economy group for comments and suggestions. Special thanks to the research department of the Institute for Empirical Research in Political Economy (IERPE) in Benin, particularly, Roumuld Anago, Kassim Assouma, Azizou Chabi, Andre Gueguehou, Late Gregoire Kpekpede, and Clement Litchegbe for their work during the data collection process. Financial support from NYU, Princeton, and IDRC (Canada) is gratefully acknowledged. The usual caveat applies.

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"An educated child is like a lantern in your house at night." Eloi Gainsi, farmer and religious instructor, Zagnanado (Benin)

1 Introduction

Education can have a profound, transformational effect on individuals and communities: this idea has received strong support not only from folk wisdom and anecdotal evidence, but from rigorous academic studies as well. A wide literature shows that the social benefit of education is only partially reflected in the advantage it gives to the individual, and that the diffusion of knowledge and human capital externalities may be fundamental factors in explaining differences in economic growth among developing countries. This literature thus far has focused primarily on the measurement of human capital at the aggregate level and has had limited success establishing a causal link between education and development outcomes. In this paper, we present direct evidence of individual-level effects of human capital on economic outcomes, as well as data on the spatial and temporal spillover of these effects

We use a unique longitudinal dataset which tracks down the first students in colonial schools founded in central and northern Benin in the early 20th Century, those students' direct descendants and extended family, as well as their contemporaries who did not get education (see Wantchekon (2012)). We use information provided by school and church archives (see Figure A.1) and face-to-face interviews with local elders to identify students in the first two cohorts from colonial schools at four sites: Kandi, Natitingou, Save and Zagnanado. Information about these first students was collected through interviews of either the students themselves (if alive) or their direct descendants. Next, the dataset contains individuals who were born at the same time and the same village but did not attend school, as well as contemporaries from a nearby village where no school was set up.[1]

[Figure A.1 about here.]

Favorable geographical conditions may have determined the colonialists' or Catholic missionaries' location choice for schools, as discussed by Nunn (2010) and Johnson (1967), inducing possibly

[1] In Section 5 we discuss in more detail how students were selected for school, and why we believe there was no positive selection.

selection bias into estimates of the effects of human capital. However, the data collection approach of Wantchekon (2012) sidesteps the issue of potentially endogenous location choice by sampling only nearby villages, within 25 km of each other, that were equally attractive to settlers. Given this relatively short distance, there is hardly any variation in geographical features relevant to Catholic missionaries and colonial settlers (see Table A.3), so the exact location of a school could plausibly be considered near-random.

It is also important to note that only regions that had no prior exposure to European influence at the time of the first schools were considered. In other words, we use data collected in areas where formal colonial institutions were established after, not before, formal education opportunities were made available to the local population. This unique feature of the data helps to isolate the effects of human capital and limits the potential for political institutions to confound the relationship between human capital and growth.

Our results reveal enormous positive treatment effect of education on a number of outcomes – the treated individuals from the first two cohorts have higher living standards, are considerably less likely to be farmers, and have better social networks. Also, students in that first generation are significantly more likely to be politically active, either by campaigning for and joining political parties, or even standing for election in a few cases. To the best of our knowledge, these results represent the first (quasi) experimental evidence in the support of the positive effect of education on political participation in developing countries.

Second, we look at the outcomes of their descendants. Parents' education has a large positive effect on their children's educational attainment, living standards, and social networks, at levels similar to the first-generation effects. Third, there are large positive village-level externalities of education in the second generation – descendants of the untreated[2] in villages with schools have substantially better outcomes than descendants in villages without. We find evidence that these externalities run partly through higher aspirations, as parents in villages with schools enhance their social networks. Fourth, the strength of extended families is documented as nephews and nieces directly benefit from education of their uncles – they are almost equally educated as the students'

[2]The untreated are a random sample of those who did not receive education but were born at the same time as students of the first two cohorts in each village where a school was established.

children, and are more educated than descendants without any educated members in their families. We show that these within-family externalities represent a family-tax, as educated uncles seem to transfer resources to the extended family.

The remainder of the paper is organized as follows: Section 2 discusses the related literature, Section 3 describes the historical context in Benin, Section 4 describes the sampling procedure, Section 6 presents results from the first generation of students, with a discussion of identification in Section 5. Section 7 presents results from the second generation, with an emphasis on extended family and village-level externalities. In Section 8 we verify that our main results are not driven by different birth patterns among the educated and the uneducated, or by non-random missing data. Section 9 concludes.

2 Literature

Diffusion of knowledge and human capital externalities are considered essential for explaining cross-country differences in growth rates (Klenow and Rodriguez-Clare 2005) as well as differences in regional development (Gennaioli et al. 2012). Moretti (2004), Lucas (1988), Romer (1989) and many others show that the social benefit of human capital is only partially reflected in the private return to education. Glaeser et al. (2004), Woodberry (2004), Huillery (2009) and Bolt and Bezemer (2009) suggest that accumulation of human capital may be a fundamental factor in explaining differences in long-term development across former colonies.

Our paper contributes to several strands of literature in economic history, development and labor economics. Most directly, we build on recent literature on the colonial legacy in the area of education (e.g. Nunn 2009, Nunn 2010, Woodberry and Shah 2004, Huillery 2009), by tracking down the first students of colonial schools and their descendants. This paper also speaks to the literature on human capital externalities (e.g. Lucas 1988, Romer 1989, Mankiw et al. 1992) by confirming its importance for economic development and providing micro-level evidence on the mechanisms for spatial and temporal spillover of human capital.[3]

[3]For a more theoretical treatment of human capital externalities see Murphy et al. (1991), Acemoglu et al. (2000), Marshall (1961), Bils and Klenow (2000), Hendricks (2002), Krueger and Lindahl (2000).

The literature on economic and labor market effects of education (e.g. Duflo 2004, Weir and Knight 2004, Kimenyi et al. 2006) finds that wage and output premiums as well as development are likely caused by increased human capital. The evidence from this paper supports this claim by showing sizable effects on living standards, occupational choice, and development in the African context.

There is also a wide literature concerning the effects of family size on education choice, which examines the quantity versus quality tradeoff. For instance, Emerson and Souza (2008) and Parish and Willis (1993) discuss credit constraints, Cornwell et al. (2005) focuses on economies of scale, and Jensen (2010) and Abeler et al. (2011) describe the importance of perceptions of actual returns to education. While the importance of extended families has been questioned in the U.S. (see Altonji et al. 1992), others find that they play a significant role in Africa and India (see Angelucci et al. 2010, Cox and Fafchamps 2007, La Ferrara 2003, Shavit and Pierce 1991). Our contribution stresses the role of extended family externalities, specifically how the presence of a successful uncle can influence educational choice, not only by relaxing the credit constraints of his family, but also by signaling the values of education through a living example.

Next, a growing theoretical literature points to the role of aspirations in education choice and poverty reduction, and describes the existence of a cognitive window and a reference point that may generate increasing returns to effort (see Dalton et al. 2010, Mookherjee et al. 2010, Ray 2006, Chiapa et al. 2012). This paper provides additional evidence of aspirations; in particular, we show that parents living closer to a school have higher social networks. We interpret this finding as evidence of aspirations, because unlike family members, untreated parents did not receive reductions in credit constraints by the educated. These enhanced social networks in turn increase the education of parents' children.

Our results are also consistent with recent findings on peer effects. For example, Lalive and Cattaneo (2009) and Bobonis and Finan (2009) find that ineligible students have benefited from the Progresa program in Mexico, due to neighborhood peer effects. In the United States, Borjas (1992) and Borjas (1995) have shown that the ethnic community in which children grow up determines, to a large extent, their later labor market outcomes, while Topa (2001) shows local spillovers are

particularly strong in areas with less educated workers. Looking at intergenerational transmission of human capital among the African-American population in the U.S., Sacerdote (2005) finds that it took about two generations for descendants of slaves to catch up with descendants of free black people in terms of education. This estimate is very similar to the speed of convergence in education outcomes that we find in our data, in villages where a school was established.

3 Context

Benin was known as the Kingdom of Dahomey before colonization, and the Republic of Dahomey 1960-75. The country was colonized in 1894 when French troops, led by General Alfred Dodds, defeated the army of the kingdom after three years of war, and Behanzin, the king, surrendered the capital city of Abomey. Prior to colonial administration and in the shadow of the slave trade, Catholic missions were established in the coastal towns of Agoue (1874) and Porto Novo (1864), and the interior town of Zagnanado (1895). There were two types of missions: those established in regions with prior European presence in the form of commercial trading posts and military settlements, such as Porto Novo and Agoue, and those with no prior European influence, such as Zagnanado.

Vatican records indicate that the evangelization of the "Slave Coast" from the Volta River in current-day Ghana to the Niger River in Nigeria was one of the main priorities of the Roman Catholic Church at the end of the 19th Century. An apostolic vicariate, a form of territorial jurisdiction of the Church, was established in 1861 in Agoue at the border between Togo and Dahomey but was limited to the littoral region (see Figure A.2). But, according to Dupuis (1961), the Kingdom of Dahomey was "closed" and "impenetrable" and made it very difficult for the Catholic missionaries to expand to the hinterland (Dupuis 1961, p. 10). It was only after the Kingdom was defeated by the French that the missionaries started expanding the boundaries of the apostolic vicariate of Agoue to the central region of Zagnanado and later Ketou. The French government later sent military explorers further north but met vigorous armed resistance in Atakora, Haut-Niger and Borgou (French Government Report 1906). The colony was completely pacified only in 1920, and its capital was located in Porto Novo. The French set up a new territorial administration in

the southern and central regions (1908) and later in the northern regions (1913 and 1936).

[Figure A.2 about here.]

Dahomey was thus under the joint administrative control of the apostolic vicariate based in Agoue representing the Vatican and the colonial government based in Dakar (with local representation in Porto Novo). The Vatican wanted to maximize religious influence and colonial authorities wanted to maximize fiscal revenues. The main obstacle to the penetration of the Catholic Church was the entrenched traditional animist religious practices in the South and the strong Islamic presence in the North (Dupuis 1961, p. 70). The main constraint to the French colonial rule was the sporadic armed resistance in the North. In addition to these difficulties, both the Vatican and the French government had very limited knowledge of the country's human resources capacity outside the coastal areas. A detailed report by the French government lamented the opacity of the local culture (p. 64-71). The report highlighted a high level of hostility towards the colonial presence, its education system and cultural influence, and provided vivid details of the strange and sometimes "diabolic" religious practices of the "indigenes" (French Government Report, 1906, p. 62).[4]

Besides the cultural distance between French settlers and the local population, the lack of diversity in the occupation and living conditions among these local populations made it nearly impossible for the colonial government and the missionaries to infer the local level of human capital without extended interaction with the people.[5] But again, cultural distance made such interaction very difficult. Given these constraints, one can understand why both the Vatican and the colonial government made primary education a precondition for their successful "civilizing mission" (Dupuis 1961, p. 69). Indeed, in all the new territories, settlements tended to start with military exploration, followed by the creation of a school after the region had been pacified. Then, six to ten years after the regional school was opened, a local civilian administration called a "cercle" was set up. The school trained religious teachers and civil servants, such as translators, nurses, accountants, and security guards. The motivation behind this curriculum was for the training native religious teachers

[4]The report states the following: "Unfortunately, there is among many natives, a high degree of mistrust and resentment vis-à-vis the White settlers, which proves that there is very little contact between White settlers and the Africans." (p. 102)

[5]The vast majority of the "indigenes" were subsistence farmers (see d'Almeida Topor 1995).

to increase the rate of Christian conversion, while the recruitment of civil servants increased fiscal colonial revenues. In line with this policy, a Catholic school was created in Zagnanado in 1895, and regional public schools were set up in Save (1913), Kandi (1911) and Natitingou (1922). None of these areas had meaningful European influence, educational or otherwise, at the time the schools were created.

4 Sampling Procedure

We examine the first schools in four communes:[6] Zagnanado, Save, Kandi and Natitingou, as shown on the map in Figure A.3. Within each commune, we compare the village in which a school was set up to a nearby village in which no school was established. These treatment and control villages are within 25 kilometers of each other and are similar in terms of various observable characteristics such as soil quality, elevation, average rainfall within each commune, as shown in Table A.3 in the Appendix.

The first-generation data was generated from a sample of students from the four schools and villagers of about the same age who did not attend school. Then the family of this first generation was sampled, so that second-generation subjects are the children, nieces or nephews of the first. The sample of treated individuals in the first generation (call this Treatment 1) is composed of the first two cohorts of the four primary schools covered in the study, and is based on information collected from various archives and village elders. In the first generation, Treatment 2 is a sample of contemporaries of Treatment 1, from villages in which a school was established, but who did not attend school. The control group is a sample of individuals from a nearby village in which no school was set up. The Treatment 2 and control samples are composed of fathers, or in some cases grandfathers, of randomly selected current inhabitants of the village. Selected parents in the Treatment 2 and control samples are about the same age as the individuals in Treatment 1, and subject characteristics were verified using several sources, including cemeteries and ID cards from colonial archives. The total sample size for the first generation was approximately 240, or 60 per site.

[6] A commune is an administrative region, called a "cercle" in the Colonial period.

The second-generation sample was generated from a census of all the children and siblings of individuals from the first generation. In addition to the children of the first generation, the children of siblings of first-generation subjects were also included, so that the second generation includes both the children of the first as well as their nieces and nephews.

5 Endogeneity of Location and Student Selection

Our identification strategy relies on two claims. First, within a given commune and a distance of about 25 km, the location of schools was partly driven by happenstance and thus near-random. Second, enrollment was in large part determined by chance or by negative selection. We will discuss each of these claims in the next subsections.

5.1 School Location: Historical Evidence and Pre-treatment Variables

Our treated units – that is, those who were selected for education – are drawn from four schools, one Catholic (Zagnanado) and three public (Save, Kandi and Natitingou). The motivations for establishing these schools were two-fold. The Vatican representative, based in Agoue, wanted to maximize its religious influence over the local population, while the colonial government based in Dakar wished to expand into the hinterland after the pacification of the Kingdom of Dahomey (Dupuis 1961, p. 70). However, both the French government and the Vatican had little knowledge of the local population aside from regional-level military reports, and government officials lamented the opacity of local culture (p. 64-71) and high level of hostility towards colonial presence, including its educational and cultural institutions (French Government Report, 1906, p. 62).[7]

[Figure A.2 about here.]

To illustrate the similarity between treated and untreated villages, Table A.3 reports several geographic and political features for both treated villages and those nearby, which were chosen as the control groups. Small differences notwithstanding, there appears to be no systematic contrast

[7]The kingdom of Dahomey, in fact, had little contact with European institutions of any kind prior to its conquest 1894, one year before the school in Zagnanado was created (Dupuis 1961, p. 10).

between villages chosen for schools and those not. Northerners were, in fact, particularly resistant to French education, which colonial officers attributed to Islamic influence in the region (p. 117). Thus, while region selection may have been subject to the financial and cultural imperatives of the Vatican and French authorities, the location of schools within those regions were quasi-random and were made without knowledge of human capital differentials or other factors that may have produced a selection bias. Moreover, the covariate balance presented in Table A.3 demonstrates that there appeared to be little if any difference between treated and untreated villages. For instance, the treatment and control villages were part of the same pre-colonial unit; feature nearly identical distances to port, soil quality, and mean elevation (except for in Zagnanado, but this difference works against finding an effect since the control village has better soil quality); and shared the same number of years of resistance to colonial rule.

There were several considerations for settlement choice in Natitingou, Kandi, and Save: the locations of armed resistance, road quality, and other geographic characteristics such as mean elevation. Once French authorities found regions that met these considerations, they were agnostic regarding the exact site within these regions (this study uses a 25 km radius) since there were a number of suitable villages that were equally as likely to be chosen. The same logic applied for Catholic missionaries in Zagnanado once they decided on the plateau region.

5.2 Student Selection: Historical Evidence and Pre-treatment Variables

On the basis of available historical evidence, the enrollment of the first generation of students was a result of two processes: school recruiters requested the local population, often through the tribal chiefs, to select students; or recruiters, upon first randomly coming across children, asked permission from the local population to enroll them. For the first scenario, we provide evidence in support of negative selection of students by families and chiefs. In the second scenario, the selection was de facto random.

5.2.1 Negative Selection

Local elites were opposed to sending their own children to school, because they feared that doing so would compromise their ability to remain independent of the colonial administration and missionaries (Centenaire 1995, p. 72). In Zagnanado and Natitingou, no students came from chief or guild families. In Save and Kandi among the about sixty students of the first cohort, only two or three were related to religious priests, traders, or chiefs. Furthermore, the local population was generally hostile towards colonial education and cultural influence, and hence lacked incentive to send their brightest children. The French Government report notes: "Unfortunately, there is among many natives, a high degree of mistrust and resentment vis--vis the white settlers....The further away you get from the centers (e.g. the coast), the less likely you are to find people you want to send their children to school" (French Government Report, 1906).

This phenomenon of negative selection of students for colonial education is not limited to Benin. Anthropologists have documented similar behavior in India, Papua New Guinea, and China.[8] Mead (2001) for example, in her ethnography of the Manus people describes her first interpreter, a schoolboy named Banyalo:

"Banyalo was a peculiar representative of historical accident, of the fruition of plans made long ago. In the early twenties a school had been established in Rabaul and pressure had been put on local district officers to find boys to send away to school. The Manus people of the South Coast, who were just coming into real and unenthusiastic contact with Europeans, did not wish to send their cherished small boys away to school. Only the orphaned, the very stupid, the unstable were selected to go – either to a school where they were to become literate in English, to be trained as clerks and schoolteachers, or to a trade school to learn carpentry, etc. We analyzed the group who were away at school in 1928 – four of the ten had histories of instability" [p. 224].

[8] Bengal authorities in 1847 reported mistrust and the dismal attendance at their schools, having to pay schoolboys to attend and only attracting the lower classes.

"A good school house has been built at Chuprah, the sudder station at the school is attended by 31 pupils, chiefly from the lower classes of society, shop-keepers and servants. The higher classes take no interest in the matter."(Bengal 1848, p. 178-181).

From 1870 to 1880s, missionary schools in China tried to attract attendance by a) appealing to girls, whose families generally did not invest in formal education for them and b) paying both boys and girls.

"Considering the kind of (religious) instruction, it was hardly surprising that even converts were often reluctant to send their children to the mission schools." (Barr 1973, p. 60-61).

5.2.2 Random Selection

Colonial authorities not only lacked detailed information on village-level characteristics, they also had limited knowledge of the villagers in their settlement sites. Because schools were established during military pacification, and before colonial administration was established in the area, there had been little prior interaction between school officials and the local population, and therefore limited ability to discriminate between potential students. For instance, the school in Zagnanado was created less than three weeks after the missionaries settled in the region and there are several accounts of children being randomly picked up from playgrounds and later enrolled with the authorization of the local chief. It is therefore unlikely that the students who were ultimately enrolled differed systematically from the local populations from which they were drawn.

The inability to discriminate between potential students may also be driven by the near homogeneity of the local population. Table A.4 in the Appendix indicates that treated and untreated families are on average the same size. Furthermore, in control villages (which should be similar to a treated village prior to treatment) 84.2% of the population are subsistence farmers, with no means of transportation (see Table 1). Thus, communities at that time appeared to have been largely homogenous with respect to any characteristics that would have been observable to colonial administrators and school officials.

Whatever heterogeneity existed within villages, the prevailing racial ideology held by the French colonizers may have led them to believe that inhabitants were all similarly ill equipped for schooling, which may have limited positive selection. As shown by Bertrand and Mullainathan (2004), racial discrimination is expressed in two ways. First, people with lighter skin are considered superior to those with darker skin (level effect). Second – and this is crucial for our identification strategy – there is little perceived variation in ability or quality among those with dark skin (distributional effect).[9] The classification of individuals by colonial administrators revolved almost exclusively around physical traits. For example, when describing the lighter skinned Fulani from Northern Benin, the 1906 Gouvernement de l'Afrique Occidentale Francaise report states:

[9]In Bertrand and Mullainathan (2004), they found that in the U.S. labor market employers are more likely to respond to a high quality resume if there is a white-sounding name than an African-American-sounding name, but among African-American resumes, the responses are no different between high and low quality resumes.

"The Fulani have special physical traits: thin, tall, with 'whitish' skin color. Their aspect does not present any negroid trait. Indeed, the Fulani have an aquiline nose, thin lips, straight forehead, big eyes, silky and non crisp hair. They have a well-deserved reputation of a more developed intelligence than their black neighbors. They have a delicate and subtle mind" (p. 164). Thus, only the Fulani were identified as possessing any inherent intelligence, predisposing them for education. Interestingly, the Fulani ethnic group was comprised largely of herders who were unavailable for colonial education.[10]

Our evidence suggests that selection by colonizers was either near-random or based on physical traits that were uncorrelated with the outcome of interest. In addition, local families were hostile to colonizers and had incentives to enroll children that were the least predisposed to education.

6 First-Generation Effects

6.1 Summary Statistics

Table 1 summarizes the most important variables for the first-generation inhabitants of the villages in our sample, and compare the first generation of students and their contemporaries. Looking down the table, we see that setting up schools appears to have had a profound and apparently long-lasting effect on the children that were chosen to attend schools and their descendants. Among the children chosen to go to school, almost all (96%) were enrolled for at least three years of primary education and 10% of them went on to complete secondary education.[11]

In terms of living standards, those chosen to attend school clearly have superior outcomes to either the uneducated from the same village, or those from untreated villages. For example, only 14% of the educated students become farmers, while farming is clearly the dominant occupation among the uneducated (about 80%). We also observe that the educated are more likely to have running water in their homes (26%), electricity (10%), and to have some means of transportation (48%). The uneducated in villages with and without schools have worse living standards outcomes

[10] Even if the Fulani possessed characteristics that could have made them more amenable to education, there is only one Fulani in our treatment group, specifically in Kandi.

[11] In the first generation, no one went on to university, which is hardly a surprise given that these children were born at the turn of the 20th century and no universities were available in Western Africa at the time.

Table 1: Summary Statistics for the First Generation

	Treated parents	Untreated parents in village w/ school	Untreated parents in village w/o school
Number of siblings	3.370	3.059	2.964
	(2.366)	(2.326)	(2.114)
	[73]	[153]	[139]
Primary education or more	0.963	0.008	0.008
	(0.189)	(0.092)	(0.091)
	[82]	[119]	[122]
Secondary education or more	0.098	0.000	0.000
	(0.299)	(0.000)	(0.000)
	[82]	[119]	[122]
Farmer	0.143	0.784	0.842
	(0.352)	(0.414)	(0.367)
	[84]	[111]	[95]
Water	0.258	0.146	0.092
	(0.440)	(0.355)	(0.290)
	[89]	[164]	[152]
Electricity	0.101	0.024	0.007
	(0.303)	(0.155)	(0.081)
	[89]	[164]	[152]
Means of transportation	0.476	0.182	0.195
	(0.502)	(0.387)	(0.397)
	[84]	[154]	[149]
Living standards scale	0.677	-0.195	-0.188
	(1.159)	(0.887)	(0.835)
	[84]	[151]	[143]
Member of party	0.425	0.107	0.050
	(0.498)	(0.311)	(0.219)
	[73]	[149]	[139]
French language	0.955	0.085	0.013
	(0.208)	(0.280)	(0.114)
	[89]	[164]	[152]
White friends	0.457	0.084	0.035
	(0.502)	(0.278)	(0.186)
	[70]	[143]	[141]
Social networks scale	1.661	-0.350	-0.451
	(0.864)	(0.539)	(0.425)
	[49]	[99]	[103]

Note: Standard deviations are in parentheses. Due to missing values, there are different number of observations across variables, shown in brackets. Means of transportation includes bicycle, motorcycle or car. Mean (SD) of 0.000 (0.000) means that none of the observations had a positive value, for example, none of the untreated individuals had secondary education or more.

and do not seem to be different from each other, as we will formally show in the next section (Table 3).

We also include a measure of living standards based on factor analysis using several indicators such as those listed in the Table 1. Other variables include house wall material, house/land/shop ownership, household equipment, means of transportation, travel patterns and type of attire. We see that also in terms of this composite measure of living standards, the educated have clearly higher scores than the uneducated. Table A.6 in the Appendix gives further details about how to interpret different values on the living standards scale.

The presence of a school in a village, however, does seem to have some indirect effect on the uneducated as well. We expect to observe that the educated are more likely to speak French, have friends among whites and score higher on a social networks scale. The interesting observation is that the uneducated in villages with schools seem to also score higher than those in villages without. The social networks scale was coded based on factor analysis using information about membership in organizations (religious, business, sports), languages spoken (national, foreign), friends among whites and other local ethnic groups, and participation in local politics. Table A.7 in the Appendix gives further details about how to interpret different values on the social networks scale.

6.2 First-Generation Effects: Living standards, Social Networks, and Political Participation

We first evaluate the effects of being treated with education at the individual or village-level among the first generation of students and their contemporaries. As we argued in the previous Section, children were chosen to attend the schools in a near random fashion, or through negative selection. Given this, the estimated effects of schooling at the individual level in the first generation can be interpreted as causal effects. To the extent that there might have been negative selection, our estimates of the individual level effects might be underestimated. The village-level effects can also be considered causal if the reader is convinced that the school locations were set up in a quasi-random manner.

The simple reduced-form OLS regressions we estimate are of the following form:

$$\text{Outcome}_{ij} = \alpha + \beta_1 I_{ij} + \beta_2 V_j + \epsilon_{ij}. \quad (1)$$

Our outcome variables are education, living standards and social ties, where i identifies the individual child, and j identifies the village in which they reside. The variables I and V are binary, and they indicate whether the individual was chosen to attend school and whether he lived in a village where a school was set up. For example, $I_{ij} = 1$ and $V_j = 1$ if child i from village j was chosen to go to school and a school was set up in village j. If a child grew up in a village where a school was set up, but was he was not chosen to attend the school, then $I_{ij} = 0$ and $V_j = 1$. Finally, if a child was not selected for school and grew up in a village with no school, then $I_{ij} = 0$ and $V_j = 0$. The key coefficients are β_1 and β_2 which estimate the causal effect of individual and village-level treatment, respectively.

Table 2 presents the coefficients on individual and village-level treatment with education as the outcome variable. These results, thus, represent a manipulation check. As expected, the coefficient on individual-level treatment is positive and highly statistically significant. In the first column in Table 2, education is measured on a scale from 0 to 3, where 0 indicates no education, 1 indicates primary school only, 2 indicates secondary school only and 3 indicates university education. From Table 1 we know that most of the treated children were enrolled for at least three years of primary school, depending on when they were recruited into the labor force by the colonial administration, and only about 10% have secondary education. Accordingly, the individual-level coefficient in column 2 of Table 2 is very close to 1, while the coefficient in column 3 is about 0.1.

Looking at the effect of individual and village-level treatment on living standards we see that in the first generation *only* the individual-level treatment contributed to higher living standards, as shown in Table 3. This result is very strong and intuitive – we can deduce that the students put their knowledge of the French language, their literacy and math skill and understanding of the colonial state and culture to good use. They were able to get better jobs, and secure better living standards for their families.[12] For example, students were as much as 65 percent less likely

[12]Many of the students from the first generation were hired as civil servants in the colonial administration and skeptics may argue that they would have better living standards even if they did not learn much in school. However, our results hold also for those who chose other occupations, such as commerce, suggesting that human capital obtained

Table 2: First-Generation Education Effects

	(1) Education	(2) Primary or more	(3) Secondary or more
Individual-level treatment	1.053***	0.955***	0.098***
	(0.058)	(0.025)	(0.033)
Village-level treatment	0.000	0.000	-0.000
	(0.001)	(0.001)	(0.000)
Observations	324	324	324

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: Dependent variables are indicated in the column header. Standard errors were calculated using blocked bootstrapping, where the full block of observations from a commune are randomly subsampled. For information on blocked bootstrapping, see Cameron and Miller (2008).

to be farmers compared to those who were not chosen to go to school, or those who lived in a village without a school.[13] In contrast, the coefficients on the village-level treatment variable are all very close to zero and statistically insignificant. This indicates that for those living in villages with schools but who did not receive education, their living standards level was no different from the level of living standards of those living in villages with no school.

What is particularly interesting is that the uneducated who grew up in treated villages did learn some French and in general had better social ties than those in untreated villages. These results are shown in Table 4, and constitute evidence of within-village externalities from the introduction of a school. These positive externalities, however, were not generated through interaction between the uneducated villagers and the colonialists. Rather, column 2 of Table 4 suggests that the uneducated did not have more friends among the white people than those from untreated villages. Hence, it appears that these uneducated villagers learned French through neighbors and friends. This is corroborated by the result in column 4 of Table 4. Using the coordinates of all the settlements within our four sites with schools, we calculated the distance between each individual's home (to

in school was useful in other professions and led to better living standards.

[13]Since most first-generation students finished only elementary school, the marginal effect of an additional year of education is quite large. Primary school consisted typically of six years of education, but many students chose to leave after three in order to join the labor force. The effect of having finished primary school on the probability of being a farmer is -0.61, or a decrease in 61%. Assuming a linear effect of additional schooling, each year of education decreased the probability of being a farmer by 15%, or around one fifth of the likelihood of being a farmer in Treatment 2.

Table 3: First-Generation Living Standards Effects

	(1) Farmer	(2) Water	(3) Electricity	(4) Means of transportation	(5) Living standards
Individual-level treatment	-0.641***	0.112***	0.077***	0.294***	0.872***
	(0.095)	(0.041)	(0.012)	(0.025)	(0.171)
Village-level treatment	-0.060	0.055	0.018*	-0.012	-0.004
	(0.116)	(0.048)	(0.010)	(0.018)	(0.164)
Observations	291	406	406	388	379

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: Dependent variables are indicated in the column header. Vehicle can include any means of transportation such as bicycle, motorcycle or car. Living standards scale is a factor score comprising a number of variables, for details see Table A.6 in the Appendix. Standard errors were calculated using blocked bootstrapping by commune.

the extent we could identify and verify its location during the relevant time after treatment) and the location of the school.[14] We find that those closer to a school had higher social networks, as measured by our factor scale, suggesting that the effect indeed may run through the neighbors. The difference in social networks score between the untreated in villages with and without schools (column 3) is statistically significant at the 5% level, suggesting a development of greater social activity and organization in the villages that had a school.

These differences in social networks among the uneducated in villages with and without schools are already suggestive evidence that the introduction of education may have long-lasting effects that go beyond those individuals who directly receive it. These positive externalities are likely particularly important in a state of utter underdevelopment, as was the case in turn-of-the-20th-Century Dahomey.

Part of the social network effect of education may run through higher political participation. Table 5 shows that students were significantly more likely to campaign for political parties, or even become full-fledged members. While very few people stood for election to political office in

[14]What we refer to as a "village" is in fact a group of interconnected smaller settlements – groups of homes. For example, in Zagnanado, Treatment 1 and Treatment 2 include 16 settlements: Agnangon, Assiadji, Assiangbome, Ayogo, Azehounholi, Dezonde, Doga, Dovi Dove, Gbenonkpo, Hougbodji, Kinbahoue, Kotyngon, Legbado, N'Dokpo, Sowe, and Zomon. We assign a location for each individual to a settlement, and calculate the distance from the location of the school. For Zagnanado, the school was closest to the settlement of Gbenonkpo and farthest from the settlement of Ayogo.

Table 4: First-Generation Social Networks Effects

	(1) French language	(2) White friends	(3) Network scale	(4) Network scale
Individual-level treatment	0.870***	0.373***	2.010***	1.999***
	(0.033)	(0.016)	(0.217)	(0.228)
Village-level treatment	0.072***	0.049	0.100***	
	(0.024)	(0.039)	(0.038)	
Distance from school				-1.102***
				(0.382)
Observations	406	355	252	238

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: Dependent variables are indicated in the column header. Network scale is a factor score comprising a number of variables. The last column includes only individuals under Treatment 1 and Treatment 2 condition. Standard errors were calculated using blocked bootstrapping by commune.

the period we cover in the first generation (only 12 people in our sample, or 3.22%), they are by and large concentrated among the treated individuals, allowing for quite a precise estimate of the treatment effect, despite the low power.[15] These findings show a clear effect of education on political participation. To the best of our knowledge, this is the first (quasi) experimental evidence in the support of the positive effect of education on political participation in developing countries.[16]

Table 5: First-Generation Political Participation Effects

	(1) Campaign for party	(2) Member of party	(3) Candidate in election
Individual-level treatment	0.339***	0.317***	0.117***
	(0.053)	(0.047)	(0.036)
Village-level treatment	0.045	0.057	-0.021***
	(0.046)	(0.061)	(0.007)
Observations	365	362	373

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: Dependent variables are indicated in the column header. Standard errors are clustered by commune.

[15]The negative and statistically significant effect for the village-level effect is due to the fact that no individuals in Treatment 2 ran for election, whereas two individuals in the control group did.

[16]See Berinsky and Lenz (2011), Campante and Chor (2011), Dee (2004), Glaeser et al. (2007), and Kam and Palmer (2008), among others.

The statistically significant results in the first generation of students are hardly a surprise, but they are important to document as a social phenomenon. Education has brought important change to the lives of the first generation of students.[17] The bigger questions are whether there were long-lasting effects of education on the descendants of the first students, and whether the differences between the descendants of the educated and the uneducated grow or diminish through generations. Before we investigate if the first-generation effects persist over time, we discuss whether these effects are causal. In the next section, we provide a wide range of evidence which indicates that both location and student selection were plausibly exogenous.

6.3 Selection on Unobserved Variables: Rosenbaum Bounds

Our goal in this section is to determine how large the differences on unobservables would need to be between the treated and control individuals in order to eliminate the treatment effect we find. We do this by following the method proposed in Rosenbaum (2002).

We perform this sensitivity analysis only on the first generation of children in villages where a school was opened. First we match individuals in treatment and control group based on the number of siblings they had (our only pre-treatment variable), their commune and their decade of birth. If all boys in the first generation had the same odds of being selected into treatment, then the treatment was truly random. Rosenbaum (2002) proposes a framework in which we assume that certain, say intelligent, kids have higher odds of being selected for treatment and are more likely to have higher living standards.[18]

Table 6 resents the results of this exercise. We focus on three binary outcome variables – whether the individual is a farmer and whether their living standards and measure of social networks are

[17]Note that in Tables 2, 3 and 4 we have no additional controls and the standard errors are clustered at the commune level. If we include indicator variables for the decade/commune of birth the estimated coefficients are very similar, but sample sizes drop by about 25% due to missing information about the year of birth. Results are also robust to controlling for the number of siblings.

[18]The details of the framework can be found in Rosenbaum (2002). Briefly, we assume that the probability of being educated, π_i is $\pi_i = Pr(D_i = 1|x_i) = F(\beta x_i + \gamma u_i)$, where D_i is the selection of individual i into treatment, x_i is the observable pretreatment variable, u_i is the unobservable variable, and we assume that F is the logistic distribution. Then the odds that i is selected are $\frac{\pi_i}{1-\pi_i} = e^{\beta x_i + \gamma u_i}$. When individuals i and j are matched on observables then $x_i = x_j$, so the odds ratio for i and j is $e^{\gamma(u_i - u_j)}$. Clearly, when there is no selection on unobservables $u_i = u_j$ and the odds ratio of being selected for treatment is 1. But if individual i is smarter than j they may have higher odds of being selected for school so the odds ratio is higher than 1. The method uses the Mantel-Haenszel test statistic as explained in Becker and Caliendo (2007).

Table 6: Rosenbaum Bounds

	Farmer	Living standards scale	Social networks scale
$\Gamma_{p<0.01}$	7.1	2.2	8.3
$\Gamma_{p<0.05}$	10.4	3	14.3
$\Gamma_{p<0.10}$	12.9	3.5	19.2

Note: If we compare individuals with the same observable characteristics, the odds of being selected for school would need to be $\Gamma_{p<0.01}$ times higher, based on unobservables, so that we cannot reject the Null hypothesis of no treatment effect at the 1% level. The second and third row show how many times higher the odds of being selected for school, based on unobservables, would need to be in order to not be able to reject the Null at the 5% and 10% level, respectively.

above or below the mean. The first row in Table 6 shows that in order to find no difference in the likelihood of being a farmer between the treated and control individuals at the 1% level of statistical significance, the selection into education would have to be so high that the "high ability" kids would need 7.1 times higher odds of being selected. Looking down the first column, we see that in order to take away the entire treatment effect at the 5% level, the "high ability" kids would need to have 10.4 times higher odds of being selected, and at the 10% level they would need to have 12.9 times higher odds of being selected. Overall, Table 6 suggests that selection on unobservables would have to be very high in order to eliminate the treatment effects we find.

7 Second-Generation Effects

7.1 Education, Living standards, and Social Networks

The second-generation effects of education are of paramount importance for human development and social mobility in Benin. If the introduction of education only affects the educated and their descendants, the country's development path may be quite different than if education also indirectly affects everyone who lives in a village with a school. In this section, we will show in several ways that descendants of uneducated people in villages with schools catch up with the descendants of the educated. In particular, they catch up both in terms of primary education outcomes as well as in terms of living standards and measures of social networks.

Table 7 shows the summary statistics for the descendants of the first-generation individuals.

Table 7: Summary Statistics for the Second Generation

	Treated parents	Untreated parents in village w/ school	Untreated parents in village w/o school
Education			
Primary or more	0.669	0.520	0.274
	(0.471)	(0.500)	(0.446)
Secondary or more	0.375	0.222	0.115
	(0.484)	(0.416)	(0.320)
University	0.104	0.050	0.006
	(0.305)	(0.218)	(0.075)
Income			
Farmer	0.079	0.166	0.386
	(0.270)	(0.372)	(0.487)
Water	0.536	0.452	0.385
	(0.499)	(0.498)	(0.487)
Electricity	0.636	0.504	0.089
	(0.482)	(0.500)	(0.284)
Television	0.536	0.362	0.075
	(0.499)	(0.481)	(0.263)
Telephone	0.480	0.281	0.079
	(0.500)	(0.450)	(0.270)
Means of transportation	0.369	0.275	0.263
	(0.483)	(0.447)	(0.441)
Living standards scale	0.400	-0.007	-0.541
	(1.021)	(0.940)	(0.652)
Networks			
Speaks French	0.655	0.494	0.248
	(0.476)	(0.500)	(0.432)
Speaks English	0.058	0.014	0.007
	(0.235)	(0.116)	(0.084)
Social networks scale	0.286	-0.066	-0.350
	(1.069)	(0.959)	(0.855)
Observations	772	1026	711

Note: Standard deviations in parentheses. Means of transportation includes bicycle, motorcycle or car.

Descendants of the first-generation students exhibit better outcomes across the board, suggesting that returns to education are strongly transferred across the two generations. But what is particularly striking is that descendants of untreated parents living in villages with schools seem to be doing markedly better than descendants of untreated parents in villages without schools. In other words, there also appears to be a strong second-generation externality from the presence of school.

We begin to examine the differences shown in Table 7 by estimating regressions of the following type:

$$\text{Outcome}_{ij} = \alpha + \beta_1 I_{ij} + \beta_2 V_j + \boldsymbol{\beta_3 \mathbf{X_{ij}}} + \mu_j + \tau_i + \epsilon_{ij}. \quad (2)$$

As before, our outcome variables are education, living standards and social ties, where i identifies the individual child, and j identifies the village in which they reside. The binary variables I and V indicate individual-level and village-level treatment of the first-generation individuals, in the same way as in equation 1. Since we have more information collected for the second generation, we also add a matrix of controls, $\mathbf{X}$, which includes gender and the number of siblings of each child. Furthermore, because descendants of different people from the first generation were born over a very long period of time spanning more than half a century, we also include dummy variables for the decade of birth of the child, τ_i, and dummy variables for the commune in which the child resides, μ_j.[19]

Note that in the second generation, the binary variable I is equal to 1 for both children as well as nieces and nephews of former students. This coding was chosen because extended families were and still are a crucial social unit in African countries. Of course, there may be differences in the opportunities available to children and nieces and nephews of the original students as they grow up. However, for the moment we disregard these differences, and we return to them in depth in Section 7.3.

[19]Note that in the second generation we are dealing with cross-sectional data observed over a long period of time. For this reason we include dummies that indicate the decade in which the child was born. While this was not necessary in the first generation when students were all of similar age, it is needed in the second generation, when the students span many decades because first generation male students had children (or nieces and nephews) at different points in life. No time subscripts are used because we do not observe individuals at multiple points in time, i.e. this is not a panel dataset. We closely follow the model setup of Duflo and Saez (2003).

Table 8: Second-Generation Education Effects

	(1) Education	(2) Primary or more	(3) Secondary or more	(4) University
Individual-level treatment	0.374***	0.144***	0.162***	0.067***
	(0.087)	(0.042)	(0.038)	(0.022)
Village-level treatment	0.566***	0.345***	0.163***	0.058***
	(0.065)	(0.035)	(0.029)	(0.015)
Observations	1898	1898	1898	1898

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: Dependent variables are indicated in the column header. Standard errors are clustered by extended family. All regressions control for gender, indicator for child or nephew/niece, number of siblings, and include commune and decade dummies.

Table 8 presents the second-generation regression results for education. The most striking finding is that the coefficient on village-level treatment, unlike in the first generation, is large and statistically significant. This indicates that descendants of the uneducated from villages with schools have significantly more education than descendants of the uneducated from villages without schools. This difference in education outcomes is substantively large, statistically significant at the 1% level, and it appears at all education levels – primary, secondary and university.

Perhaps even more striking is the finding from columns 1 and 2, that the coefficient on village-level treatment is greater than the coefficient on the individual-level treatment indicator. This means that simply growing up in village with a school has a big positive effect on descendants' education, while the additional positive effect of having an educated parent or uncle is smaller. Looking at the individual and village-level coefficients for secondary and university education (columns 3 and 4), both are still highly statistically significant, but now they are of comparable magnitude. This suggests that at higher levels of education, the descendants of educated fathers or uncles are twice as likely to go to secondary school or university as descendants of uneducated parents from villages with schools. For example, in the case of secondary education, a descendant of uneducated parents from a village with a school, *ceteris paribus*, has about a 17% chance of attending secondary school, while the chance that a descendant of an educated parent or uncle attends secondary school is 16 percentage points higher. These are sizable effects.

Table 9: Second-Generation Living Standards Effects

	(1) Farmer	(2) Water	(3) Electricity	(4) TV	(5) Phone	(6) Means of transport	(7) Living standards
Individual-level treatment	-0.061*	0.118***	0.142***	0.194***	0.211***	0.135***	0.396***
	(0.034)	(0.040)	(0.050)	(0.047)	(0.050)	(0.038)	(0.095)
Village-level treatment	-0.299***	0.054	0.426***	0.312***	0.228***	0.011	0.578***
	(0.035)	(0.033)	(0.040)	(0.034)	(0.035)	(0.033)	(0.068)
Observations	1791	1924	1924	1924	1924	1894	1653

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: Dependent variables are indicated in the column header. Standard errors are clustered by extended family. All regressions control for gender, indicator for child or nephew/niece, number of siblings, and include commune and decade dummies. Vehicle can include any means of transportation such as bicycle, motorcycle or car.

A similar pattern emerges for living standards among the second-generation descendants, as shown in Table 9. We see that simply having been raised in a village with a school has important positive effects on measures of living standards and living standards. For example, results from column 1 of Table 9 suggests that being born in a village with a school reduces the descendants' probability of being a farmer by about 30%, and having an educated father or uncle reduces the likelihood of being a farmer only by an additional 8.5%. Hence, while being a descendant of an educated person clearly one ahead, descendants of the uneducated in villages with schools have nearly caught up over the course of only one generation.

For most other measures of living standards, such as having running water in the house (column 2), having a television or a telephone[20] (columns 3 and 4), the individual- and village-level effects are of comparable magnitude. Also, when we look at the composite measure of living standards, generated by factor analysis, the individual and village-level effects are comparable. In the case of ownership of any means of transportation, however, descendants of educated parents or uncles have a greater additional likelihood of ownership.

The effect of village-level treatment on social networks is also very large, statistically significant

[20]Note that in the first generation we did not report results for ownership of telephone or television set because in early 20th Century neither the educated nor the uneducated had this equipment.

Table 10: Second-Generation Social Networks Effects

	(1) Speaks French	(2) Speaks English	(3) White friends	(4) Social networks scale
Individual-level treatment	0.167***	0.052***	0.050**	0.423***
	(0.044)	(0.017)	(0.023)	(0.090)
Village-level treatment	0.326***	0.011	0.039***	0.427***
	(0.037)	(0.008)	(0.014)	(0.083)
Observations	1925	1925	1496	1841

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: Dependent variables are indicated in the column header. Standard errors are clustered by extended family. All regressions control for gender, indicator for child or nephew/niece, number of siblings, and include commune and decade dummies.

and consistent across measures. In particular, when looking at knowledge of French we again see that just growing up in a village with a school increases the likelihood that the descendent speaks French by about 33%, and the additional effect of being a descendent of an educated person is a further 16 percentage points. In the case of knowledge of English and having white friends, however, the additional effect of being a descendant of an educated person is large, which is reasonable since it requires interaction with people outside the traditional social milieu.

Overall, there is one very big difference in results across the first and second generation. In the first generation, only those who were randomly picked to attend schools reaped the benefits of education. In other words, only the individual-level treatment variable produces positive and statistically significant effects on our two main outcomes of interest – education and living standards. The only discernible positive effect on the contemporaries of students who did not go to school is that they learned a bit more French and began to develop better social networks than those in villages where no schools were set up.

In contrast, in the second generation across all outcomes we see that just having grow up in a village with a school positively affects education and living standards. That is, the village-level treatment effect is now consistently positive and statistically significant, in addition to the individual-level treatment effect. We take this as an indication that the descendants of the une-

ducated are catching up, and catching up fast, especially in terms of living standards and social networks.

We argue that higher human capital is the primary reason why the descendants of the educated and the uneducated converge in outcomes in treated villages. This convergence, especially in living standards, may also run through greater development of the village, better employment opportunities after the school was established, or social networks and connections developed through the school. However, with the data that we have, we cannot and do not attempt to separate these effects.[21] This challenge is left for future research.

7.2 Human Capital as Mechanism: Suggestive Evidence

In this section, we look at a subset of the second generation who were born in one village and after schooling-age moved to another village. In this way we attempt to separate the effect of human capital and the effect of other things (such as connections) that an individual also gains through education. We assume that when a person moves to another village they only carry their human capital with them, which they obtained in school or by interacting with educated people in the village with a school. In contrast, we assume they cannot carry with them the networks and connections they acquired in the village where they grew up. Hence, if we see that being treated leads to better outcomes among migrants, we are more likely to believe a human capital story.

Table 11 presents results for the subset of migrants. Overall, the results are quite similar as those in Tables 9 and 10, of the same sign and typically comparable magnitude. The result for white friends is no longer statistically significant, which is likely due to the small sample size and the fact that there are generally few individuals in our sample with white friends. The results are broadly supportive of our claim that better economic outcomes among descendants of the treated are due to higher human capital.

Of course, we treat these findings only as suggestive evidence in support of the human capital mechanism. A major problem is that the migration decision is not random. Those who move are

[21] We can observe, however, that there are "development effects" from opening a school. In Table A.5 in the Appendix we see that treated villages today typically have more schools than control villages, suggesting that the opening of a school during colonial times had a long-lasting effect of "attracting" more new schools.

Table 11: Second-Generation Social Networks Effects among those who migrated

	(1) Farmer	(2) Living standards scale	(3) Social networks scale	(4) White friends
Individual-level				
	-0.140***	0.573**	0.509***	0.079
treatment	(0.049)	(0.222)	(0.161)	(0.049)
Village-level				
	-0.349***	0.434***	0.561***	0.027
treatment	(0.078)	(0.148)	(0.133)	(0.040)
Observations	472	434	490	353

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: Dependent variables are indicated in the column header. Standard errors are clustered by extended family. All regressions control for gender, indicator for child or nephew/niece, number of siblings, and include commune and decade dummies.

likely to move because they expect good earning opportunities at the new location. The same characteristics that make an individual success at the new location might also be correlated with the success of their parent or uncle and their education level.

7.3 Family Tax: Do Nieces and Nephews Perform as Well as Children?

So far we have shown that in the first generation the educated have better outcomes than the uneducated, and that in the second generation the descendants of the educated have better outcomes. Under "descendants" we included both the direct descendants (i.e. children of the original students) as well as the indirect descendants (i.e. nieces and nephews of the students). The natural question arises – do the children accrue higher benefits from their parent's education than nieces and nephews? The answer to this question is given in Table 12 where we compare the average outcomes of children of the students, and nieces and nephews of the original students to all other descendants.

Some readers may find it surprising that children of the students do not seem to be performing any better than their nieces and nephews, as indicated through the F-test in the last row of Table 12. We find that this demonstrates the strength of extended family networks in Western Africa and the pressure on successful individuals to support their kin. It is true that the children of the

Table 12: Outcomes for Children and Extended Family Descendants of the Students

	(1) Education	(2) Primary or more	(3) Secondary or more	(4) University
Student child				
× Ind. treatment	0.642***	0.287***	0.259***	0.096***
	(0.101)	(0.047)	(0.045)	(0.029)
Student niece/nephew				
× Ind. treatment	0.503***	0.205***	0.184***	0.114***
	(0.086)	(0.040)	(0.038)	(0.029)
Observations	2396	2396	2396	2396
F-test p-value, child=niece/nephew	0.171	0.082	0.138	0.655

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: Dependent variables are indicated in the column header. Standard errors are clustered by extended family. All regressions control for gender, number of siblings, and include commune and decade dummies. The last row shows the p-value from an F-test of the difference between the two coefficients presented in the table (null hypothesis is that of no difference).

former students tend to have more primary education than nieces and nephews, but this difference is statistically significant only at the ten percent level. For all the other education levels, the difference between children and nieces and nephews is statistically insignificant.

If we acknowledge the strength of extended family networks, we would expect that nieces and nephews of the former students, even though they were born to uneducated parents, to do significantly better than descendants of uneducated parents who do not have any educated members in the extended family. This is confirmed in the second row of Table 12.[22]

We see that across all education outcomes having just one educated person in the extended family makes a large difference to the outcomes of the nieces and nephews. These descendants have better education at all education levels than descendants (either children on nieces and nephews) in families where no one was educated. These effects are statistically significant and substantial – they are 20% more likely to have primary school education, 19% more likely to have secondary school education and 11% more likely to go to university.

[22]We confirm that children and nieces and nephews in Treatment 2 and control do not have different outcomes – as they should not, given that none of the parents in their extended family had formal education. Results are available upon request.

What may be happening is that educated uncles tend to support their nieces and nephews almost as much as their own children – we call this the extended family tax on education. One way to test this mechanism is to compare educational attainment of daughters/sons and nieces/nephews in small and large extended families. If the family tax mechanism exists, we could imagine that as the extended family increases, the ability of the educated uncle to support all the nieces and nephews may be stretched too thin. In order words, the difference between daughters/sons and nieces/nephews may be increasing as the size of the extended family size increases.[23] Results presented in Figure 1 seem to support this mechanism.

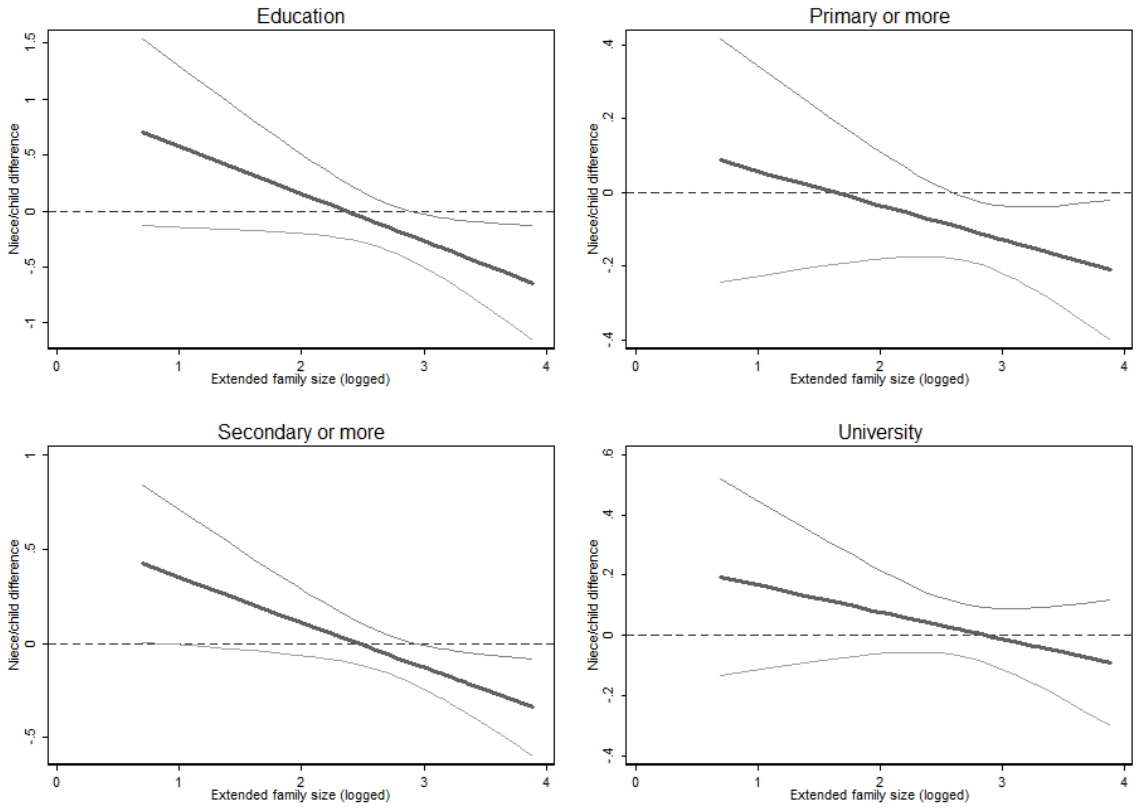
In Figure 1 we see that the difference in education outcomes between daughters/sons and nieces/nephews becomes negative and statistically significant if the logged extended family size exceeds about 3 (i.e. the true extended family size exceeds about 20). Given such a large extended family, the educated uncle must prioritize between educating his own children and educating the extended family, and the data suggest that at around this threshold level, education of own children becomes more important than education of the nieces and nephews.

Note that our finding of an extended family tax is in discord with findings in the developed world that extended families are *not* altruistically linked (Altonji et al. 1992).[24] How does the existence of an extended family tax affect the human development of West Africa, and Benin in particular? Clearly, in the aggregate, there is a positive side of the family tax as it allows more promising children to get high levels of education, especially university education. However, there is also a negative side. As shown in Table 14 uneducated siblings of initial students choose to have more children than their uneducated counterparts in the same villages with a school. Hence, these parents choose to have more children then they could raise independently. Educated parents,

[23] An alternative plausible explanation might be that extended family externality runs through *aspirations*. The educated uncle may serve as a role model to both nieces and nephews and their parents. Similarly, nieces and nephews may increase their educational attainment through emulation and learning from the children of the educated uncle. It is possible that as the extended family grows, ties to the educated uncles of any one niece and nephew become weaker, thus weakening the power of aspirations and emulation. However, based on our knowledge of extended family networks in Benin, this is unlikely.

[24] Our findings are also related to the literature on sibling rivalry in developing countries. In Burkina Faso, Akresh et al. (2010) have found that if one child has higher IQ than his or her sibling, this child receives a disproportionately large share of the families investment in education. In other words, a child is picked as the "hope of the family" and supported at the expense of less-abled siblings. Other papers that have found evidence of sibling rivalry in developing countries include Morduch (2000), Garg and Morduch (1998), Parish and Willis (1993) and Binder (1998), often in the context of allocation of resources across male and female children.

Figure 1: Education and Family Tax in Extended Families



Note: All models control for gender, number of siblings, parents' wealth, and include commune and decade dummies. Marginal effects are calculated by keeping all remaining regressors at their means or medians. Gray lines represent the 95 percent confidence interval based on the standard errors clustered by extended family.

knowing that their siblings will expect support, may decide that exerting high effort to earn more may not be optimal given that they will have to give up an increasing amount to their increasing extended family. With our analysis here, we only acknowledge the apparent existence of family tax. Currently, we cannot discern the magnitude of the positive and negative effects of family tax and we leave these challenges for future work.

The results contribute to the growing development research on the institutions of kin system, a "social contract of mutual assistance among members of an extended family" (Hoff and Sen 2005, p. 2).[25] Our results documents the way in which the kin system can both be a "vehicle of progress"

[25]See also Platteau (2000), Comola and Fafchamps (2012), and Barr and Stein (2008).

or "instrument of stagnation" (Hoff and Sen 2005). On the one hand, it allows the benefits of education to spill over quite rapidly to a large number of near and distant relatives and neighbors. On the other hand, it creates a strong distributive pressure on the educated and successful member of the extended family in the form of a family tax. Faced with harsh social sanctions if they do not redistribute, they can choose to invest in less profitable activities, so long that they are less observable to family members.[26]

7.4 Aspirations: A Determinant of Village-Level Externalities

In this section we aim to explain how the children of the uneducated in villages with schools began to catch up with the children of the educated. We saw in section 7 that village-level treatment is associated with higher education, living standards and social network outcomes in the second generation. But, of course, there is variation in outcomes. In this section, we identify which uneducated families in villages with schools are more likely to produce educated children. The channel that we focus on is that of higher aspirations developed through greater social ties with the educated locals and the white colonialists.

A problem in estimating the causal effect of a parent's social network on his children's education is possible endogeneity. Parents with extensive social networks are more likely to be more ambitious and possess superior abilities, characteristics that lead to greater social networks and greater aspirations for their children's education, living standards, etc. One way to solve this problem is to instrument for parents' social networks with their distance from the location of the school where they could interact with the better educated locals and colonialists. This is what we do in Table 13.[27]

The first stage in Table 13 is the same in each regression because we are using distance from school to instrument for parents' social networks in each regression. Next, we use the variation in parents' social network that can be explained by distance and find that it is associated with higher

[26] See Baland et al. (2011), Dupas and Robinson (2009), and Jakiela and Ozier (2012) for evidence for this type of behavior.

[27] In Table 13 we can only use information on parents and their own children, hence the number of observations is relatively low (499). We cannot use the full set of nephews and nieces because we do not have information about the social networks of the parents of nephews and nieces (we only have information about their one uncle).

Table 13: Parents' Social Networks as Determinants of Descendants' Education

	(1) Education	(2) Primary or more	(3) Secondary or more	(4) University
Social networks scale	0.808**	0.406**	0.293**	0.109*
	(0.349)	(0.207)	(0.139)	(0.065)
First stage				
Distance from school	-3.623***	-3.623***	-3.623***	-3.623***
	(1.228)	(1.228)	(1.228)	(1.228)
Observations	499	499	499	499
F-statistic	8.698	8.698	8.698	8.698
p-value	0.004	0.004	0.004	0.004
Anderson-Rubin p-value	0.005	0.010	0.027	0.114

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: Dependent variables are indicated in the column header. Social network scale is a factor score comprising a number of variables. Social network scale is that of the parents. It is instrumented by the distance of the parents' household from the school in the nearest village. Standard errors are clustered by extended family. All regressions control for gender, number of siblings, and include commune and decade dummies.

education of their children.[28] This positive relationship is statistically significant at the 5% level.

What do these findings suggest? Our interpretation is that greater interaction with the educated locals and the colonialists, simply because they set up a school in the vicinity, increases the aspirations that parents have for their children. This then induces parents to invest more in their children's education and leads to better outcomes at the village-level where schools were set up.

8 Robustness Checks

8.1 Addressing Possible Bias due to Different Birth Patterns

We have found significant differences among the descendants of the educated and the uneducated from villages with and without schools as described in Section 7, yet we must be careful when

[28] To alleviate concern about distance to school being a weak instrument, we report the Anderson-Rubin p-values which test whether all the excluded instruments are relevant. This test is satisfied in the case of primary and secondary education, and the overall education variable. However, because the AR test also includes the second stage regressors, and there is little variation in the data when it comes to university education, the result is weaker and should be treated with additional caution.

interpreting these differences. For causal interpretation, we need the individual- and village-level assignment to be random. However, in the second generation the individual-level treatment is not entirely random because parents *choose* how many children to have. In particular, in Benin, more educated parents tend to be richer and to have more children, nieces and nephews, as documented in Table 14.

Table 14: Treatment Assignment and Family Size

	Treated parents	Untreated parents in village w/ school	Untreated parents in village w/o school
Children			
Average number	5.49	4.98	3.17
Difference from treated		-0.51	-2.32
p-value		0.20	0.00
Descendants			
Average number	5.88	4.02	2.88
Difference from treated		-1.86	-3.00
p-value		0.00	0.00

Note: Extended family descendants include all reported nieces, nephews and foster children. For very large families, our sampling design includes only a random subsample of all extended family descendants; this design should not affect the accuracy of the test reported. p-values are based on the Mann-Whitney-Wilcoxen difference of means test. Results are qualitatively equivalent if using the traditional two-groups difference of means t-test or the Kolmogorov-Smirnov test of the equality of distributions.

How may this bias our results? When the treatment assignment affects the number of children and nieces and nephews born to the educated, we are faced with a selection problem. A good way to think about this problem is in terms of "principal strata" (Frangakis and Rubin 2002). Among the descendants of the educated, there are some children who would have been born regardless of parent's/uncle's treatment status (i.e. always-takers, or always born) and there are children who were born only because their parent/uncle was treated (i.e. compliers), and hence had funds to raise an additional child. Among the descendants of the uneducated, there are again the always-takers, who would have been born regardless of treatment status, and possibly some defiers, i.e. those who are born only if their parent is uneducated.

The estimator in equation 2 makes a "naive" comparison of the treated and control descendants, assuming that the underlying populations and their potential outcomes are the same. However, we

infer that the two groups do not represent the same population because of the evidence shown above – that treated parents have more kids than parents in the control group. For causal interpretation, we may only compare the always-takers, i.e. those who would have been born regardless of treatment status (Horowitz and Manski 2000, Lee 2009, Zhang and Rubin 2003, Zhang et al. 2009). We try to do this in two ways.

First, conditional on having children, a family will at least have a first-born. Hence, for families with children, it is reasonable to consider the first-born the always-born. We also need to assume that monotonicity holds, i.e. that there are no defiers in the control group. This framework allows us to assume that the only subpopulation in the control group is the always-born.[29] Table 15 shows the main results, for education, living standards scale, and social network scale on this subpopulation. The results are substantively very similar to those shown above.

Table 15: Outcomes for First-Born Descendants Only

	(1) Education	(2) Living standards scale	(3) Social networks scale
Individual-level treatment	0.297**	0.535***	0.374***
	(0.126)	(0.141)	(0.128)
Village-level treatment	0.451***	0.393***	0.408***
	(0.084)	(0.102)	(0.112)
Observations	383	341	375

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: Dependent variables are indicated in the column header. Standard errors are clustered by extended family. All regressions control for gender, number of siblings, and include commune and decade dummies.

Another approach is to calculate *bounds* on the treatment effect, according to Lee (2009), which we present in Table 16. The key assumption again is that monotonicity holds. In order to calculate the lower and upper bounds for our treatment effect, we need to focus on the compliers in the treatment group. To determine the share of compliers in the treatment group, we should take the difference between those who were born in the treatment group and those who were born in the

[29]Is the monotonicity assumption reasonable in our case? We believe that it is. If this assumption were violated, then there exist people who have fewer kids if they are educated then if they had been uneducated. In the aftermath of the slave trade that decimated the local population over four centuries, people in 20th Century Dahomey had as many children as they could afford (see Manning 1982).Hence, the educated would almost never have fewer children then the uneducated.

control group (i.e. the difference between the always-born and compliers in the treatment group and the always-born in the control group), and express that as a share of the born individuals in the treatment group. Next, since we cannot identify exactly who these compliers are, just how many of them there are, we construct the best- and the worst-case scenarios, as in Lee (2009). In the *best case*, all compliers have the *lowest* education level among the treated who were born. We then "trim" the low end of the distribution of education among the treated by the share of the compliers, and recalculate the mean education among treated and calculate the treatment effect with this mean (by subtracting the mean education of the control group). Since the low end of the distribution is trimmed, the new mean of the treated will be higher, and the new treatment effect will be higher. This is the *upper bound*. In the *worst case*, all compliers have the *highest* education level among the treated who were employed. We then trim the high end of the distribution of education among the treated by the share of the compliers, and recalculate the treated mean and the treatment effect. Now, the treatment mean and the treatment effect will be lower, which gives the *lower bound*.

The calculated best and worst case bounds are presented in Table 16.[30] Individual-level effects are positive and both the lower and upper bound of the ATE are statistically significant. This is true for all outcomes – education, living standards and networks scales. For village-level effects, the worst- and best-case bounds are wider, because the difference in the number of descendants in villages with and without schools is larger (see Table 14). The estimated lower bound for the village-level effect is typically just below zero, suggesting that in the worst-case scenario, we cannot claim the existence of a village-level effect. Yet, the worst-case scenario – that compliers have higher potential outcomes than the always-born – is pretty extreme, and most likely a positive effect remains.

[30]Note that results in Table 16 are calculated only for children. If nieces and nephews were included, then the ATE shown in this table would be the same as the ATE in column 1 of Table 8, column 7 of Table 9 and column 4 of Table 10. We exclude nieces and nephews because we do not have precise information as to which nuclear family they belong (i.e. how many brothers and sisters they have) which is necessary for the computations.

Table 16: Bounds on Treatment Effect for Children with Selective Birth

	Treatment	ATE	"Worst Case" Bound	"Best Case" Bound
Education				
	Individual-level	0.483 (0.108)***	0.400 (0.102)***	0.539 (0.105)***
	Village-level	0.427 (0.075)***	-0.115 (0.048)**	0.929 (0.076)***
Living standards scale				
	Individual-level	0.502 (0.116)***	0.416 (0.112)***	0.568 (0.118)***
	Village-level	0.569 (0.077)***	-0.069 (0.054)	1.093 (0.082)***
Networks scale				
	Individual-level	0.444 (0.121)***	0.293 (0.103)***	0.529 (0.118)***
	Village-level	0.231 (0.090)**	-0.260 (0.075)***	0.777 (0.087)***

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: The main entries in cells are estimates from the regression of each dependent variable, indicated in the first column, on individual-level and village-level treatment. The entries in the parentheses are the standard errors, clustered by extended family. Bounds are obtained using the method of Lee (2009). No other controls are used, but the results (available upon request) are qualitatively similar when controls from the models reported in previous tables are included. The share of unborn children in any group (the "never-born"), needed for the trimming procedure, is not observed. It is assumed that the largest family within the wealthiest 50 percent in the treatment group had attained an ideal family size. The unborn are obtained for every other family by subtracting their number of children from the family with the largest number of children. Results are qualitatively similar when the share of the unborn children in each control group is alternatively calculated by taking the ratio of the average number of children in that group and the average number of children in the treatment group.

8.2 Addressing Possible Bias due to Non-Random Missingness

A natural concern is that our dataset fails to capture the less successful and prosperous individuals in the first generation, as well as their descendants. Since we have shown that success is correlated with education, this may imply that we are less likely to observe individuals in control groups than in treatment. Therefore, our comparisons may overestimate the returns to education. There are two ways in which this bias may arise. First, we may fail to observe any data on less successful individuals due to biased sampling. However, our design discussed above ensures against that.

Second, conditional on sampling, we may fail to observe less successful individuals if they are more likely to have missing values for outcomes of interest. This may be a consequence of recall bias – our respondents may be more likely to remember the outcomes of the more successful relatives. There is some evidence of this in our data. For example, the rate of missingness on education is significantly lower among the treated first-generation individuals (8%) than those in control 1 and

control 2 (27% and 20%, respectively). Since our estimates in the previous sections discard missing values, our estimates may be biased. We therefore perform several checks of the robustness of our findings to the potentially non-random patterns of missingness.

First, we perform a worst-case scenario exercise similar in logic to that in the previous section. We assume that a missing value on some variable of interest is due to the value of that particular variable, i.e. that missingness is non-ignorable (Little and Rubin 1987). As we focus on the outcomes examined in the previous sections, we are assuming that missingness is caused by treatment status. We further assume that all missing values in treatment contain the lowest outcome, and all missing values in control groups contain the highest outcome. This is the worst-case scenario for our estimates: assigning the lowest (highest) outcome in treatment (control) will down-weight the effects of education shown above in proportion to the share of missing values in each treatment group.

Table A.1 shows the first-generation results of this exercise.[31] While the results are somewhat weaker, even under the worst-case scenario the sign and the significance of most of our earlier results are entirely preserved. This is the case even for the outcomes for which missingness is relatively substantial, such as the farmer indicator, where almost 30% of observations are missing.

[Tables A.1 and A.2 about here.]

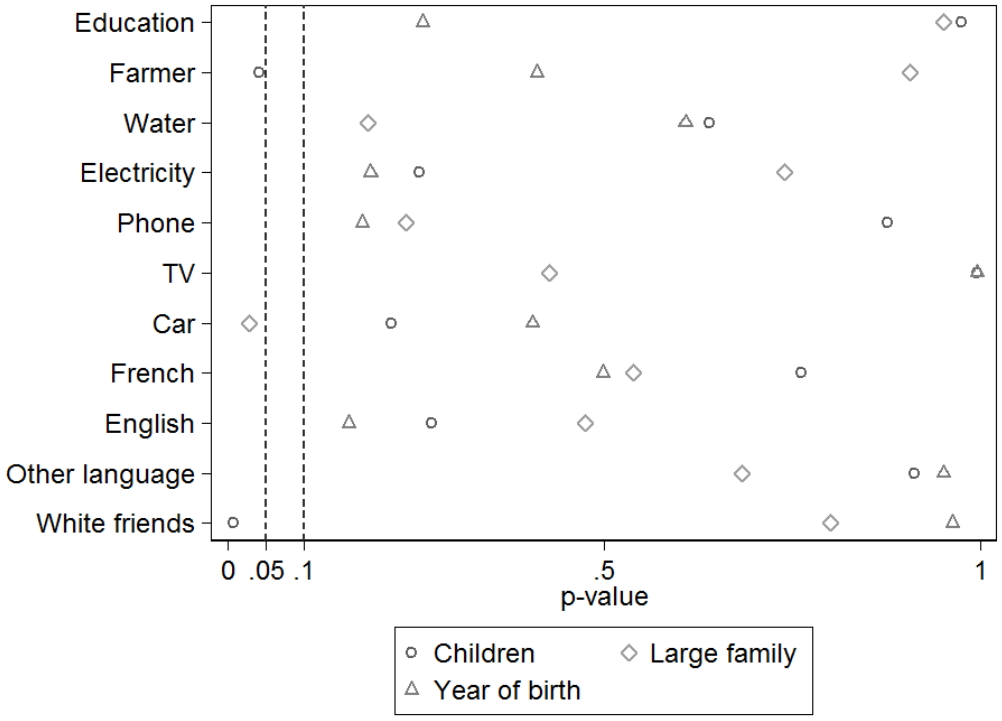
Table A.2 shows the results of the same exercise for the descendants. Again, our results are mostly identical. Note for example that the worst-case scenario assumes that all missing data on education in the control group contain the achievement of university education, whereas all missing data in treatment indicate individuals without education. Nevertheless, the worst-case scenario estimates still point to significant positive effects of parents' education on descendants' outcomes.

Recall bias may not be entirely non-ignorable, i.e. missingness on our outcome data may be due to some other factors observable in the data. For example, our respondents may be more likely to recall outcomes for children than for nephews and nieces, for smaller families, or for individuals who were born later. Figure 2 examines the evidence for such possibilities. It plots the p-value

[31] We exclude the factor scales for living standards and networks because we show the results of many of their individual components. Moreover, these scales are continuous, and it is less clear what value to assign. For example, assigning the minimum in treatment and the maximum in control represents an extremely conservative test.

from separate regressions of the missing value indicator for our outcome variables on: the dummy variable for children (circles), the log of the number of siblings (diamonds), and the year of birth (triangles). The figure shows that there is no systematic evidence of recall bias based on any of the three plausible sources. In most regressions, the coefficient on all three variables of interest is not significant at the conventional levels, indicated by the vertical dashed lines.

Figure 2: Evidence for Recall Bias



We perform one more check of the evidence for recall bias. We have shown in section 7.3 that direct offspring do not have higher educational attainment than extended family descendants. Even though Figure 2 does not suggest that missingness is less likely among direct offspring, it may be that our respondents are less likely to recall outcomes for less successful nieces/nephews than less successful daughters/sons. This would bias our results towards zero when comparing daughters/sons and nieces/nephews. One way to examine the robustness to such recall bias is to compare only sons and nephews, as male descendants were likely more successful on average than

females.[32] In this subsample, the recall bias towards zero – if it exists – should be lower. Table 17 reruns models from Table 12 on the subsample of men. Our results are unchanged, further suggesting that recall bias is not an issue.

Table 17: Outcomes for Children and Nieces and Nephews in the Second Generation – Males only

	(1) Education	(2) Primary or more	(3) Secondary or more	(4) University
Children	0.034	0.033	0.031	-0.030
	(0.113)	(0.048)	(0.059)	(0.055)
Observations	452	449	449	449

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: Dependent variables are indicated in the column header. Standard errors are clustered by extended family. All regressions control for gender, number of siblings, and include commune and decade dummies.

9 Conclusion

We use data from the first elementary schools established in areas of colonial Benin with no prior European influence to estimate economic and social effects of education. We find a large positive impact of education on measures of living standards, professional achievements and occupational diversity. We also find significant peer effects and intergenerational living standards effects, and we argue that they are driven in large part by aspirations. Finally, the paper presents the first empirical analysis of the offsetting effects of kin systems in Africa. We find both sizable education spillovers across family and neighbors as well as redistributive pressures within extended families.

Our results provide rigorous estimates of human capital externalities and illustrate their impact on development. However, it is unclear whether the documented impact was driven more by knowledge spillovers or by colonial investment in local public goods. An important contribution resides in the empirical strategy we use to investigate the comparative effects on human capital and colonial institutions for long-term development. When institutions and human capital shocks are simultaneous, one might disentangle the effects on these competing factors by comparing development outcomes in areas where education came before formal intuitions with the outcomes in

[32] One reason to believe this is that we are more likely to observe nephews than nieces relative to the ratio of sons to daughters.

areas where schools were created after formal institutions were established.

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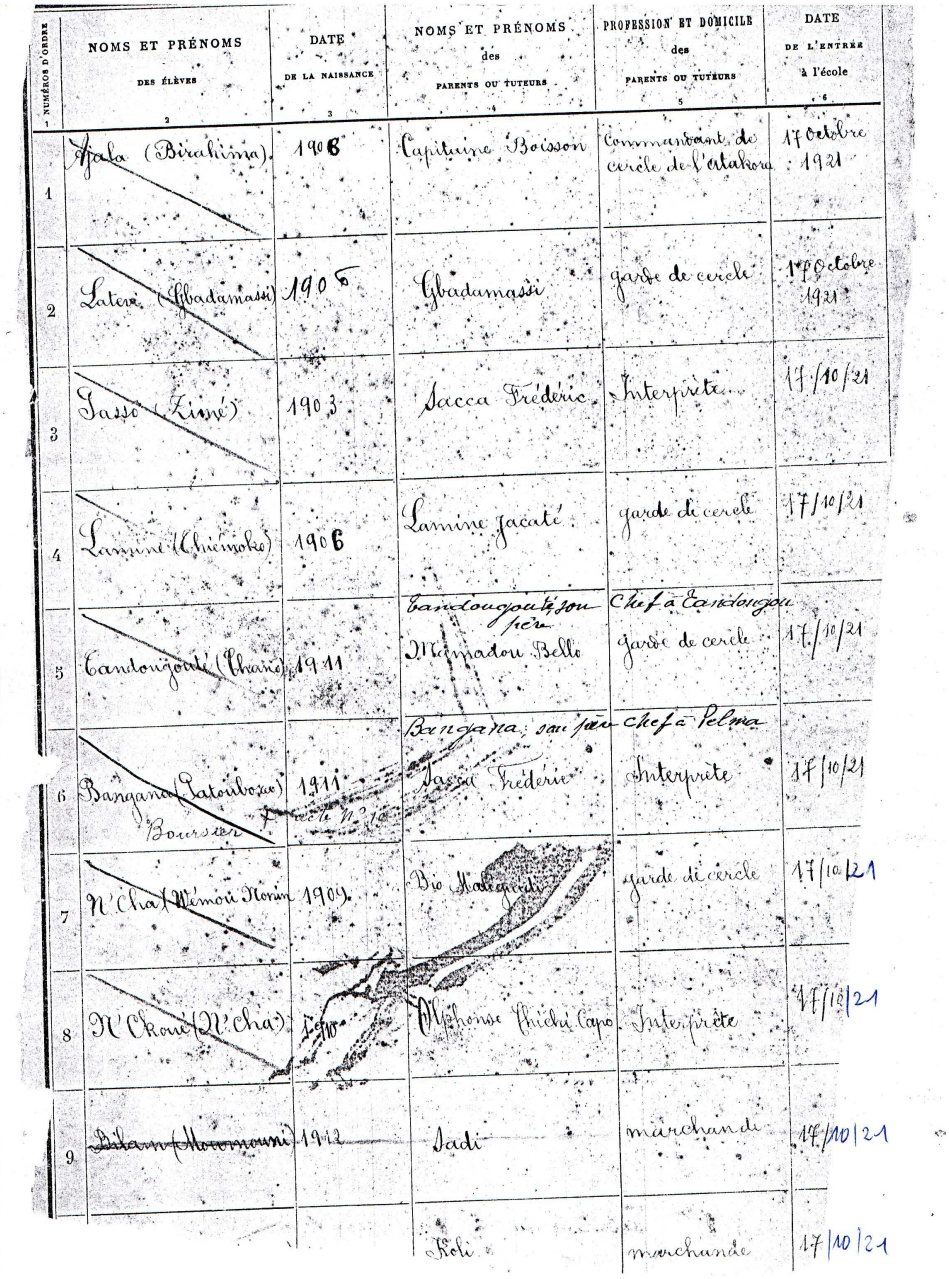
Appendix

Figure A.1: List of the first students in the school in Natitingou (fragment)



NUMÉROS D'ORDRE	NOMS ET PRÉNOMS DES ÉLÈVES	DATE DE LA NAISSANCE	NOMS ET PRÉNOMS des PARENTS OU TUTEURS	PROFESSION ET DOMICILE des PARENTS OU TUTEURS	DATE DE L'ENTRÉE à l'école
1	Ayala (Birachima)	1908	Capitaine Boisson	Commandant de cercle de l'Atakora	17 Octobre 1921
2	Latera (Gbadamassi)	1905	Gbadamassi	garde de cercle	17 octobre 1921
3	Sasso (Zimé)	1903	Sacca Frédéric	Interprète	17/10/21
4	Lamine (Chicmoko)	1906	Lamine Jacaté	garde de cercle	17/10/21
5	Cantonjoaté (Chaire)	1911	Cantonjoaté son père Mamadou Belle	Chef à Cantonjoaté garde de cercle	17/10/21
6	Bangana (Latoubzae) Boursier	1911	Bangana son père Sacca Frédéric	Chef à Pelma Interprète	17/10/21
7	N'Cha (Mémoi Norim)	1904	Bio Nalegnon	garde de cercle	17/10/21
8	N'Koué (N'Cha)	1910	Alphonse Chidia Capo	Interprète	17/10/21
9	~~Bikam (Nkonmouni)~~	1912	Sadi	marchand	17/10/21
			Koli	marchande	17/10/21

Figure A.2: Map of Benin, 1936

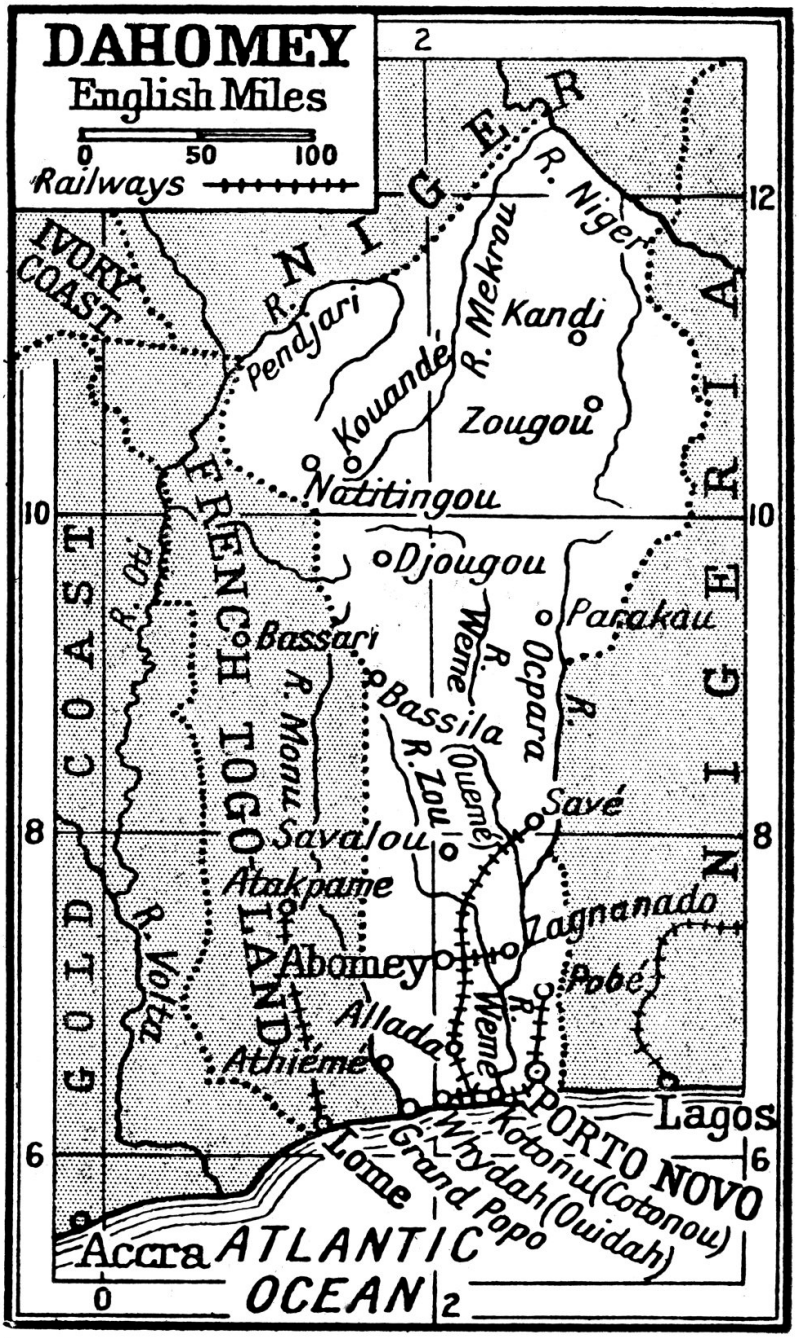


Table A.1: Robustness to Non-Ignorable Missingness – First Generation

	(1) Education	(2) Farmer	(3) Water	(4) Electricity	(5) Vehicle	(6) French language	(7) Other language	(8) White friends
Individual-level treatment	0.697**	-0.339*	0.112**	0.077**	0.218**	0.870***	0.061**	0.158*
	(0.170)	(0.141)	(0.040)	(0.020)	(0.057)	(0.035)	(0.014)	(0.059)
Village-level treatment	0.050	0.004	0.054	0.018	0.021	0.072*	0.006	0.096**
	(0.044)	(0.222)	(0.052)	(0.011)	(0.056)	(0.027)	(0.006)	(0.027)
Observations	405	405	405	405	405	405	405	405
Missing share	0.20	0.28	0.01	0.01	0.04	0.01	0.01	0.13

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: Dependent variables are indicated in the column header. The worst-case scenario is explained in the text. Standard errors are clustered by commune.

Table A.2: Robustness to Non-Ignorable Missingness – Second Generation

	(1) Education	(2) Farmer	(3) Water	(4) Electricity	(5) TV	(6) Phone	(7) Car	(8) French	(9) English	(10) White friends
Individual-level treatment	0.292***	-0.042	0.080	0.128**	0.169***	0.194***	0.061	0.158***	0.040***	-0.209***
	(0.103)	(0.029)	(0.067)	(0.051)	(0.048)	(0.047)	(0.039)	(0.054)	(0.015)	(0.030)
Village-level treatment	0.412***	-0.196***	0.070	0.418***	0.291***	0.207***	0.026	0.249***	0.011	0.169***
	(0.085)	(0.038)	(0.067)	(0.037)	(0.031)	(0.030)	(0.034)	(0.047)	(0.007)	(0.039)
Observations	2509	2509	2509	2509	2509	2509	2509	2509	2509	2509
Missing share	0.03	0.08	0.02	0.02	0.02	0.02	0.04	0.02	0.02	0.25

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Note: Dependent variables are indicated in the column header. The worst-case scenario is explained in the text. Standard errors are clustered by extended family. All regressions control for gender, number of siblings, and include commune and decade dummies.

Pretreatment Variables

Table A.3: Pretreatment variables, village level

District	Save		Natitinguou		Kandi		Zagnanado	
Village	Boni (T)	Okpara	Winke (T)	Koudengou	Keferi (T)	Sonsoro	Assiadji (T)	Kpedekpo
Elevation (m)	211	163	462	444	294	289	100	37
Soil Quality	0.49	0.49	0.51	0.51	0.51	0.51	1.77	3.22
Average Rainfall (mm)	150	150	108	108	92	92	83	83
Distance Port (km)	188	186	451	462	530	525	96	98
Navigable Rivers	1	1	0	0	1	1	1	1
Kingdom	0	0	0	0	0	0	1	1
Trading Post	0	0	0	0	0	0	0	0
Years of Resistance	4	4	23	23	22	22	3	3

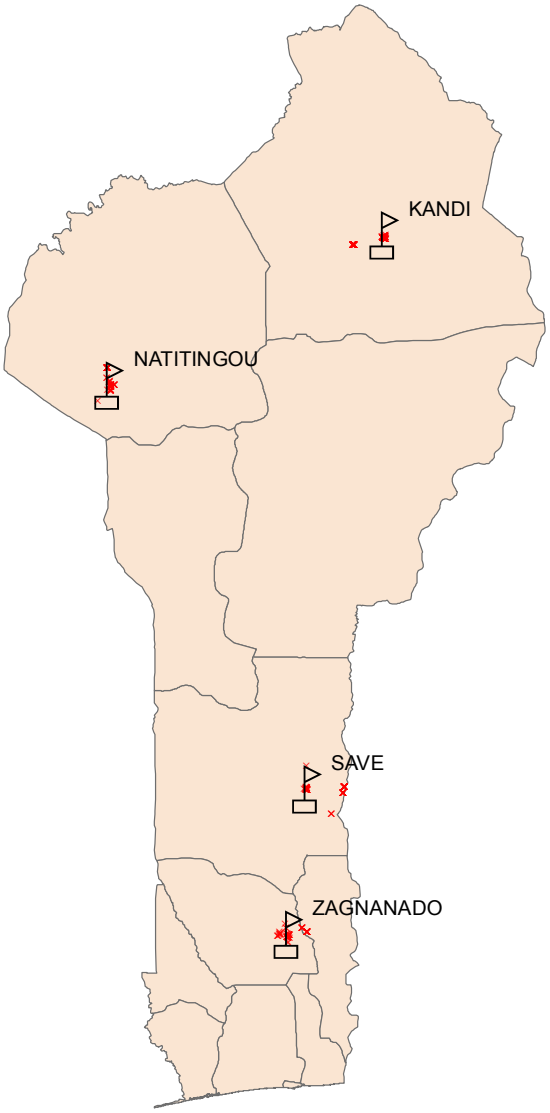
Table A.4: Pretreatment variable, individual level

	Treated parents	Untreated parents in village w/ school	Untreated parents in village w/o school
Pre-treatment			
Number of siblings	3.370	3.059	2.964
	(2.366)	(2.326)	(2.114)

Note: Standard deviations in parentheses.

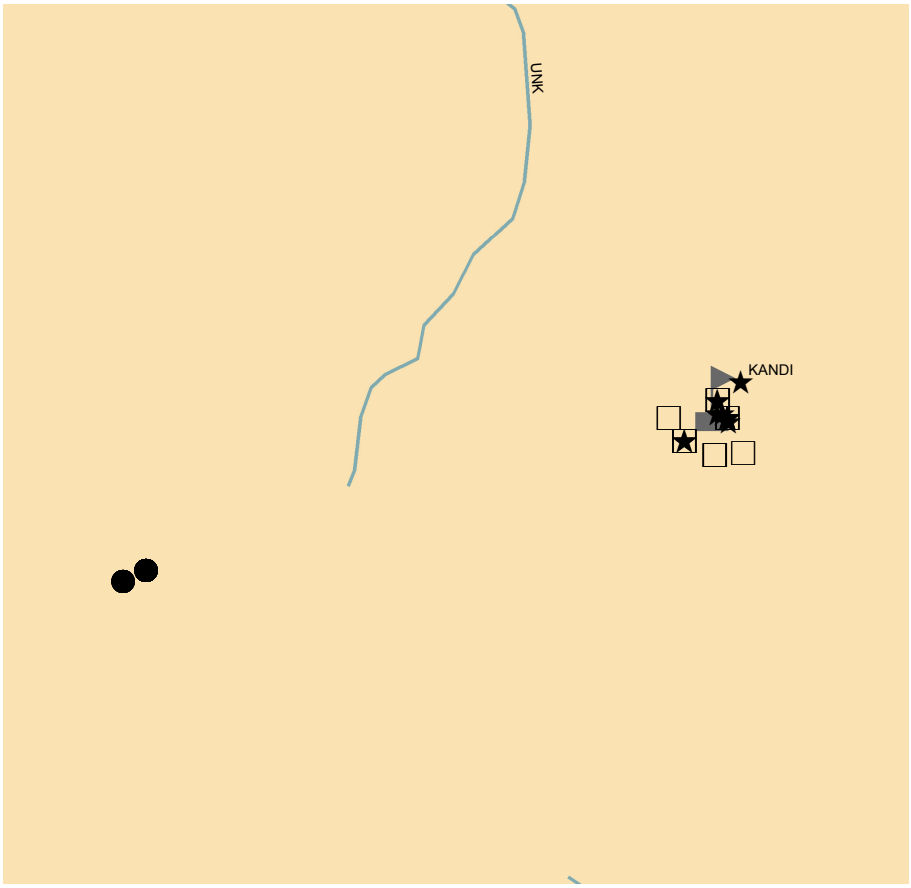
Maps

Figure A.3: Map of Benin with the Four School Sites



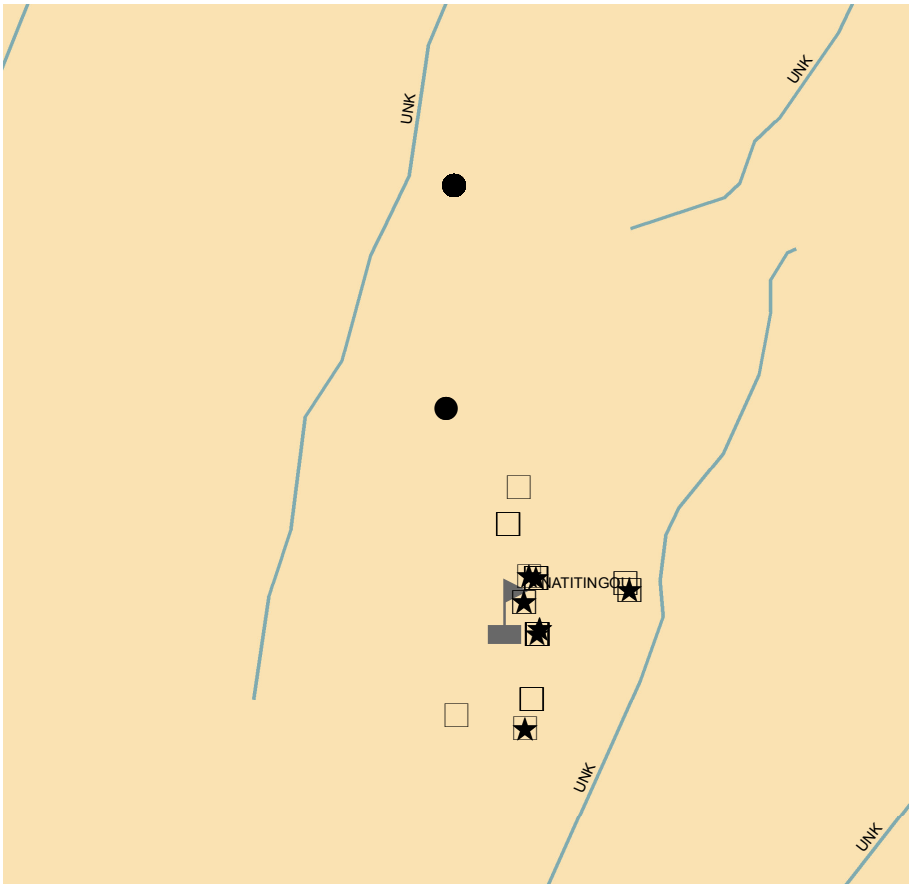
Note: Each of the four schools are flagged and the location of the first-generation individuals is shown with crosses.

Figure A.4: Map of Kandi



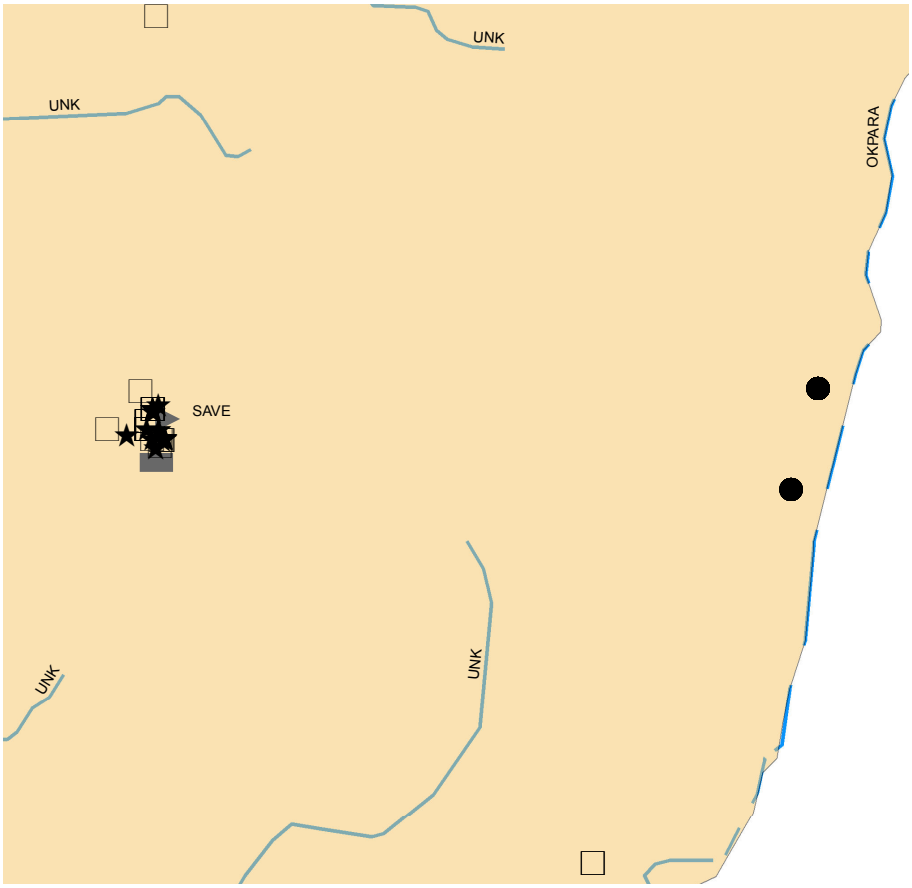
Note: Locations with treated individuals are indicated by stars, those with untreated individuals within sites with school are indicated with squares, and those with untreated individuals in sites without schools are indicated with circles. Multiple individuals living at the same location are represented by one symbol.

Figure A.5: Map of Natitingou



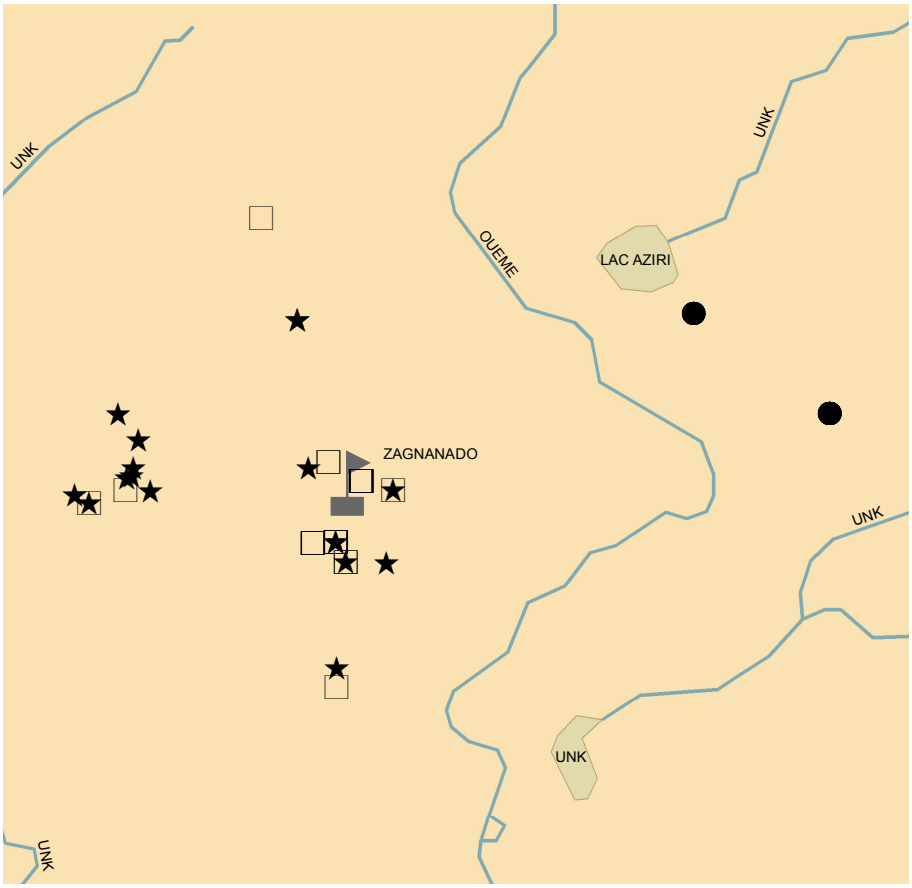
Note: Locations with treated individuals are indicated by stars, those with untreated individuals within sites with school are indicated with squares, and those with untreated individuals in sites without schools are indicated with circles. Multiple individuals living at the same location are represented by one symbol.

Figure A.6: Map of Save



Note: Locations with treated individuals are indicated by stars, those with untreated individuals within sites with school are indicated with squares, and those with untreated individuals in sites without schools are indicated with circles. Multiple individuals living at the same location are represented by one symbol.

Figure A.7: Map of Zagnanado



Note: Locations with treated individuals are indicated by stars, those with untreated individuals within sites with school are indicated with squares, and those with untreated individuals in sites without schools are indicated with circles. Multiple individuals living at the same location are represented by one symbol.

Dates of School Openings

Table A.5: Dates of School Openings

Village	Number of Schools	Years
Savè		
Treated	3	1913, 1958, 1970
Control	3	1953, 1971, 1972
Kandi		
Treated	2	1913, 1951
Control	1	1960-1963
Zagnanado		
Treated	24	1895, 1919, 1951, 1952, 1953, 1957 1959, 1962, 1964, 1969, 1970, 1971, 1973, 1977, 1978 (5 schools), 1980, 1982, 1996 (2 schools), 1998
Control	2	1966 (2 schools)
Natitingou		
Treated	6	1921, 1948, 1969, 1970, 1972, 1973
Control	1	1971

Living standards and Networks Scale

The living standards scale and networks scale were both generated using factor analysis. While we could not directly observe the income of our subjects at different points in time, we recorded various indicators of their standard of living and thus accumulated income. For example, if a subject had a home with running water and/or a well, electricity and a radio, they likely had better income over the years than someone without any of these facilities, and thus a higher living standard. The full set of variables that we use to construct the living standards scale is given in Table A.6. For example, almost everyone in the control villages in the first generation lived in a traditional house with terracotta walls, while in the treatment villages, some houses were made of cement and other more durable materials.

Table A.6 presents examples of a few individuals from our data to give a sense of what different values on the living standards scale mean and to help interpret the results from regressions in Section 6. We see that a low living standards score (-1.11) can be characterized by an individual with no water, electricity, radio, or other household appliances, who lives in a traditional house, with terracotta walls, travels on foot and wears traditional clothing. This individual is a farmer who owns land but shares his house with extended family. Increasing living standards on our standardized[33] scale by approximately 1, from -1.11 to -0.09, can be associated with an individual who owns a separate home and wears French-style clothing. Increasing living standards from -0.09 to 0.88, in our example, the individual's occupation is commerce/business, he travels by bicycle and owns some furniture and devices such as a kitchen table and a radio in the household. Finally, at the living standards score of 1.91 the individual might own a motorbike or occasionally a car, and enjoy electricity and running water. Of course, there are variations in exact characteristics associated with each living standards score, but these examples are indicative.

Similarly, Table A.7 presents examples of individuals in the first generation with various scores on the networks scale. The networks scale was also generated by factor analysis, using the variables listed in column headings of Table A.7. A person has better social networks if they are a member

[33]Note that both the living standards and social networks scale are standardized to have a mean of zero and standard deviation of one. The smallest value on the living standards scale was -1.17, the largest was 2.64 and the median was -0.33.

of various associations such as, religious, sports, professional (i.e. business), if they are members of a political party, if they speak foreign languages and have friends among whites or other ethnic groups. For example, at the lower end, an individual with networks scale value of -1.01 is not a member of any association, does not speak foreign languages or has friends outside his ethnic group. Increasing the networks score by 1 to 0.005 we would expect an individual to be a member of at least some association and speak an additional language.

Table A.6: Living standards scale examples in first generation

Income scale	Water	Electricity	Radio	Owns shop?	Owns land?	Occupation	Tradition. house?	Terracota walls?	Owns devices?	Residence owned?	Means of transport	Attire	Travel distance
-1.113	No	No	No	No	Yes	Farmer	Yes	Yes	No	Family	On foot	Trad.	Region
-0.089	No	No	No	No	Yes	Farmer	Yes	Yes	No	Owner	On foot	French	Village
0.875	No	No	No	No	Yes	Commerce	Yes	Yes	Yes	Owner	Bicycle	Trad.	Region
1.911	Yes	Yes	Yes	Yes	No	Law	Yes	Yes	Yes	Owner	Other	Trad.	Region

Table A.7: Social networks scale examples in the first generation

Networks scale	Religious assoc. member?	Political party member?	Business assoc. member?	Sports assoc. member?	Other assoc. member?	Speaks French?	Speaks other nat. lang?	Speaks other foreign lang.?	Friends with Whites?	Friends with other ethnicities?
-1.012	No	No	No	No	No	No	No	No	No	No
0.005	No	No	Yes	No	No	No	Yes	No	No	No
1.03	No	No	No	Yes	Yes	Yes	Yes	No	No	Yes
2.12	No	Yes	No	Yes	No	Yes	Yes	No	Yes	Yes