Education and Human Capital Externalities: Evidence from Colonial Benin*

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Abstract

Using a unique dataset on students from the first regional schools in colonial Benin, we investigate the effect of education on living standards, occupation and political participation. Since both school locations and student cohorts were selected with very little information, treatment and control groups are balanced on observables. We can therefore 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.

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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. However, this literature has thus far 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 evidence 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 families, as well as their contemporaries who did not get education (see Wantchekon 2012). We use information provided by school and church archives and face-to-face interviews with 289 informants and 325 counter-informants (informants intended for cross-validation of information) to identify students in the first two cohorts from colonial schools at four sites across the central-north region. Information about these first students was collected through interviews of either the students themselves (if alive) or their direct descendants. We also collected data on individuals who were born at the same time and in the same village but did not attend school, as well as contemporaries from nearby villages where no school was set up.

Favorable geographical conditions may have determined the colonialists' or Catholic missionaries' location choice for schools, as discussed by Nunn (2010) and Johnson (1967), possibly inducing 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 20 km of each other, that were equally accessible to settlers. Given this relatively short distance, there is hardly any variation in geographical features relevant to Catholic missionaries and colonial settlers, so there should be little difference between treatment and control villages (see Table A.1 in Online Appendix A).

It is also important to note that we only consider regions that had no prior exposure to European influence at the time the first schools were set up. 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 development, at least in the first generation exposed to education.

Our results reveal enormous positive treatment effects of education on a number of outcomes. The treated individuals from the first two cohorts have higher living standards, better social networks, and are considerably less likely to be farmers. 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 in villages with schools have substantially better outcomes than descendants in villages without schools.[1] We show qualitative evidence that these externalities run partly through higher aspirations of uneducated parents in villages with a school. Fourth, the strength of extended families is documented as nephews and nieces directly benefit from education of their uncles. They are almost as educated as the students' children, and are more educated than descendants without any educated members in their families.

[1]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.

We show that these within-family externalities may 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 and illustrates the sampling procedure and gives more details on the selection of school locations and students; Section 5 presents results from the first generation of students, and Section 5.3 discusses their sensitivity to selection on unobservables; Section 6 presents results from the second generation, with an emphasis on extended family and village-level externalities. In Section 7 we verify that our main results are not driven by different birth patterns among the educated and the uneducated, by non-random missing data, or by any one location. Section 8 concludes. Additional information is given in a number of files in the Online Appendix.

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. 2013). Moretti (2004), Lucas (1988), Romer (1989) and many others have shown that the social benefit of human capital is only partially reflected in the private returns 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, by tracking down the first students of colonial schools and their descendants, we build on recent literature on the colonial legacy in education (e.g. Cagé and Rueda 2013, Caicedo 2014, Cogneau and Moradi Forthcoming, Huillery 2009, Nunn 2009, Nunn 2010, Okoye and Pongou 2014, Wietzke 2015, Woodberry and Shah 2004). 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.[2]

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 and occupational choice in the African context.

There is also a large 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), studies 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 by relaxing the credit constraints of his family.

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). Through our interviews, we document qualitative evidence of aspirations; where uneducated parents in villages where the school was established emulate the educated and invest in their children's education. We observe such patterns much less frequently in villages where no school was set up, but which otherwise share a similar history.

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 that local spillovers are particularly strong in areas with less-educated workers. Looking at intergenerational transmission

[2]For a more theoretical treatment of human capital externalities see Murphy et al. (1991), Acemoglu et al. (2001), Marshall (1961), Bils and Klenow (2000), Hendricks (2002), Krueger and Lindahl (2001).

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 during 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 one of the main priorities of the Roman Catholic Church at the end of the 19th century was the evangelization of the "Slave Coast," a region stretching from the Volta River in current-day Ghana to the Niger River in Nigeria. 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.1 in Online Appendix A). 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 established 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).

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 the colonial authorities wanted to maximize fiscal revenues. The main obstacle to the penetration of the Catholic Church was the entrenched traditional animistic 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).[3]

Besides the cultural distance between French settlers and the local population, little 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.[4] But again, the cultural gap 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 Zagnanado (1895), Kandi (1911), Save (1913) and Natitingou (1922) the schools were opened immediately after the end of the colonial war in their respective regions. This was followed six to ten years later by the establishment of civilian colonial administrations, or "cercles." Thus, in each case, human capital investment preceded colonial institutions and the schools were established to train civil servants, such as translators, nurses, accountants, and security guards to

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

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

serve in the new colonial administration.[5]

4 Data Collection and Sampling Procedures

The data used in this paper originate from survey and archival research on the first four regional schools in Benin. These schools, founded by Catholic missionaries and colonial authorities, were located in Zagnanado (1895), Save (1911), Kandi (1913), and Natitingou (1922). This section provides further details about the selection of: (1) treated villages where the schools were established and control villages where the schools could have potentially been located, and (2) treated students who were the first to attend these schools and control children who could have potentially been chosen.

Our sampling of locations and students focuses on what could be considered valid historical counterfactuals to villages where the schools were set up and children who were chosen as first students. To assess the suitability of these counterfactuals, we show balance in pre-treatment variables between the treated school locations and control villages, and for treated students and untreated children living near schools and those living in the control villages away from the schools. Moreover, we believe that the selection process of both the school locations and the first students had an important haphazard component due to limited information from the standpoint of the colonial authorities and the missionaries.

4.1 Site Selection

The four sites considered in this study lie 100 km or more from the Atlantic Coast, which formed the southern border of the Kingdom of Dahomey. Prior to the establishment of the schools, these sites had no meaningful European institutions, commercial, religious, or political.[6] Exposure to European institutions prior to the establishment of schools would present two potential problems

[5]Note that Zagnanado was a catholic school that also trained religious teachers along with civil servants.

[6]Dupuis (1961) wrote: "Despite being the first settlement of the '*Societes des Missions Africaines*' the Kingdom of Dahomey was inaccessible for Europeans. Religious conversion in the Central and Northern part of the territory became possible only after the Kingdom was conquered by the French colonial troops" (p. 10). Even after the conquest, only the coastal part of territory was known, albeit imperfectly, by Europeans (d'Almeida Topor 1995, p. 20).

for this study. First, there is an issue of self-selection into schools. Coastal areas such as Porto Novo, Cotonou, and Agoue had significant interaction with European traders. In these areas, residents with exposure to French language and formal education would be more able to engage in trade with the European merchants. Thus, certain residents in these areas may have self-selected into education. The second issue involves separating the impact of previous European institutions and newly established schools on future development outcomes at the individual and regional level. In the coastal areas, for example, villages simultaneously experienced the growing presence of European institutions and the introduction of colonial schools, making it difficult to disentangle the effect of colonial institutions from that of education. For these two reasons, villages within 100km of the coast were excluded from this study.

Importantly, these sites contained no formal educational institutions, European or otherwise, prior to the construction of the colonial schools. Such an absence eliminates the potential for a self-selection problem in which residents with more information about the benefits of education would be more likely to enroll in schools.[7] Since prior generations had no access to education, the initial cohorts were the first generation in their communities to be educated. Thus, the two defining characteristics of the sites are no or very little European economic institutions and no prior formal European-style schools. The four sites selected contained the first regional schools in the hinterland of Benin (see Fig A.1).

4.2 Selection of Control Locations

If the control and treatment villages were different prior to the construction of the colonial schools, then the results could derive from these initial disparities as opposed to the impact of the schools. Since the treatment villages are already given, we identify villages that we believe were just as likely to be selected for school establishment to serve as a comparison group.

We do this in two ways. First, we exclude villages that lie within a 6–7km radius from the school location because they are near enough to the schools that children from these villages could have been selected to attend. The vast majority of the students from the first cohorts that we

[7] See pp. 17–18 in Dupuis (1961).

consider did not have access to boarding facilities and, therefore, had to commute to the schools daily by foot. Even students within the 7km radius would have faced a three to four hour commute given that villages were only connected through unpaved trails.[8] Therefore, villages beyond a 7km radius from the site are unlikely to have had local children attending the school. This assumption is verified in the data from the first generation: there is no student living with their parents having to walk more than 6km to school.[9]

We identify candidates for control villages as those located within a 7–20km band around the school. Villages further than 7km away would not have had students attending the schools. Villages over 20km away from the schools would be so far away that they would be located in a region with potentially different geographic or ethnic characteristics. We assume that any of the villages within a 7–20km band around the actual school site could also have been selected by the missionaries or colonial authority as a viable location for a school. We justify this assumption with an example from Zagnanado, discussed shortly below (see Section 4.2.1). Table A.1 in Online Appendix A shows that the control and treatment villages had very similar geographic, social, ethnic, and political characteristics before the establishment of the schools. Map A.8 of Zagnanado in Online Appendix A shows the school at the circle's center; the small circle represents a treatment area, corresponding to a 7km radius from the school, and the bands represent the control areas 7–20 km from the school.

Within the 7–20km band around the schools, we select one village at random as control village.[10] We use current maps from the four sites to illustrate the selection process of the control locations. The maps reflect the current distribution of villages, which is, to the best of our knowledge, identical to the distribution at the time when the schools were created. We checked colonial maps and population census from 1931 and found no evidence of the emergence of a new village or complete disappearance of a village after the establishment of the schools. There are 18 potential control

[8] According to d'Almeida Topor (1995), it usually took adults about 10 hours of walk to travel about 25km from their homes to visit the local markets. Thus, it should take at least three to four hours for 10- to 14-year-old students to walk 7km to school.

[9] There were 12 students (mostly from Zagnanado) whose parents were living more than 6km away from the school. However, school records and qualitative evidence show that those students from Zagnanado were staying in a dormitory while those from Natitingou were staying with foster families near the school (*Centenaire*, p. 17). The six children from Zagnanado were perhaps children of villagers from Baname and Cove converted by Schenkel and Steinmetz early in 1895 during their trip back from Pira through Zagnanado (*Centenaire*, pp. 10–14).

[10] In Save, the population size of the first control village selected at random was significantly lower than in the treatment village; we chose another one at random.

villages in Natitingou, 17 in Kandi, 10 in Save and 15 in Zagnanado. In the latter case, all the villages in our sample can be seen on the map of the area published by the missionaries in 1895 (see Maps A.2, A.4 and A.6 in Online Appendix A).

4.2.1 Illustrative Example: Zagnanado

One missionary school was built in Zagnanado in the Agonlin region – an ethnically homogeneous province of the Kingdom of Dahomey – in 1895 by Catholic missionaries from the *Societe des Missions Africaines* (SMA) in Lyon. The missionaries had increased access to the interior of Benin following the fall of the Kingdom of Dahomey in late 1894. Yet, as of 1895, the French had not yet instituted a formal colonial administration in the area.[11]

The pamphlet "*Centenaire de l'Arrivee des Missionnaires au Pays Agonlin*" based on diaries and reports of founders of the Catholic school of Zagnanado provides interesting details of the process leading to the creation of the school (see Map A.10 in Online Appendix A).

> At the start of 1895, two missionaries of the *Societe des Missions Africaines* (SMA) Priests Pierre Schenkel and Francois Steinmetz (who will become Bishop Steinmetz) traveled inside the Dahomey; a journey on foot lasting more than two months which leads them from Agoue to Pira, passing by Djaloukou and Savalou. They are the first two Europeans to head to the sources of the Zou.
>
> The two priests came down from Pira through Dassa and Abomey where they diverted slightly towards the East to Agonlin, which takes two days to reach on foot from Dassa. (...)
>
> In a neighboring village, so much sympathy was shown towards one of the priests who was ill which was in itself unbelievable; they want him to stop his journey for some time because of his poor health and the rough roads that lay ahead of him. *Faced with his refusal to stop, they cleared and weeded out two kilometers to ease his travel. It is as a result of this trip, and based on the report and instructions of the two priests that the mission of Zagnanado was founded* (*Centenaire*, pp. 10–12, emphasis added).

The opening of the school took place two months after the trip, when Father Schenkel returned to the region, this time with Michaud (not Steinmetz). They traveled from the coastal city of Porto Novo up the Oueme River and stopped at the small town of Sagon, at the center of the Agonlin-Zagnanado region. They decided to settle on the left bank of the river, about 5km away in a small town called Assiadji (*Centenaire*, p. 12). There is nothing in the diary that demonstrates

[11] See *Centenaire de l'Arrivee des Missionnaires au Pays Agonlin* (1895–1995), pp. 10–14.

a preference by the missionaries to go left as opposed to the right of the river. We interpret their choice to settle on the left rather than the right as essentially arbitrary. There is no evidence that there are any characteristics of the right side of the river that made it unfit for a school. Ten years later, in 1905, colonial missionaries did, in fact, build a school on the right side of the river at Ketou.

At that time, the village of Zagnanado and the surrounding hamlets of Doga, Houegbo, Don, and Agnangon had a population of nearly 2,000 residents. This cluster, located within 7km of the school, is considered to be in the treatment area. The band of 7–20km around the school, considered to be the control area, includes other villages such as Sagon, Houinhi, Kpedekpo, Wakon, and Agonve. We randomly selected Kpedekpo from this group of 15 potential control villages.

As can be seen in Table A.1 in Online Appendix A, Kpedekpo and Zagnanado are nearly identical on observables factors, such as distance from the port, ethnic composition, and political and institutional history. In fact, the only differences between Kpedekpo and Zagnanado are in regards to land fertility and mean elevation, with Kpedekpo having somewhat higher average land fertility than Zagnanado.

4.3 Individual-Level Data Collection and Survey

We have three groups of individuals in our research design: those who lived near the school and enrolled (TG1), the group proximate to the school that did not enroll (TG2), and the control group outside the radius of the school (C). The data on these individuals were collected in two phases by a team of researchers from the Institute for Empirical Research in Political Economy (IERPE) in Benin. Phase 1 consisted of identifying the first two cohorts of students from colonial schools (TG1), along with a sample of their unschooled contemporaries (TG2, C) at four sites: Kandi, Natitingou, Save, and Zagnanado. Phase 2 consisted of a social and demographic survey of informants with close ties to the subjects identified in Phase 1. The information used to identify the individuals in the three groups came from the school, colonial and family archives, as well as face-to-face interviews of local informants.

Below we provide further details on the data collection, survey logistics, and the measures taken to ensure the reliability and credibility of the data.

4.3.1 Sampling and Archival Sources

To identify the individuals in the three groups and their descendants, we used extensive archival sources. These sources were critical in identifying the names of the treated individuals and providing the lists of the first cohorts of students, as well as determining some of the relevant variables and adjudicating conflicting information provided by the informants. There are roughly three types of archival resources: school records, colonial administration records (e.g. ID cards), and family archives.

The IERPE research team was able to find student records (individuals in TG1) for two of the four schools: the Catholic school of Zagnanado, and the public school of Natitingou. The records for Save were incomplete, and no records were available for Kandi. In Kandi, the only school in the sample without any archive of student records, the IERPE team used mainly colonial administrative archives and face-to-face interviews of individuals (Lafia N'Gobi Gouda and Demon Komkom) who were born around 1916 and knew the students fairly well. The team was also able to locate family archives in Save and a monograph written by former Kandi students on the history of the school. Since Zagnanado had a Catholic school, its archives were preserved by the Church. Some additional information was available at the *Societe des Missions Africaines* in Lyon (France). The archives were created by school officials (a priest in Zagnanado and a French civil servant in the other locations) and were kept either at the school or at the home of the principal.[12] The school records have the names of the students, their age, their parents' names and professions, and an evaluation of their performance in school (see Documents I and II in the Online Appendix).[13]

Naturally, there were no school archives on unschooled subjects in treatment and control villages (TG2 and C groups, respectively). To identify individuals in these groups, we used a backward-sampling procedure. Enumerators were sent to the treatment and control sites where they systematically sampled from inhabitants of the village who are at least 40 years old. That is, enumerators

[12]The IERPE team in Zagnanado found the student records at the home of Mr Aihounton, a former principal at the school, who took it from the school when it was closed in 1975 by the military government.

[13]Documents III and IV in the Online Appendix show photographs of the first two cohorts of the Zagnanado school and its founders.

chose a random starting point from the sampling frame, proceeding to choose households at regular intervals (e.g. fifth, tenth, fifteenth, etc.), with the constraint that to be in the final sample, an individual had to be at least 40 years old, and their father or grandfather had to be close in age to students from the regional school.[14] The selected individuals were asked to identify their predecessors. If the predecessor was from the same age-cohort as those in the treatment group, then this individual became a subject. If the predecessor was not of the same age-cohort, we do not include data about this individual. We then used available colonial and family archives, as well as face-to-face interviews to obtain and corroborate information about these subjects. The main concern about potential bias from this type of sampling of untreated individuals stems from the relationship between social status and the probability of being sampled. There is a risk of over-sampling the wealthy because they have more descendants, or under-sampling them because the wealthy may migrate more often. However, as we discuss in detail in Section A1 in Online Appendix A, we believe these concerns are minor because of very low pre-treatment inequality in social status and low tendency to migrate, and because any sampling bias likely attenuates our results.

Colonial administration archives are kept at the office of the current local governments. About 6–10 years following the creation of the schools, formal colonial administrations had been established in the four sites studied. Later on in the post-colonial period, these colonial districts became sub-prefectures or communes, now the site of Benin's local governments. National ID cards were of particular interest in the archives. For instance, Document V in the Online Appendix is the ID card of Jean Chrysostome Adoko, who was in the first cohort of students at the Zagnanado Catholic school. His ID shows that he was born in 1888 and became a shopkeeper. According to colonial records, some of the treated subjects were politically active, especially in the post-WWII period. For instance, Document VI in the Online Appendix indicates that Donou Marc, part of the first

[14] As in Afrobarometer surveys, IERPE enumerators were given the following instructions: "Start your walk pattern from the start point that has been randomly chosen by your Field Supervisor. Team members must walk in the opposite directions to each other. If A walks towards the sun, B must walk away from the sun; C and D must walk at right angles to A and B. Walking in your designated direction away from the start point, select the 5th household for the first interview, counting houses on both the right and the left (and starting with those on the right if they are opposite each other). Once you leave your first interview, continue on in the same direction, this time selecting the 10th household, again counting houses on both the right and the left. If the settlement comes to an end and there are no more houses, turn around."

cohort enrolled in the Catholic school of Zagnanado, was president of the local traditional court and was cast by the colonial administrator as a deceptive and anti-French provocateur.

The IERPE research team was also able to glean information about subjects through photo archives provided by their families. For instance, we obtained pictures of some of the subjects' assets (houses, bicycles, and clothing), as well as private family correspondence that provided some indication of their social status and political views. For example, Documents VII and VIII in the Online Appendix show a subject from Save in colonial-style dress in his house. The availability and quality of the archives varied from one site to another. Zagnanado and Natitingou had more robust archives, whereas Save and Kandi's were comparatively lacking. To compensate for this, the interviews of the informants in Kandi and Save were much longer and involved more field researchers than in Zagnanado and Natitingou.

4.3.2 The Survey

In addition to the archives, field researchers collected data via questionnaires administered to local respondents. Respondents were either informants, the primary sources of information, or counter-informants, who we relied on for cross-validating the information provided by the informants. All respondents have close familial or personal ties to the original students or the students' unschooled contemporaries (with the exception of Pedro Boni from the second cohort in Natitingou; see Document IX in the Online Appendix).

Potential respondents included children, grandchildren, siblings, and neighbors, who were surveyed to ascertain their personal characteristics and ties to the subject. Depending on their availability and reliability, on average two of them were selected to be informants with another selected to be a counter-informant. For example, in Zagnanado, the original list of potential respondents for H. Litchegbe included his daughters Micheline and Emilienne, and his nephew Thomas. Only his two daughters were available for an interview; we picked Emilienne to serve as the informant and the oldest woman in the extended family, Sokponto, to serve as the counter-informant. When several informants were available, preference was given to the oldest (in terms of age) or the informant closest to the subject. Provided that the informants were lucid, the oldest informant would most likely have more reliable information since he or she had more interaction with the subject than their younger counterparts.

A separate questionnaire was also given to informants in order to assess the quality and the reliability of the information they provided. This questionnaire included questions about the nature and the timespan of their relationship with the subject. In each location, a number of counter-informants were also selected to corroborate the data provided by the informants. A set of key questions were taken from the informant questionnaire and posed to counter-informants in order to verify that the responses of these two groups matched.

Survey Logistics

The project had 33 field researchers, with about eight in each location working for six years from 2008 to 2014. There were two types of field researchers: local field researchers, typically primary or high school teachers who live permanently in the village; and research assistants (RAs) from the IERPE, typically masters students in geography, sociology, or demography. The RAs spent one to two weeks in the location every month. The reason for hiring both local residents and RAs was to combine familiarity with the location with empirical research experience. While RAs provided technical expertise in the implementation of the research protocol, permanent local field researchers helped locate the correct set of informants by virtue of their connections and the trust they had cultivated with local residents. This allowed for more flexible interview scheduling. It also facilitated the collection of evidence about the assets of the first cohort of students and their contemporaries, particularly with regard to housing, profession, and educational attainment, as found in archival data such as family photos, ID cards, and personal letters.

The typical interview took place in the evening and lasted an average of two hours. In each location, interviews were given to 51 to 97 informants and 52 to 116 counter-informants (see Table B.1 in Online Appendix B). There was one informant and between one and three counter-informants by subject, depending on availability. Below we describe the profile of informants and provide more details on the content of the interviews.

Informant Questionnaire

Data collection directly from the subject was limited mainly because of subject's death, and, in some cases, family migration. Therefore, family members, neighbors, family friends, and village elders were essential in the collection of the data. Family members include children and dependents, siblings and cousins, nieces and nephews, and grandchildren. There were a total of 289 informants and 325 counter-informants. Figures B.1 and B.2 in Online Appendix B describe the age distribution of informants and counter-informants, respectively.

As mentioned earlier, the informant survey included personal information and questions on personal ties with the respondent (see Questionnaire 1 in the Online Appendix). Personal information includes gender, education level, occupation, the family link, and whether the informant had ever directly met and/or talked with the subject. The counter-informant survey essentially included the same types of questions as the informant survey. In addition, counter-informants were asked a sample of questions about the subject originally posed to the informant.

Each informant received a questionnaire about the subjects (see Questionnaire 2 in the Online Appendix). Informants were asked for basic information pertaining to the subjects, including family name, place and date of birth, and date and cause of death. They were then asked to list the subject's classmates, level of education, profession, and living standards. More specifically, we posed questions about the highest level of educational achievement (Q11); profession or occupational status (Q13); type and building material of the house in which the subject resided (Q16); the subject's living standards such as access to electricity, water, television, radio, etc. (Q18); the subject's method of transportation (Q20); style of dress (Q21); business ownership (Q22); and membership in associations (Q30). Furthermore, questions were asked about the demographic and social characteristics of the subject, including the number and names of their children (Q25) and the number and names of their siblings. This was followed by questions about the subject's social milieu, including their frequency of travel (Q39), number and list of languages spoken (Q41), number of European friends (Q44), and number of friends of a different ethnicity (Q45). Finally, there were questions on the subject's political participation, including whether the subject was active in a political party (Q47), an electoral candidate (Q49), or part of a political campaign

(Q54).

4.3.3 Data Credibility

Credibility Criteria

The main challenge of the data collection process was to measure the credibility and reliability of the information provided by the informant. The criteria used to establish credibility depended on the extent of past interactions between the respondent and the subject, specifically regarding the nature of the relationship and the time span of the interaction. For instance, family members were preferred over informants from outside of the family such as neighbors or friends.

Since most of the informants were family members, there was also a need to define credibility in terms of family distance and length of interaction with the subject. When choosing between two informants with family ties to the subject, preference was given to the older respondent who had plausibly known the subject for a longer period of time. Additionally, children or siblings who had lived with the subject were considered more reliable than children who had not lived with the subject – or who had, but for a shorter time.

Assessing Reliability

Two strategies were employed to assure the reliability of information provided by the informants. The first was to demand supplementary data from informants; they were asked to furnish additional proof for every piece of information provided. The preferred form of proof was physical data such as photos of the subject or documents pertaining to the subject. In the absence of this proof, we prioritized respondents who had resided extensively with the subject. The second strategy involved the use of counter-informants, who were selected to answer the same questions posed to the original informants with supporting evidence when available. The objective of this second round was to verify that there was no mismatch between the data provided by the informants and counter-informants. Information provided by informants was considered reliable when counter-informants confirmed it. When there was a mismatch, the information provided by the closest informant was selected (see Online Appendix B for additional details).

To summarize, data credibility was classified as follows. Archival evidence was given the highest priority, followed by tangible evidence, such as photos provided by informants or the subject's actual dwelling. In the absence of archival evidence, preference was given to information that elicited a match between informants and counter-informants. Table B.3 in Online Appendix B indicates that the best matched variables are housing (78.65%), means of transportation (63.77%) dressing style (82.68%) and to a lesser extent, profession (49.81%).[15] In case of a mismatch, information from the informant who had resided longer with the subject was weighted more heavily. Descriptive statistics of the credibility measures of the variable are available in Section B.4 in Online Appendix B.

5 First-Generation Effects

We now proceed to examine the first-generation effects of schooling.

5.1 Summary Statistics

Table Isummarizes 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 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.[16]

[Table I about here.]

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 became farmers, while farming is clearly the dominant occupation

[15] Birth dates are not well matched (30.54%), which is not surprising given the absence of birth certificates and maternity hospitals prior to the establishment of the colonial administration.

[16] 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.

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

We also include a composite measure of living standards based on factor analysis using several indicators such as those listed in Table I. 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 clearly have higher scores than the uneducated. Table A.4 in Online Appendix A gives more details about how to interpret different values on the living standards scale and its construction.

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 composite scale was coded by applying factor analysis, using information about membership in social organizations (religious, business, sports), languages spoken (national, foreign), friends among whites and other local ethnic groups, and participation in local politics. Table A.5 in Online Appendix A gives further details about how to interpret different values on the social networks scale.

5.2 First-Generation Effects: Living Standards, Social Networks, and Political Participation

We now more formally 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 at a time when there was almost no information about the value of education. As a result, strong self-selection into schooling was unlikely. Given this, the estimated effects of schooling at the individual level in the first generation can potentially be interpreted as causal effects. If anything, there is anecdotal evidence of negative selection. 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 there was no systematic difference between treatment and control villages. In Sections 5.3 and 7 below we show that our estimates are insensitive to a number of threats to causal interpretation.

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

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

Our outcome variables are education, living standards and social networks, 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 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 effect of individual- and village-level treatment, respectively.

Table II 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 II, 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 I 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 about 10% enrolled for secondary education. Accordingly, the individual-level coefficient in column 2 of Table II is very close to 1, while the coefficient in column 3 is about 0.1.

[Table II about here.]

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 III. This result is very strong and intuitive – we can deduce that the students put their knowledge of the French language, their literacy and math skills, and their 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.[17] For example, students were as much as 65 percent less likely to be farmers compared to those who were not chosen to go to school, or those who lived in a village without a school.[18] In contrast, the coefficients on the village-level treatment variable are all very close to zero and statistically insignificant. This indicates that the living standards of those living in villages with schools, but who did not receive education, were no different from the living standards of the uneducated living in villages with no school.

[Table III about here.]

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 IV, and constitute evidence of first-generation, within-village externalities from the introduction of a school. Furthermore, we use the coordinates of all the settlements within our four sites with schools to calculate the distance between each individual's home (to the extent we could identify and verify its location during the relevant time after treatment) and the location of the school.[19] We find that those closer to a school had larger social networks, as measured by our factor

[17]Many of the students from the first generation were hired as civil servants in the colonial administration. 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 in school was useful in other professions.

[18]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 of 61 percentage points. Assuming a linear effect of additional schooling, each year of education decreased the probability of being a farmer by 15 percentage points, or around one fifth of the likelihood of being a farmer in Treatment Group 2.

[19]What we refer to as a "village" is in fact a group of interconnected smaller settlements – groups of homes. For example, in Zagnanado, Treatment Group 1 and Treatment Group 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.

scale, suggesting that some of the externality may run through the neighbors.[20] 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.

[Table IV about here.]

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 the 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 V shows that students were significantly more likely to campaign for political parties (column 1), or even become full-fledged members (column 2). While very few people stood for election to political office in 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 (column 3).[21] These findings show a clear effect of education on political participation. To the best of our knowledge, this is the first quasi-experimental evidence of a positive effect of education on political participation in developing countries.[22]

[Table V about here.]

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

[20] Some of the externality may run through the contact with the colonialists. The results from Table IV are inconclusive, given that the difference between the share of individuals in Treatment Group 2 who spoke French and had white friends is quite similar. We thank an anonymous reviewer for noting this.

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

[22] See Berinsky and Lenz (2011), Campante and Chor (2012), Dee (2004), Glaeser et al. (2007), and Kam and Palmer (2008) or related evidence.

to the lives of the first generation of students.[23] 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 can be interpreted as causal. In the next section, we provide evidence that our results are insensitive to a large degree of selection of children based on unobservables.

5.3 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 TG1 and TG2 based on the number of siblings they had, 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 or better-fed, kids have higher odds of being selected for treatment and are more likely to have higher living standards.[24]

[Table VI about here.]

Table VI shows the results of this exercise. We focus on three binary outcome variables – whether the individual is a farmer (column 1) and whether their living standards and social networks are above or below the mean (columns 2 and 3, respectively). The first row in Table VI shows that

[23] Note that in Tables II, III and IV 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.

[24] 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).

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 biased 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. The results for social networks in column 3 are stark. While the results for living standards are less pronounced, the selection on unobservables would still have to be more than twice as high. Overall, Table VI suggests that selection on unobservables would have to be very high in order to eliminate the treatment effects we find. In Section 7, we show additional evidence that our first-generation effects are quite robust.

6 Second-Generation Effects

6.1 Education, Living standards, and Social Networks

The second-generation effects of education are of paramount importance for human development and social mobility. 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 show in several ways that descendants of uneducated people in villages with schools catch up with the descendants of the educated – particularly in terms of primary education outcomes, living standards, and size of social networks.

[Table VII about here.]

Table VII shows the summary statistics for the descendants of the first-generation individuals: they exhibit better outcomes across the board, suggesting that returns to education are strongly transferred across 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 a school.

We begin to examine the differences shown in Table VII by estimating regressions of the following form:

$$\text{Outcome}_{ij} = \alpha + \beta_1 I_{ij} + \beta_2 V_j + \beta_{\mathbf{3}} \mathbf{X_{ij}} + \mu_k + \epsilon_{ij}. \tag{2}$$

As before, our outcome variables are education, living standards and social networks, where i identifies the individual child, and j and k respectively identify the village and commune in which they reside. The binary variables I and V indicate individual-level and village-level treatment of the first-generation individuals, respectively, 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, $\mathbf{X}_{ij}$, which contains the gender and number of siblings of each child. Furthermore, because descendants of different people from the first generation were born over more than half a century, $\mathbf{X}_{ij}$ also contains decade-of-birth fixed effects. Finally, we include dummy variables for the commune in which the child resides, μ_k.

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, we set aside these differences for the moment, as we will discuss them in depth in Section 6.2.

[Table VIII about here.]

Table VIII 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.

Also striking is the finding from column 2 that the coefficient on village-level treatment is greater in magnitude than the coefficient on the individual-level treatment. This means that simply growing up in village with a school has a big positive effect on descendants' primary education, while the additional positive effect of having an educated parent or uncle is somewhat smaller. The difference between the effect sizes is statistically significant, as evidenced by the test statistics for the equality of the two coefficients. 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 and statistically indistinguishable from each other. 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 IX about here.]

A similar pattern emerges for living standards among the second-generation descendants, as shown in Table IX. We see that simply having been raised in a village with a school has important positive effects on measures of living standards. For example, results from column 1 of Table IX suggests that being born in a village with a school reduces the descendants' probability of being a farmer by about 30 percentage points, and having an educated father or uncle reduces the likelihood of being a farmer only by an additional six percentage points. Hence, while being a descendant of an educated person clearly puts one ahead, descendants of the uneducated in villages with schools have nearly caught up over the course of only one generation. The individual- and village-level effects are of comparable magnitude for most other measures of living standards, such as having running water in the house (column 2), having a television or a telephone (columns 3 and 4), as well as the composite measure of living standards.[25]

[Table X about here.]

The effect of village-level treatment on descendants' social networks is also large, statistically significant, and consistent across measures, as shown in Table X. When looking at knowledge of French language, we again see that just growing up in a village with a school increases the likelihood that the descendent speaks French by about 33 percentage points, and the additional effect of being a descendent of an educated person is a further 16 percentage points. The village-level effect is statistically significantly larger than the individual-level treatment (see the last two rows of the table). 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 the results across the first and second generation. In the first, only those who were 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 ties than those in villages where no schools were set up. In contrast, in the second generation we see that having grown up in a village with a school positively affects all measures of 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.[26] In Section 6.3 below, we examine the evidence for aspiration as one potential mechanism behind these second-generation village-level externalities.

[25] There is some evidence that village-level effects can be stronger than individual-level effects (see the last two rows of the table), although the differences between the two effects are not as pronounced as for primary education outcomes (seen in column 2 of Table VIII).

[26] We have the following categories for occupation: civil servants, private sector employees, artisans, farmers and traders. The majority of the treated (37%) were civil servants, and 17% worked in the private sector. Interestingly, 16% of the students who became civil servants also possessed a shop, the most common form of entrepreneurship among the native Africans early in the 20th century. In addition, siblings of the treated who owned a shop were 20% less likely to be farmers, and their descendants tended be more educated. Specifically, 40% of descendants of the educated shop owners had some secondary school education compared to only 20% for those without shops. Of course, we need to take these statistics with caution, as the correlation between treatment and shop ownership might be due to access to credit, motivation or networking skills, not entrepreneurial skills per se.

6.2 Family Tax: Do Nieces and Nephews Perform as Well as Daughters and Sons?

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 XI where we compare, to all other descendants, the average outcomes of the original students' children and their nieces and nephews.

[Table XI about here.]

Some readers may find it surprising that children of the students do not seem to be performing any better than nieces and nephews, as indicated by the F-test in the last row of Table XI. 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 former students tend to have more primary education than nieces and nephews, but this difference is statistically significant only at the 10% 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 XI.[27]

We see that across all education outcomes having just one educated person in the extended family makes a large difference for the outcomes of the nieces and nephews. These descendants have better education at all levels than descendants (either children on nieces and nephews) in families where no progenitor was educated. These effects are statistically significant and substantial

[27] We confirm that children and nieces and nephews in Treatment Group 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.

– such descendents 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.

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 children 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 thin. In other words, the difference between children and nieces/nephews may be increasing as the size of the extended family size increases.[28] Results presented in Figure I are consistent with this mechanism.

[Figure I about here.]

In Figure I we see that the difference in education outcomes between children and nieces/nephews becomes negative and statistically significant if the logged extended family size exceeds about three (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.

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).[29] 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

[28] 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.

[29] Our findings are also related to the literature on sibling rivalry in developing countries. In Burkina Faso, Akresh et al. (2012) 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.

promising children to get high levels of education, especially university education. However, there is also a negative side. As shown in Table XII, 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 than they could raise independently. Educated parents, 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.

These results also 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 2006, p. 2).[30] Our results document the way in which the kin system can both be a "vehicle of progress" or "instrument of stagnation" (Hoff and Sen 2006). 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 as they are less observable to family members.[31]

6.3 Aspirations: A Determinant of Village-Level Externalities

There are two competing explanations for the village-level externalities from school that we observe in the second generation. The first is a demand-side mechanism by which non-educated residents invest in education after witnessing the success of educated individuals living nearby. Through this mechanism of aspiration, untreated parents imitate their educated neighbors and want their children to do just as well. An alternative channel is a supply-side mechanism, whereby education leads to the opening of additional schools and other state institutions as vehicles of human capital

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

[31] See Baland et al. (2011), Dupas and Robinson (2013), and Jakiela and Ozier (2012) for evidence for this type of behavior.

externalities. Thus, increased school enrollment could simply be due to the new availability of schools and other government institutions.

The supply-side mechanism is undoubtedly an important component behind the observed village-level externalities. For example, Table A.3 in Online Appendix A shows 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. However, we also believe that aspirations play an important role as well. To attempt to disentangle the aspiration channel from the supply-side mechanism, we collected additional qualitative evidence in Zagnanado and Kandi. First, we took four pairs of villages in those two locations; three pairs in Zagnanado: (Vedji, Veme), (Ayangon, Bame), and (Ahossouhoue, Dezonnoude); and one pair in Kandi: (Angaradibou, Bah Para). The matched pairs of villages are roughly equidistant from the local school and the center of local government and about one to two miles apart from each other.[32] An important difference, however, is that one village in each matched pair had at least one child from the first cohort of students, whereas the other village in the matched pair had none. In Zagnanado for example, Vedji, Ayangon and Dezonnoude had one student (named Aniwanou, Hessou and Houedete, respectively), whereas the villages of Veme, Bame, and Ahossouhoue had none. In Kandi, Angaradibou had two students (named Issiakou and Toungou) and Bah Para had none.

If the institutional supply-side mechanism is the only channel at work, then we should see little difference between the paired villages, because the presence of a school in the area would be likely to encourage similar patterns of enrollment in both locations. However, we find that the villages with the first-generation students exhibited notably higher second-generation primary school enrollment than matched villages where no one in the first generation interacted with the educated. In our Zagnanado matched pairs, there were 33 uneducated parents from Vedji who enrolled at least one of their children in the local school, whereas in Veme we found no parents who enrolled their children. In Ayangon, there were 25 parents who enrolled their children versus zero in Bame. In Dezonnoude we found 12 second-generation students while Ahossouhoue had only one. In our matched pair from

[32]None of the paired villages had their own schools, since at that time there was only one regional school for the entire area.

Kandi, the patterns are quite similar. Over one generation, Angaradibou had about 30 families with at least one child enrolled while Bah Para had four.

In addition to examining the primary school enrolment rates across matched pairs of villages, we interviewed 43 descendants of the uneducated in the first generation about the behavior of their parents. A consistent storyline was direct parental involvement in their children's education, coming from the observation of the behavior of educated parents toward their children. For example, nearly all untreated parents monitored their children's completion of school assignments on a daily basis. Some even hired more educated individuals as tutors, and established networks so that their children could only interact with children of educated parents. By keeping close contact with individuals who exemplified the success they desired for their children, they had the opportunity to learn ways to invest in the education of those children.[33]

7 Robustness Checks

7.1 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 6, yet we must be careful when 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 XII.

[Table XII about here.]

[33]In one interview, Thomas told us that his father, Houedete, an illiterate subsistence farmer from Dezonnoude, insisted he make friends only with children who were attending school and stay away from the uneducated children. Thomas had to read his lecture notes to his father every night, and Houedete regularly visited the teacher to check on his progress. In another interview, Mohammed from Kandi told us that his mother would get him up at 5AM everyday so that he could be in school and study on his own for at least an hour before the start of classes at 8AM. Many educated parents showed this kind of dedication.

How might 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 a parent's or uncle's treatment status (i.e. always-takers, or always born) and there are children who were born only because their parent or 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 the 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 as 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.[34] Table XIII shows the results based on this subpopulation, for education, the living standards scale, and the social networks scale. The results are substantively very similar to those shown above.

[Table XIII about here.]

Another approach is to calculate the *bounds* on the treatment effect, according to Lee (2009), which we present in Table XIV. The key assumption again is that monotonicity holds. In order to

[34]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.

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 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. Since we cannot identify exactly who these compliers are, or just how many of them there are, we can only construct the best- and 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 the 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. 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, giving the *lower bound.*

[Table XIV about here.]

The calculated best- and worst-case bounds are presented in Table XIV.[35] Individual-level effects are positive and both the lower bound and the upper bound of the ATE are statistically significant. This is true for all outcomes – education, living standards and social networks. 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 XII). 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 quite extreme, and most likely

[35]Note that the results in Table XIV 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 VIII, column 7 of Table IX and column 4 of Table X. We exclude nieces and nephews because we do not have precise information as to which nuclear family they belong to (i.e. how many brothers and sisters they have), which is necessary for the computations.

a positive effect remains.

7.2 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, as we discussed above and in Online Appendix A, we believe this is not a serious threat.

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, i.e. 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 Treatment Group 2 and the control group (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 the treatment (control) group will downweight the effects of education shown above in proportion to the share of missing values in each treatment group.

Table XV shows the first-generation results of this exercise.[36] 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.

[Table XV about here.]

[Table XVI about here.]

Table XVI 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 individuals with missing data on education in the control group achieved the university education, whereas all individuals with missing data in the treatment group had no formal education at all. 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 II examines the evidence for such possibilities. It plots the p-value 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 each of the three variables of interest is not significant at the conventional levels, indicated by the vertical dashed lines.

[Figure II about here.]

We perform one more check of the evidence for recall bias. We have shown in Section 6.2 that direct offspring do not have higher educational attainment than extended family descendants.

[36]We exclude the factor scales for living standards and social 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.

Even though Figure II 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 children. This would bias our results towards zero when comparing children with 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.[37] In this subsample, the recall bias towards zero – if it exists – should be lower. Table XVII reruns models from Table XI on the subsample of men. Our results are unchanged, further suggesting that recall bias is not an issue.

[Table XVII about here.]

7.3 Re-estimation with different subsets of communes

Since we are dealing with a natural experiment in which treatment villages are not identical, and the treatment and control village are not perfectly comparable, we re-estimate our results when each of the communes is excluded one at a time. These results are presented in Table XVIII and Table XIX below, for the first and second generation, respectively.

[Table XVIII about here.]

[Table XIX about here.]

We see that the individual-level treatment results are quite consistent across the specifications, when we exclude a commune at a time, in both the first and second generation. In particular, all the individual-level effects are statistically significant and are generally of similar magnitudes. The fact that the effect drops when we exclude Save is indicative of the power of colonial investment in enhancing human capital externalities. Indeed, the school was set up at the time of the construction of the railway from the main port city of Cotonou to Save, which later became the main transport hub of the country (Manning 1982, p.171). As a result, many educated Save natives set up trucking companies and commercial farms, providing employment opportunities for non-educated parents

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

and neighbors as well as their descendants. The village-level treatment estimates are also quite consistent in the second generation (Table XIX), but they do vary when we exclude different communes in the first generation (Table XVIII). The first-generation differences are at least partly due to much smaller sample sizes, as compared to the second generation. However, this also contributes suggestive evidence that the communes were more different among each other further in the past than they are now. The first generation heterogeneity might be the reflection of a variation in the nature and the timing of colonial rule across the country. In Natitingou, the school was set up at a time of violent repression of the peasant uprising against forced labor in the region, which led to a complete militarization of the local government (Gratz 2000, Garcia 1971). While students in Natitingou were living under military government, those in Save grew up at a time of relative economic prosperity due to the newly completed railroad.

8 Conclusion

We estimate the economic and social effects of education using data from the first elementary schools established in areas of colonial Benin with no prior European influence. 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 to a degree 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. One important contribution of this paper 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 those in areas where schools came first. Using this strategy we find that, even in the absence of prior European institutions, human capital has a large impact on economic development in colonial Dahomey (Benin).

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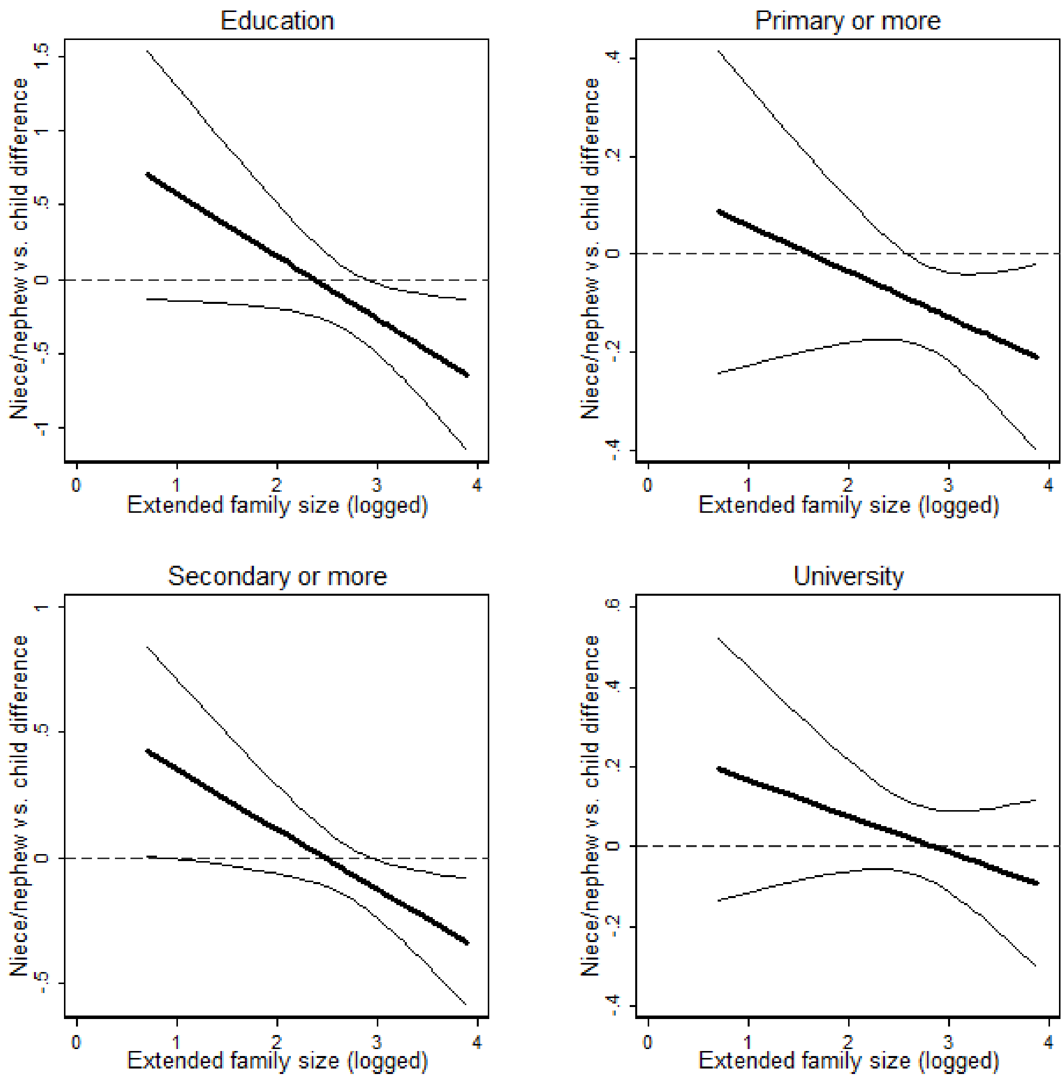
Figure I: Education and family tax in extended families



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.

Figure II: Evidence for Recall Bias

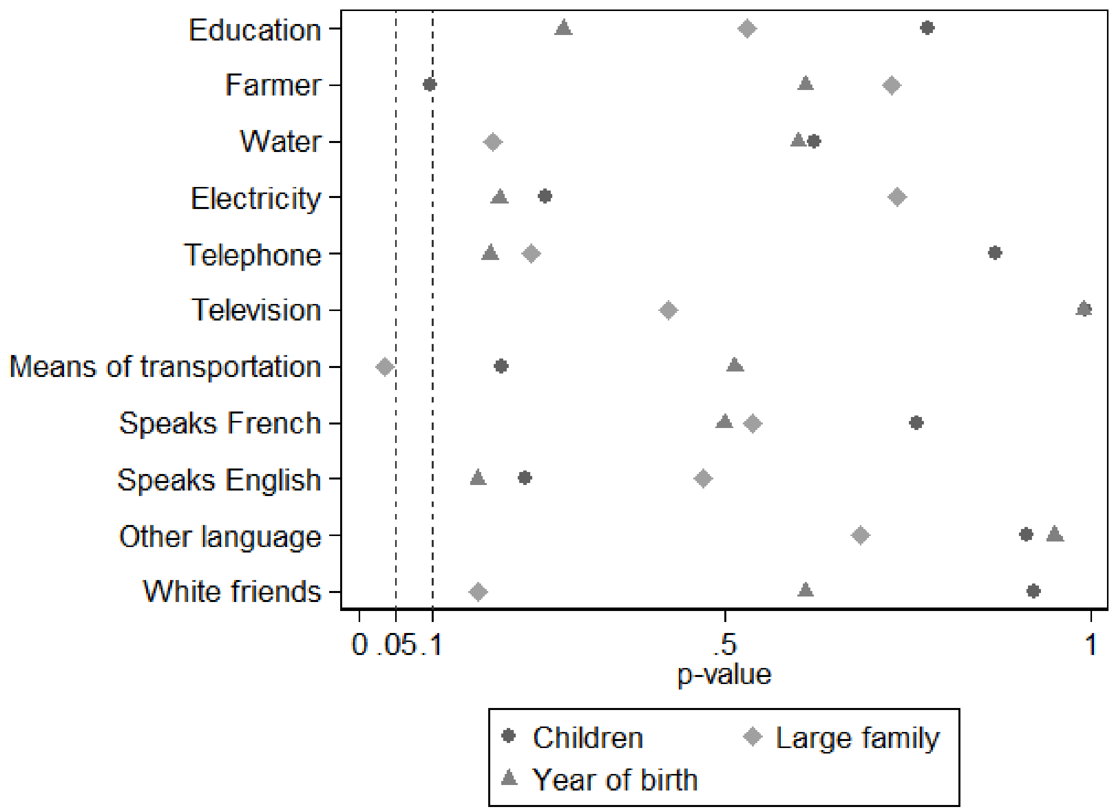


Table I: Summary statistics for the first generation

	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	0
	(0.299)	-	-
	[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. The entry of "-" for secondary school or more in columns 2 and 3 implies that there are no individuals with these education levels in the relevant samples.

Table II: 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	0.000273	0.000273	-
	(0.00112)	(0.00112)	-
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 (Cameron et al. 2008), where the full block of observations from a commune are randomly subsampled. The coefficient and the standard error on the village-level treatment in the third column are missing because there are no individuals with secondary or higher education in villages without a school, and therefore, the outcome variable is perfectly collinear with village-level treatment.

Table III: 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.4 in Online Appendix A. Standard errors were calculated using blocked bootstrapping by commune.

Table IV: First-generation social networks effects

	(1) French language	(2) White friends	(3) Social networks scale	(4) Social networks 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. The social networks scale is a factor score comprising a number of variables. The last column includes only individuals in the Treatment Groups 1 and 2. Standard errors were calculated using blocked bootstrapping by commune.

Table V: 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.

Table VI: 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.

Table VII: Summary statistics for the second generation

	Treated parents	Untreated parents in village w/ school	Untreated parents in village w/o school
Primary or more	0.669	0.520	0.274
	(0.471)	(0.500)	(0.446)
	[761]	[1004]	[702]
Secondary or more	0.375	0.222	0.115
	(0.484)	(0.416)	(0.320)
	[761]	[1004]	[702]
University	0.104	0.050	0.006
	(0.305)	(0.218)	(0.075)
	[761]	[1004]	[702]
Farmer	0.079	0.166	0.386
	(0.270)	(0.372)	(0.487)
	[745]	[945]	[643]
Water	0.536	0.452	0.385
	(0.499)	(0.498)	(0.487)
	[771]	[1020]	[711]
Electricity	0.636	0.504	0.089
	(0.482)	(0.500)	(0.284)
	[771]	[1020]	[711]
Television	0.536	0.362	0.075
	(0.499)	(0.481)	(0.263)
	[771]	[1020]	[711]
Telephone	0.480	0.281	0.079
	(0.500)	(0.450)	(0.270)
	[771]	[1020]	[711]
Means of transportation	0.369	0.275	0.263
	(0.483)	(0.447)	(0.441)
	[742]	[999]	[706]
Living standards scale	0.400	-0.007	-0.541
	(1.021)	(0.940)	(0.652)
	[674]	[846]	[605]
Speaks French	0.655	0.494	0.248
	(0.476)	(0.500)	(0.432)
	[771]	[1021]	[711]
Speaks English	0.058	0.014	0.007
	(0.235)	(0.116)	(0.084)
	[771]	[1021]	[711]
Social networks scale	0.286	-0.066	-0.350
	(1.069)	(0.959)	(0.855)
	[718]	[948]	[705]

Note: Standard deviations in parentheses. Means of transportation includes bicycle, motorcycle or car. Number of observations for each variable is shown in brackets.

Table VIII: 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
L = D F-stat	2.104	9.335	0.000067	0.079
L = D p-value	0.148	0.002	0.993	0.779

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

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 commune and decade dummies. The last two rows show the F-statistic and the associated p-value from a hypothesis test that the coefficients on individual- and village-level treatment are equal.

Table IX: 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
L = D F-stat	16.169	0.990	13.039	2.829	0.053	4.283	1.789
L = D p-value	0.000	0.321	0.000	0.094	0.818	0.039	0.182

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 commune and decade dummies. Vehicle can include any means of transportation such as bicycle, motorcycle or car. The last two rows show the F-statistic and the associated p-value from a hypothesis test that the coefficients on individual- and village-level treatment are equal.

Table X: 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
L = D F-stat	5.007	4.623	0.129	0.001
L = D p-value	0.026	0.032	0.720	0.979

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

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 commune and decade dummies. The last two rows show the F-statistic and the associated p-value from a hypothesis test that the coefficients on individual- and village-level treatment are equal.

Table XI: 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).

Table XII: 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.

Table XIII: 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.

Table XIV: 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)***
Social 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.

Table XV: Robustness to non-ignorable missingness – first generation

	(1) Education	(2) Farmer	(3) Water	(4) Electricity	(5) Means of transportation	(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)	(0.128)	(0.041)	(0.012)	(0.056)	(0.033)	(0.012)	(0.058)
Village-level treatment	0.050	0.004	0.054	0.018*	0.021	0.072***	0.006	0.096***
	(0.044)	(0.190)	(0.048)	(0.010)	(0.052)	(0.024)	(0.005)	(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 were calculated using blocked bootstrapping by commune.

Table XVI: 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.

Table XVII: 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.

Table XVIII: First generation results re-estimated when each of four communes is excluded

	Individual-level treatment		Village-level treatment		Observations
Living standards scale	Coeff.	SE	Coeff.	SE	
All communes	0.872***	(0.171)	-0.004	(0.164)	379
Excl. Zagnando	0.971***	(0.175)	-0.083	(0.164)	304
Excl. Kandi	0.721***	(0.154)	0.193***	(0.052)	286
Excl. Natitingo	0.938***	(0.204)	-0.111	(0.195)	271
Excl. Save	0.830***	(0.222)	-0.003	(0.185)	276
Social networks scale					
All communes	2.010***	(0.217)	0.100***	(0.038)	252
Excl. Zagnando	2.040***	(0.236)	0.123***	(0.014)	227
Excl. Kandi	2.044***	(0.295)	0.089*	(0.050)	195
Excl. Natitingo	2.194***	(0.217)	0.117**	(0.057)	159
Excl. Save	1.726***	(0.101)	0.072	(0.052)	175

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

Note: Living standards (social networks) scale is the dependant variable in the first (second) five rows. Column headers indicate the regressor. Standard errors were calculated using blocked bootstrapping, and reported next to the coefficient.

Table XIX: Second generation results re-estimated when each of four communes is excluded

	Individual-level treatment		Village-level treatment		Observations
Living standards scale	Coeff.	SE	Coeff.	SE	
All communes	0.408***	(0.092)	0.538***	(0.065)	2087
Excl. Zagnando	0.485***	(0.093)	0.514***	(0.065)	1957
Excl. Kandi	0.464***	(0.120)	0.396***	(0.076)	1357
Excl. Natitingo	0.385***	(0.097)	0.631***	(0.075)	1611
Excl. Save	0.262**	(0.117)	0.624***	(0.083)	1336
Social networks scale					
All communes	0.429***	(0.084)	0.454***	(0.072)	2331
Excl. Zagnando	0.490***	(0.085)	0.454***	(0.074)	2181
Excl. Kandi	0.433***	(0.115)	0.406***	(0.088)	1566
Excl. Natitingo	0.485***	(0.093)	0.368***	(0.088)	1767
Excl. Save	0.251**	(0.098)	0.598***	(0.083)	1479

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

Note: Living standards (social networks) scale is the dependant variable in the first (second) five rows. Column headers indicate the regressor. Standard errors are clustered by extended family, and reported next to the coefficient. All regressions control for gender, number of siblings, and include commune and decade dummies.