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Analysis

Unintended impacts from forest certification: Evidence from indigenous Aka households in Congo

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A B S T R A C T

Does Forest Stewardship Council (FSC) certification of “responsible” commercial forestry change nutrition, health and wealth for indigenous peoples, like the Aka of the Congo Basin? Using hand-collected data from the boundary of a certified and an uncertified forest in the Republic of Congo five years after certification, I compare nutrition, health, and wealth using questions that are locally salient and survey timing designed to reach semi-nomadic hunter-gatherers. Though I only observe outcomes after certification, using a spatial regression discontinuity design I find suggestive evidence that activities to satisfy forest certification may cause increased food insecurity and illness frequency for Aka households. I find no evidence of increased material wealth; instead, the poorest 15th percentile is poorer for Aka households. Non-Aka households are unaffected. Activities to satisfy FSC include a road connection, likely requested by non-Aka households, which in combination with hunting restrictions may decrease food security for Aka hunter-gatherers.

1. Introduction

The Congo Basin is home to the second largest tropical forest in the world and an estimated 30–60 million forest-dependent people (Chao, 2012), of whom about one million may be indigenous hunter-gatherers (Olivero et al., 2016).[1] For Aka hunter-gatherers of northern Congo, well-being depends on daily interactions with the forest.[2] Yaka commonly believe that “Komba (God) created the forest for Yaka people to share” (Lewis, 2000: 3).[3] Over the last few decades, Yaka people have increasingly shared the forest with commercial timber firms as seen in Fig. 1.

Tensions between commercial timber extraction and local forest users' rights catalyzed the creation of the Forest Stewardship Council (FSC). FSC is a non-governmental organization that defines an international standard for “responsible” forest management. At an early meeting in 1992, stakeholders insisted that changes to the standard's Principles and Criteria “should include a strong role for indigenous peoples and for local forest communities” (Dingwerth, 2008: 56). A broad principle to protect forest-dependent and indigenous peoples' use rights resulted (FSC, 1996).

Are indigenous peoples living within a commercial forest better off if the forest certifies FSC? So far, we know that FSC's commercial forestry standard fails to change deforestation (Blackman et al., 2018), forest degradation (Doremus, 2015), and forestry practices (Nebel et al., 2005). We might expect a similar null result for how FSC changes indigenous peoples' use rights and economic well-being.

However, a credible assessment of how FSC certification affects indigenous peoples' well-being faces three empirical challenges. First, consistent assessment of indigenous peoples' well-being is difficult. Hunter-gatherers move seasonally within the forest and prefer areas far from roads, making them costly to access (Olivero et al., 2016). In most cases, we lack basic cross-sectional data. Second, specificity of

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[1] Estimates of the number of forest-dependent and indigenous peoples vary greatly because they are not consistently sampled in large-scale, public datasets.

[2] Indigenous peoples are recognized as those who have ancestral claims to natural resources, are culturally distinct from majority ethnic groups, and have been historically marginalized from policy processes governing their use (Colchester, 1994). Aka in Northern Congo call themselves “first people” (Lewis, 2000) and fit within Colchester's definition of indigenous peoples. They share the forest with Kaka and Bondongo, ethnic groups that spend more time in the village but would satisfy several of the criteria of forest-dependent people (Newton et al., 2016).

[3] The plural of Aka is Yaka or BaAka.

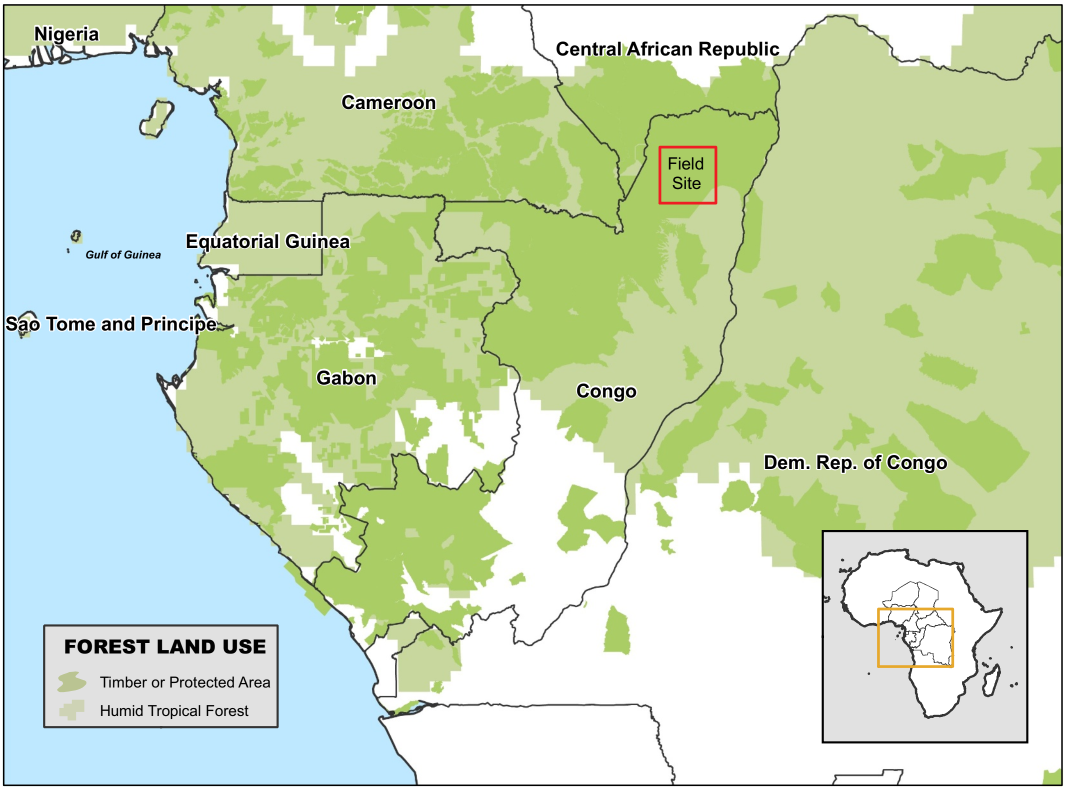

Fig. 1. Site within Congo Basin.

livelihoods to the local environment, when combined with low population density, makes it difficult to construct an appropriate counterfactual. Finally, measuring hunter-gatherer well-being is fraught. Indigenous peoples may have different ethical systems and definitions of well-being that do not map neatly into utilitarian frameworks (Choy, 2018).[4] Many hunter-gatherers in the Congo Basin live in extreme material poverty, in an exchange economy, with low literacy and numeracy rates (Jackson, 2006). Forests contribute toward a large share of indigenous peoples' consumption or earnings (Godoy et al., 2002) and contributions to income from forests may be missed in most survey instruments (Wahlén, 2017).

I test my null hypothesis, that certification fails to change outcomes for indigenous households, by addressing these empirical challenges head-on. I designed and implemented a survey to measure economic well-being of Aka and non-Aka households along the boundary of a certified commercial forest and an uncertified commercial forest in the Republic of Congo. Congo leads the world in the share of its timber that is FSC certified. The certified firm likely chose to certify because of an environmental ethos among firm leaders (Poulsen and Clark, 2012), which is consistent with the literature (Nakamura et al., 2001; Galati et al., 2017). If there is no discontinuous change in local population characteristics along the forest boundary, exposure to certification is quasi-random. Balance checks confirm that households and individuals look similar across the boundary. Using a spatial regression discontinuity design, people on the uncertified side serve as a counterfactual for people exposed to certification. This design ensures that hyper-local features are similar for treated and control groups.

I focus on three outcomes that proxy for economic well-being — composition of the household's previous meal, whether or not an individual was recently ill, and an inventory of material assets. The survey was built on other surveys used in the region (INS, 2006; Riddell, 2011) and extensively pre-tested. Questions emphasized locally salient outcomes with timeframes designed to elicit good data in a context without access to written records (Lenzner et al., 2010). The survey was explicitly timed to reach Aka hunter-gatherers when they were most likely to spend time in the village.

I find that Aka households' nutrition, health, and wealth differ after exposure to certification activities in a surprising way: they are worse. Aka households in certified villages are more likely to go hungry over the previous two days. They are more frequently ill and have greater material wealth inequality: the poorest 15% are poorer than the poorest 15% of households in uncertified villages. Non-Aka households are largely unaffected.

Despite the certified firm's environmental ethos, innovation, and commitment to ensuring indigenous peoples' forest access, exposure to FSC activities likely made Aka households worse off. Why? It may be precisely because it is so difficult to meet with the Yaka. For example, auditors consistently pointed to roads as an example of benefits to the local community, citing appreciation from people living in certified villages along the Motaba River. Yet among the 110 Aka household interviewed, not one cited a road connection as a benefit from commercial forestry. Instead, the most frequently cited benefit from commercial forestry was "nothing."

That Yaka refrain from lauding a road connection is probably unsurprising to anthropologists, researchers trained to consider the world from the Yaka's perspective. For example, Jost Robinson and Remis (2016) found that among Aka women exposed to market integration and hunting restrictions, older women had lower body fat and increased inflammatory markers. Riddell (2013) found that Aka likelihoods changed more in villages exposed to commercial forestry and conservation activities as compared to villages with less exposure. For FSC

[4] Choy (2018) describes the system of *adat* used in by the Sarawak in Malaysia, as well as difficulties in reconciling it within a price-based cost-benefit analysis framework. For the Aka of the Congo Basin, anthropologist Jerome Lewis describes an exchange system where goods are given freely when demanded but knowledge is intellectual property that is exchanged for goods or money (Lewis, 2015).

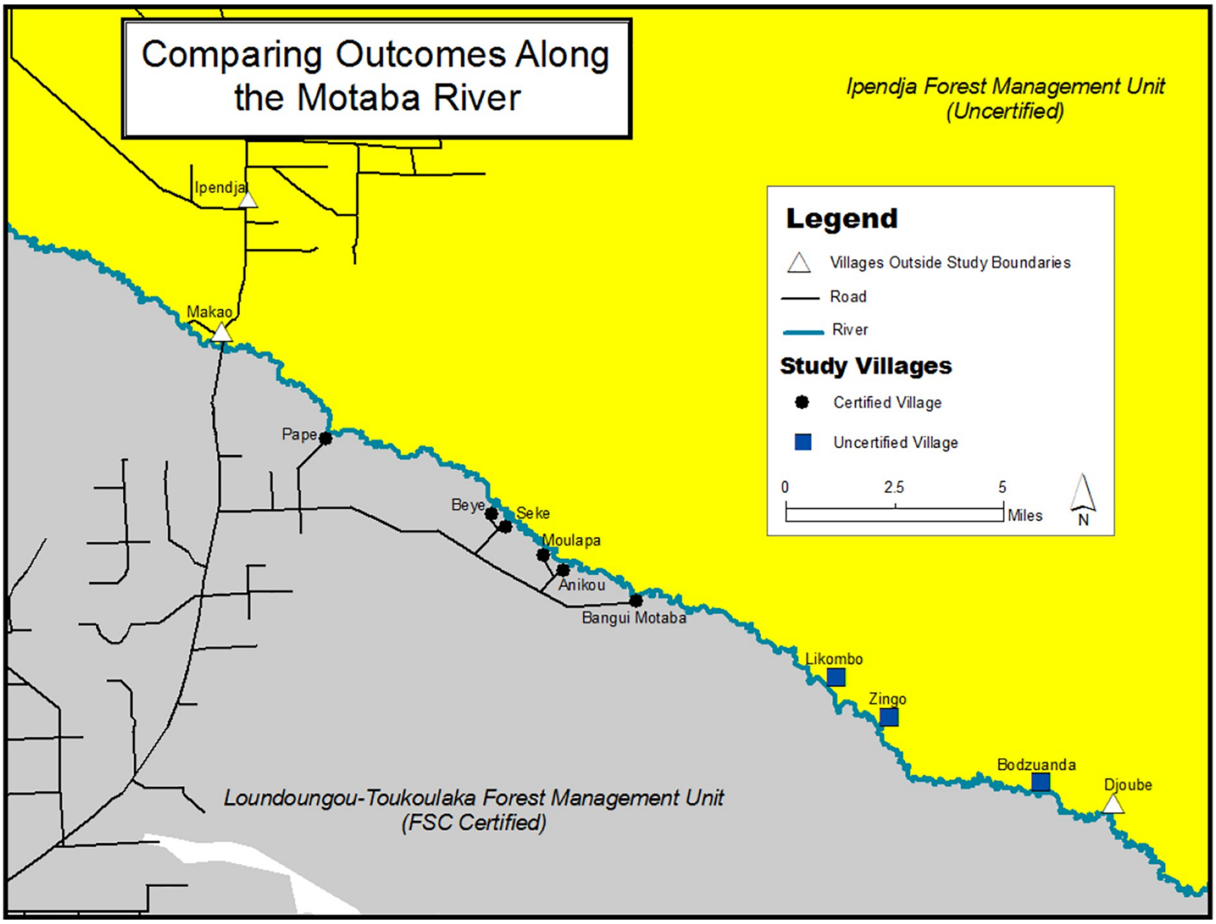

Fig. 2. Treatment boundary.

to succeed, indigenous peoples' need to be involved beyond "consultations to edit and refine the principles" (Dingwerth, 2008: 56) — they need to be actively engaged in imagining and defining what sustainable forestry looks like within their forests.

2. Study setting

The study takes place along the Upper Motaba River in the Republic of Congo. Fig. 1 locates the study site within the Congo Basin forest, second in size only to the Amazon forest and heavily allocated to commercial logging. I begin by describing firm activity and then briefly characterize the village economy along the Motaba River.

2.1. Timber firm activities along the Motaba River

Situated in northern Republic of Congo, the Motaba River acts as a boundary between a certified and uncertified forest management unit (FMU). Both FMUs are large, over half a million hectares. They each include about a dozen small villages and forest camps and their estimated populations are around five thousand people each. Firms cannot certify only part of their forest concession. The households surveyed in the certified forest make up about 10% of the total inhabitants of the Loundoungou-Toukoulaka forest management unit. Loundoungou-Toukoulaka is the last of three contiguous FMUs managed by Congolaise Industrielle du Bois (CIB) to be certified, making CIB's total certified area over 1 million ha. I discuss CIB's motivation to certify in Section 3.4.1.

De jure, all forest management units have obligations to the communities that live in their forests. These obligations, known as *cahiers des charges*, are the product of negotiations between the firm and the Congolese State. A frequent critique is that they are incompletely implemented (Karsenty and Pierre, 2005). For the uncertified FMU along our boundary, the only *cahiers des charges* activities reported by village leaders were the distribution of some agricultural tools. This was confirmed in a ministerial report (MINDDEFE, 2011). By selecting the Motaba River boundary, we construct a counter-factual of local outcomes under typical uncertified timber management, which implies few services for local households.

2.2. Compliance with FSC standard

Forest Stewardship Council's (FSC) Forest Management Certification is the best-known forestry management standard and is highly regarded among all types of eco-labels. Ten principles and criteria are used to assess compliance. Among these, three are most relevant to this study: those concerning rural development, indigenous peoples' rights, and controlled hunting. Principle three states that "The legal and customary rights of indigenous peoples to own, use and manage their lands, territories, and resources shall be recognized and respected." Principle four states that "forest management operations shall maintain or enhance the long-term social and economic well-being of … local communities" (FSC, 1996).

CIB, the certified firm, implemented two innovative programs to respond to principles three and four of the FSC standard. The first was a participatory mapping program developed with help from The Forest Trust, the World Bank, anthropologist Jerome Lewis, and Helveta, a technology company. The result was a unique GPS mapping process co-developed by Aka men and women that used icons and did not require literacy (Clark and Poulsen, 2012: 67). Yaka used the GPS units to create community maps that identified key resources, like trees preferred by caterpillars or sacred places. These resources were then built into CIB's harvesting plans and marked in the forest to ensure protection. Second, CIB hosted a local language radio program called *Biso na Biso*, which featured programs in twelve local languages on 88.0 FM. Yaka songs and interviews were featured on the radio, as well as information related to CIB's timber harvesting and FSC practices (Clark and Poulsen, 2012: 68).

The timeline for the study site, Fig. 2, begins in 2002. CIB received rights to timber production in Loundoungou FMU for a period of fifteen years in 2002. Production began in 2003, the same year that production began in the uncertified Ipendja FMU by Thanry Congo.[5] In 2007, CIB resurfaced the road connecting the Loundoungou sawmill town to the village of Makao and built a bridge across the Motaba River. This is the road sweeping from the bottom left corner of Fig. 2 to the towns of Makao and Ipendja.[6] Among CIB's activities to satisfy FSC's rural development obligation was the construction of a feeder road that connected five villages to the broader road network in 2007.

Road-resurfacing is a common way firms fulfill these obligations in the Congo Basin. For example, in CIB's FSC audit for their Kabo concession, certified in 2006, the auditor notes that to satisfy criteria 4, Community Relations and Workers' Rights, "CIB has also created a number of social services in the region including education, housing, communication (radio, television, telephone), road infrastructure etc." (SGS 2009: 46). Similarly, in the 2010 pre-audit for Loundoungou, the auditor SGS spoke to people in Bangui-Motaba and Beye, two villages included in this study, and notes they have "enjoyed the social infrastructure supplied by CIB… CIB opened a road that leads through the various villages of Upper Motaba" (SGS 2011: 77). In the auditor response, they note that other villages "entered CIB in their good books … because of the road that opened the access for their village, thus giving them the opportunity sell their agricultural produce on Pokola market." Though addressed in more detail in Section 5, it is likely these stakeholder comments came from non-Aka households, who are easier for CIB employees and FSC auditors to access and who, as net exporters of agricultural goods, stand to gain from a road connection.

Though the risks of roads to indigenous peoples' health and wellbeing in the Congo Basin is infrequently recognized, the risks to local defaunation are well-understood (Wilkie et al., 2000). Roads facilitate access to fauna by hunters and trade of bushmeat. Recognizing this, the Congolese state requires all forests to implement controlled hunting protocols, however this requirement is poorly enforced. FSC's criterion 6.2 states that "inappropriate hunting, fishing, trapping and collecting shall be controlled" (FSC, 1996). The FSC-certified firm is recognized as having one of the most sophisticated and far-reaching anti-poaching programs in the region (Clark and Poulsen, 2012). Activities to satisfy criterion 6.2 include control posts on roads manned by armed "ecoguards" to check for illegal trade in wildlife, required seasonal and annual hunting permits, and patrols of hunting zones. Critics have pointed out that controlled hunting policies hurt hunter-gatherers because they restrict access to the forest and are unfairly enforced (Lewis, 2016).

2.3. *The rural economy of the Motaba River*

The Motaba River forms the boundary between the certified and uncertified forest concessions. Even with the new road connection, households remain focused on local consumption and production of agricultural goods. Logging activities are concentrated in the town that accompanies the sawmill and in the timber camp, which moves around the FMU. The sawmill site for Thanry is Ipendja, visible in Fig. 2, and for CIB it is Loundougou, which is south of this map. Both are several hours drive from the villages along the Motaba. Some households do seasonal work for the timber company, such as helping with forest inventories but firms typically prefer to hire individuals with better literacy and greater experience in timber production.

For this paper, we classify households into two subgroups. Yaka are semi-nomadic indigenous hunter-gatherers that are genetically distinct from people who arrived more recently during the Bantu expansion tens of thousands of years ago (Quintana-Murci et al., 2008). All households along the Motaba can be considered forest-dependent but I use the term "indigenous" to refer to Yaka households and "Bantu" or "non-Aka" to refer to non-indigenous households. Among Bantu households, two clans live within the study site, Kaka and Bondongo. Kaka are the dominant clan. Both Kaka and Bondongo spend more time in the village and spend more time on agricultural practices than Yaka, who spend more time in hunting and fishing camps in the forest.

Over time, Yaka and Bantu farmers developed exchange relationships related to farming. Yaka spend time in the village during the dry season, trading their labor for calorie-rich farmed starch, clothes, medicine, salt, and other non-forest goods from a Bantu household known as their *nkumu* (Moukassa et al., 2005; Riddell, 2011; Lewis, 2000). Historically, access to farmed starch during the dry season has been critical for Yaka for three reasons. One, they need to replenish glycogen stores after spending a long period of time in the forest. Two, foraged starch is less abundant during this time of year, thus farmed starch forms part of an annual starch strategy (Kitanishi, 1995). And three, labor for farmer households is physically taxing, requiring a more calorie dense starch.

Before FSC certification, for villages on either side of the Motaba River the primary method of trade was either to travel to a town with a market or to trade with merchants traveling up or down the Motaba by boat (Kitanishi, 1995). As seen in Fig. 2, directly north and south of the study site are two towns, Makao and Djubé. These towns are much larger, with over one thousand inhabitants each. They have weekly markets active since before the road connection. Merchants travel down the Motaba River by boat selling manufactured goods and buying agricultural goods. People also paddle to markets to sell their goods. The new road connection on the west bank of the Motaba brought merchants and logging employees directly to the village by truck or motorcycle. There is excess demand for food because many of the logging company employees living in Thanry and Loundoungou are from Cameroon, Central African Republic, or the Democratic Republic of the Congo and have trouble negotiating access to land they can cultivate.

3. Methodology

This section begins by describing the process by which I selected the site and collected the data. I then present the empirical framework and assess different threats to identification.

3.1. *Site selection*

To investigate how forest certification impacts forest-dependent and indigenous households, I began by identifying all boundaries between a certified and uncertified forest in Congo. I then refined this set to include boundaries with both a sufficient number of villages nearby on either side of the boundary as well as being located within one administrative unit (département). This resulted in a single possible site, the upper Motaba River. Using a census from a 2010 management plan and anthropologists' records of the seasonality of production and consumption activities (Kitanishi, 1995), I conducted a power analysis to ensure sufficient power to identify changes from certification and planned survey timing. Given the share treated, at 80% power we would need 71 households for each ethnic subgroup (Aka, non-Aka) to detect differences at the 5% level. At 90% power, we would need 95 households in each subgroup to detect differences at the 5% level. The

[5] In 2005, Loundoungou and Toukoulaka FMU were combined into one FMU. The FMU in Fig. 2 includes both FMUs.

[6] The Loundoungou sawmill was constructed between 2008-2009.

estimated total population across all study villages was about 250 households. Given the power analysis, we elected to use a census. The study site includes nine study villages between Makao and Djoubé, two larger towns. Six of the villages were on the west bank and thus exposed to certification activities. Table A1 reports village names, location, date of visit, number surveys started, and number finished for each village.

3.2. Data collection

Survey data collection began November 2012 and ended February 2013, spanning 79 days during the main dry season. I chose this time period because it made it most likely we would meet with Aka households in the village (Kitanishi, 1995; Riddell and Obongo, 2011) and to ensure that all surveys occurred during the same agricultural season. I randomized the order of the timing of visits to each village prior to the start of the survey.

I translated and adapted a survey instrument to better reflect local characteristics and piloted the survey in the town of Makao. The *Troisième Enquête Camerounaise auprès des Ménages* (ECAM3) survey instrument used in Cameroon in 2007 served as the base (INS, 2006), to which I added questions used by an anthropologist assessing changes in hunting behavior (Riddell, 2011). I describe outcome variables in more detail in Section 4. After translating the survey into English and Lingala, I piloted question wording and defined local units of measurement during pre-testing in Makao. The survey was given in either French or Lingala, with Yaka translating on the fly if needed. Each household survey took no more than 2 h. I used a tablet, coding the survey in XML and used Kobo Collect software, which accommodated built-in skip logic. Questions and participation remuneration were approved by the University of Michigan Internal Review Board. Funding for data collection came from the Department of State's Fulbright Program and several small grants managed by the University of Michigan.

Each round of surveys began with an initial census of all dwellings in the village. Comparing surveys to census dwelling data, 51 households were absent during our stay in the village and 197 households (896 individuals) started the survey.[7] Participation was voluntary and either the enumerator or the household could refuse participation. Nine households refused to participate when initially approached. The primary reason given for refusal was that the occupants were too old or unwell to participate. Participants or the enumerator could choose to stop the survey at any time. Eight households started but did not finish the survey. These households provided data on their meals and health but not their material wealth data, which was collected at the end of the survey.[8]

3.3. Empirical framework

This study's empirical strategy exploits a discontinuous change in forest management regime across the Motaba River. I compare households on the certified bank of the river to households on the uncertified bank with a focus on differential impacts for indigenous households. The Motaba River boundary forms a discontinuity in longitude-latitude space. The main regression specification is

$$Outcome_{iv} = \alpha + \beta_1 Cert_v + \beta_2 [Cert_v * Non\text{-}Aka_i] + \beta_3 Non\text{-}Aka_i + \gamma X_i + \epsilon_{iv} \quad (1)$$

where $Cert_v$ is the an indicator equal to one if the village is on the certified east bank of the Motaba River and equal to zero otherwise. The coefficient β_1 describes the average marginal effect of certification for indigenous households, the omitted group. $Non\text{-}Aka_i$ is a dummy variable equal to one if not indigenous. The interaction term captures the differential effect of certification on non-Aka households.

The unit of observation i and suite of covariates X_i vary across outcomes. The exact regression used for each outcome is described in Section 4. Because the balance test fails to find differences in most household and individual characteristics across exposure to treatment (see Table 2), and because the study has low power, covariates in the main specifications are restricted. Unless stated otherwise, the direction and magnitude of the β_1 estimate is robust to including the characteristics described in Table 2.

The identification strategy echoes Dell et al. (2017), who compare historical and contemporary outcomes across a colonial administrative boundary in Vietnam. My regression specification is much simpler than Dell et al. (2017) because my data, study area and variation along the boundary is more limited.[9] First, due to budget constraints I only observe villages directly along the study boundary; this eliminates the need for a border bandwidth. Second, the pattern of settlement along the Motaba River limits the latitudinal and longitudinal variation within the treatment and control groups. The implication for the regression specification is that we cannot separately identify differences in latitude and longitude from differences from certification. I address this threat to identification in more detail below.

Selection criteria for the study was at the village level because villages are assigned to treatment based on their location in latitude and longitude space. Treatment is clustered within a village and thus we cluster standard errors at the village level (Abadie et al., 2017). This choice is similar to Dell et al. (2017), who cluster by sub-administrative unit. However, unlike Dell et al. (2017), we have few clusters. There are nine villages; with this few clusters, we are likely to over-reject the null (Bertrand et al., 2004). The best way to address this problem is to use a wild cluster bootstrap (Cameron et al., 2008). However, for very few clusters, like in my case, the wild cluster bootstrap may over- or under-reject. MacKinnon and Webb (2018) show that the subcluster wild bootstrap performs better when there are fewer than 13 clusters. We use the subcluster wild bootstrap, clustering regression standard errors at the village level but bootstrapping at the individual or household level using the *boottest* command (Roodman et al., 2018). We use a six-point distribution because this performs better than traditional Rademacher weights when there are less than eleven clusters (Webb, 2013).

3.4. Identifying assumption and threats to identification

Regression discontinuity designs are "a much closer cousin of randomized experiments than other competing methods" (Lee and Lemieux, 2010: 289). Within a narrow bandwidth, assignment to treatment is as good as random, as long as there is no sharp change in the population at the treatment boundary. Thus, our identifying assumption is that there is no discontinuous change in unobserved household characteristics across the Motaba River or at 17.32° longitude.[10] If unobserved household characteristics change sharply at these boundaries, estimated differences across treatment status would

[7] The likelihood a household was absent when conducting the survey was higher in control villages, 20%, but similar to treated villages, 16%.

[8] Surveys were intended to include all inhabitants of the Bobate neighborhood of Bangui-Motaba (certified). However, after surveys began, leaders within the neighborhood insisted on additional neighborhood fees and gifts for continued participation. Surveys ceased, resulting in completed surveys for 11 households and 31 households that were present but not surveyed. Because the conflict that provoked termination of survey activity occurred among leaders and not households, I do not expect systematic bias across households missing and included from Bobate. Regressions include the 11 Bobate households but results are robust to restricting the population to neighborhoods with full participation.

[9] Like Dell et al. (2017), I also experimented with including a distance to the nearest town. This variable failed as a significant predictor for all study outcomes, likely because there are towns on either end of the study area so there is little variation within treatment and control groups.

[10] The historical settlement pattern along this short stretch of the Motaba River is such that villages north of 17.32° longitude are on the west (certified) bank and villages south of this are located on the east (uncertified) bank.

Table 1
Summary statistics.

	Mean	SD	Min	Max	*N*
Households					
No starch yesterday	0.142	0.350	0	1	197
No starch day before yesterday	0.152	0.360	0	1	197
No starch last two days	0.076	0.266	0	1	197
No protein yesterday	0.330	0.471	0	1	197
No protein day before yesterday	0.259	0.439	0	1	197
No protein last two days	0.137	0.345	0	1	197
Certified village	0.619	0.487	0	1	197
Non-Aka * Certified	0.264	0.442	0	1	197
Non-Aka	0.442	0.498	0	1	197
Female	0.137	0.345	0	1	197
Value of material assets, Log (FCFA)	11.338	1.022	7.199	14.986	189
Individuals					
Sick last two weeks	0.771	0.420	0	1	896
Certified village	0.610	0.488	0	1	896
Non-Aka * Certified	0.246	0.431	0	1	896
Non-Aka	0.415	0.493	0	1	896
Female	0.502	0.500	0	1	896
Age in years	22.664	21.378	0	90	896

Notes: The number of observations differs for value of material assets because eight households did not finish the survey; the inventory of household assets was near the end of the survey questions.

Table 2
Comparing persistent characteristics across exposure to treatment.

	Certified	Uncertified	*p*-Value
All households (N = 197)			
Aka	0.574	0.533	0.582
Fisher	0.795	0.787	0.889
Hunter	0.505	0.593	0.271
Planted field	0.868	0.841	0.616
Educated	0.767	0.743	0.716
Can read	0.174	0.100	0.143
Residents in household	3.992	3.920	0.805
Men in household	1.098	1.160	0.535
Children in household	2.164	2.213	0.830
Visitors in household	0.0246	0.0933	0.313
Female	0.0902	0.213	0.0251
Aka households (N = 110)			
Fisher	0.757	0.775	0.833
Hunter	0.627	0.706	0.428
Planted field	0.855	0.794	0.462
Educated	0.721	0.686	0.719
Can read	0.0290	0	0.159
Residents in household	4.200	4.125	0.849
Men in household	1.114	1.175	0.502
Children in household	2.286	2.375	0.781
Visitors in household	0.0143	0.175	0.203
Female	0.0429	0.150	0.0905
Non-Aka households (N = 87)			
Fisher	0.846	0.800	0.590
Hunter	0.318	0.440	0.323
Planted field	0.885	0.886	0.988
Educated	0.827	0.800	0.757
Can read	0.365	0.200	0.0894
Residents in household	3.712	3.686	0.952
Men in household	1.077	1.143	0.728
Children in household	2	2.029	0.932
Visitors in household	0.0385	0	0.159
Female	0.154	0.286	0.159

Notes: The table presents results from *t*-tests where the unit of observation is the household head or household. The first and second columns report averages for households in Certified and Uncertified villages. The third column reports the *p*-value from a two-tailed test where the population sizes are unequal. "Fisher" is a dummy variable equal to one if the household reported any fishing tools among their assets. "Educated" is a dummy variable equal to one if the head of household reported ever attending school.

reflect ex ante differences instead of changes from certification.

Ex ante, we do not expect to find changes in persistent household and individual characteristics at the boundary because both sides of the Upper Motaba River form part of the settlement area for the Kaka clan (Pierre, 2005: 30). This implies that people on either side of the river are kin and likely to be similar. Indeed, households on the uncertified side of the river reported an average of about 3.9 family members living in certified villages and households on the certified side of the river reported 3.7 family members living in uncertified villages.

Under randomization, balanced covariates across treated and control groups "gives us reason to hope and expect that other variables, not measured, are similarly balanced" (Rosenbaum, 2002: 21). Table 2 tests whether persistent household characteristics vary across treated and control groups. The left column reports the average for certified villages, the right the average for uncertified villages, and the final column is the *p*-value for a two-sided *t*-test. I fail to find differences across likelihood to fish, farm, attend school, be able to read, the household size, and other characteristics. The only characteristic that differs significantly is the likelihood the household head is female, which I explore in the second identification threat below in Section 3.4.2. Female-headed households are poorer and less likely to report consuming protein. A higher frequency of female-headed households in uncertified villages means we may be more likely to overestimate meal and wealth benefits from certification. To control for persistent differences across male- and female-headed households, I include a dummy variable, $Female_i$, in all regression specifications.

3.4.1. Identification threat: firm selection into treatment

The decision to certify is voluntary, meaning firms select into certification. An emerging body of literature characterizes what drives firms to volunteer to pursue additional regulation. Nakamura et al. (2001) and Galati et al. (2017) find that moral or ethical reasons seem to dominate economic reasons for participating. Alternatively, having the appearance of an environmental ethos may be what matters: Mikulková et al. (2015) find that certified firms enjoy benefits from an image associated with better environmental performance. In their chapter describing the origins of the partnership between CIB and a conservation organization, Poulsen and Clark offer support for both of these motivations (Poulsen and Clark, 2012). The founder of CIB, Dr. Hinrich Lueder Stoll, was personally sympathetic to sustainable timber practices, having studied under Sir Dietrich Brandeis, "the father of tropical forestry "(Poulsen and Clark, 2012: 37). In the eighties, Stoll was struck by what he learned about Yaka well-being after a presentation from Jerome Lewis, an anthropologist and eventual collaborator with CIB on their participatory mapping program. However, the long-term success of the partnership between CIB and a conservation organization was credited to the "unique friendship" between the son of CIB's operational director and a researcher for the conservation organization.

Consistent with a narrative that CIB leadership was environmentally inclined, the FSC audit of their first certified forest concession states that "CIB has a long term commitment to have all of its FMU's FSC certified" (SGS Qualifor, 2005: 16). This suggests that CIB's decision to certify their forest as sustainably managed stemmed from values held by CIB leadership. However, this was not CIB's only motivation. Poulsen and Clark cite "projecting a 'green' image" as one of eight motivations for CIB (Poulsen and Clark, 2012 : 25), noting that CIB wanted to rehabilitate its image after they were accused of killing apes (World Rainforest Movement, 2003).

These institutional details suggest that CIB's motivation to certify was not based on differences in the characteristics of the local population on their side of the upper Motaba River. If the certifying firm is somehow intrinsically different than the conventional firm, e.g. more environmentally inclined, and this difference causes them to certify, this does not threaten our identifying assumption that unobserved household characteristics do not change discontinuously across the

Table 3
Selection into treatment.

Certification activity	Low cost	Data to test	*t*-Test
Hunting restrictions	Less hunting	Hunted this year	$p = 0.271$
Market integration	More ag production	Any field	$p = 0.616$

Notes: The table presents results from *t*-tests where the unit of observation is the household head or household. The first and second columns report averages for households in Certified and Uncertified villages. The third column reports the *p*-value from a two-tailed test where the population sizes are unequal.

Motaba River or at 17.32° longitude.

Though not emphasized in the history of CIB, theoretical models of certification point to low compliance costs as a driver of certification (Fischer and Lyon, 2014; Li and van't Veld, 2015). There is empirical support that higher compliance costs firms are less likely to certify (Carlsen et al., 2012; Tian et al., 2018) or that low compliance cost firms are more likely to certify (Cubbage et al., 2010; Doremus, 2018). For selection into FSC based on compliance costs to threaten this study's empirical design, four things would have to be true. First, activities targeting local people must be sufficiently costly as to affect the decision to certify. This seems possible in our context. In Kabo, a nearby forest concession, CIB spent about 31% of annual FSC implementation cost on hunting restrictions, 10% on funds for rural development initiatives, and about 5% on programs oriented to indigenous peoples of a total $1.6 M spent annually.

Second, the company would need fine data on local population characteristics when making the decision to certify. This seems less likely. In the third corrective action request of their 2016 audit report, the auditor cites CIB for not monitoring social impacts from logging operations and cites outdated socioeconomic data, collected in 2005, as objective evidence (SGS Qualifor, 2016: 65–66).[11]

The third necessary condition is that costs from complying with FSC certification would have to depend on characteristics of the local population along the study's segment of the Motaba River. The main FSC activities considered here are the construction of a road connecting villages to the road network and restrictions on hunting to reduce poaching. Table 3 relates each activity to how we would expect observables to differ, based on firm selection.

The fourth and final necessary condition is that local population characteristics differ at the forest boundary.[12] The final columns in Table 3 describe data available to test for selection and the outcome of the test. Given how costly hunting restrictions are to implement, firms with populations that hunt less frequently may be more likely to certify. I test whether men in certified villages are more or less likely to report hunting in the previous year in Table 2 and find no difference. Next, market integration activities would be less costly if households in certified villages frequently trade agricultural goods in the regional market. I test a measure of agricultural effort, whether a household has a field, and fail to find a difference across households in certified and uncertified villages. Because hunting may fall in response to restrictions and agricultural investment may rise in response to higher prices, we would expect these tests to have a higher type I error. Finding no difference across the boundary is encouraging.

In sum, in this setting the decision to certify may have been based on CIB leadership's values using outdated data on socioeconomic characteristics of the local population. Certification costs related to hunting are sizable. Those related to rural development and indigenous peoples are less so. However, we failed to find evidence that local characteristics differ across the boundary in ways that would make the certifying firm more likely to certify.

3.4.2. Identification threat: sorting in response to treatment

Even if exposure to certification is as good as randomly assigned, there remains a second identification threat: household sorting in response to certification activities. With sorting, the estimated coefficient β_1 would reflect changes in the distribution of households instead of the difference between treated and control households. The balance tests in Table 2 provide some assurance that we do not find strong evidence of sorting. However, a higher frequency of female-headed households in uncertified villages could be an indication of higher emigration out of uncertified villages.

One certification activity in particular, a road connection, could change village composition by changing emigration incentives. Roads may increase or decrease emigration, depending on how changes in local wages and prices compare to changes in migration costs (Morten and Oliveira, 2016). Empirically, several authors find that roads reduce emigration.[13]

I use three different approaches to assess whether patterns of emigration differ across certified and uncertified villages. First, I compare populations during my census, in 2012, with a 2007 census done by the uncertified logging company (Bikoumou and Mboussi, 2010). For each census, the unit of observation was ethnicity-village population. A two-sided *t*-test comparing the change in population since 2007 failed to reject the null hypothesis that population change was the same in certified and uncertified villages (p-value = 0.43).

Second, I plot a histogram of household formation across time across treatment status in Fig. AI. If we see a sharp uptick in arrivals in certified villages starting about five years before, this would be evidence of increased immigration into certified villages. Differences in the pattern of arrivals over time could also indicate difference in emigration. From the figure, we see that the patterns of household arrival look very similar across exposure to certification activities.

Third, to evaluate emigration out of the study area I can compare the fraction of households absent when the survey was conducted. The likelihood a household is absent is similar across certified and uncertified villages, 16% and 20%, respectively. Fourth, we may expect migrants to be more educated than those that remain and men may migrate more frequently (Chiquiar and Hanson, 2005; Fafchamps and Shilpi, 2013). Table 2 reports *t*-tests from survey questions related to education and sex. Though certified villages have higher rates of literacy and school attendance, we fail to reject the null that the rates are the same for certified and uncertified villages.

Together, there is weak evidence that certification activities reduced emigration out of certified villages. However, if road connection reduced emigration by educated, productive men in treated villages, we would expect our estimates of protein meal frequency and the value of material assets to be biased upward.

4. Specifications and results

I consider how certification activities affect nutrition, health and wealth. In each subsection, I describe the outcome variable, regression

[11] In response, CIB collected data and issued a report. All of this took place in 2015, three years after the survey for this paper was finished.

[12] If a firm selects in to certification because they have fewer inhabitants in their forest, and thus will have lower compliance costs, this is not a threat to identification because it does not imply a discontinuous change in unobserved characteristics of local people along the Motaba River. Given that, in this setting, costs directly related to the local population are only about 15%, we might not expect them to be important when choosing to participate, anyway. Doremus (2018) finds population density fails to predict participation in either weak or strong certification schemes in Cameroon.

[13] In Nepal, road presence was associated with less emigration (Fafchamps and Shilpi, 2013) and in Tanzania, roads reduced migration on average (Gachassin, 2013). In India, roads do not lead to changes in village populations (Asher and Novosad, 2016). Authors in Bangladesh cite zero attrition in panel households before and after road rehabilitation, though this may not capture emigration by individuals within the household (Khandker and Koolwal, 2010).

specification and then report results.

4.1. Nutrition

4.1.1. Outcome variables

The outcome variables for nutrition are derived from a survey question on the household's meals over the previous two days. This question was used by an anthropologist studying hunting along the Motaba from 2007 to 2008 (Riddell, 2011) . [14] Using the dietary recall of the meal the previous day, I classified whether meals had any protein or any farmed starch. A household was recorded as having not consumed farmed starch if they did not list manioc, sweet potato, taro, yam, plantain or rice. A household was recorded as having not consumed protein if they failed to list meat or fish.

Given that the dietary recall period was so short, food security is measured in the very immediate past. I consider three horizons: the previous day's meal, the meal two days ago, and a two day period (previous day and day before). The outcome variable is equal to one if the household failed to report ever eating farmed starch or protein over the horizon. In Table 1, we see that 14.2% of households failed to report eating starch the previous day, 15.2% the day before that, and 7.6% failed to report eating starch either day. Protein is missed more frequently; 33% of household failed to report eating protein the previous, 25.9% the day before that, and 13.7% report no protein either day.[15]

4.1.2. Specification

I estimate the difference in nutrition outcomes between households in certified and uncertified villages using a linear probability model estimated via ordinary least squares,

$$Outcome_{iv} = \alpha + \beta_1 Cert_v + \beta_2 [Cert_v * Non\text{-}Aka_i] + \beta_3 Non\text{-}Aka_i + \gamma_1 Female_i + \epsilon_{iv} \tag{2}$$

where i indexes a household. $Female_i$ is a dummy variable equal to one if the household head is female. The coefficients α and γ_1 are estimated but not reported in Table 4. Standard errors are clustered by village and reported in parentheses. p-Values calculated using Roodman et al.'s (2018) subcluster wild bootstrap program are reported in brackets below standard errors.

4.1.3. Results: starch

The top panel of Table 4 reports the difference in starch consumption frequency across exposure to FSC activities. Though, on average, all Aka households report missing farmed starch more frequently, Aka exposure to certification activities further increases the frequency of missing farmed starch. Across each horizon – the previous day, two days ago, or across the last two days – the coefficient is positive. For the previous day, the estimate is not statistically significant when using the subcluster wild bootstrapped standard errors. However, it is significant for the previous day or over the last two days. For the meal two days ago, households in certified villages are 30 percentage points more likely to report failing to consume farmed starch; the estimate is significant at the 1% level. This is an increase of over 200% as compared to households in uncertified villages. This pattern holds for consumption over the last two days. The effect is more precise, suggesting a more consistent difference for the poorest households.

Access to farmed starch is critical to Yaka nutrition strategy in this season. According to anthropologist Koichi Kitanishi, "for Aka living in village camps, the main dietary items were agricultural foods and wild meat kept while hunting with loaned Ikenga firearms" (Kitanishi, 2000: 151).[16] In part because staple forest foods are less available at this time of year, Yaka have seasonally spent time in the village, exchanging their labor for farmed food, clothing, and other non-forest goods in a long-term exchange partnership with a non-Aka family who needs help with clearing and planting fields during the dry season (Kitanishi, 1995). This agricultural labor is intense, increasing calorie and amino acids needs. Increased frequency of missed farmed starch could indicate insufficient calories. Insufficient calories may lead to difficulty fighting infections. Later I examine whether households more often report that they are ill.

Non-Aka households do not share the decrease in farmed starch consumption frequency. The second coefficient is the differential effect of exposure to certification activities on non-Aka households. The interaction term is negative, statistically significant, and nearly perfectly offsets the average effect. For each regression, we fail to reject the null that certification has no effect on starch consumption for non-Aka households. As seen in Table 2, non-Aka households are more likely to have planted and fallow fields. For non-Aka households, trade increases the opportunity cost of consuming own farmed starch as prices increase. However, even with a higher opportunity cost, households seem to

Table 4
Differences in food consumption.

	Yesterday	Two days ago	Last two days
Panel A: No farmed starch consumption			
Certified village	0.145	0.297**	0.126*
	(0.08)	(0.06)	(0.04)
	[0.222]	[0.004]	[0.045]
Non-Aka & certified	−0.143+	−0.325*	−0.149+
	(0.06)	(0.08)	(0.05)
	[0.083]	[0.017]	[0.077]
Observations	197	197	197
R^2	0.118	0.210	0.078
Panel B: No protein consumption			
Certified village	0.324*	0.189+	0.154**
	(0.10)	(0.08)	(0.04)
	[0.048]	[0.066]	[0.007]
Non-Aka & certified	−0.206	0.093	−0.069
	(0.12)	(0.11)	(0.07)
	[0.231]	[0.491]	[0.442]
Observations	197	197	197
R^2	0.104	0.085	0.093

Notes: The table presents results from six separate regressions where an observation is a household. The outcome variable for Panel A is a dummy variable equal to one if the household failed to report eating starch the previous day (column 1), the day before (column 2), or anytime over the last two days (column 3). The outcome variable for the bottom panel is a dummy equal to one if the household failed to report eating protein over the same horizons. The omitted category is Yaka, the indigenous group. All regressions include a dummy for non-Aka, female, and a constant. Coefficients are followed by standard errors clustered by village, in parentheses. p-Values from hypothesis testing based on subcluster wild bootstrapping using boottest (Roodman et al., 2018) are reported in brackets.

+ $p < 0.1$.

* $p < 0.05$.

** $p < 0.01$.

[14] This is, admittedly, a blunt instrument for food security. I chose this question, in part, because I wanted to create data that would be comparable with Riddell (2013) across time. The question also performed better in pretesting than the alternative considered, a question in ECAM3 that asked whether the household ate three meals per day, on average (INS, 2006: Section 10.2, "Conditions de Vie," Question 12, subsection 01). Households found it easy to recall. This was the only question in the survey that indicates food insecurity. Given my results, future studies should consider adding survey questions shown to be strong predictors of food insecurity, such as the Household Dietary Diversity Score.

[15] About 8% of households report eating neither starch nor protein the previous day or the day before.

[16] Note, Ikenga is a clan within the Kaka group.

prefer to continue consuming farmed starch. This is consistent with high calorie needs during the dry season.

4.1.4. Results: protein

In the second panel, we find a similar pattern for protein consumption. Aka households exposed to forest certification activities more frequently report not consuming protein in recent meals. The effect size is larger for the most recent horizon: households in certified villages are 32 percentage points more likely to report no protein in their last meal. They are also 19 percentage points more likely to report no protein in a meal in the recent past and 15 percentage points more likely to report no protein consumption over the last two days. Like in the case for starch, the estimated effect is most precise for the poorest households, those who fail to consume protein for the past two days.

Unlike for starch, there is some evidence that decreased protein consumption is shared across Aka and non-Aka households exposed to certification activities. For the previous day, we reject the null that there is no change in consumption for non-Aka households at the 5% level. A shared decrease may come from hunting restrictions.

Considered jointly, the estimates from Table 4 bring us pause. Aka households exposed to FSC activities consume less starch and less protein. More troubling, we see an increase in the frequency that households report not consuming any starch or any protein over the last two days. These are likely the poorest household along the left bank of the Motaba River, who may not be getting enough food to eat. To test further for food insecurity, the next section investigates the frequency of illness among household members.

4.2. Health

4.2.1. Outcome variable

For health, I asked respondents whether they had been ill over the last two weeks. This question came from ECAM3 and did well in pretesting.[17] This question was posed to each individual within the household, for a total of 896 observations, of which 77% reported being ill recently. Though illness frequency may vary by season, these differences should not affect our results because the survey was limited to one season, the dry season, and village visitation timing was randomized. We would expect illness to respond to the interaction between an individual's activities and their nutrition and health status at the time of these activities. In our setting, changes in illness could reflect an increase in labor without an increase in food, no change in labor and a decrease in food, or an increase in labor and a decrease in food.

4.2.2. Specification

Like in the case of nutrition, I estimate the change in the likelihood of an individual being ill in over the last two weeks after exposure to certification activities using a linear probability model estimated via ordinary least squares,

$$Outcome_{iv} = \alpha + \beta_1 Cert_v + \beta_2 [Cert_v * Non\text{-}Aka_i] + \beta_3 Non\text{-}Aka_i + \gamma_1 Female_i + \gamma_2 Age_i + \gamma_3 Age_i^2 + \epsilon_{iv}$$

where i indexes an individual. Unlike in the case of nutrition, I include a linear and quadratic age_i term. The constant, $Female_i$,Age_i and Age_i^2 coefficients are estimated but not reported in Table 5. Standard errors clustered by village are in parentheses and p-values calculated using Roodman et al.'s (2018) subcluster wild bootstrap program are reported in brackets below the standard errors

To assess differential effects for subgroups, I run the regression and restrict the population by gender and age. Panel A of Table 5 includes all individuals, while panels B and C restrict to women and men.[18] I break out age into three categories, adults older than 45, people between 10 and 45, and children under ten. Within each panel, each column comes from a single regression, for a total of 12 regressions.

Table 5
Differences in reported illness.

	All	Older adults	Middle aged	Children
Panel A: All individuals				
Certified village	0.108*	0.194	0.071	0.090
	(0.02)	(0.10)	(0.03)	(0.06)
	[0.023]	[0.189]	[0.149]	[0.351]
Non-Aka & certified	−0.165*	−0.292*	−0.108	−0.129
	(0.03)	(0.09)	(0.08)	(0.09)
	[0.013]	[0.040]	[0.313]	[0.354]
Observations	896	155	337	404
R^2	0.034	0.095	0.013	0.059
Panel B: Women and girls				
Certified village	0.117*	0.387	0.102	−0.007
	(0.03)	(0.13)	(0.06)	(0.14)
	[0.037]	[0.119]	[0.254]	[0.971]
Non-Aka & certified	−0.195+	−0.418*	−0.125	−0.113
	(0.07)	(0.13)	(0.11)	(0.14)
	[0.087]	[0.075]	[0.388]	[0.563]
Observations	450	85	170	195
R^2	0.050	0.279	0.012	0.102
Panel C: Men and boys				
Certified village	0.102	−0.037	0.036	0.166+
	(0.04)	(0.10)	(0.04)	(0.05)
	[0.101]	[0.813]	[0.567]	[0.065]
Non-Aka & certified	−0.136	−0.151	−0.087	−0.104
	(0.07)	(0.14)	(0.07)	(0.13)
	[0.166]	[0.406]	[0.352]	[0.604]
Observations	446	70	167	209
R^2	0.022	0.060	0.019	0.049

Notes: The table presents results from four separate regressions where an observation is an individual. The outcome variable for all regressions is a dummy variable equal to one if the individual reported being sick within the previous two weeks. The first two columns includes all individuals; column 2 restricts to women and column 4 restricts to men. The omitted category is Yaka, the indigenous group. All regressions include a dummy for non-Aka, female, linear and quadratic age, and a constant. Coefficients are followed by standard errors clustered by village, in parentheses. p-Values from hypothesis testing based on subcluster wild bootstrapping using boottest (Roodman et al., 2018) are reported in brackets.

+ $p < 0.1$.

* $p < 0.05$.

4.2.3. Results

On average, exposure to certification activities increases the frequency of illness. Individuals exposed to certification are 11 percentage points more likely to report recently being ill. Moving to the second column, we see that this effect may be driven by older adults: the coefficient on the regression for older adults is much larger than for other groups, though the estimate is imprecise due to low power. Like in the case of starch consumption, the interaction term for the effect of certification on non-Aka individuals is negative and offsets the average effect.

Panels B and C group the population by sex. The average effect is similar for men and women and has the same magnitude as the population estimate: 10–11 percentage points. However these similarities mask different responses across older and younger people. Among women, it is older Aka women who report the greatest increase of illness: a 39 percentage point increase. The coefficients for women of childbearing age and girls are much smaller. This result concords with results from anthropologists in nearby Central African Republic, who found that older women in particular had lower BMI and increased inflammatory markers in villages with greater market integration and hunting restrictions (Jost Robinson and Remis, 2016). Among males, Aka boys exposed to certification activities are 17 percentage points more likely to report being sick recently. Boys illness could decrease if

[17] Section 2, question 2 (INS, 2006).

[18] Note that panels B and C include the age covariates but not gender.

they have less opportunity to practice and grow skilled at hunting through defaunation or reduced access to the forest.

4.3. Material assets

4.3.1. Outcome variable

For material wealth, I used an inventory of belongings adapted from an anthropologist (Riddell, 2013) and prices assessed across two vendors in Makao in 2012.[19] The assets in the inventory are reported in Table A2. The outcome variable is the natural log of the value of a household's assets, measured in FCFA.[20] The average value of assets was about 84,000 FCFA or 168 US dollars. However, as seen in Fig. 3, the median Yaka and non-Yaka household asset value differs greatly.

4.3.2. Specification

To estimate the difference in wealth between households in certified villages and those in uncertified villages, I use both a linear regression model estimated via ordinary least squares (OLS) and a quantile regression model to estimate the following specification,

$$Outcome_{iv} = \alpha + \beta_1 Cert_v + \gamma Female_i + \epsilon_{iv} \quad (3)$$

where i indexes a household. $Female_i$ is a dummy variable equal to one if the household head is female and is estimated but not reported in Table 6. Fig. 3 plots histograms of material asset value for Yaka and non-Aka households. The distributions differ – Yaka households tend to be much poorer, as seen in the figure – creating a bimodal distribution when grouped. For this reason, I broke the population into two when estimating the difference in material wealth for households exposed to certification activities.

The first column of Table 6 reports estimates from an OLS regression for each group. The second to fourth columns use quantile regressions at the 15th, 50th, and 85th percentiles with bootstrapped standard errors. I selected these quantiles to give a sense of how higher and lower parts of the distribution differ after exposure to certification.[21]

4.3.3. Results

Focusing first on the top panel of Table 6, the coefficient for the certification dummy in the OLS estimate, in column one, is −0.10 and statistically insignificant. This estimate is not far from the coefficient for the quantile regression of the 50th percentile, which is −0.089 and also statistically insignificant. Comparing across the quantile regressions in columns 2 through 4, we see that the distribution of wealth of households exposed to certification has greater inequality: the coefficient for certification is negative for the bottom of the distribution and positive for the top of the distribution. The increase in inequality is driven by decreases at the bottom of the distribution: the poorest 15th percent of certified indigenous households are 34 percentage points poorer than the poorest 15th percent of uncertified indigenous households and the estimate is statistically significant at the 5% level. A decrease in household wealth for the poorest households fits with the food insecurity results, where the estimates were most precise for households who failed to consume any starch or any protein over the past two days. In the Democratic Republic of the Congo, households with the lowest income and asset value tend to have less agricultural land and are further from markets (Nielsen et al., 2012). If this holds true along the Motaba, we might expect a decrease in wealth in response to road-induced higher agricultural product prices. Turning to the second panel of Table 6, the pattern of results and coefficient magnitudes are similar for non-Aka households, however the quantile for the 15th percentile is no longer statistically significant.

Fig. 4 plots the estimated coefficient for certification at different deciles from a quantile regression using the grouped population, Aka and non-Aka households. The dashed line is the OLS estimate of the certification coefficient, which is −0.069. Near the median, the two estimates are similar. The coefficient is increasing in quartiles, which implies that households in certified villages have greater income inequality. The confidence interval for lower quartiles is below zero; we reject the null that there is no difference in material assets across treatment status for households in bottom quartiles.

5. Discussion

In this section, I explore the role of the FSC process in bringing about negative outcomes for Yaka. Though we cannot trace out the mechanism directly, higher prices for farmed starch from the road and restrictions on hunting seem plausible pathways that could explain changes in nutrition, health, and wealth for Aka households. This begs the question, would the road have been resurfaced without FSC certification? Internally, CIB recognized the risks roads pose. In his 2005 report for CIB, Pierre warns that the opening up, by road, of north Congo must be anticipated and accompanied by safeguards (44). Auditors, on the other hand, cite road construction several times as an example of benefits to local communities. Roads as a benefit reflects non-Aka preferences. When asked what frustrated them about STC, the uncertified firm, 26 non-Aka households cited the lack of road resurfacing. The majority of Aka households said they had no complaints with STC. Not one Aka household complained of a lack of road resurfacing by STC.

Without FSC auditor pressure, STC had not resurfaced a road to Motaba villages on the right bank. Given CIB's internal concerns and STC's reluctance despite local interest, it seems likely that FSC auditor preferences encouraged CIB to resurface roads. Timber firms like CIB in the Congo Basin choose to satisfy FSC by resurfacing roads because road quality is particularly poor in the Congo Basin[22] and firms are skilled at road-building. Experts at selective logging, firms in the Congo Basin can resurface at low cost, in terms of capital, management costs, and materials.

Roads are also a popular choice because they are easily monitored by FSC auditors. This last reason hints at the main limitation of the FSC certification process, as it concerns indigenous peoples. Audit visits are short and focused on documentation. In an analysis of FSC corrective actions across several forests, Thornber found that most corrective actions were related to documentation, monitoring, and environmental concerns (Thornber, 1999). Nebel et al. (2005) found a similar pattern: the number of FSC auditor corrective actions related to indigenous peoples was less than 6%, while those related to environmental impact (33%), management plan (18%) and monitoring and evaluation (12%) made up over half. For Loundoungou, this pattern – a focus on documentation and neglect of engaging with indigenous peoples – emerges in the audit reports themselves. For example, in the notes from surveillance visits in Loundoungou's final audit report, auditors failed to meet with Yaka leaders or focus groups (2016). In this same audit report, there are, however, allegations that a different CIB road connection, opened at the request of village leaders in 2016, led to a fatal epidemic for Yaka children under five.[23]

[19] Since eight households did not complete the survey, material wealth has 189 observations instead of 197.

[20] In 2012, along the Motaba, 500 FCFA was about 1 USD.

[21] We are limited to a coarse set of quantiles because the set of observations is small.

[22] Development agencies, such as the World Bank, the IMF, the US Agency for International Development, and the United Nations Development Program all have large programs financing road resurfacing in the Congo Basin.

[23] Notably, this concern was waived off by the auditor in a telling way. The response to the comment was that “the life style and believes [sic] of the forest people especially the semi-nomadic also facilitate the spread of diseases amongst them but also malnutrition” (2016: 84). In one sentence, the auditors both blame Yaka lifestyle, which they do not understand, and recognize Yaka food insecurity.

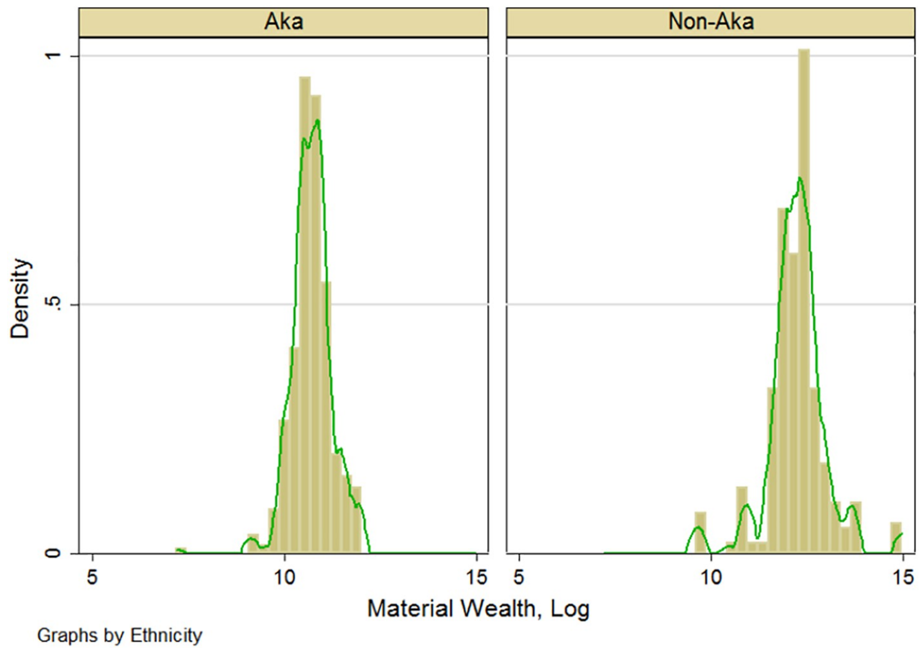

Fig. 3. Histogram of material wealth. Notes: The figure presents histograms and kernel density plots for the natural log of household material wealth, measured in FCFA, for Yaka (right) and non-Aka (left) household. The bar areas are scaled such that their sum equals one. The kernel density uses the Epanechnikov kernel.

Table 6
Differences in asset value quantiles.

	OLS	p15	p50	p85
Panel A: BaAka households				
Certified village	−0.102	−0.342*	−0.0888	0.0699
	(0.128)	(0.160)	(0.115)	(0.223)
Female	−0.498*	−0.376	−0.465**	−0.578*
	(0.213)	(0.260)	(0.163)	(0.255)
Constant	10.78**	10.42**	10.83**	11.13**
	(0.109)	(0.111)	(0.0807)	(0.196)
Observations	102	102	102	102
R^2	0.053			
Panel B: Bantu households				
Certified village	−0.179	−0.373	−0.107	−0.0553
	(0.184)	(0.315)	(0.159)	(0.236)
Female	−0.175	−0.932+	0.125	0.0955
	(0.223)	(0.507)	(0.327)	(0.345)
Constant	12.27**	11.87**	12.25**	12.82**
	(0.154)	(0.166)	(0.140)	(0.212)
Observations	87	87	87	87
R^2	0.016			

Notes: The table presents results from eight separate regressions where an observation is a household. The outcome variable for all regressions is the natural log of household's asset value (in FCFA). The first column estimates the conditional expectation using OLS with clustered standard errors. Columns 2–4 use quantile regression to estimate the 15th, 50th, and 85th percentiles using bootstrapped standard errors. Panel A restrict the population to Yaka households, Panel B restricts the population to non-Aka households.

+ $p < 0.1$.

* $p < 0.05$.

** $p < 0.01$.

In a discussion about why CIB was opening roads to begin with, the auditor notes the "road was opened by CIB on request of village communities" (SGS, 2016: 84). Who put in this request? If non-Aka village leaders have better access to FSC auditors and logging company personnel, roads may reflect their rural development project preferences. In certified villages, responses to the question "what do you appreciate most about CIB?" emphasize this point. Among non-Aka households, eight said the road, five said no they saw no benefit, and 39 listed some other benefit. Aka responses had a different pattern: 28 cite some other benefit and 42 said no benefit. Not one Aka household said the road.

6. Conclusion

This paper uses a spatial discontinuity in exposure to activities to satisfy Forest Stewardship Council's forest management standard in northern Republic of Congo to assess how FSC certification changes nutrition, health, and wealth for indigenous and forest-dependent households. Outcomes were assessed using a survey timed to target indigenous Aka households and measure salient outcomes in a low literacy, extreme material poverty context.

I found that Aka households exposed to certification had worse nutrition outcomes: they more frequently failed to report eating farmed starch or protein. Aka individuals exposed to certification also reported more frequent recent illness, particularly older women. The poorest 15th percentile of Aka households exposed to certification were poorer than the poorest 15th percentile of households not exposed to certification. The distribution of material wealth among households in certified villages was more unequal than those in uncertified villages. Anthropologists Remis and Jost Robinson (2014) and Riddell (2013) found similar patterns when comparing Yaka across villages more and less exposed to market integration and hunting restrictions.

A negative impact on wealth stands in contrast to recent work investigating the effectiveness of FSC-certified *community* forestry. FSC-certified community forestry is associated with increased incomes in Guatemala (Bocci et al., 2018) and Tanzania (Kalonga and Kulindwa, 2017). In community forestry, the local community has the right to fell and sell certified timber, in contrast with commercial forestry, where a firm extracts timber and is mandated to respect local use rights and offer compensation for extraction.

Two features of the study limit the generalizability of these results. Like other settings that exploit a spatial discontinuity (Dell, 2010; Dell et al., 2017), I only observe outcomes after treatment. I cannot directly verify that households did not sort in response to treatment. However, I test persistent household characteristics and find them to be comparable. Second, the population of households interviewed is small and I only investigate one boundary. These are tradeoffs from keeping the local environment very similar across the treated and control groups

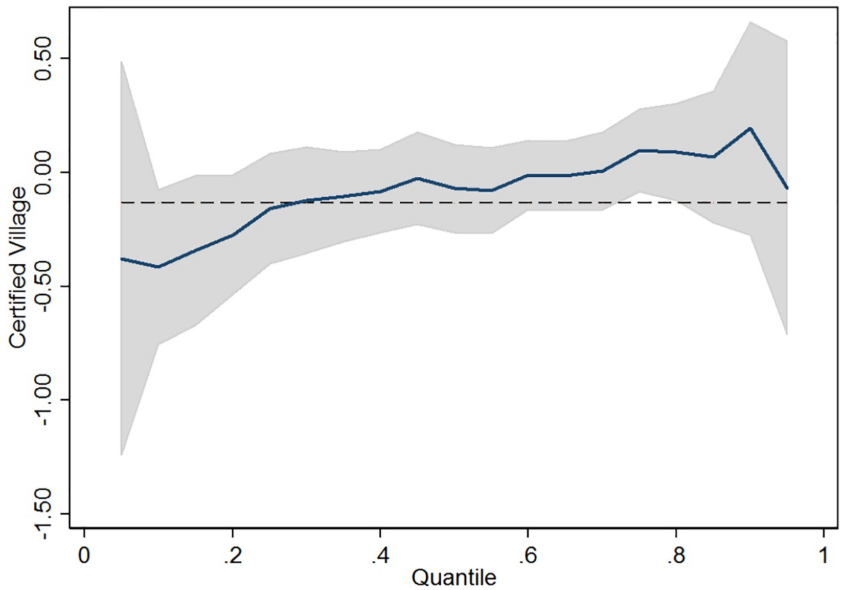

Fig. 4. Quantile regression coefficients. Notes: The figure presents results from a quantile regression for the median including both Yaka and non-Aka households using the *grqreg* command (Azevedo, 2004). The outcome variable is the natural log of household's asset value in FCFA. The regression includes a dummy variable for female-headed households and non-Aka ethnicity. The dashed line is the OLS coefficient for the Certified Village dummy variable. The blue line connects the quantile regression coefficient estimates for Certified Village for each decile and the shaded area is the 95% confidence interval. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

and ensuring good Yaka representation. These results may not generalize to other settings, particularly those with strong institutions where indigenous peoples can seek legal protection from harm.

Reduced food security and wealth run counter to the goals that catalyzed the creation of FSC and made it such a promising policy innovation in the nineties. It is unlikely that these results come from an explicit agenda by the certified timber company to harm indigenous peoples. Indeed, the certified firm pioneered innovative policies for indigenous peoples and may have certified, in part, due to an environmental ethos among firm leadership. It is more likely that these results are an unintended consequence of some FSC activities, including road-building and restricted hunting and forest use, and that activities were selected because they are easy to monitor and they reflect auditor biases and non-Aka preferences.

More broadly, these results add to an emerging body of evidence that shows that despite tough criteria and credible monitoring, FSC certification of commercial timber firms may fail to change behavior in desired ways. Blackman et al. (2018) find no evidence that participation in FSC changes deforestation rates in Mexico. Doremus (2015) finds no change in forest degradation due to firms gaming the auditor's baseline in Cameroon. Nebel et al. (2005) finds no change in harvesting practices in Bolivia. In contrast to these cases, which find no effect, this paper finds FSC activities likely had a negative impact on indigenous peoples.

Given this, FSC may be more useful in credibly revealing the firms that find it less costly to practice less destructive harvesting. Doremus (2018) finds that forests with the lowest opportunity cost participate in FSC and Foster and Gutierrez (2013) find that FSC is useful for targeting local monitoring resources. However, FSC as a signal for less destructive harvesting is insufficient for ensuring use rights and well-being for indigenous peoples. When asked to compare the past to today, Maindja, a 45 year old Aka grandmother, said “Our life has turned upside down! And nobody cares. If we walk in the forest we are taken by eco-guards… Listen! We don’t eat meat anymore! This is what the state has done to us” (Lewis, 2016: 376). Given auditor reliance on documentation, for FSC to succeed certified forests should develop and monitor locally appropriate measures of indigenous peoples' well-being.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.ecolecon.2019.106378.

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