Do Cities Impact Gender and Family Norms in Africa?*

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January 19, 2022

1 Introduction

Around the world and certainly in Africa, city dwellers appear to differ from rural populations in their norms about gender and family. Patriarchal family structures are often followed more closely outside of cities, yet at the same time rural women frequently perform work outside of the household. This project seeks to provide causal evidence about the impacts of access to cities in rural Africa on gender and family norms.

We study the randomized rollout of a program promoting urban access in rural villages in the Democratic Republic of the Congo (DRC). Implemented by a local NGO called Congo Helping Hands (CHH), this 'City Access Program' (CAP) provides regular weekly transportation by motorbike taxi to the city of Kananga to individuals living in rural villages surrounding the city. CHH's City Access Program has two different components, which form the treatment arms of our study. In a first 'market' arm, CHH provides weekly transportation directly to Kananga's central market, allowing villagers to sell produce and buy goods there as they please. In a second 'social' treatment arm, CHH provides villagers weekly transportation to the city along with an invitation to attend a church group. Churches are the main hub of social networks in Kananga and many African cities. Our project studies the effects of CHH's programs on individuals' beliefs and values.

*This study has been approved by the Harvard Institutional Review Board (Protocol IRB17-0724) and the University of British Columbia Behavioural Research Ethics Board (Protocol H22-01318) and is funded by the National Science Foundation, the Weiss Fund for Research in Development Economics and the John Templeton Foundation.

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2 Background and Setting

The study takes place in the city of Kananga, in the Kasaï Central Province of the Democratic Republic of Congo (DRC). Kananga, a city of roughly 1.6 million (the fourth largest in Congo), is the seat of the Provincial Government of Kasai Central. Transport infrastructure in Kasaï Central is in severe disrepair, due to heavy rain and a lack of maintenance. As a result, transportation in rural areas is difficult even for 4x4 vehicles. Traveling 50 kilometers out of the city can take up to 4 hours on a motorbike. But most villages are unable to afford motorbikes or other forms of transport, and so they spend days walking to reach the city, or they simply remain in their villages. Congo Helping Hands' City Access Program was designed to help solve this problem.

3 Data

3.1 Research Design

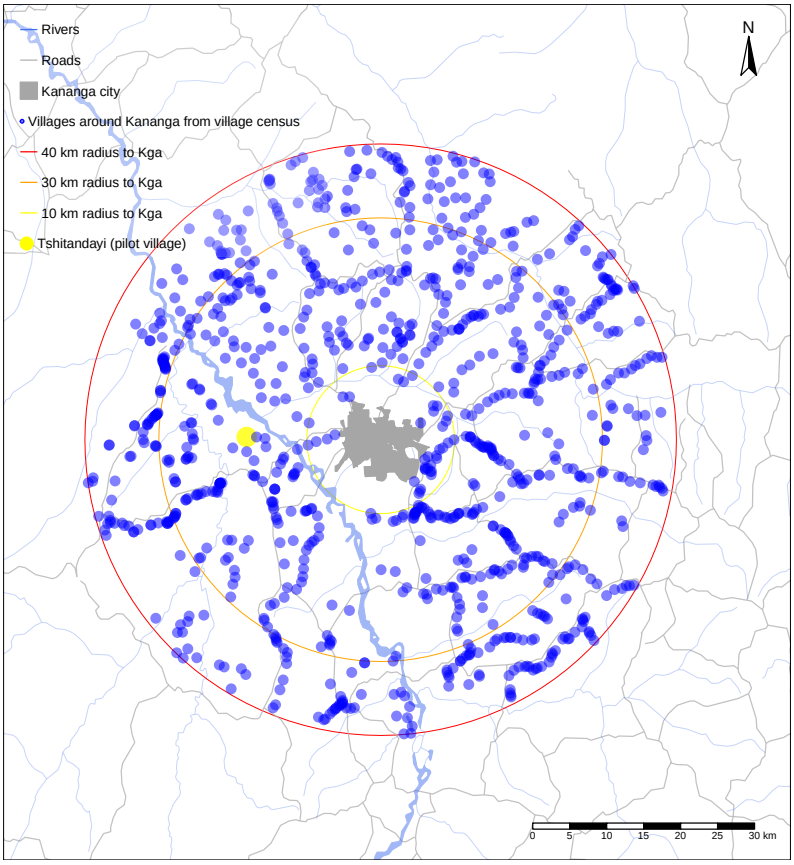
We study Congo Helping Hands' City Access Program, which aims to increase access of rural villages to Kananga. The program provides personalized round-trip transportation to and from Kananga for individuals living in rural villages around the city.[1] The City Access Program has both 'market' and 'social' components. Individuals in the market arm receive transportation directly to Kananga's central market and are invited to transport goods if they like, or to buy products they could resell in the village. Individuals in the social arm receive transportation along with an invitation to join an urban church congregation.

CHH agreed to randomize villages into the 'market' or 'social' arms of their program or to a control group of otherwise similar villages. We collaborated with CHH to achieve a randomization that will enable an impact evaluation of the program. Sampling of respondents and random assignment of villages into the treatment arms occurs in several steps. First, using satellite data and driving time data, we identified all villages that are less than a 3-hour drive from the city's limits. We conducted a village census to collect basic information such as village size and accessibility (Figure 1). We then worked with CHH to identify a set of 300 villages that would be eligible for their program according to the following criteria: (*i*) accessibility by motorbike, (*ii*) a population of fewer than 300 families (where access to services found in cities is especially limited), and (*iii*) continual settlement all year round (rather than only during harvest season, e.g.). We selected the 300 villages that are closest to Kananga by straight line distance, but further than 10 km from the city centre, that fulfilled these criteria.

Second, in all eligible villages, our enumerators randomly sample households and invite

[1] The treatments are similar to the transport subsidy analyzed by Abebe et al. (2021), with the key difference that we study rural-to-urban transport rather than transport within cities.

Figure 1: Map of village census around Kananga



This map shows the 988 villages mapped in our village census.

them to participate in a baseline survey. Enumerators follow a village-specific house skip pattern to conduct a screening survey. Based on the screening survey, we randomly select main respondents for the baseline survey. Since the CHH program works with couples, we randomly select three couples, i.e. six main respondents per village.[2]

To enable estimation of spillovers, our enumerators also conduct a shorter baseline survey with additional individuals with and without connections to the main respondents. They interview (*i*) one close friend of the main respondents, as revealed in a social network module, and (*ii*) two additional randomly sampled individuals without connections to the main respondents in each village.[3] The survey will enable us to estimate spillover effects on non-participating individuals connected through social networks to participating individuals as well as more generalized spillover effects on individuals sampled randomly in the village.

[2] Note that this sampling approach generates random variation in the share of the population that is treated. We will use this random variation to explore if treatment and spillover effects are more pronounced if a larger share of the village is treated.

[3] All of these surveys occur before villages are assigned to treatment or control, allaying concerns that enumerators' sampling or respondents' availability could be endogenous to treatment.

Third, we randomly assign villages to the two treatments or to control. We stratify the randomization on (*i*) distance from Kananga, and (*ii*) village size.[4] Once the treatments are randomly assigned at the village level, CHH staff invite the main respondents to participate in their program. Table 1 summarizes the numbers of participants across all treatment arms. There are 100 villages in the each of the three treatment groups (including control). In each village, there are six main respondents, or 600 total participants. With six main respondents, up to six network respondents, and two pure control respondents in each village, we expect a full sample size of around 4,200.

To shed light on the mechanisms of the market arm, we randomize the location of selling at the market within the market arm at the village level. The 'retail' and 'wholesale' sub-treatment arms should introduce further variation in wholesale and retail selling, the number of interactions with customers, and the probability of having repeat customers. The villages in the 'social' arm are randomly assigned to one of 30 churches that CHH works with, which are broadly representative of the landscape of churches throughout Kananga.[5] CHH works with the largest churches in Kananga of different denominations, such as Pentecostal, Protestant, Neo-Apostolic, and Kimbangu. We see this natural heterogeneity of denominations, doctrines, and practices as an asset to our investigation of the program. We plan to examine heterogeneous treatment effects of this treatment as we describe in more detail below (see Section 3.2).

Table 1: Allocation of units across treatment groups

	Urban social treatment	**Urban market treatment**	**Pure control**
Main Respondents	600	600	600
Network Respondents	600	600	600
Non-Network Respondents	200	200	200
Total Respondents	1,400	1,400	1,400
Villages (clusters)	100	100	100

Finally, we plan to collect an endline survey in all villages with the same set of 4,200 respondents sampled at baseline. These surveys will be conducted roughly six months after the conclusion of the CHH programs (in treatment villages and nearby control villages).

[4] Note that this generates geographical variation in distance to other treated and control villages. We will use this random variation to explore spillover effects across villages.

[5] The one exception is that CHH does not work with the Catholic Church because of logistical problems: there are only Catholic services in Tshiluba—the only language understood by most rural residents—at 7 am on Sundays, which is too early for the villagers to arrive on time. Later services are conducted in French without Tshiluba translation.

3.2 Other Data

We collect additional data to study mechanisms and alternative hypotheses:

1. Administrative data on the City Access Program collected by Congo Helping Hands staff in both the market and social arms. These include weekly data on attendance and other details on participation (e.g., the goods bought and sold).
2. Village census around Kananga. Collected by our enumerators, these data provide information about the location and amenities in villages around Kananga.
3. Chief village survey. Collected by our enumerators, these surveys ask the chief about the village and its history.
4. Church census in Kananga. Collected by our enumerators, these data provide basic information about the size and denomination of all houses of worship in the city.
5. Pastor surveys. Collected by our enumerators at a subset of the largest churches in the city and in all villages, this survey focuses on doctrine and congregant details.
6. Church service recordings and surveys. We also collect audio recordings of church services to enable text analysis of their content. Enumerators also record the elements and proceedings of services.

4 Analysis

Around the world and certainly in Africa, city dwellers appear to differ from rural populations in their norms about gender and family (Evans, 2018, 2019). Traditional practices associated with marriage are followed more closely outside of cities. While women in rural areas do agricultural work, focus group discussions we conducted in Kananga showed that there can be a large heterogeneity in female labor force participation and attitudes about marriage, gender, and family norms in the city.

However, past work on the urban-rural gap cannot easily distinguish selection from causal effects of cities. Because it is difficult to find exogenous variation in exposure to urban areas, the observed differences noted in the previous paragraph could simply arise from the sorting of different types of people into different areas. The random assignment of Congo Helping Hands' City Access Program will help fill this gap. By comparing participants in treated and control villages, we can bring experimental evidence to bear on claims about the impacts of cities on gender and family norms. The program also helps us shed light on the drivers behind any observed differences between urban and rural populations. By comparing market and social group participants and exploiting church heterogeneity in the social arm, we can disentangle income effects and exposure effects from church doctrine effects.

We measure such norms using survey modules on the role of women and girls, marriage traditions and preferences, and discord within the household. The aforementioned literature

predicts that exposure to cities will lead participants to adopt more liberal views about women and marriage and simultaneously to view customary practices as less important. While both arms of the experiment will offer evidence on this relationship, we expect more heterogeneity in the social arm according to the specific doctrine and norms espoused during church services. In particular, certain churches, such as the Pentecostal Branhamist church, promote strict patriarchal beliefs — e.g., that women should primarily tend to the children and household, that they should be obedient to their husband, that they wear long skirts, etc. These beliefs might in fact be more patriarchal than traditional beliefs in Congo before contact with European missionaries. We therefore anticipate considerable heterogeneity when we examine the effects of exposure to urban churches on rural participants' family and gender norms. Our analysis will ultimately be guided by the specific doctrines and practices that our data collection reveals to characterize the participating churches in CHH's program. That is, we will use the detailed data on church sermons and doctrine that we are collecting to characterize the beliefs espoused by each church and then examine heterogeneous effects of the social treatment arm accordingly.[6]

5 Heterogeneity

We plan to investigate the following as sources of heterogeneity in the impact of the CAP on outcomes:

1. *Distance to Kananga.* The City Access Program is more of a shock to villagers' access to Kananga in more remote villages. We therefore anticipate larger treatment effects farther from Kananga.

2. *Market landscape in the village.* Participants vary in their baseline access to markets. We expect more pronounced treatment effects of the market arm where participants had less access to markets before the CAP. We will use data from our initial village census as well as baseline surveys to measure market access.

3. *Religious landscape in the village.* Participants vary in their baseline access to churches and religiosity. We expect more pronounced treatment effects in villages with less prior exposure to Christian churches, especially Pentecostal churches (which remain predominantly an urban phenomenon). We will use baseline data on participants' religiosity as well as data from the village census and chief survey to estimate access to churches,

[6] As a different example, many churches in Kananga emphasize household harmony as a key objective of their teachings. We would thus expect any negative causal effects on household conflict to be more pronounced in churches that focus on these teachings.

including mission stations. We will also explore how treatment effects vary by specific types of religious beliefs held by participants. Generally, there may be two countervailing forces at play: those with prior beliefs more concordant with those espoused at the urban church might be more inclined to participate every week, which would magnify effects; but, at the same time, the treatment would also be less novel for this subgroup and likely have a smaller effect. Which of these effects dominates is an empirical question we hope to explore using program administrative data on attendance and a combination of baseline and endline data on beliefs.

4. *Urban church doctrine and practices.* The 30 churches participating in the CAP are heterogeneous in their doctrines, practices, and social networks. As noted throughout, we therefore anticipate studying heterogeneity by different types of beliefs, practices, and other church characteristics. We will use detailed data from surveys with pastors as well as recordings of sermons and church service proceedings to characterize this variation and study its heterogeneous impacts on outcomes.

5. *Agricultural productivity.* Among the villages participating in the CAP, there are different climatic zones with variable suitability for different crops that can be sold in Kananga. We have natural variation in these crop suitabilities and the seasons during which the CAP was running. We can use this variation to study whether villages in zones with suitabilities for more lucrative crops conditional on the season exhibit more pronounced treatment effects.

6. *Exposure to Kamuina Nsapu.* A recent violent conflict, known as the Kamuina Nsapu insurgency, triggered large-scale displacement and claimed thousands of lives. We expect impacts of the program on welfare to be more pronounced in areas that were more affected by this violent conflict.

7. *Time gap before endline survey.* Because of the staggered rollout of the intervention and endline survey, there will be natural variation in the time gap between the two. We will use this variation to study whether treatment effects decay or persist over time.

8. *Duration and frequency of attendance.* We expect stronger effects where participation was exogenously higher. Although participation may often be endogenous, we will explore exogenous shocks like weather, pregnancy, and family deaths as exogenous shifters of participation to obtain variation in treatment intensity.

9. *Village size.* We have natural variation in the size of villages and thus the share of

the village that is treated by the CAP. We can use this variation to study spillovers to non-participants in the treatment village. For instance, we can assess whether such spillovers are larger when a larger share of the village is treated, and whether we find evidence for tipping-point effects.

10. *Age.* Research often finds that children and young adults are more plastic in their beliefs than the elderly. Although we do not have children or young adults in our sample, we will examine whether younger participants are similarly more responsive when examining belief outcomes.

11. *Gender.* Women and men often have distinct economic roles. For instance, in focus groups, we learned that some agricultural products are typically sold by women, while others are typically sold by men. This means that the market arm might have differential effects by gender — if for instance the type of customers with whom men and women interact in the city different because of the products they sell (or for some other reason). Similarly, churches often discuss gender and family issues extensively in sermons, and these discussions might impacts the sexes differently. Some churches have gender segregated seating or activities. We will therefore explore gender heterogeneity.

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