

THE EFFECTS OF CITIES ON PSYCHOLOGICAL AND BEHAVIORAL TRAITS.*

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January 19, 2022

1 Introduction

As economists have focused on how cities promote economic growth by facilitating knowledge spillovers, human capital accumulation, agglomeration effects, and production technologies with increasing returns to scale, sociologists and anthropologists have highlighted another key impact of cities: their effects on psychological and behavioral traits. However, past work cannot easily distinguish selection from causal effects of cities. This project seeks to provide causal evidence about the impacts of access to cities in rural Africa on individuals' behaviors and psychological traits.

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

*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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programs on the psychological traits of its participants.

2 Background and Setting

The study takes place in the city of Kananga, in the Kasai 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 Kasai 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

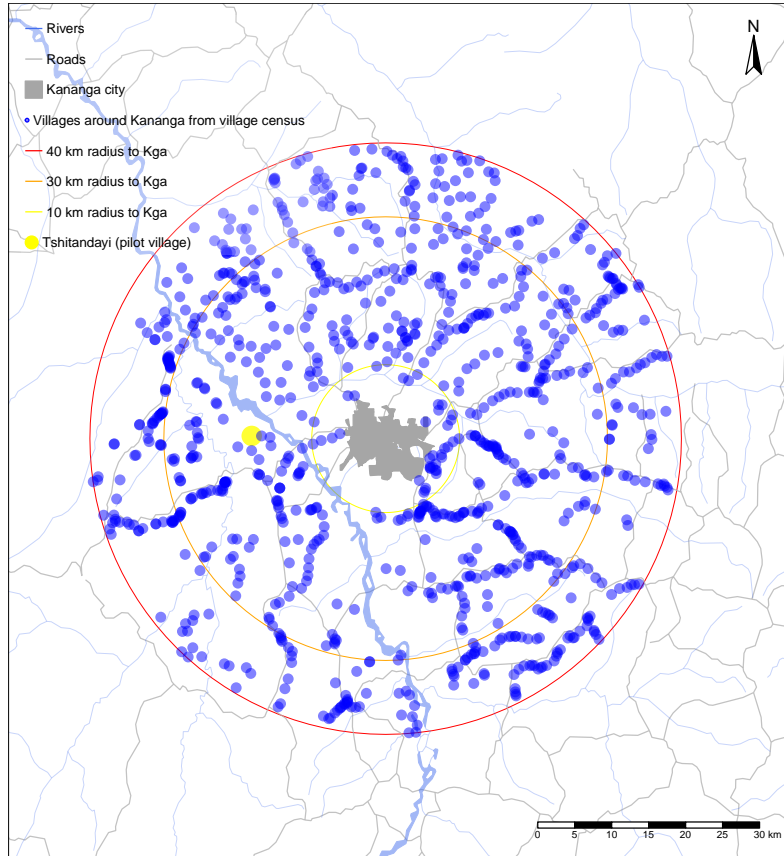
We study Congo Helping Hands’ City Access Program, which aims to increase access of rural villages to Kananga. The program provides personalized round-trip transportation to and from Kananga for individuals living in rural villages around the city.¹ The City Access Program has both ‘market’ and ‘social’ 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

¹The 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.

Kananga by straight line distance, but further than 10 km from the city centre, that fulfilled these criteria.

FIGURE 1: MAP OF VILLAGE CENSUS AROUND KANANGA



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

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

²Note 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.

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.

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

³All 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.

⁴Note 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.

⁵The 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.

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).

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

The City Access Program of Congo Helping Hands provides a rare opportunity to provide causal evidence on well-established ideas in the literature on cities, culture, and development. As economists have focused on how cities promote economic growth by facilitating knowledge spillovers, human capital accumulation, agglomeration effects, and production technologies with increasing returns to scale (Romer, 1990; Lucas, 1988; Glaeser and Maré, 2001; Greenstone et al., 2010; Bryan et al., 2014), sociologists and anthropologists have highlighted another key impact of cities: their effects on psychological and behavioral traits (Gugler et al., 1978; Henrich et al., 2004; Henrich, 2014).

During focus group discussions we conducted in Kananga, participants often asserted that city dwellers have a vastly different *mentalité* from those in rural areas.⁶ Those from the city are thought to have greater perseverance, grit, self-control, and self-efficacy. 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

⁶Similar urban-rural differences have been documented in the United States (Enke, 2019).

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 these claims about the impacts of cities on psychology.

Our experiment also affords opportunities to investigate specific mechanisms that could explain such causal effects. First, a large and varied literature going back at least to Max Weber posits effects of religion on psychology. Advocates of the [Weber \(1958\)](#) hypothesis argue that the Protestant Reformation caused a mental revolution that resulted in modern capitalism. The view propagated by Protestantism broke with traditional orientations because of its increased emphasis on personal diligence, frugality, thrift, and individual responsibility ([Iannaccone, 1998](#)). Relatedly, a growing ethnographic literature in anthropology argues that urban pentecostal churches in Africa are changing believers' psychology and basic outlook on life.⁷ This literature argues that urban pentecostal churches have promoted grit, self-control, self-efficacy, resilience, and economic optimism. Given that the modal participating church in CHH's program is pentecostal — and many non-pentecostal churches have responded to the popularity of pentecostal preachers by emulating aspects of their doctrine and practices — the field experiment offers a unique opportunity to bring causally identified evidence to bear on this hypothesis.⁸

Additionally, many have argued that religious participation improves mental health. Marx famously (and censoriously) called religion the opiate of the people. Religion — and, in particular, giving oneself up to a higher power — is a key part of the 12 steps of Alcoholics Anonymous. A meta-analysis reports largely positive correlations between religion/spirituality and mental wellbeing ([Chen and Koenig, 2006](#)). However such correlations could reflect causality in either direction, as it is common for individuals to turn to religion in times of stress.⁹

Moreover, others have argued that certain religious beliefs can have negative impacts on mental health. For instance, in Weber's famous account, the Calvinist uncertainty about whether one has been chosen to be saved can lead to misery and existential angst. One might imagine a similar version of this hypothesis in the case of some of the beliefs attributed to Pentecostalism above. For example, in a society like DRC with high poverty and low economic mobility, hearing in sermons that you are responsible for your economic (mis)fortune might cause people to feel stressed and worthless. That said, our focus group participants also

⁷Some of the key citations in this literature include [Smith \(2001\)](#); [Martin \(2002\)](#); [Meyer \(2004\)](#); [Ranger and Ranger \(2008\)](#); [Marshall \(2009\)](#); [Van Dijk \(2012\)](#); [Freeman \(2012\)](#); [McCain \(2013\)](#); [Swidler \(2013\)](#); [Ojo \(2015\)](#).

⁸However, as we note below, we plan to explore heterogeneous treatment effects of the social arm according to the specific doctrines and practices that our data collection reveals to characterize the participating churches in CHH's program. One would expect the predictions of this ethnographic literature to be more pronounced among churches that more strongly emphasize self-control, self-efficacy, and individual responsibility for one's economic wellbeing.

⁹For instance, [Chen \(2010\)](#) provides evidence that negative economic shocks increase demand for religion.

emphasized the importance of the idea of rebirth — a clean break from past misfortune and a chance to start anew — as a source of hope and a reason for the popularity of Pentecostal churches in Kananga. We therefore plan to examine heterogeneity by the types of beliefs espoused during services by different churches participating in the CAP.¹⁰

Our survey also allows us to explore several potential mechanisms (or zero-stage outcomes) behind some of the overall impacts of city access on psychology and wellbeing. For instance, an increase in Christian religiosity, and in the specific set of beliefs associated with Pentecostalism, is the implied mechanism behind the ethnographic literature noted above (e.g., [Freeman, 2012](#)). As such, we collect detailed data on religious participation, beliefs, and religiosity. In addition, many urban churches emphasize the importance of refraining from the use of alcohol, cigarettes, and drugs. By contrast, urban areas might also present participants with more opportunities to access alcohol, cigarettes, and drugs. We thus also measure substance abuse via surveys and alcohol consumption experimentally. Finally, church teachings that one’s ability to be saved and reach heaven is independent from the probability that others are also saved could also counteract a zero-sum view of the world ([Foster, 1965](#)). We measure zero-sum beliefs using survey questions.

There is perhaps less past work exploring how exposure to markets specifically impacts psychology and mental health. Better market access could impact wellbeing through an income effect and an occupational shift away from strenuous agricultural labor (e.g., [Coye, 1985](#)).¹¹ However, exposure to the market — and the city more generally — might also alter participants’ beliefs about economic mobility, i.e., the chances they and their children could advance up the economic ladder ([Henn and Robinson, 2021](#)). As [Banerjee and Sequeira \(2020\)](#) document, it may be the case that villagers are in fact over-optimistic about mobility, in which case learning such “bad news” could offset more salutary effects on wellbeing generated by greater access to the city. The net effects of access to urban markets on psychology and mental health are thus empirical questions we will investigate in our analysis.¹²

We measure optimism, grit, self-control, resilience, self-efficacy, envy, zero sum beliefs, and locus of control using standard survey modules from economics and psychology.¹³ Addition-

¹⁰We also anticipate studying heterogeneity by exposure to a recent violent conflict. In 2017–2018, there was a violence insurgency in Kasai Central known as the Kamuina Nsapu, which triggered large-scale displacement and claimed thousands of lives. One might anticipate that any impacts of the CAP on mental health would be more pronounced in areas that were more affected by this violent conflict. We will thus examine heterogeneous impacts of the social arm by exposure to the Kamuina Nsapu insurgency.

¹¹CAP participants, especially those in the market arm, might benefit from access to greater supply of health care in the city. While city exposure could induce participants to substitute from traditional medicine towards modern medicine, a substitution towards other Christian faith-based medicine in the social arm is also plausible.

¹²Markets could also impact beliefs that the world is zero sum. For instance, if market exchange is truly positive sum, and the experience of such exchange is novel for participants, then assignment to the market treatment could reduce zero-sum beliefs.

¹³For instance, we include in the survey the Short Grit Scale from [Duckworth and Quinn \(2009\)](#).

ally, we measure perseverance, patience, and reliability through observational measures of survey performance and punctuality noted by the enumerator. We measure time preferences through a survey module and an experimental savings game. Closely related to optimism, we also collect beliefs about social and economic mobility using standard survey modules. We measure different dimensions of mental wellbeing: happiness, emotions, satisfaction with life, and depression through the SRQ-20 survey module.¹⁴

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, 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 het-

¹⁴We also ask a few questions about physical wellbeing and health care seeking behavior, both of which could be increased through placebo effects and for psychosomatic illnesses.

erogeneous 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 impact the sexes differently. Some churches have gender segregated seating or activities. We will therefore explore gender heterogeneity.

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