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Essays on Technology Adoption and Urbanization in East Africa

by

Daniel Agness

A dissertation submitted in partial satisfaction of the

requirements for the degree of

Doctor of Philosophy

in

Agricultural and Resource Economics

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Professor Jeremy Magruder, Chair
Associate Professor Marco Gonzalez-Navarro
Professor Christopher Walters

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Essays on Technology Adoption and Urbanization in East Africa

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Abstract

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University of California, Berkeley

Professor Jeremy Magruder, Chair

Low and middle-income countries are urbanizing rapidly, and nowhere is urbanizing faster than Sub-Saharan Africa. The rush of urbanization presents multiple challenges to policymakers, including how to accommodate rural-to-urban migrants in growing cities while simultaneously improving productivity and promoting food security for those who remain in the agricultural sector. This dissertation speaks to each of these areas. In Chapter 1, I study the intergenerational impacts of public housing expansion in Addis Ababa, Ethiopia – one of Africa’s fastest growing cities. In Chapters 2 and 3, I study technology adoption and agricultural infrastructure expansion in Kenya. Combining the two helps us to make sense of the simultaneous and potentially competing challenges faced by governments in low-income countries during a period of rapid structural transformation. Using tools drawn from the literatures on labor and urban economics, and combining large-scale survey activities with administrative and spatial data, the dissertation employs natural experiments, randomized controlled trials, and structural models to identify causal effects and answer policy-relevant questions.

In Chapter 1, “**Housing and Human Capital: Condominiums in Ethiopia**”, co-authored with Tigabu Getahun, we evaluate a major government response to the pressures of urbanization: the largest expansion of public housing on the African continent. The policy subsidizes homeownership, allowing households to move from low-quality housing or to use their unit as an income generating asset. We make use of the lottery mechanism that has been used to allocate these units to more than 200,000 households in Addis Ababa, Ethiopia, over the past 15 years. To do so, we combine extensive surveys from 2,987 households, surveys with children of affected households, and administrative data collected in collaboration with

policy partners to study how this policy has impacted the human capital accumulation of children.

We find large and positive effects of a housing policy in a low-income setting. Specifically, we show that children in lottery winning households experience 4.5-11% increase in school attendance, a 10.5% increase in secondary school completion rates, and a 16% increase in post-secondary school matriculation. Our surveys allow us to observe additional measures of human capital that are typically unavailable to researchers using only administrative data. Namely, we show evidence of improved cognition and socioemotional development for children in lottery winning households, as well as increases in aspirations. We make use of spatial and temporal variation to disentangle mechanisms and reach a novel conclusion: policy impacts are driven by children in households that own and occupy the unit that they win. This rules out the possibility that treatment acts through a wealth effect alone and suggests that neighborhoods of residence may play an important role. We then adapt a structural model of selection from new work in the policy evaluation literature to characterize patterns of selection and estimate margin-specific treatment effects. To our knowledge, our work represents the first use of these methods in the evaluation of a policy in a low-income setting.

My second chapter, “**New Technology and Network Change**”, uses dyadic regressions, a social network panel, and a randomized controlled trial to show how networks respond to the introduction of a new technology. Technology recipients become more central within village networks, driven both by differential maintenance of existing links and through the creation of new ones. I apply these results to a peer effects model with directed networks and show that failing to consider network change overestimates treatment effects and that failing to account for new connections due to the intervention underestimates the importance of peer effects.

In my third chapter, coauthored with Travis Baseler, Sylvain Chassang, Pascaline Dupas, and Erik Snowberg, “**Valuing the Time of the Self-Employed**”, we consider a critical but understudied component of policy evaluations – people’s value of their own time. Accurately estimating the value that the self-employed assign to their own time is essential for estimating the profitability and welfare impacts of interventions. The majority of the literature assumes this value to be zero, which may explain low adoption of seemingly profitable technologies that require increases in worker time. The market wage is likely to be a poor reference in low-income settings, where labor market frictions inhibit individuals from easily converting their time into a wage. To make progress on this question, we use choice data from farming households in Kenya and a structural model to identify behavioral wedges in choices. Accounting for these wedges, we estimate that valuing the time of the self-employed

at 60% of the market wage is a reasonable rule of thumb.

Together, the three chapters of my dissertation highlight the two-sided policy challenge presented by urbanization and structural transformation in East Africa. Applying econometric methods and experimentation, we focus on communities, both rural and urban, to shed light on how they function and adapt, and how they shape human welfare.

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Chapter 1

Housing and Human Capital: Condominiums in Ethiopia

1.1 Introduction

Modern urbanization is concentrated in low- and middle-income countries (LMICs): in the past two decades, they have been urbanizing 4-8 times faster than North America and Europe (UN-HABITAT, 2022). This is particularly true in Sub-Saharan Africa, where structural transformation away from agriculture has rapidly increased the shares of populations living in urban settings. By 2050, 1.5 billion Africans will live in cities, nearly triple the number of urban Africans today (Haas et al., 2023). As urban populations grow, so too does demand for all types of urban infrastructure, foremost amongst which is housing. But housing construction has struggled to keep up with rapid city growth, leaving tens of millions of urban residents living in slums and informal housing (Marx et al., 2013; Laros and Jones, 2014).

Governments across the globe have responded to this housing crisis with large investments in public housing, often located in the peripheries of major cities. While these investments will play a major role in determining the shape and function of developing cities, the real allure of these programs lies in their potential to provide a stable foundation for families who otherwise would have been living in low-quality, “slum” housing.¹ However, evaluations of housing lotteries and rental subsidies in low-income settings generally conclude that they

¹We use the term slum in a manner consistent with the UN-HABITAT definition (UN-HABITAT, 2002). Households are said to live in a slum if their residence lacks one or more of the following five elements: 1) access to adequate drinking water; 2) access to adequate sanitation; 3) housing with adequate space; 4) housing with adequate structure to protect against climatic conditions; 5) secured tenure.

fail to be transformative, with null or negative impacts on most household-level economic outcomes, (Galiani et al., 2017; Barnhardt et al., 2017; Franklin, 2020b; Rojas Ampuero and Carrera, 2022; Belchior et al., 2023) echoing findings from the United States and Europe (Kling et al., 2007; Van Dijk, 2019).

How could policies that address a need as fundamental as housing be so inconsequential? One potential explanation is that by focusing on adults, much of the previous literature misses policy impacts on the population that has been shown to be most sensitive to changes in home quality and neighborhood of residence: children (Chetty et al., 2016; Chyn, 2018; Kumar, 2020; Rojas Ampuero and Carrera, 2022; Camacho et al., 2022). Particularly in LMICs, where administrative data is scarce, the impacts of housing policy on children, and long-run policy impacts more generally, remain understudied.

An alternative proposed in the literature is that decreases in social cohesion, labor market access, and public service quality associated with relocation to far-flung neighborhoods, which depress policy adoption, ultimately outweigh improvements in home quality. Testing this hypothesis is confounded by policy environments that leave households with large choice sets, even conditional on a randomized program offer: households first choose whether to take up a program, then choose the neighborhood in which to live (Heckman and Pinto, 2018). In cases where policies involve home ownership, as opposed to a rental subsidy, households further choose whether to occupy, rent out, or sell their unit. This sequence of endogenous choices implies that the typical reduced form, intent-to-treat analysis employed in the literature may disguise heterogeneity across “hidden treatments” (Rothstein and Von Wachter, 2017) that depend on the full set of household choices.² Disentangling this heterogeneity is essential for understanding mechanisms and estimating policy counterfactuals. We provide evidence supporting both of these explanations.

In this paper, we use a natural experiment associated with the largest expansion of public housing on the African continent to answer two questions: (1) How do shocks to neighborhoods of residence and parental wealth impact the human capital development of children? (2) What are the relative contributions of changes in neighborhood versus changes in wealth? Our project combines new survey data, matched administrative data, reduced form and policy-derived instrumental variable impact analysis, and a structural selection model to understand the policy’s medium-run impact on children and families. Through a partnership with the Policy Studies Institute in Ethiopia (PSI) and the Addis Ababa Housing Development Agency (AAHDA), we conducted an extensive household survey with 2,987 households, drawn from the universe of applicants for subsidized condominium units in urban Addis Ababa, Ethiopia. We combine our household surveys with supplementary

²In our setting, households can own and occupy, rent out, or sell their unit. The causal effects of these outcomes are pooled in ITT analysis.

data on wages, firms, neighborhood amenities, and administrative budgets. With these data, we are able to study a battery of outcomes typically unavailable to researchers relying on administrative data alone (Chetty et al., 2016; Chyn, 2018).

Since its inception in 2005, the policy has been massively oversubscribed; an estimated 50% of all households in Addis Ababa have registered for the program, with more than 900,000 applications to date. These applicants were all urban residents at the time of their application, generally living in low-quality homes near the city center. Through 2023, approximately 210,000 units were completed and transferred to residents via random lottery in 14 lottery rounds. Lottery winning households have the opportunity to purchase a subsidized unit, paired with a low-interest mortgage through the city administration, provided that they are able to make a 20% down payment upfront. These condominiums are spread throughout the city; the majority are located 8-12 kilometers from the city center, while others are located in Addis Ababa’s urban core.

Our analysis relies on the lottery mechanism employed by the AAHDA to assign subsidized condominium units to applicants. The lotteries in our study are for home *ownership*, not rental, which distinguishes it from most policies studied in high-income settings (Van Dijk, 2019; Chyn, 2018; Chetty et al., 2016; Pinto, 2021). This common feature of housing policy in lower-income settings expands household decision sets – they can occupy, rent, or sell their unit (Barnhardt et al., 2017; Kumar, 2020; Belchior et al., 2023). We compare children in lottery winning households to those in similar households that remain on the waitlist for a condominium unit. Critically, the location of the winning households’ units and the lottery round in which they win are exogenous, allowing us to use spatial and temporal variation to disentangle impacts and understand mechanisms.³

We show that nearly all winning households purchase the unit that they won. Nearly perfect take-up, conditional on winning, is due to the substantial subsidy associated with winning a unit (Franklin, 2020b). In our sample, 82% of winning households still own the unit that they won an average of 8 years after winning. However, many winning households chose not to move into their unit: 35% rent out their unit, 17% sell (often before the 5-year embargo had elapsed, suggesting it was not enforced), and a small share either leave the unit unoccupied or allow it to be used rent-free by friends and family. The remaining 40% of the winning households own and occupy the unit they won. We expect treatment effects to vary with this decision: only households that move into condominiums will experience the change in housing quality and neighborhood characteristics attributable to the policy, but all winning households experience an increase in wealth via a government-subsidized

³While households can choose the number of bedrooms in their unit, the lottery round in which they win and the unit’s location are random. This policy approximates “double randomization” (Graham, 2018), whereby households are randomly grouped and randomly assigned to a neighborhood.

asset. Consequently, our reduced form estimates of the average treatment effects of winning a lottery pool impacts driven by direct exposure to condominiums and their associated neighborhoods with impacts due to increases in familial wealth – a “neighborhood” effect and a wealth effect. To separate treatment channels, we develop a structural model adapted from the policy evaluation literature (Kline and Walters, 2016; Mountjoy, 2019) to account for the fact that, conditional on winning, households make an endogenous choice of whether to occupy the unit that they win.

An average of 8 years after their lottery, winning households live in better neighborhoods in terms of public infrastructure and in higher quality homes. However, these neighborhoods are farther from the city center, relatives, and close friends. This result is consistent with previous work that highlights the potential for housing policy to disrupt social networks (Barnhardt et al., 2017; Harding et al., 2023; Rojas Ampuero and Carrera, 2022).⁴

We next show that winning a condominium lottery meaningfully improves child outcomes across a range of measures associated with children’s human capital: school enrollment, educational attainment, cognitive skills, aspirations, and socioemotional development. The policy increases active educational enrollment for children of winning households by 4.5-11%, secondary school completion rates by 10.5%, and post-secondary attendance rates by 16%. Increases in attendance rates are greater for older children, for whom school attendance is no longer compulsory, and are increasing in years of childhood exposure to the policy. These impacts on educational attainment are greater than many school expansion programs, Head Start in the United States (Bailey et al., 2021), and are about half as large as some of the most generous scholarship programs (Duflo et al., 2021). Despite large increases in educational attainment, we find no evidence that children of winning households are attending schools of differential quality.

In the sample of 6-17 year old children that we interviewed directly, we see substantial gains in measures of cognition and aspirations an average of eight years post-lottery. Specifically, we see that children in winning households score significantly better on Raven’s matrix tests and complete a numerical Stroop exercise faster, and more accurately. Children in lottery winning households are additionally more likely to aspire to an advanced degree or an occupation that requires an advanced degree, are more confident in their academic performance, are more optimistic about their future, and more satisfied with the neighborhood in which they live. Finally, we find small improvements in socioemotional development for male children as measured by the Strengths and Difficulties Questionnaire (SDQ) asked about children and administered to children’s parents. These results highlight policy impacts

⁴However, we find no evidence of thinner social networks for lottery winners in measures of neighborhood social connectivity and trust, which may be explained by measurement error in social connectivity or the capacity of social networks to develop in new sites.

that may be missed when looking only at traditional economic outcomes.

Previous research on this policy has found that it had limited short-term impacts on adult economic outcomes (Franklin, 2020b; Andersen et al., 2022) despite increases in household wealth. At the household-level, we consider many of the same outcomes an average of eight years after the lottery. While increases in household wealth and job transitions rates are similar to those found in Franklin (2020b), we find that lottery winning households have higher incomes, driven by heads of winning households being eight percentage points more likely to be formally employed. This increase in formal sector employment rates is caused by household heads leaving casual employment, not by changes in overall rates of employment. We show that the formalization and household impacts documented in our survey are increasing in years since winning the lottery, implying that the policy’s impacts on these outcomes may only accrue over longer time horizons. This implies that short-term evaluations may miss policy-induced changes in household welfare. This paper focuses on the potential externality to children and outcomes in the medium-run, neither of which have been previously studied in this setting.

Our results for children may be unsurprising if they simply represent a wealth effect: winning a condominium bequeaths households with a valuable, subsidized asset, dramatically increasing familial wealth. Understanding the extent to which our results can be explained solely through changes in parental wealth motivates two empirical approaches that move beyond the intent-to-treat effects estimated in our reduced form analysis.

To separate mechanisms – a wealth effect due to a randomly allocated subsidized asset versus an effect driven by exposure to improved housing and condominium neighborhoods – we first turn to an instrumental variables (IV) approach. The temporal and spatial variation in our setting allows for the creation of rich sets of instruments that influence the household’s decision of whether to *own and occupy* or capital (*rent out or sell*) the units that they win. Using these instrument sets, interacted with the lottery offer, enables us to identify a model with multiple endogenous treatment states under an assumption of constant complier effects (Hull, 2018; Kline and Walters, 2016; Kirkeboen et al., 2016; Pinto, 2021).⁵

In our preferred specification, using the difference between the realized distance to the winning condominium from the expected distance to all condominiums as an instrument (Borushak and Hull, 2020), we show that the positive effect on educational outcomes for children are driven almost entirely by children in households which choose to *own and occupy* the unit that they win. Households winning condominiums that are closer to their current residence than expected are significantly more likely to own and occupy their units, consistent with evidence on the preference for maintaining employment and social ties (Barnhardt et al.,

⁵Variation in multiple instruments creates different complier groups into each treatment. Constant complier effects assumes that treatment effects are identical across these complier groups.

2017; Franklin, 2020b), and suggests that variation in the quality of a match between a household and their assigned unit, reflecting household preferences over maintaining local connections, may lead to heterogeneous treatment effects and selection. These results imply that the intergenerational impacts of this policy cannot be explained by a wealth effect alone.

To relax the assumption of constant complier effects and characterize the nature of household selection into treatment states, we adapt a structural selection model with multiple, unordered treatments from the policy evaluation literature (Kline and Walters, 2016; Moun-tjoy, 2019; Heckman and Pinto, 2018; Kamat and Norris, 2020; Heinesen et al., 2022; Steven-son et al., 2023). Results from this model support the IV model, with treatment effects for children concentrated amongst those in families that own and occupy the unit that they win. We additionally document Roy-style selection: if anything, children in households with higher tastes for occupying their unit are less likely to attain secondary and tertiary educa-tion, but children in lottery winning households with high propensities for unit occupation experience larger gains in educational attainment.

The results of our study make contributions across three strands of literature. First, we contribute to the literature on the impacts of public housing and slum redevelopment. Policies to improve housing quality and remove slums are ubiquitous in low-income countries, (Franklin, 2020a; Michaels et al., 2021; Camacho et al., 2022) just as they were historically in the United States and Europe (LaVoice, 2013; Collins and Shester, 2013). We are the first to show large, positive impacts of a housing policy in a low-income setting (Barnhardt et al., 2017; Franklin, 2020b; Hoagland, 2020). Evaluating a policy that focuses on *ownership* rather than rental subsidies distinguishes us from more commonly studied programs (Kling et al., 2007; Oreopoulos, 2003; Chetty et al., 2016; Chyn, 2018; Van Dijk, 2019; Pinto, 2021) or those that limit a household’s ability to sell or rent out the unit that they own (Kumar, 2020; Camacho et al., 2022; Belchior et al., 2023). By documenting heterogeneity in treatment effects based on household choices, conditional on winning a unit, we are able to separate a wealth effect from one related to ownership and neighborhoods. Pooling these two have confounded policy evaluation of other programs (Pinto, 2021).

The relationship between unit proximity and occupation rates implies that in-site slum redevelopment policies may have much larger impacts on beneficiaries than redevelopment policies that expand housing in the city periphery (Lall et al., 2008; Camacho et al., 2022). Our results help to explain the negative effects found in evaluations of other slum redevelop-ment policies that mandate relocation to suburban neighborhoods, and support the findings of Camacho et al. (2022) that emphasize the importance of housing placement. They fur-ther suggest that an allocation mechanism that incorporates household residential location, matching households to close developments, would likely improve outcomes for lottery par-ticipants and their children. Beyond location, by considering outcomes in the medium-term, we are able to document household-level impacts that were not observed in a short-run eval-

uation of the same policy (Franklin, 2020b).⁶ We therefore add to the small set of papers on the long-run impacts of slum redevelopment policies in low-income countries (Picarelli, 2019; Michaels et al., 2021; Rojas Ampuero and Carrera, 2022; Belchior et al., 2023).

Second, we contribute to the literature on the intergenerational impacts of public policy. Failure to consider longer-term effects on children may dramatically underestimate a policy’s impact, (Bailey et al., 2020, 2021; Nakamura et al., 2021; Duffo et al., 2023) and previous studies on housing have generally focused on adults. A critical exception is the long-term analysis of the Moving to Opportunity (MtO) experiment, in Chetty et al. (2016) where the authors find large impacts of housing rental subsidies on income and educational attainment for children. The results from lower-income settings are mixed: Kumar (2020) shows that a housing lottery in India leads to only modest increases in measures of housing quality and assets, but children in winning households have higher employment and educational attainment; Camacho et al. (2022) shows substantial gains in educational attainment for children whose parents win houses in desirable Chilean neighborhoods; Rojas Ampuero and Carrera (2022) finds decreases in employment for children effected by a slum clearance program in Brazil. We offer the first long-term evaluation of a lottery for full homeownership on children, and consider measures of human capital that are unavailable in administrative data.

Finally, studies on the impacts of public housing generally emphasize intent-to-treat results that do not account for households’ endogenous response to treatment. Adapting new methods from the policy evaluation literature (Kline and Walters, 2016; Kirkeboen et al., 2016; Kamat and Norris, 2020; Lee and Salanié, 2018; Stevenson et al., 2023), we make use of the full set of information embedded in ex-post household responses to treatment in order to disentangle potentially competing mechanisms (Pinto, 2021). Our study represents one of the first applications of these methods to an evaluation of a policy in a low-income setting, and in doing so is one of the first to characterize the importance of neighborhoods and homeownership in a developing city (Michaels et al., 2021; Belchior et al., 2023).

1.2 Context

Ethiopia is one of the fastest urbanizing countries in the world; Addis Ababa, the capital, has doubled in size since 2000 and is expected to nearly double again by 2035 (Koroso et al., 2021). Rapid urban population growth has stressed the existing housing stock in Ethiopian cities, raising rental prices, and private sector development has not kept up with demand – over 70% of households in Addis Ababa live in slums or informal settlements (Franklin,

⁶Other than Franklin (2020b), the only other papers to study the Ethiopian housing lotteries are Andersen et al. (2020) and Andersen et al. (2022) which document how winning a lottery changes household preferences for redistribution and subjective well-being.

2020b). Beginning in 2005, with the rate of construction increasing rapidly since 2015, the Ethiopian government launched an ambitious public housing policy to build hundreds of thousands of residential units for urban dwellers in Addis Ababa. Through 2022, approximately 200,000 units have been built and occupied, with thousands more expected to be completed in 2023. Appendix Figure A.1.1 shows the total units built over time. The stated goals of the program were to provide housing for low- and middle-income urban dwellers and to support the domestic construction industry.

There were two rounds of registration for the lotteries, which took place in 2005 and 2013. An estimated 50% of all households in Addis Ababa registered for the program, with over 900,000 applications in total.⁷ Only one application was allowed per household, and eligibility required that the heads of household could not own property in Addis Ababa. Registrants were also required to have been residing in the city for at least six months at the time of their registration. Households were free to choose the size of the desired unit – studios, 1-bedroom, 2-bedroom, and 3-bedroom units – but not the unit’s location.

Critically for our research design, condominiums were allocated via random lottery. Due to over-subscription and limited construction capacity, the lottery was conducted in rounds as units were completed. There were 14 lottery rounds through 2022. The lotteries were random within pre-determined strata for female-headed households, government employees, and disabled households. Lottery winners were announced publicly in the media with substantial fanfare. The city government went to great lengths to ensure that lotteries were viewed as fair by the community, and there is no evidence of corruption in the lottery implementation for the rounds considered in this paper (Franklin, 2020b).⁸ The policy has not been without controversy, however, as condominium sites built in the city outskirts have spilled into land in the surrounding Oromia region, aggravating issues related to Addis Ababa’s urban sprawl.

In order to be eligible for the lotteries, after submitting an application, the households were required to open a tagged bank account with the Central Bank of Ethiopia (CBE) and to make deposits towards a down-payment. The required payments corresponded to the unit’s size and the down-payment program to which the household belonged.⁹ The households were not required to have completed the full down-payment at the time of the lottery, but needed to have made consistent deposits. Only after making the entirety of the

⁷Approximately 300,000 households registered in the 2005 registration round and the remaining 600,000 registered in 2013.

⁸There has been an accusation of lottery corruption in the 14th lottery round (Borkena, 2022). We do not sample winners from this round in our analysis.

⁹There are three program types: 10/90, 20/80, 40/60, where the first number is the percentage of the unit’s total cost that must be paid via down-payment. Households were mapped into down-payment programs via rough means-testing, with lower down-payments required for low-income households. All registrants from the first registration, and were part of our sampling frame, were in the 20/80 program.

down payment were households given the keys to their unit. The remainder of the total unit cost was financed via a low-interest mortgage at CBE. During the 11th round in 2019, the total condominium unit price was \$6,400 for a one-bedroom, \$8,800 for a two-bedroom, and \$11,700 for a three-bedroom. These prices represent, on average, a 40% subsidy relative to the cost of production per unit (Franklin, 2020b).

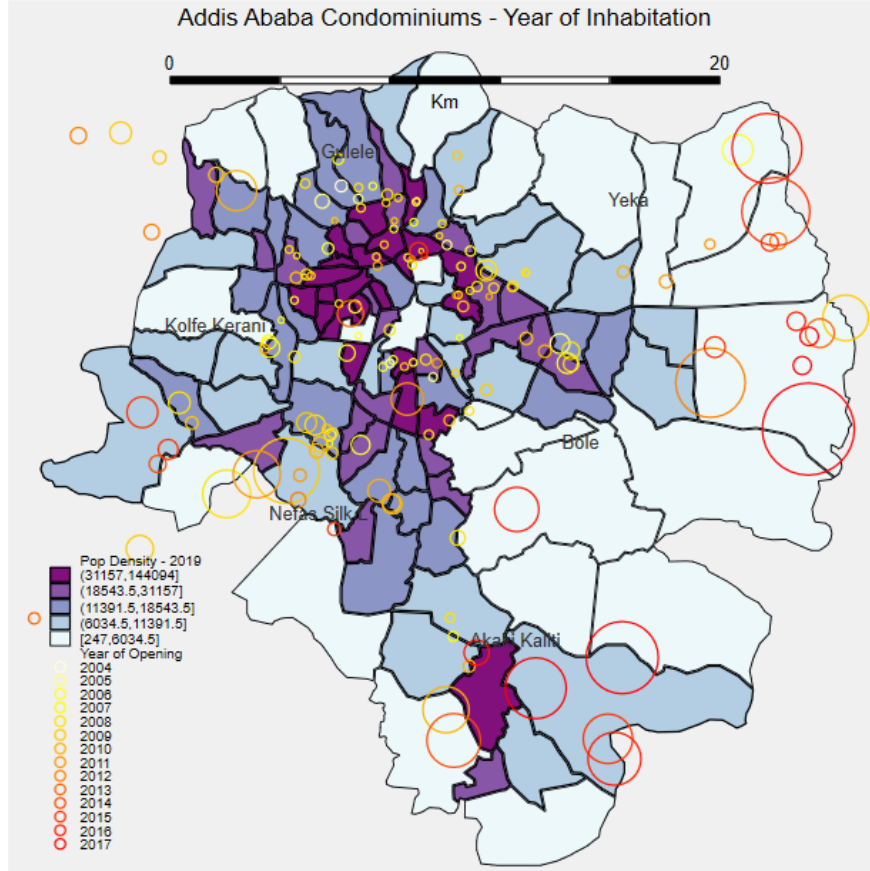
While early lottery rounds included more centrally located units, the condominium policy functionally reallocated families from low-quality, dense housing in the city center to higher-quality housing on the outskirts of the city. Due to their peripheral location, many condominium neighborhoods had worse labor market access, sparser social networks, and lower quality schools and infrastructure. Figure 1.1 shows the spatial distribution and timing of condominium openings in Addis Ababa through 2017. Recent developments are increasingly located in peripheral locations and are substantially larger than early developments, often consisting of 30,000 or more condominium units. Condominiums were virtually identical in size, quality, and appearance across sites. Anecdotal evidence suggests that some winning households were unsatisfied with the construction, but winning households were free to invest in upgrading their units.

Relative to public housing programs in North America that focus on moving families from “bad” neighborhoods to “good” ones (Kling et al., 2007; Oreopoulos, 2003; Chetty et al., 2016), and a similar policy in Colombia (Camacho et al., 2022), condominium neighborhood exhibit substantial heterogeneity along multiple dimensions of neighborhood quality. This heterogeneity makes the expected effects on households and child welfare unclear *ex ante*.

The design of the policy corrects for key margins of selection that confound the estimation of neighborhood effects. Typically, households make an endogenous choice of where to live, matching a household to a neighborhood. They similarly choose with whom they wish to live, leading to residential sorting across neighborhoods. In our case, the scope for neighborhood matching and residential sorting are diminished. Since households could be assigned to any condominium unit, those who choose to move into their unit are exogenously matched to a neighborhood. Similarly, a household’s neighbors in their new units are also randomly assigned, at least amongst the set of winners who occupy their unit.

However, after winning, there is no requirement that the household move into the unit that they win. That is, they are free to rent it out or leave it unoccupied. Although there was a technical requirement that households not sell their unit for five years after winning, this requirement was unenforced and often ignored (Andersen et al., 2020). Thus, there remains the potential for residential matching and sorting, such that the policy falls short of the ideal “double randomization” experiment as described in Graham (2018). This ideal experiment would only be possible under mandated relocation, which is infeasible in most settings.

Figure 1.1: Condominiums in Addis Ababa



The map divides Addis Ababa into woredas, the smallest formal administrative unit within the city. Woreda color represents population density; denser woredas have darker shading. Circles represent the location of condominium sites. The size of the circle represents the number of units in the site, and the color of the circle indicates the year the site opened, with darker colors being more recent.

1.3 Data

Through our partnership with the AAHDA we obtained the universe of condominium applicants, both winners and “waitlist” households who had yet to win a unit as of 2019. This administrative data was used as our sampling frame from which we sampled households to participate in our survey.

1.3.1 Sampling Frame

Before sampling the households for our survey, some cleaning of the sampling frame was required. We first excluded lottery rounds for which no winner contact information was available. We further excluded Round 13, which took place in 2020, as we believed this to have been too short a period to observe changes in key outcomes of interest. Round 14 was not included in the survey as it occurred after the project had started. We were left to draw our sample from 9 of the 14 completed lottery rounds.

We further limited the sample to the subset of households who had applied during the first registration period in 2005. The 2005 registrants were prioritized during the first 14 lottery rounds, and few of the 2013 registrants had won a unit by 2022. Finally, we excluded all households who applied for a 3-bedroom unit since nearly all of these households from the 2005 registration had won before the 13th lottery round, leaving few comparable controls. After these restrictions, we were left with 171,183 lottery winning households and 48,932 waitlist households in the sampling frame. Appendix Table A.1.1 shows the totals by lottery round.

1.3.2 Household Sampling

We used a two-step stratified sampling strategy to sample winning households for our survey. We first sampled condominium site-by-round pairs from across the 9 eligible rounds, oversampling from early lottery rounds, and stratifying by the round-specific median condominium site size. Since some condominium sites were allotted over multiple rounds, we allowed single sites to be sampled multiple times. In order to ensure that we could characterize neighborhood characteristics for winning households, we targeted approximately 50 households per condominium site. This left us with 32 site-by-round units in our sample.

Households were selected within these site-by-round pairs via stratified random sampling. The strata were the interactions of the gender of the head of household, the number of bedrooms applied for ¹⁰, and the sub-city where the household resided at the time of its registration. In total, there were 60 strata. We sampled a total of 1,648 lottery winning households.

The waitlist households were selected using simple stratified random sampling, relying on the same strata as the above winners. Since waitlist households have not yet been assigned a condominium unit, this sampling did not include the first site sampling step. Waitlist households had been eligible during each of the 12 rounds through 2019, but had not won. A small share of these households won during rounds 13 and 14 and were included in the survey. In total, we sampled 1,500 waitlist households to participate in the survey.

¹⁰These were either a studio, 1-bedroom, or 2-bedroom unit.

Our sample of waitlist and condominium winning households is balanced across the baseline covariates used in the strata, as seen in Appendix A.2. To achieve balance, households with female leaders were over-sampled from the wait list. This reflects the fact that, on average, female headed households were 10p.p. more likely to win a unit due to specific quotas. This left relatively fewer female headed households in the remaining waitlist.

Site Re-sampling Due to security issues, six of these sites were re-sampled and replaced with alternative sites drawn from the same round and strata. Details on this process can be found in Appendix A.2.

1.3.3 Household Survey

Households were first screened via phone before being surveyed by our team of trained enumerators. Since a primary focus of our study was labor market and educational outcomes for children, we required households to have a child who was less than 35 years old to be eligible. Since our survey took place in-person, we required that the household still be living in Addis Ababa. Of contacted households, 96% of contacted respondents still lived in Addis Ababa and amongst these, 89% had a child under 35, with balance across the treatment and waitlist groups. In total, 85% of contacted households were eligible for the survey. Our primary sample consists of 2,236 households, which include over 6,000 children. We supplement this primary sample with a sample of 661 households sampled through a separate, in-person sampling strategy in four condominium sites. We use this supplementary sample to validate our primary sampling strategy, as describe in further detail in Appendix A.3.

Summary Statistics Basic summary statistics for our surveyed households are displayed in Table 1.1. Column (1) is the waitlist households, while columns (3) and (4) show statistics based on the decision of winning households. These figures suggest that our population is positively selected relative to the entirety of Addis Ababa, as approximately 50% of all household heads were formally employed at the time of the condominium registration and over 60% of household heads have obtained at secondary level of education. Our sample is comparable to the sample drawn from a single lottery round in Franklin (2020b), though more likely to be married and have a female head of household. When we compare households in our sample drawn from the same round as those in Franklin (2020b), the two samples looks similar.

Table 1.1: Summary Statistics and Balance

	DV Mean (DV SD) (1)	Lotto coef. (SE) (2)	Own and Occupy Condo (3)	Rent Out Sold Condo (4)
HHH Age	49.775 (9.637)	1.001 (0.684)	51.946 (9.784)	48.822 (8.903)
HHH Years in Addis	38.713 (11.547)	-0.340 (1.031)	37.509 (12.217)	39.215 (11.001)
HHH Married	0.686 (0.464)	-0.025 (0.040)	0.686 (0.465)	0.683 (0.466)
HHH Years Ed	10.094 (4.244)	0.928** (0.389)	10.901 (4.569)	10.232 (4.153)
Orthodox	0.679 (0.467)	0.032 (0.036)	0.695 (0.461)	0.686 (0.464)
Amharic	0.700 (0.458)	0.020 (0.037)	0.672 (0.470)	0.732 (0.443)
BL: HHH Wage Emp	0.488 (0.500)	0.056 (0.036)	0.575 (0.495)	0.469 (0.499)
BL: HHH No Income	0.251 (0.434)	0.031 (0.033)	0.221 (0.415)	0.287 (0.453)
HHH Father Wage Emp	0.393 (0.488)	0.027 (0.046)	0.315 (0.465)	0.435 (0.496)
HHH Father Casual/Self Emp	0.571 (0.495)	-0.025 (0.048)	0.671 (0.471)	0.512 (0.500)
HHH Mother Wage Emp	0.103 (0.304)	-0.017 (0.024)	0.071 (0.257)	0.109 (0.311)
HHH Mother Casual/Self Emp	0.211 (0.408)	-0.024 (0.035)	0.170 (0.376)	0.232 (0.423)
BL: HH Size	3.619 (2.216)	-0.042 (0.199)	3.786 (2.130)	3.519 (2.070)
Observations	2326	1176	480	622
Samp Weights	X	X	X	X
Joint F-Stat		1.423		

Household-level OLS regressions of time-invariant household head (HHH) characteristics on an indicator for whether the household won a condominium lottery. Standard errors are clustered at the household level. The joint F-stat in column (2) is from a test that all lottery winner coefficients are equal to zero. Columns (3) and (4) present summary statistics for winning households that alternatively own and occupy or have rented out or sold the unit that they won. Orthodox is an indicator for the HHH belonging to the Ethiopian Orthodox Church. Amharic is an indicator equal to one when the HHH's mother tongue is the Amharic language. Baseline (BL) employment measures reflect household head employment states at the time of the condominium registration in 2005. HHH [Father/Mother] Wage Emp and HHH[Father/Mother] Casual/Self Emp are indicators equal to one when the parent of the HHH was primarily employed in a given sector. BL HH size is the number of members in the household at the time of lottery registration in 2005. ***, **, * indicates significance at 1, 5, and 10%.

Attrition We successfully contacted 65% of sampled households. This is comparable to similar phone surveys conducted in this setting, and attrition was largely due to our reliance on dated administrative data. Attrition is balanced across lottery winners and the waitlist, though higher for early lottery round winners. These higher rates of attrition in early rounds are due to households not updating their phone numbers with the AAHDA after winning. Additionally, female-headed households and two-bedroom applicants are slightly less likely to be contacted. We believe that these attrition rates are reasonable given that we are tracking households up to 17 years after they win a lottery in an urban setting where relocation and phone number changes are common. Of those contacted, 11% refused to participate in the survey. While this refusal rate was unusually high, recently political instability in Ethiopia had led to significant tension amongst respondents.

Upon receiving consent, households were asked to respond to an extensive survey which included information on education, employment, and residential history for all household members and any of the respondent’s children who may be living outside the household. We additionally surveyed a subset of children directly. The survey with children covered aspirations, education, measures of cognition, and basic numeracy and literacy exercises.

1.3.4 Administrative Data

We supplement our household survey with administrative and survey data on administrative budgets, school quality, wages, roads, and neighborhood characteristics.

Administrative Budgets In partnership with the Addis Ababa City Administration, we have collected line-item administrative budgets for each woreda within Addis Ababa between 2014 and 2018. These data are used to build measures of per capita spending on education and public services.

School Quality The Ministry of Education tracks school quality for all primary, secondary, and tertiary educational institutions throughout the country. For primary and secondary schools, we obtained school quality data collected in 2018 and 2019. These data rank primary schools along 26 distinct standards and five aggregate performance measures. We use these data in our analysis of school quality.

We also obtained a comprehensive list of all tertiary institutions – universities, colleges, and technical training institutes – from the Ministry of Education. These data include school location, year of establishment, and the institution’s ownership status. We combine this list with data from the Ethiopian Higher Education Relevance and Quality Agency (HERQA) which monitors school accreditation. These data are used to build measures of postsecondary school quality.

Firms We collected matched employer-employee from the Private Sector Employer’s Social Security Agency (POESSA) to build measures of firm density and average wages. With quarterly observations between 2011 and 2021, we observe firm location, sector, employment, and wages for the set of private sector firms which contribute to social security. While Addis Ababa has a large informal sector, this data represents the most comprehensive data on formal sector wages and employment.

Roads We build a biannual road network panel of all roads built in Addis Ababa. This data has been used in prior work in Ethiopia (Adamopoulos et al., 2019), and includes measures of road quality, allowing us to construct measures of and document changes in neighborhood-level market access.

Neighborhood Characteristics We also use survey data collected by the Central Statistics Agency and the Stanford University African Urbanization and Development Research Initiative (AUDRI) (Abebe et al., 2018). For the former, we use the Urban Employment and Unemployment Survey collected in 2012, 2014, 2016, and 2018 to build neighborhood-level measures of unemployment and poverty rates. We separately use a survey of all woreda-level administrators from the AUDRI project, which asks specifically about public services, spending, and local population changes.

1.4 Impacts of Condominium Lotteries

1.4.1 Policy Uptake

Before showing how the policy affects households and their children, we begin by documenting how the policy was used. First, 99% of winning households purchased the unit that they won. Nearly perfect take-up, conditional on winning, is due to the substantial subsidy associated with winning a unit. Households who won a condominium are given full ownership, able to rent out or sell their unit immediately. In our sample, 82% of winning households still own the unit that they won, even up to 17 years after winning. However, many winning households chose not to move into their unit. In Table 1.2 we see that of winning households, 35% rent out their unit, 17% sell, and a small share either leave the unit unoccupied or allow it to be used rent-free by friends and family. The remaining 41% of the winning households live in and own the unit they won.

Table 1.2: Condominium Usage

	(1)	(2)	(3)
	All	Winners	Waitlist
Own Lottery Condo	0.82 (0.38)	0.82 (0.38)	. (.)
Own Any Condo	0.44 (0.50)	0.85 (0.36)	0.01 (0.10)
Sold - Lottery Condo	0.17 (0.37)	0.17 (0.37)	. (.)
Occupy - Lottery Condo	0.41 (0.49)	0.41 (0.49)	. (.)
Rent Out - Lottery Condo	0.40 (0.48)	0.40 (0.48)	. (.)
Rent In Condo	0.04 (0.21)	0.02 (0.13)	0.07 (0.26)
Observations	2326	1176	1150

Variables defined over any condominium or lottery condominium. Lottery condominium refers to the particular unit that winners obtained via the lottery. Any condominium is the unit won via lottery or any other condominium unit.

1.4.2 Changing Neighborhoods

Having shown how households interact with the policy, we now turn to documenting policy-induced changes in neighborhood characteristics that may impact household and child welfare. In Table 1.3 we show that winning a condominium leads to significant changes in neighborhood quality. Lottery winners live, on average, 3.3 kilometers further from the city center than waitlist households, a 53% increase. This effect is much larger for winning households that occupy the unit that they win. These households live an average of 10.3 kilometers from the city center, compared to 5.9 kilometers for waitlist households not living in condominiums (column (2)). Despite living in more peripheral neighborhoods, we show that a neighborhood quality index, composed of measures of household satisfaction with their current neighborhood, is 0.86 SDs larger for winning households. Gains in this index are again concentrated amongst households that occupy their units, as shown in column (4). Appendix Figure A.1.2 displays how each of the index components varies based on lottery

winner and condominium occupation status.

Table 1.3: Neighborhood Quality and Proximity

	Km to City Center		Qual Index		Prox Index		1(Feel Secure)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1(Won Condo)	3.331*** (0.323)	1.704*** (0.351)	0.863*** (0.220)	0.609*** (0.232)	-0.310* (0.185)	-0.317 (0.221)	0.075*** (0.021)	0.085*** (0.024)
1(Occupy)		3.978*** (0.619)		1.151*** (0.294)		-0.986*** (0.232)		0.020 (0.049)
Winner X Occupy		0.919 (0.717)		-0.266 (0.420)		0.780** (0.332)		-0.041 (0.058)
Constant	6.319*** (1.306)	6.948*** (1.067)	-0.299 (0.840)	-0.236 (0.821)	2.209*** (0.721)	2.278*** (0.717)	0.740*** (0.074)	0.733*** (0.073)
N	2269	2269	2269	2269	2269	2269	2269	2269
Wait/Non-Dwell Mean	5.758	5.866	-0.455	-0.380	0.141	0.151	0.831	0.859
Samp Weights	X	X	X	X	X	X	X	X
HHH Controls	X	X	X	X	X	X	X	X

Household-level regressions. Standard errors are clustered at the household level. Regressions include controls for household head characteristics. The quality and proximity indices are the first principal component from the amenities and infrastructure show in Appendix Figures A.1.2 and A.1.3. Quality is measured on a 1-5 scale. Proximity is measured by one-way travel time. ***, **, * indicates significance at 1, 5, and 10%.

We similarly construct an index of neighborhood proximity to key amenities and social venues in columns (5) and (6) of Table 1.3. We do not find evidence that condominium households live significantly further from these amenities; if anything, column (3) implies that winning households live marginally closer to these amenities. Looking at the components of the index in Appendix Figure A.1.3 we observe that there is substantial heterogeneity based on the household’s decision to occupy their unit. Condominium dwellers do live further from close friends and family members, but not public services or other infrastructure. This echos findings from Barnhardt et al. (2017) and Franklin (2020b) that suggest that moving to new housing may disrupt social networks. We discuss additional results on household social networks and neighborhood cohesion in Section 1.4.5. In column (7) of Table 1.3 we show that winning a condominium increases respondent’s feelings of security in their neighborhood by 7.5 percentage points.

Using administrative data on school inspections and woreda-level budgets, we build measures of average neighborhood school quality and per capita spending. These results are presented in Appendix Tables A.1.2 and A.1.3. These results imply that while condominium winning households live in neighborhoods with slightly lower quality schools, school density is higher, driven by an increase in the number of (lower-quality) proximate private schools.

Further, winning households live in neighborhoods with lower per capita spending on education, health, and women’s and children’s affairs. Variation in these measures are displayed visually in Appendix Figure A.1.4.

Together, these results show that winning a condominium lottery impacts household neighborhoods of residence. These new neighborhoods are not clearly of lower quality: there is substantial variation in neighborhood characteristics across outcome measures and within the set of winning households, reflecting the varied locations of condominium units across the city. Differences in quality measures, conditional on the household’s decision to move into their unit, previews the key margin of heterogeneity that we will explore further in Section 1.5.

1.4.3 Empirical Strategy

Having documented that the policy was utilized and meaningfully changed neighborhoods of residence for the winning households, we turn to estimating the impacts of the policy on children’s human capital and household welfare. Equation 1.1 is our primary reduced form specification, which estimates an intent-to-treat (ITT) effect for households and families who win condominiums in their youth. Specifically, we estimate:

$$y_i = \alpha + \beta_1 T_i + \mathbf{X}_i \gamma + \epsilon_i \tag{1.1}$$

$$y_i = \alpha + \kappa_1 Exp_i + \mathbf{X}_i \gamma + \epsilon_i \tag{1.2}$$

where T is a treatment indicator for winning households for child or household i . \mathbf{X} is a vector of child and household covariates, including the child’s birth cohort. Given that there was nearly perfect take-up for lottery winners, and almost zero waitlist households acquired a unit, β_1 approximates the ATE in this setting.¹¹

Our setting allows for an additional reduced form specification for estimating policy impacts for children. Drawing on the literature on intergenerational mobility and housing in the United States (Oreopoulos, 2003; Chetty and Hendren, 2018), and leveraging exogenous temporal variation in the timing of condominium lotteries, we can separately estimate a linear exposure design, show in Equation 1.2. Here, the treatment variable Exp is defined as the number of years after a child’s household wins a condominium lottery before they turn 18 years old. Children in waitlist households are defined as having zero years of childhood exposure to the policy. We can additionally include the primary treatment indicator T from specification 1.1 to control for the main effect of winning a lottery, such that κ_1 is identified by exogenous variation in policy timing within the subset of winning households. We show the variation in Exp for children in winning households in Appendix Figure A.1.6.

¹¹In the parlance of instrumental variables (Imbens and Angrist, 1994), our setting has almost no never-takers or always-takers. We return to this point in Section 1.5.

Balance We use specification 1.1 to test for balance in our sample of lottery winning and waitlist households. As we lack a true baseline survey for respondents, prior to condominium application or lotteries, we examine whether time-invariant characteristics of winning and waitlist heads of household are comparable. In Table 1.1 we regress various baseline household head characteristics on an indicator for whether the household won a lottery. We include sampling weights that reflect the sampling probability due to our household sampling strategy. We additionally include two controls: an indicator for the condominium site sample group and an indicator for households who had already adopted a cell phone at the time of the 2005 registration. We discuss the inclusion of these controls and robustness in Appendix A.3. We obtain similar balance in unweighted regressions controlling for strata fixed effects. We believe the weighted regressions to be more conservative and thus preferred for estimation.

In column (2) we observe that we obtain balance on all but one of the included covariates. However, we cannot reject the joint balance test across all baseline covariates. The imbalanced characteristic, years of education of the household head, is included along with sampling weights and the controls discussed above in all subsequent analysis.

1.4.4 Children’s Human Capital

We now turn to our primary research question, how winning a housing lottery during childhood affects human capital. First, we focus on educational attainment and school quality, two of the principal components of human capital. Next, we consider direct measures of learning and cognition using exercises on cognition, literacy, and numeracy. We then use our household and child surveys to consider outcomes that are unavailable in administrative data. Specifically, we focus on measures of soft skills and children’s aspirations that may be associated with human capital, but are infrequently measured. Finally, we present results on children’s downstream employment and income.

Educational Attainment The results on child educational attainment are presented in Table 1.4. In column (1) we show that the policy increases active educational enrollment for children of winning households by 4.5% amongst 5-30 year-olds and 11% amongst 14-30 year-olds, shown in column (2). The effects are larger for the older cohort for whom school attendance is no longer compulsory. These results are consistent with visual evidence in Figure 1.2 which estimates marginal effects on enrollment by age group. We see that increases in school enrollment are largest for children between 15 and 20 years old, which coincides with the period in which children are completing secondary schooling and enrolling in post-secondary education. These results are robust to the inclusion of additional household

and child controls, the consideration of different age-restricted subsamples, and unweighted specifications with strata fixed effects.

Similarly, in the upper-left panel of Appendix Figure A.1.5 we observe that trends in enrollment rates between lottery winning and waitlist households until age 15 at which point children in lottery winning households are consistently more likely to be enrolled in school. In contrast, we find no impact on primary school completion rates. Primary school is mandatory and free in Ethiopia, leading to high completion rates of over 90%. We would not expect the lottery to significantly impact these rates and consider this a placebo test our estimation strategy.

In column (3) of Table 1.4 we show that treatment increases secondary school completion rates by 10.5%. Post-secondary attendance rates – defined as attendance at any college, university, or technical training program – increase by 16% for children in lottery winning households. These impacts on post-secondary attendance are larger than many school expansion programs, and about half as large as some of the most generous scholarship programs (Snilstveit et al., 2015; Duflo et al., 2021).

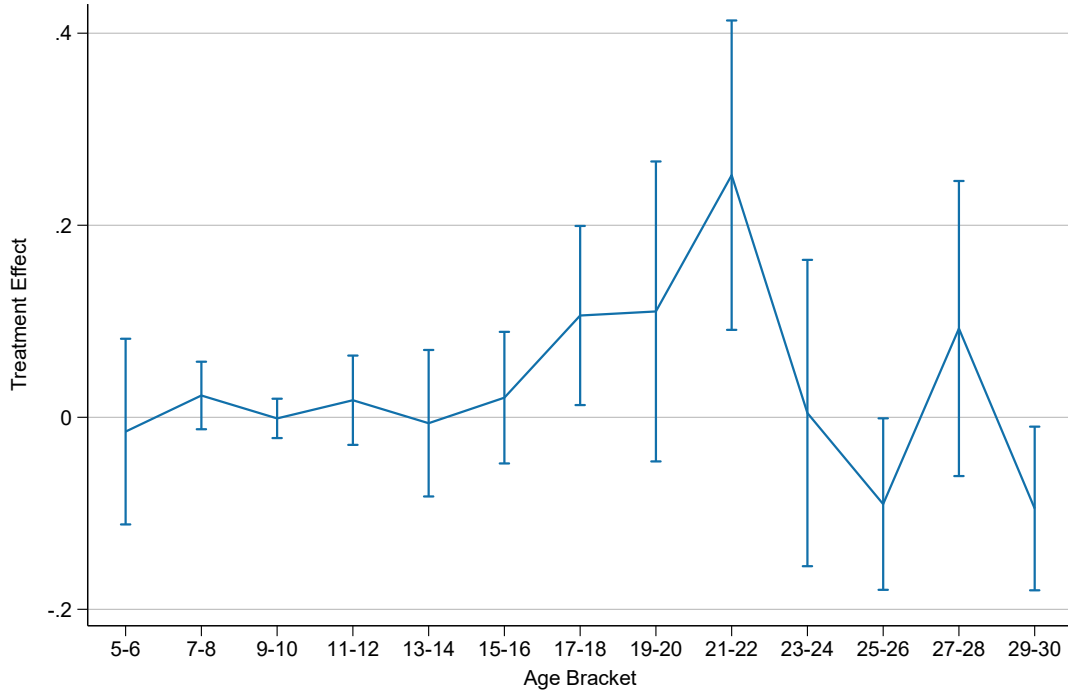
Table 1.4: Children’s Educational Attainment

	(1)	(2)	(3)	(4)	(5)
	1(Enrolled)	1(Enrolled)	Primary	Secondary	Post-Sec Att
1(Won Lottery)	0.029*	0.057**	0.012	0.070**	0.079**
	(0.016)	(0.024)	(0.025)	(0.031)	(0.039)
1(Male)	-0.011	-0.022	0.042**	-0.036	-0.123***
	(0.015)	(0.024)	(0.017)	(0.028)	(0.036)
Constant	0.632***	0.421***	0.849***	0.629***	0.344***
	(0.030)	(0.039)	(0.027)	(0.037)	(0.062)
N	4558	2614	2210	1812	1812
Waitlist Mean	0.742	0.558	0.913	0.620	0.406
Birth Cohort FEs	X	X	X	X	X
Sample	5-30	14-30	14-30	18-30	18-30

Weighted child-level OLS regressions. Standard errors are clustered at the household level. All specifications are weighted using sampling weights and include household head controls, birth cohort, and birth order fixed effects. ***, **, * indicates significance at 1, 5, and 10%.

Second, in Table 1.5 we estimate the linear exposure model from specification 1.2. Binning children’s age into 3-year groups to improve power, as in Chetty et al. (2016), and we find evidence of large exposure effects for children in winning households. Even after controlling for the main effect of winning a condominium, three additional years of childhood exposure

Figure 1.2: Children’s Enrollment By Age



Weighted child-level OLS regressions with 95% confidence intervals. Standard errors are clustered at the household level. Each estimate in the figure represents the marginal effect from an OLS regression of a treatment indicator interacted with a child age group at the time of the survey, controlling for base age-cohort effects.

to the policy increases school enrollment rates by 1-4% percentage points. This result makes an important point: lottery winning children of the same age experience larger gains in enrollment rates when their parents win a lottery when they are younger.

In contrast, columns (4) and (5) show that the entirety of policy’s effects on secondary school completion and post-secondary attendance are attributable to a level effect due to winning a lottery. We expand on our enrollment results in Appendix Table A.1.13 and estimate a “sibling design” (Oreopoulos, 2003). We include household fixed effects, such that treatment effects are estimated off of within-household variation in years of childhood exposure to the policy. While we lose power, our results are remarkably stable. Comparing children within the same household who had varying years of childhood exposure, an additional 3 years of childhood exposure increases school attendance rates by 2-5%. These results are

Table 1.5: Children’s Education - Exposure Design

	(1)	(2)	(3)	(4)	(5)
	1(Enrolled)	1(Enrolled)	Primary	Secondary	Post-Sec Att
Exposure (Years)	0.007*	0.017***	-0.006	-0.016	-0.005
	(0.004)	(0.005)	(0.005)	(0.010)	(0.011)
1(Won Lottery)	-0.009	-0.021	0.050*	0.119***	0.094*
	(0.029)	(0.035)	(0.027)	(0.039)	(0.053)
1(Male)	-0.009	-0.018	0.040**	-0.029	-0.114***
	(0.016)	(0.025)	(0.017)	(0.029)	(0.038)
Constant	0.618***	0.398***	0.859***	0.646***	0.343***
	(0.030)	(0.041)	(0.025)	(0.041)	(0.065)
N	4558	2614	2210	1812	1812
No Exposure Mean	0.638	0.434	0.912	0.715	0.521
Birth Cohort FEs	X	X	X	X	X
Sample	5-30	14-30	14-30	18-30	18-30

Weighted child-level OLS regressions. Standard errors are clustered at the household level. All specifications are weighted using sampling weights and include household head controls, birth cohort, and birth order fixed effects. Exposure is defined as years of treatment before the child turned 18 years old. ***, **, * indicates significance at 1, 5, and 10%.

consistent with evidence from the United States that the impacts of housing interventions are concentrated among younger individuals (Chetty et al., 2016; Chyn and Katz, 2021).

School Quality While we have shown that the policy leads to large increases in educational *attainment*, one may also believe that the quality of education received may also change based on treatment. On the one hand, children who live on the city’s periphery may have access to lower quality schools. On the other, an increase in parental wealth may allow parents to enroll their children in better schools.

To make progress on this, we first use administrative data on primary school quality from HERQA. These data are collected by the agency annually and rates schools from 0 to 100 along 26 standards ranging from facility quality to curriculum and testing performance. These 26 standards are aggregated into 4 indices – Performance, Input, Process, Output. Finally, these indices are combined to assign each school a quality level between 1 and 4; a grade of 1 corresponds to very low quality and a grade of 4 corresponds to very high quality.

We match the children with their most recently attended primary school. In Table 1.6 we show that treatment does not change the quality of primary schools attended by children.

We find consistently null effects across all measures used by HERQA and a quality index derived from the first principal component of HERQA’s 4 aggregate indices. Furthermore, children in lottery winning households are no more likely to attend a private primary school, which is generally considered to be of higher quality in Addis Ababa.

Table 1.6: Primary School Quality

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Standard Avg	Performance	Input	Process	Output	Quality Index	1(Private)	Level
1(Won Lottery)	-0.047 (0.095)	-0.051 (0.096)	0.032 (0.092)	-0.070 (0.094)	-0.063 (0.094)	-0.076 (0.177)	0.008 (0.042)	0.029 (0.053)
1(Male)	-0.007 (0.069)	-0.006 (0.068)	-0.017 (0.071)	-0.002 (0.067)	0.003 (0.066)	-0.011 (0.127)	0.056* (0.030)	0.011 (0.042)
Constant	-0.135 (0.132)	-0.152 (0.129)	-0.139 (0.128)	-0.103 (0.132)	-0.177 (0.112)	-0.284 (0.243)	0.378*** (0.059)	2.292*** (0.087)
N	2872	2872	2872	2872	2872	2872	2872	2872
Waitlist Mean	0.020	0.013	-0.009	0.038	0.003	0.023	0.378	2.423
Sampling Weights	X	X	X	X	X	X	X	X
Sample Controls	X	X	X	X	X	X	X	X
Birth Cohort FEs	X	X	X	X	X	X	X	X
Sample	5-30	5-30	5-30	5-30	5-30	5-30	5-30	5-30

Weighted child-level OLS regressions. Standard errors are clustered at the household level. All specifications are weighted using sampling weights and include household head controls, birth cohort, and birth order fixed effects. Children are matched to their most recently attended primary school. “Standard Avg” is the unweighted, normalized average of 26 quality components. Performance, Input, Process, and Output are normalized, aggregate quality measures. Quality index is the first principal component of the 4 aggregate quality measures. Level is measured on a scale of 1-4 with 1 representing significant failures and 4 representing exemplary performance. ***, **, * indicates significance at 1, 5, and 10%.

Next, we used data on all colleges, universities and technical institutions in Ethiopia from the Ministry of Education to match children with the post-secondary schools they attended. The results are presented in Table 1.7. Although treatment effects are positive in all specifications, our results are imprecise. As with primary school quality, we find no significant differences in the quality of lottery winning children’s post-secondary institutions. In column (1) we show that children of winning households are no more likely to attend post-secondary school in Addis Ababa. The results in columns (2) and (3) show that they also do not attend Addis Ababa University, the country’s top university, or any of the flagship public universities. Reconciling these results with the significant increase in post-secondary attendance, we see in columns (3) and (5) that children in winning households are marginally

more likely to attend public universities or technical training institutes (TVETs). This result holds if we additionally condition on any post-secondary attendance.

Table 1.7: Post-Secondary School Quality

	(1)	(2)	(3)	(4)	(5)
	1(Post-Sec in AA)	1(AAU)	1(Public Uni)	1(Private Uni)	1(TVET)
1(Won Lottery)	0.037 (0.038)	0.002 (0.014)	0.032 (0.029)	0.007 (0.031)	0.023 (0.032)
1(Male)	-0.060 (0.040)	-0.038** (0.017)	-0.083** (0.033)	-0.068* (0.037)	0.040 (0.032)
Constant	0.326*** (0.048)	0.027*** (0.010)	0.146*** (0.034)	0.204*** (0.043)	0.095** (0.045)
N	1814	1731	1731	1731	1731
Waitlist Mean	0.322	0.024	0.108	0.157	0.127
Sampling Weights	X	X	X	X	X
Sample Controls	X	X	X	X	X
Birth Cohort FEs	X	X	X	X	X
Sample	18-30	18-30	18-30	18-30	18-30

Weighted child-level OLS regressions. Standard errors are clustered at the household level. All specifications are weighted using sampling weights and include household head controls, birth cohort, and birth order fixed effects. Children are matched to their most recently attended post-secondary institution, if any. Post-Sec in AA is an indicator for attending tertiary education in Addis Ababa. AAU is an indicator for attending Addis Ababa University. TVET is an indicator for attending a technical or vocational training institute. ***, **, * indicates significance at 1, 5, and 10%.

Cognitive Skills, Literacy, and Numeracy Our educational results are not limited to attainment. In a sample of 6-17 year old children who we interviewed directly, we see substantial gains in measures of fluid intelligence. These results are presented in Table 1.8 and are consistent with household wealth and residential stability being important drivers of human capital (Bursztyn and Jensen, 2015; Card and Giuliano, 2013). Specifically, in column (1) we see that children in winning households complete a numerical Stroop exercise 28% faster, and more 88% more accurately. In column (5) we show that winning a condominium lottery improves children’s performance on a Raven’s matrix exercise by 24%. These measures are commonly used in economics and the child development literature and have been previously validated in Ethiopia (Mekonnen et al., 2020; Abebe et al., 2021). Details on the implementation of these tests can be found in Appendix A.4. Columns (2), (4), and (6) of Table 1.8 replicate the exposure design in Equation (1.2). Columns (2) and (6) provide

Table 1.8: Stroop Test & Raven’s Matrices

	Stroop Time (Sec)		Stroop Num Mistakes		Raven Score	
	(1)	(2)	(3)	(4)	(5)	(6)
1(Won Lottery)	-19.042** (9.492)	-10.681 (6.668)	-2.420* (1.400)	-2.430* (1.355)	1.370** (0.615)	-1.064 (0.963)
Exposure [3 Yr]		-2.878 (3.744)		0.004 (0.555)		0.840** (0.342)
1(Male)	-9.289* (5.440)	-9.405* (5.571)	0.142 (1.005)	0.142 (1.012)	0.795* (0.420)	0.820** (0.411)
Constant	70.613*** (7.803)	71.468*** (8.633)	1.501 (1.025)	1.500 (1.114)	5.699*** (0.489)	5.559*** (0.493)
N	98	98	98	98	223	223
Waitlist Mean	67.378	67.378	2.705	2.705	5.594	5.594
Birth Cohort FEs	X	X	X	X	X	X
Test FEs					X	X
Sample	13-17	13-17	13-17	13-17	6-17	6-17

Weighted child-level OLS regressions. Standard errors are clustered at the household level. All specifications are weighted using sampling weights and include household head controls, birth cohort, and birth order fixed effects. Stroop Time is the number of seconds to complete the Stroop exercise. Stroop Num mistakes is the count of mistakes on the Stroop exercise. Raven Score is the numerical score on the Raven’s matrix exercise. Test fixed effects reflect control for average scores on age-specific sets of Raven’s matrices. ***, **, * indicates significance at 1, 5, and 10%.

evidence of an exposure effect on Stroop exercise timing and Raven’s matrix performance, though the effect for the Stroop test is imprecisely estimated. Focusing on Raven’s matrix performance for which we have a larger sample, after controlling for the main effect of the lottery, an additional three years of childhood exposure to the housing policy increases Raven’s scores by 15%.

In Appendix A.4, we discuss additional results for additional literacy and numeracy exercises. While each of the results across a numeracy index, a literacy index, and a combined testing index are positive, the effects are insignificant. We show results for each of the index components in the Appendix Table A.1.4 and note that the effects are positive for 5 of the 7 components. Children in winning households score significantly better on the math component, which is also the component in which we successfully induce significant variation in scores – students generally scored very well on the tests, with many getting perfect scores. We view these results as suggestive of improved learning, although our failure to generate

significant variation in components and our relatively small sample of children hinder our ability to detect differences.

Soft Skills & Aspirations Outside of education and learning, we find broadly positive impacts of winning a lottery on children’s soft skills, aspirations, and general well-being. First, we conduct the Strengths and Difficulties Questionnaire (SDQ) for a random 50% sample of male and female children. This questionnaire, standard in the literature on child development and validated in Ethiopia (Hoosen et al., 2018; Mekonnen et al., 2020), is administered to parents, asking about their children, and is designed to measure the child’s emotional and behavioral development. More details on its implementation can be found in Appendix A.4.

Table 1.9: Strengths & Difficulties

	SDQ Scores (2-18)			
	(1)	(2)	(3)	(4)
Winner	0.028 (0.088)	-0.062 (0.101)	-0.017 (0.151)	-0.131 (0.198)
Winner \times 1(Male)=1		0.179* (0.104)	0.179* (0.104)	0.385* (0.217)
Exposure (Years)			-0.007 (0.021)	0.009 (0.028)
Exposure (Years) x Male				-0.029 (0.027)
1(Male)=1	0.035 (0.056)	-0.073 (0.058)	-0.073 (0.058)	-0.073 (0.058)
Constant	-0.291** (0.122)	-0.224* (0.123)	-0.222* (0.124)	-0.225* (0.124)
N	2443	2443	2443	2443
Waitlist Mean	-0.038	-0.038	-0.038	-0.038
Birth Cohort FEs	X	X	X	X
Resp Controls	X	X	X	X

Weighted child-level OLS regressions. Standard errors are clustered at the household level. All specifications are weighted using sampling weights and include household head controls, birth cohort, and birth order fixed effects. SDQ score is the normalized score out of 40 SDQ questions. Scores are reversed such that higher values indicate fewer behavioral issues. ***, **, * indicates significance at 1, 5, and 10%.

The results for the SDQs are presented in Table 1.9. First, we show in column (1) that there is no distinguishable difference between the SDQ scores of lottery winning and waitlist children. However, as seen in columns (3)-(5), we observe marginally significant positive

effects amongst male children in lottery winning households. While the results are relatively imprecise, they provide further suggestive evidence of policy impacts on soft-skills.

In our survey with 6-17 year old children, we follow the literature on aspiration measurement (Bernard and Seyoum Taffesse, 2014) to ask about educational and occupational goals as well as children’s general well-being. The results are presented in Table 1.10 where we aggregate educational goals, occupational goals, and well-being into indices, and build a composite index covering all measures. More information on aspiration measurement can be found in Appendix A.4. Treatment effects of for lottery winner are positive across all measures, with statistically significant impacts for educational aspirations show in column (1). These increases in educational aspirations, combined with marginal increases in the other index measures, drives the significant increase in the composite index measure in column (4).

Table 1.10: Aspirations

	(1)	(2)	(3)	(4)	(5)
	Ed Aspir Index	Occ Aspir Index	WB Index	Tot Index 1	Tot Index 2
1(Won Lottery)	1.363** (0.566)	0.333 (0.327)	0.461 (0.516)	1.359*** (0.496)	0.112 (0.307)
1(Male)	-0.774 (0.486)	0.023 (0.350)	-0.152 (0.442)	-1.235*** (0.429)	0.704*** (0.257)
Constant	0.414 (0.576)	-0.259 (0.304)	0.668 (0.560)	0.856 (0.524)	-0.015 (0.300)
N	98	98	98	98	225
Waitlist Mean	-0.236	-0.071	-0.163	-0.217	-0.168
Birth Cohort FEs	X	X	X	X	X
Sample	13-17	13-17	13-17	13-17	6-17

Weighted child-level OLS regressions. Standard errors are clustered at the household level. All specifications are weighted using sampling weights and include household head controls, birth cohort, and birth order fixed effects. Educational and Occupational Aspirations, and the well-being indices are the first principal components of the outcomes presented in Appendix Tables A.1.5, A.1.6, and A.1.7 respectively. The first Total Index is the first principal component of all education and occupation aspiration measures. The second Total Index additionally includes the well-being outcome measures. ***, **, * indicates significance at 1, 5, and 10%.

Income and Employment We do not find significant evidence that the policy increases children’s earnings or employment. Focusing on children 17-35 years of age who are not currently enrolled in school, we show in Table 1.11 that treatment is not associated with

higher employment rates, higher earnings, or more days worked in the past month. We believe two features of our setting may explain these findings. First, children in winning households stay in school longer, such that those who are not currently enrolled may be negatively selected within the sample of winning children. We expect that these results may change once more of the children impacted by the policy finish schooling and enter the labor market. Second, we note the extremely high levels of unemployment among young entrants into the labor market. The unemployment rate among 17-35-year-olds in our sample is over 50%, which matches recent reports in Ethiopian media (Sahlu, 2023). The lingering impacts of the recent civil war in Ethiopia and macroeconomic instability are likely causes. One interpretation of our results is that the condominium policy is insufficient to overcome these macro-conditions. Knowledge of these poor labor market conditions may also partially explain why children stay in school longer.

Table 1.11: Children’s Income

	(1)	(2)	(3)	(4)	(5)	(6)
	Any Income	Formal Emp	Self Emp	Days Worked	Asinh(Prim Inc)	Asinh(All Inc)
1(Won Lottery)	-0.012 (0.031)	0.020 (0.030)	-0.008 (0.017)	0.239 (0.762)	-0.077 (0.276)	-0.260 (0.332)
1(Male)	0.061** (0.026)	-0.067*** (0.025)	0.074*** (0.015)	1.415** (0.648)	0.551** (0.231)	0.548** (0.272)
Constant	0.430*** (0.031)	0.343*** (0.030)	0.042*** (0.015)	9.975*** (0.770)	3.736*** (0.275)	4.310*** (0.326)
N	1515	1515	1515	1515	1515	1515
Waitlist Mean	0.480	0.330	0.086	11.297	4.250	4.898
Birth Cohort FEs	X	X	X	X	X	X
Sample	17-35	17-35	17-35	17-35	17-35	17-35

Weighted child-level OLS regressions. Standard errors are clustered at the household level. All specifications are weighted using sampling weights and include household head controls, birth cohort, and birth order fixed effects. All outcome measures are for the past 30 days. Primary income is income from the individual’s main income source. All income includes the primary source and any other income sources. ***, **, * indicates significance at 1, 5, and 10%.

1.4.5 Household-Level Impacts

Our results for children may be unsurprising if they simply represent a wealth effect: winning a condominium bequeaths households with a valuable, subsidized asset, dramatically increasing familial wealth. As was similarly found in Franklin (2020b), we observe large increases in family assets (Appendix Table A.1.8), home quality (Appendix Table A.1.9), and

estimated home value (Appendix Table A.1.10). That is, regardless of whether the family occupies the unit that they win, condominium winning households are wealthier and live in higher quality homes.

As in Franklin (2020b), we find no impacts on overall adult employment rates, but observe 21% increases in formal sector employment for household heads driven (Appendix Table A.1.11). This formalization represents a specific type of job-switching, where adult heads of household are moving from causal to formal sector employment. Given the wage premium associated with formal sector employment, it is unsurprising that we also see substantial increases in household income (Appendix Table A.1.12), which Franklin (2020b) does not find. Specifically, in column (1) we see 10% increases in household head employment, 11% increases in household head and spouse income (column (2)), and 13% increases in total household income per capita.

We believe that the differences between our results and those in Franklin (2020b), which only studies short-term impacts of a particular lottery round, is likely due to our study looking at longer-term impacts. Consistent with this, we find that the impact on formal sector employment is increasing in years since winning the lottery. By looking across various lottery rounds, we may pick up on average treatment effects that might be missed by only considering a single lottery round. Furthermore, we believe that our results on household income are plausible given the wage premium associated with formal sector employment. In Appendix A.5 we document changes in additional household-level outcomes.

1.5 Model & Mechanisms

As was previously noted, only 41% of lottery winners move into their unit after winning, while the remainder sell or rent out their unit to others. Thus, our reduced form estimates reflect the combined treatment effect for the two groups: movers and non-movers. The decision to move into a unit, conditional on winning, is an endogenous choice made by the household. Although we can focus only on the subset of households that move, we expect that these households differ from non-moving households in meaningful ways. Since we do not know, ex-ante, which of the waitlist households would have moved if they had won the lottery, treatment effect estimates in this reduced sample of movers may be contaminated by selection.¹²

Taking into account selection into condominium occupation presents an empirical challenge, but also allows for the opportunity to disentangle the channels through which the

¹²Consider, for instance, a scenario where only financially vulnerable households sell their unit. Then a model considering the full set of waitlist households and only winners who move into their unit would be biased. The sign of the bias is unclear ex-ante.

condominium policy may operate. All households who decide to purchase a government-subsidized condominium through the housing lottery experience a large increase in wealth due to the associated subsidy. Only households who move in, however, will experience the changes in neighborhood and peer characteristics that are the focus of the literature on neighborhood effects (Kling et al., 2007). In order to disentangle the effect of a parental wealth shock from the effects of moving into a condominium unit during childhood, we take two empirical approaches. In the first, we consider an interacted two-stage least squares (2SLS) model with multiple endogenous treatment states. To relax assumptions required for the estimation of causal effects in the 2SLS model, we extend the structural selection model developed in Kline and Walters (2016).

1.5.1 Interacted 2SLS

Before introducing our selection model, we first outline a multivariate 2SLS estimation strategy and consider its limitations.

Let *owning and occupying* (**O**) and *selling or renting out* the condominium (**W**) to be the treatment incentivized by the lotteries. We consider a single fall-back state (**S**) that represents the outside option of the household absent the winning a lottery.¹³ Thus, households choose between three mutually exclusive treatments $k \in \{\mathbf{S}, \mathbf{W}, \mathbf{O}\}$. Let D_{ki} represent the binary indicator corresponding to each treatment, $D_{ki} = \mathbb{1}[D_i = k]$ for each household i such that:

$$D_{Si} + D_{Wi} + D_{Oi} = 1$$

We're interested in the impact of the causal effect of each treatment on a child's later life outcome Y (e.g. college attendance, income):

$$Y = Y_{Si}D_{Si} + Y_{Wi}D_{Wi} + Y_{Oi}D_{Oi}$$

where Y_{ki} is the potential outcome for household i if assigned to treatment k . This implies that $Y_{Wi} - Y_{Si}$ represents the effect of a shock to parental wealth, $Y_{Oi} - Y_{Si}$ represents the combined neighborhood and wealth effects, and under an assumption that the wealth effect associated with ownership is the same as the wealth effect associated with renting or selling the unit, $Y_{Oi} - Y_{Wi}$ represents the effect of condominium occupation net of the wealth effect.

This setup would suggest an OLS regression with two endogenous treatments, $\mathbf{W}_i = D_{Wi}$ and $\mathbf{O}_i = D_{Oi}$:

$$y_{h(i)} = \alpha_0 + \alpha_1 \mathbf{O}_i + \alpha_2 \mathbf{W}_i + \mathbf{X}_i \gamma + \epsilon_i \tag{1.3}$$

¹³The model can be extended to separately consider selling versus renting out, or including rental in as an additional fall-back state. One simply needs instruments that differentially predict these choices.

The lottery offer, as used in the reduced form analysis, $z_1 \in \{0, 1\}$, can be used as a first instrument. Fortunately, our setting allows for the creation of multiple sets of exogenous instruments leveraging temporal variation in lottery rounds and spatial variation in condominium locations. In our main specifications, we interact the lottery round indicator with the difference between realized and expected distance of the household to their condominium unit:

$$\mathbf{z}_2 = \mathbf{1}(\text{Won Lottery}) \times (\text{Distance to Unit} - \mathbb{E}[\text{Distance to Unit}])$$

The instrument z_2 is re-centered, as in Borusyak and Hull (2020), accounting for the fact that household residential location, and therefore the distance to condominium lottery units, is endogenous. Borusyak and Hull (2020) establish how re-centered instruments can avoid omitted variable bias when shocks (i.e. distance to condominium units) are exogenous but actor characteristics (i.e. household residential location) that influence the exposure to such shocks are not. The instrument z_2 is constructed by calculating, for each household, the distance to all lotteries that they could have won based on their residential history since the time of lottery registration. These distances are then weighted by the number of units in the condominium site that were dispersed. For waitlist households, this expectation is calculated for all units transferred through the first 13 rounds. For the lottery winning households, we calculate the expected distance in two ways: (1) the expected distance to all units transferred in years before a household wins; (2) the expected distance to all units transferred both before and after a household wins. We prefer the first approach, as household location after winning is impacted by the lottery offer. In practice, results look similar using either version of the instrument.

Here, z_2 can be thought of as a measure of whether the unit that was won was closer to or farther away from the household than expected, given its location of residence. Negative values indicate that a unit was close, while positive values indicate the opposite. Notably, the realized distance to a lottery condominium is only defined for lottery winning households, not waitlist households. By interacting the instrument with the lottery offer, all waitlist households are necessarily assigned a value of zero. We consider alternative constructions where waitlist households are assigned their expected distance, their maximum distance, or a fixed large value. These alternative instruments can be used directly, without an interaction with the lottery offer. Results are robust to these alternative instrument constructions.

With two instruments, the lottery indicator and the interacted deviated distance measure, we then estimate Equation (1.3) instrumenting \mathbf{W} and \mathbf{O} with z_1, z_2 controlling for the main effects of the interacting variables. Following Kline and Walters (2016) we can build additional instruments by further interacting z_1 and z_2 with exogenous (time-invariant) household characteristics. In the following, we outline the assumptions required for identification of causal effects in this model.

Let Z be an L vector of instruments with support $\mathcal{Z} \subseteq \mathbb{R}^L$. Let $Y(k, z)$ denote latent potential outcomes for children or households with $k \in \{S, W, O\}$ and $z \in Z$. We adapt assumptions E.1-E.3 from Mogstad et al. (2020) and suppress household characteristics \mathbf{X} for notational simplicity. Consequently, all assumptions should be thought of as holding conditional on \mathbf{X} .

Exclusion

$$Y(k, z) = Y(k, z') \equiv Y(k) \text{ for all } k \in \{S, W, O\} \text{ and } z, z' \in \mathcal{Z} \quad (1.4)$$

Equation 1.4 is the traditional exclusion restriction as in Imbens and Angrist (1994), that instruments have no direct causal effects on outcomes except through choices, extended to a setting with multiple choices and instruments. Our instruments are functions of a random lottery offer, so violations of this assumption would require that lottery offers themselves, not choices influenced by the offer, have direct impacts on outcomes.

Independence

$$\mathbb{E}[Y(k)|Z, \{D(z)\}_{z \in \mathcal{Z}}] = \mathbb{E}[Y(k)|\{D(z)\}_{z \in \mathcal{Z}}] \text{ and } \mathbb{E}[Y(k)^2] < \infty \text{ for all } k \in \{S, W, O\} \quad (1.5)$$

$$\{D(z)\}_{z \in \mathcal{Z}} \text{ is statistically independent of } Z \quad (1.6)$$

The mean independence assumptions in Equation 1.5 are weaker than the full independence assumption of Imbens and Angrist (1994) and are common in the marginal treatment effect (MTE) literature (Mogstad et al., 2020; Borusyak and Hull, 2020). We observe the full set of information used by the AAHDA and the lottery is believed to have been implemented correctly.

Partial Unordered Monotonicity Divide $z \in \mathcal{Z} \subseteq \mathbb{R}^L$ into its l th component and all other $(L - 1)$ components, z_{-l} .

$$\text{For any } l \in L \text{ let } (z_l, z_{-l}) \text{ and } (z'_l, z_{-l}) \text{ be any two points in } \mathcal{Z}. \text{ Then either } D_k(z_l, z_{-l}) \geq D_k(z'_l, z_{-l}) \text{ or } D_k(z_l, z_{-l}) \leq D_k(z'_l, z_{-l}) \text{ almost surely for all } k \in \{S, W, O\}. \quad (1.7)$$

In a setting with multiple instruments and multiple treatments, the standard notion of monotonicity from the binary treatment, binary instrument case is insufficient to achieve identification. We adapt the notion of partial unordered monotonicity (PUM) from Mountjoy (2019) and Mogstad et al. (2020). The standard monotonicity assumption, when $L = 1$, implies partial monotonicity, and PUM is strictly weaker. While maintaining the “no defiers”

condition from the binary case, this assumption requires that each shift in the instrument render each treatment state either weakly more or less attractive for all households.

Consider the case of the lottery offer, that greatly subsidizes treatment **W** and **O**. While this instrument is not targeted directly at either treatment state, PUM implies that households may only flow into one of these two treatment states in response to a winning a lottery.¹⁴ For the deviated distance measure, conditional on the lottery offer, PUM requires that a household winning a condominium closer (further) from their residence only be more or less likely to *own and occupy* their unit. That is, winning a closer unit cannot induce some compliers into **W** and others into **O**. We believe this assumption to be reasonable in this setting: the deviated distance instrument can be thought of as a cost shifter for unit occupation, but not necessarily for unit rental or sale. Thus, we would expect that winning a lottery closer to one’s home than expected increases the probability of occupation, which we confirm in the first stage of the analysis below.

According to the discussion in Kline and Walters (2016), Hull (2018), and Heinesen et al. (2022), this model can be characterized by the assumption of constant complier effects. In a setting with multiple unordered treatments, the local average treatment effect (LATE) estimated through an interacted 2SLS model is a weighted average of “subLATEs” reflecting compliers drawn from alternative treatment states. We can write the composite LATE_O as:

$$\text{LATE}_O = \omega_S \text{LATE}_{SO} + (1 - \omega_S) \text{LATE}_{WO}$$

where ω_S is the share of compliers would have remained in the base residential state absent the lotteries. The subLATE terms, LATE_{SO} and LATE_{WO}, each represent a LATE for a separate complier margin – those drawn from **S** to **O** and those drawn from **W** to **O** given their realization of the instruments, $z \in \mathcal{Z}$. These two complier groups can be loosely thought of as always takers of either **W** or **O**, conditional on winning a lottery, and marginal takers whose treatment is influenced by the cost of **O** versus **W**. The composite LATE_W is defined analogously. In general, the subLATEs are not separately identified, such that we can only interpret the composite LATE_O as a causal estimate of a given treatment state under an assumption of constant complier effects, LATE_{SO} = LATE_{WO}.

Based on the construction of our 2SLS model, we think of the coefficients for *ownership and occupation*(**O**) and *renting out or selling* (**W**) as representing average effects of condominium occupation, rental, or sale across all condominium neighborhoods. We are not modeling heterogeneity in outcomes based on variation in condominium neighborhood amenities like those documented in Section 1.4.2. One might be concerned that winning a condominium closer than expected implies that the unit is located near the city center, and

¹⁴The assumption that the inequality in PUM holds strictly for at least some households implies an instrument relevance condition.

consequently worth more. Then changes in occupation, rental, and sale rates estimated in the first stage of our 2SLS model will reflect not just distance but also the value of the unit that was won.

Fortunately, the spatial dispersion in the lottery condominium sites induces exogenous variation in these condominium neighborhood characteristics that we can control for or stratify by. If we assume that condominium rental value represents an index measure of neighborhood quality, which is observed for all condominium sites, then we can control for exogenous neighborhood quality directly.¹⁵ But as we show in the red line in Figure 1.3, condominium value is only weakly correlated with z_2 . This implies that variation in neighborhood and condominium quality cannot be driving our first stage results.

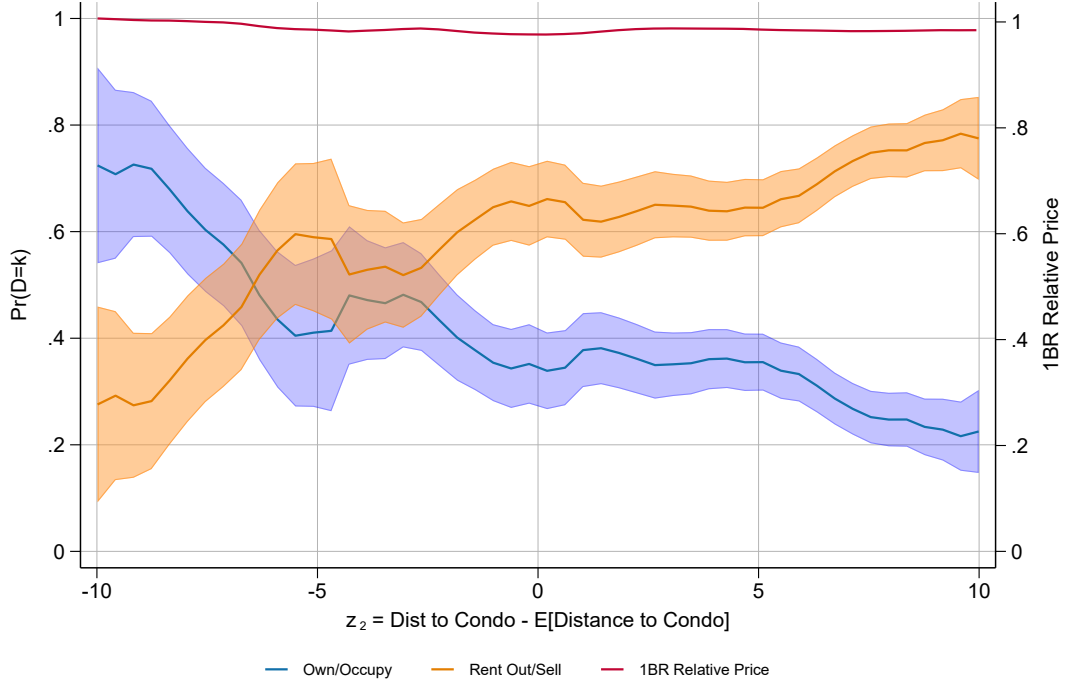
Results from the interacted 2SLS model are presented in Table 1.12. We have a strong first stage, with Angrist-Pischke partial F's between 45 and 175. The first stage results imply that households winning a condominium one kilometer closer to their residence are 1.2-2.6 percentage points more likely to occupy those units. We depict this relationship graphically in Figure 1.3. In the second stage, we show that nearly all the positive treatment effects in educational enrollment and attainment accrue to children in households that own and occupy the condominium unit that they win. Columns (1)-(2) and (4)-(5) of Table 1.12 show positive and significant increases in active educational enrollment, secondary, and post-secondary attainment for children in lottery winning households. While positive, the coefficient on **W** is close to and indistinguishable from zero in all specifications. Together, these results imply that the positive intergenerational effects of lottery winning on education cannot be explained solely through increases in familial wealth and that the occupation of condominium units, in their associated neighborhoods, plays a key role in the intergenerational transmission of policy impacts.

1.5.2 Selection Model

To relax the assumption of constant complier effects, we adapt the model developed in Kline and Walters (2016) to our context. We incorporate household preferences and potential outcomes over three treatment states: *ownership and occupation* (**O**), *renting out or selling* (**W**), and an outside option of *staying* outside condominium housing (**S**). Like the interacted 2SLS approach, we use the lottery instrument interacted with household and condominium site covariates (e.g. z_2) to identify causal effects for each treatment. The model allows for different margin-specific treatment effects, which was the primary limitation of the 2SLS approach.

¹⁵A similar assumption is often made in research studying neighborhoods in the United States and the MtO policy (see e.g. Kling et al. (2007)). These papers use neighborhood-level poverty rates as their quality index measure.

Figure 1.3: Recentered Distance (z_2) First Stage and Condominium Prices



This figure graphs local polynomial regressions of the predicted treatment states **O** and **W** for lottery winners as a function of the deviated distance instrument, z_2 . These predictions are based on the first stage of the 2SLS regressions presented in Table 1.12 and use the left axis. Using the right axis, the red line graphs the average rental price of a 1BR condominium unit as a function of z_2 with $z_2 = -10$ normalized to 1.

Model Setup There is a population of households, indexed by h , each of which has one or more children, indexed by i , who have applied to the condominium lotteries. Assume that households have preferences over choices given by:

$$\begin{aligned}
 U_{h(i)}(S) &= 0 \\
 U_{h(i)}(W, Z_{h(i)}) &= \Psi_W(Z_{h(i)}, X_{h(i)}) + \nu_{h(i)W} \\
 U_{h(i)}(O, Z_{h(i)}) &= \Psi_{bm}(Z_{h(i)}, X_{h(i)}) + \nu_{h(i)O}
 \end{aligned}$$

where we normalize the value of staying in non-condominium housing to zero. Here, Ψ_k is the mean treatment-level utility for treatment k while ν_k are unobserved idiosyncratic

Table 1.12: Interacted 2SLS

	(1)	(2)	(3)	(4)	(5)
	Enrolled	Enrolled	Primary	Secondary	Any Tertiary
Own/Occupy - O	0.090*	0.147*	-0.007	0.167**	0.161
	(0.047)	(0.077)	(0.074)	(0.085)	(0.124)
Rent Out/Sell - W	0.002	0.015	0.036	0.028	0.014
	(0.033)	(0.058)	(0.048)	(0.072)	(0.090)
N	4558	2614	2210	1812	1812
Waitlist Mean	0.742	0.558	0.913	0.620	0.406
Birth Cohort FEs	X	X	X	X	X
Sample	5-30	14-30	14-30	18-30	18-30
O First-stage F	78.30	63.67	58.43	44.94	44.94
W First-stage F	175.02	97.53	100.20	54.92	54.92

IVs: 1(Lotto Winner) ; 1(Lotto Winner) \times (Distance to Unit - E[Distance to Unit])
 Weighted 2SLS regressions. Standard errors are clustered at the household level. The excluded instruments are z_1 and z_2 . The first stage includes household head and sampling controls. The second stage includes child birth cohort, birth order, and gender fixed effects. First-stage F's are Angrist-Pischke partial F's. ***, **, * indicates significance at 1, 5, and 10%.

components that vary across households. Households maximize state-specific utility:

$$D_{h(i)}(X, z) = \underset{k \in \{S, W, O\}}{\operatorname{argmax}} U_{h(i)}(k, z_1, z_2, X)$$

where $D_{h(i)}(X, z) = k$ represents the observed outcome. We further assume that the stochastic components are distributed multinomial probit:

$$(\nu_{h(i)O}, \nu_{h(i)W}) | X_{h(i)}, Z_{h(i)} \sim N \left(0, \begin{bmatrix} 1 & \rho(X_{h(i)}) \\ \rho(X_{h(i)}) & 1 \end{bmatrix} \right)$$

Following Heckman (1979), we can write potential outcomes for each treatment as:

$$E [Y_{h(i)k} | X_{h(i)}, Z_{h(i)}, \nu_{h(i)O}, \nu_{h(i)W}] = \mu_k (X_{h(i)}) + \gamma_{k,O} \nu_{h(i)O} + \gamma_{k,W} \nu_{h(i)W}$$

such that the γ terms govern selection on unobservables. They are assumed to enter into the potential outcome framework linearly and to be additively separable from observables.

Using the law of iterated expectations, we can write the conditional expectation of realized outcomes as:

$$E [Y_{h(i)} | X_{h(i)}, Z_{h(i)}, D_{h(i)} = k] = \mu_k (X_{h(i)}) + \gamma_{k,W} \lambda_W (X_{h(i)}, Z_{h(i)}, k) + \gamma_{k,O} \lambda_O (X_{h(i)}, Z_{h(i)}, k)$$

where $\lambda_k (X_{h(i)}, Z_{h(i)}, D_{h(i)}) = E [\nu_{h(i)k} | X_{h(i)}, Z_{h(i)}, D_{h(i)}] \forall k \in \{O, W\}$ are variations of the Mills ratio terms from a two-step Heckman selection model.

In our setting, with almost zero never-takers (i.e. lottery winners who do not purchase their unit) and few always takers (i.e. waitlist households who purchase a unit) our model reduces to a single index model. To see this, we show in Figure 1.3 that conditional on winning a lottery, $Pr(\mathbf{O}) \approx 1 - Pr(\mathbf{W})$. Relatedly, less than 1% of waitlist households are in treatment state \mathbf{O} and none are in treatment state \mathbf{W} . This implies that there is a single threshold governing the selection of \mathbf{O} versus \mathbf{W} conditional on z_1 . Consequently, we estimate a single Mills ratio term, λ_O that governs selection into the occupation treatment state.¹⁶

Kline and Walters (2016) describe identification of this model using a two-step procedure. Following their work, in a first step we estimate the multinomial probit model using simulated maximum likelihood, relying on the GHK probability simulator. We then use the parameters from our probit model to build our single control function estimate, which is included in a second step regression to estimate treatment effects for compliers and selection-adjusted average treatment effects.

Identification is obtained under a few critical criteria. First, we require the additive separability of potential outcomes in observables and unobservables, as is common in this literature (Heckman et al., 2006). This rules out selection coefficients (γ) depending on household characteristics, which is testable by comparing selection coefficients across different subsets of households. Further, we require that (1) the instruments shift choice probabilities across the support of z_2 conditional on winning the lottery; (2) the instruments must shift the conditional mean values of $\nu_{h(i)k}$ in a non-proportional manner for all $k \in \{O, W\}$.

Model Results The results of our second step estimates are presented in Table 1.13. Children in households that own and occupy their unit are 6-12pp more likely to be actively enrolled in school, 14pp more likely to finish secondary school, and 16pp more likely to start post-secondary education relative to children in households in the outside option state \mathbf{S} . The control function term, λ_O exploits experimental variation in the lottery assignment and the recentered distance to the condominium unit. Adjusting for selection on unobservables

¹⁶An alternative framing of this model would be as a sequential choice, where households only select between \mathbf{O} and \mathbf{W} conditional on an exogenous lottery offer.

Table 1.13: Control Function Model Estimates

	(1)	(2)	(3)	(4)	(5)
	1(Enrolled)	1(Enrolled)	Primary	Secondary	Post-Sec Att
O	0.060** (0.028)	0.116*** (0.041)	0.027 (0.039)	0.137*** (0.050)	0.158*** (0.060)
W	-0.015 (0.054)	-0.004 (0.089)	0.025 (0.082)	0.076 (0.104)	0.042 (0.126)
λ_O	0.027 (0.027)	0.052 (0.047)	-0.033* (0.017)	-0.085** (0.033)	-0.048 (0.042)
O \times λ_O	-0.003 (0.038)	0.018 (0.049)	-0.009 (0.023)	0.128*** (0.048)	0.169*** (0.057)
W \times λ_O	-0.081 (0.091)	-0.098 (0.153)	0.046 (0.144)	0.016 (0.173)	-0.066 (0.215)
Constant	0.669*** (0.025)	0.474*** (0.032)	0.874*** (0.022)	0.681*** (0.033)	0.464*** (0.047)
N	4558	2614	2210	1812	1812
Waitlist Mean	0.742	0.558	0.913	0.620	0.406
Birth Cohort FEs	X	X	X	X	X
Sample	5-30	14-30	14-30	18-30	18-30

Weighted child-level OLS regressions. Standard errors are clustered at the household level. The first-stage multinomial probit specification is estimated using simulated maximum likelihood and includes sampling weights and household head controls. λ_O is the generalized Mills Ratio estimated in the first-stage multinomial probit regression. ***, **, * indicates significance at 1, 5, and 10%.

decreases the estimated average impact of **O** relative to **S** when compared to the results in the 2SLS model. The results of the control function estimates are somewhat imprecise, however we reject the hypothesis of no selection on gains for secondary and post-secondary education in columns (4) and (5). In these cases, we document Roy-style selection in which children whose families are more likely to own and occupy the unit that they win achieve larger gains in educational attainment when shifted from the outside option to occupation of the condominium unit. This suggests smaller gains for households with unobservables that make them less likely to own and occupy their unit. The results for renting out or selling the unit are similar consistent with the 2SLS model, with all estimates indistinguishable from zero. It is worth noting, however, that the coefficients on **W** in columns (4) and (5) are positive and larger than their 2SLS counterparts.

1.6 Conclusion

The expansion of public housing and slum redevelopment are two of the primary policy levers used by policymakers in low-income countries to manage urban growth. This paper has empirically explored the largest of these policies on the African continent to understand how it impacts children’s human capital and household welfare.

By interviewing households an average of 8 years after winning a condominium, we are able to observe outcomes that may be missed in short-run followups. Further, through extensive surveys with households and children, we are able to document changes in key human capital outcomes that are unavailable in administrative data. The empirical results show that winning a condominium meaningfully changes neighborhoods of residence and significantly increases children’s educational attainment, cognitive performance, and aspirations. These impacts are concentrated amongst children in households that own and occupy the unit that they win. This suggests that the policy’s impacts cannot be explained through a wealth effect alone.

Using an instrumental variables approach and a structural model, we show that households winning a condominium relatively close to their existing residence is an important driver of their decision to occupy the unit, but this is mediated by selection. Taken together, these results suggest that in-site housing redevelopment, as opposed to peripheral construction, would substantially increase the policy’s impacts on children and households.

We believe that our results may help inform policy in other contexts. Housing upgrading and slum redevelopment in Ethiopia is not an outlier, as it shares many characteristics with policies that are actively implemented in low-income, urbanizing countries around the world. We hope that based on our findings, deeper consideration may be given to long-run policy effects on children and the importance of homeownership and neighborhoods in developing cities.

Chapter 2

New Technology and Network Change: Experimental Evidence from Kenya

2.1 Introduction

That social interactions are foundational for learning, technology diffusion, and risk sharing has been well known and well studied by economists for decades (Griliches, 1957; Townsend, 1994). This topic is of particular importance in the field of development economics: in the face of credit or labor market failures and barriers to information acquisition, all of which are common in developing economies, social networks are likely to play an out-sized role in the economic lives poor and rural populations (Chuang and Schechter, 2015; Breza et al., 2019). A growing literature shows that the structure of social networks, and an individual's position within that network, are important for economic outcomes (Jackson et al., 2015). However, relatively little is known about how social networks changes over time or in response to policy. Network dynamics inform how we think about models of diffusion and peer effects. Only recently have development economists have attempted to observe social networks directly (Conley and Udry, 2010) rather than attempt to infer information about their features (Townsend, 1994). Social network data is difficult to collect and standard strategies of network elicitation are prone to measurement error (Comola and Fafchamps, 2014; Griffith, 2019) which may bias estimates of network summary statistics (Chandrasekhar and Lewis, 2010). Consequently, researchers often make simplifying assumptions about the nature of social connections in order dispel concerns related to measurement error and missing data (Banerjee et al., 2013).

The first standard assumption is that networks are fixed over time. The prohibitive cost of data collection makes panels of social network data, which would allow researchers to study network change, exceedingly rare. The second typical assumption is that social connections are bilateral or “undirected”. This may not be appropriate if one believes that learning and diffusion are better described as unilateral (“directed”) processes. A final standard assumption is that social networks are monolithic in which a connection along any dimension is presumed sufficient to describe a comprehensive network link. This assumption ignores the varied depth of social connections and may bias results if key network function such as information transmission and risk sharing occur only through particular types of social connections.

In this paper I leverage a new, rich panel of network data, collected in conjunction with a technology adoption randomized controlled trial (RCT) in Kenya, that allows me to relax the assumptions of fixed network structure, bilateral connections, and uniform connection types. I discuss the importance of these assumptions in light of new work on models endogenous network change and peer effects (Comola and Prina, 2019; Banerjee et al., 2019). I show that failure to account for endogenous changes in network structure biases treatment and peer effect estimates. First, using a unique method of network elicitation, I show that agricultural networks in this context are dense: farmers discuss agriculture with many neighbors and frequently help on each others farms. This stands in contrast to previous findings in the literature (Banerjee et al., 2019). Next, I show that that for a given farmer, there is substantial variation in their links across different types of social connections. My data allows me to credibly separate these network sub-graphs and analyze them individually, while using only undirected graphs masks much of this variation.

Having described the network structure, I leverage the panel nature of my data to show that networks respond endogenously to the introduction of a new irrigation technology. My results are consistent with a model of network formation in which farmers seek information from treated households (Dar et al., 2020; Fernando and Sharma, 2019). Using directed agricultural sub-graphs, I show that receiving a new technology as part of the RCT led treated farmers to be much more centrally located within their village social network 3 years after treatment. These results are explained by treated and non-treated households differentially maintaining existing network links, and creating new links, with households that receive the new technology. Finally, I highlight the importance of accounting for changing network structure by applying these results to a new peer-effect model developed in a recent paper by Comola and Prina (2019) that incorporates endogenous network response to an intervention.

This paper uses data collected as part of a technology adoption RCT with small-scale farmers across 390 villages in Western Kenya between 2014 and 2018. The intervention was receiving a new type of manual irrigation pump designed for use in this context. Villages

were first randomized into treatments where pumps were then assigned to two individuals randomly or via “selective trial” lotteries in which individuals could probabilistically influence their treatment status (Chassang et al., 2012). Related to the literature on seed targeting (Beaman et al., 2018), selective trials should disproportionately treat individuals with a high propensity for experimentation. Using random pump allocation as a baseline, the design allows me to compare effects for individuals who self-select into treatment by increasing their odds with those who are randomly given a pump. At the village level, I can test whether outcomes differ for villages randomized into the selective trial arms. A more detailed description of the experiment can be found in Section 2.

Full, dyadic social networks for geographically concentrated subsets of villages were collected at baseline (2014-2015) and endline (2018) for 190 of the villages. In another 200 villages, baseline networks were elicited only for a sample of respondents and endline networks were collected for all respondents. Consequently, in order to leverage the panel nature of the data, the majority of the analysis in this paper focuses on the first 190 villages. When possible, I incorporate the latter set of villages using a simple differences approach to study endline network outcomes.

Relying on this RCT design, I present three sets of reduced-form results. First, I show that at the village-level the treatment does not cause detectable changes in the overall village network structure. Through ANCOVA and difference-in-difference specifications, I find that there is no change in network density or other network statistics that describe village connectivity. This holds across network sub-graphs and for all treatment arms. This differs from related literature that shows decreases in network connectivity and closure in response to the introduction of microfinance and community development projects (Banerjee et al., 2019; Heß et al., 2019).

The second set of results leverages the random allocation of pumps within villages: I show that winning a pump substantially increases an individual’s network centrality within the village. Relative to low-value non-winners, the number of respondents who mention receiving farming help or advice from the winner increases by 19.4%. The pump winners’ betweenness centrality, a measure of their position on the shortest connecting path between any other two nodes in the network, increases by 49.8% and their closeness centrality, a measure of an individual’s distance to all other network nodes, increases by 5.2%. Results are similar for other agricultural sub-graphs.

The final set of reduced-form results rely on a set of dyadic regressions to study link evolution over time. Consistent with a story of individuals “seeking the treated”, I find that a pump winner’s links with another pump winner (WW) are 22.8pp more likely to be sustained at endline, conditional on having existed at baseline, in the agricultural sub-graph, relative to a link between two non-winners (NN). This represents a 58% increase in the probability of observing WW links at endline relative to a two non-winner dyad. The

same is true when considering link formation for the set of links that did not exist at baseline: WW links are 16.9pp more likely to have formed by endline (83.6% increase relative to a two non-winner dyad).

There are similar trends when considering the directed non-winner to winner links (NW). Conditional on having existed at baseline, NW links are 8.5pp more likely to be sustained at endline in the primary agricultural sub-graph. If the links did not exist at baseline, then relative to a two non-winner dyad, non-winners are 5.4pp more likely to report a new link with respect to receiving farm help/advice from a pump winner. These results are consistent with recent literature that shows that farmers may expand or change their social network to acquire new information and resources (Dar et al., 2020; Fernando and Sharma, 2019; Magnan et al., 2015) and inform a model of strategic network formation in which the utility from a link is an increasing function of a partner's pump ownership status.

In the final section of the paper, I highlight the importance of considering endogenous network change by adapting a peer-effect model that accounts for changes in response to the intervention. Extending Comola and Prina (2019) to a setting with direct graphs, I show that failure to incorporate network change when estimating peer effects may bias the estimated direct effects of treatment upwards. I provide evidence that farmer crop income is affected not only by the changes in baseline peer income but *also* by changes in the income of their new peers. Standard peer effect models do not measure the latter term.

This paper contributes to a large literature in development economics on technology diffusion and social learning. There is substantial evidence that social learning occurs among farmers (Foster and Rosenzweig, 1995; Conley and Udry, 2010; Banerjee et al., 2013; Magnan et al., 2015). By focusing on endogenous changes in network structure, I emphasize a primary channel through which learning may occur. This also relates to the active literature on seed targeting in networks (Beaman et al., 2018; Dar et al., 2020; Akbarpour et al., 2017; Kondylis et al., 2017). While the RCT does not target adopters based on their network characteristics, it does focus on farmers who signal their desire to experiment with a new technology which is an important first step for diffusion (Munshi, 2004).

Second, this paper contributes to the burgeoning literature on network response to policy interventions. Recent papers by Dupas et al. (2019) and Comola and Prina (2019) document positive spillovers with respect to intra-village connections after the introduction of savings accounts. In Binzel et al. (2013), people substitute away from informal borrowing within their networks following the opening of a bank branch. Heß et al. (2019) show that large-scale international aid projects lead to declines in network connectivity. The paper that relates most closely to this one is Banerjee et al. (2019). They find that the introduction of microfinance decreases network connectivity over time and explain these trends by developing a new model of intra-village social interactions. They, along with Comola and Prina (2019) are two of the few papers who directly observe social networks at multiple points in time.

This paper extends these findings in the context of an agricultural technology and makes use of an especially rich social network panel to study changes in separate network sub-graphs. Relative to a financial intervention, the irrigation pumps in this paper can be easily shared which may make strategically linking with treated households particularly attractive. Finally, by incorporating the model of dynamic peer effects outlined by Comola and Prina (2019) this paper adds to the literature studying peer effects using network data with endogenous network structure (Goldsmith-Pinkham and Imbens, 2013; Graham, 2017; Bramoullé et al., 2009). I extend the model in Comola and Prina (2019) to a setting with directed networks and adapt it to selective trial experimental design.

The rest of the paper is structured as follows. Section 2 describes the context and technology adoption RCT. Section 3 describes the data. Section 4 presents the main empirical findings on network change. Section 5 uses the empirical findings to motivate a simple model that allows for endogenous network response to treatment and discusses the implications for seed targeting and estimating peer effects. Section 6 outlines planned future work and concludes.

2.2 Context, Experiment Design, and Data

2.2.1 Selective Trials for Technology Adoption

This paper uses multi-wave panel data collected as part of a large-scale randomized controlled trial (RCT) studying technology adoption by small-scale farmers in Western Kenya. The RCT was a field application of Chassang et al. (2012) which extends a standard RCT design to disentangle treatment effects when they depend on heterogeneous subject effort. The authors use the moniker “selective trials” to describe their principle-agent approach to technology adoption RCT design: the principle’s goal is to maximize information about a new technology and agents may express preferences by probabilistically selecting themselves into or out of treatment. They show that the information available to the researcher via a selective trial represents a superset of the information that would have been obtained via a comparable RCT. As a result, selective trials may improve RCT external validity by allowing the experimenter to recover the distribution of returns to a technology, or marginal treatment effects, as a function of willingness to pay.

The technology considered by the project is the Kickstart MoneyMaker Hip Pump (henceforth “pump”), a low-cost manual irrigation pump designed for use in rural Kenya. For approximately USD \$60, the basic pump system includes the pumping unit, 2 hoses, an intake filter, and a handheld sprinkler attachment. Given its limited range, users need a water source close to where they intend to use the pump. Use of the basic system requires

2 people: one working the pump and the other holding the sprinkler. Pumps were designed to be used for horticulture cultivation during the dry season and for other household tasks (e.g. brick making, pumping drinking water from a borehole well). While the pump was already available for purchase in regional cities when the project began, at baseline almost no farmers in the sample owned any type of irrigation system or were familiar with the pump.

The project took place in rural Western Kenya between 2014 and 2018, an ideal location to study technology adoption by small-scale farmers. The typical farmer in the sample owns 2.2 acres of land and reported total incomes in the previous year of 50,000 Kenya Shillings (USD \$505) at baseline. Prior to the introduction of the pump, 40% of farmers regularly irrigated and virtually all of those used non-mechanized means for irrigation (e.g. watering can, bucket).

2.2.2 Experimental Design

The experiment used 2-stage randomization. The first stage took place at the village level, allocating villages to one of 4 treatment arms or a control group. Within each treatment arm, individuals were randomized into treatment via a public lottery for a pump with winners chosen either through a “selective trial” or via purely random selection. Lottery winners received a voucher redeemable for a free pump at the local agro-dealer. All winners were free to do with the pump as they pleased, but were encouraged to try using it and to share what they learned with their neighbors. All pump winners ultimately redeemed their vouchers.

Prior to treatment, all households (including the control group) were invited to a pump information session where they were shown how to use the pump and could try it for themselves. Irrespective of attendance at the information session, all sampled households received a 15% discount voucher for the pump redeemable at the local agro-dealer. Discount vouchers were redeemable for 1 year. During the information session, households in non-control villages were provided information about their village’s treatment, a lottery for one of the irrigation pumps, which would take place approximately one month later.

Below I describe the details of the various treatment arms. The implementation of the latter three were changed for the projects’ third round of data collection. More details can be found in the forthcoming paper by Chassang, Dupas, and Snowberg.

Random: In the *Random* treatment arm, all farmers were given a lottery ticket with uniform probability of winning a pump ($\sim 5\%$ chance). The lottery was held in public approximately one month after the baseline survey. Farmers could win a pump whether or not they attended the lottery. Two tickets were randomly drawn from a bucket and the winners were given their vouchers for a free pump. In Waves 1 and 2, a subset of villages assigned to the random treatment arms had only one winner at the time of the initial lottery. For these villages, a additional winner was randomly selected a few months later and given a

voucher for a free pump such that all villages ultimately had two free pump winners. Unless otherwise specified, I group the first and second random lottery winners in my analysis.

Cash: For Waves 1 and 2, all farmers in the *Cash* treatment were given a lottery ticket with uniform odds of winning a pump, as in the Random treatment arm. Farmers were then given the option to purchase an additional ticket for 150 Kenyan Shillings ($\sim 2.5\%$ of the pump’s value) that would improve their odds of winning. Farmers who paid for an additional ticket were entered into a second lottery, with identical odds of winning as the first. Given the fixed odds, there was no guarantee of a winner in the second lottery. Farmers were then given 2-3 weeks to come up with the money which could be paid at multiple “collection days” before the lottery or on the day of the lottery; farmers could pay in cash or with an equivalent in-kind payment in maize.

In Wave 3, the project used a quantile targeting mechanism to determine lottery participants. Using a BDM mechanism to elicit willingness to pay for a lottery ticket, there was an auction for 7 total lottery tickets where farmers bid in cash. The 7 highest bidders paid the maximum of 50 Kenyan Shillings or the 8th highest bid in the village on the day of the lottery. This ensured that the lottery enrolled a fixed quantile of high value participants in each village. Two winners were selected in each village from the 7 high-value participants in a public lottery.

Task: The *Task* treatment arm mirrored the Cash treatment arm but farmers were asked to “pay” for a lottery ticket by working for a number of hours on a farm in a neighboring village. This is comparable to “ordeal” mechanisms used elsewhere in the willingness to pay literature (Nichols and Zeckhauser (1982); Alatas et al. (2012)). In waves 1 and 2, the “price” of a ticket was 3 hours of work, while in the third wave prices were determined via quantile lottery. Respondents could work on one of two “task days” scheduled over the 2-3 weeks.

Group Vote: The final treatment arm asked respondents to vote for other villagers in the sample who they believed would be the best recipient of a pump. In Waves 1 and 2, a self-vote was imposed such that each farmer had at least one chance of winning. Farmers were then asked to cast two additional votes, a first and second choice, where they could not vote again for themselves. Respondents submitted 2 ballots with a first and second choice listed on each: on the first, the respondent’s self-vote was listed as the first choice by default, and their first choice besides themselves was the 2nd choice; on the second, the respondents two non-self votes were recorded. Two winning ballots were selected per village. If the first choice on a given ballot had already won on the first draw, then the second choice on that ballot became the second winner. Respondents not in attendance at the lottery could still win a pump: the ballot with a self-vote was submitted regardless of attendance and other respondents could still vote for people not in attendance.

For the *Group Vote* treatment arm in Wave 3, two pumps were randomly allocated among

the top 7 vote recipients in the village, again ensuring that a constant quantile of “high value” participants was entered in the lottery. A full evaluation of this RCT with additional details about implementation can be found in Chassang et al. (forthcoming). Figure B.1.1 provides a graphical representation of the design.

2.2.3 Defining High Value Farmers

As previously mentioned, selective trials elicit willingness to pay (WTP) for treatment and allows those with high WTP to positively influence their treatment probability. It is useful in the later analysis to label this group as “high-value”. In the *Cash* and *Task* arms, high-value respondents were those who self-selected into the second set of lotteries (Waves 1 & 2) or were part of the upper quantile in WTP for a lottery ticket (Wave 3). This results in 16.5% of *Cash* respondents being listed as high-value in Waves 1 & 2 and 41.4% of respondents in the *Task* arm. In the *Group Vote* village, I follow Dupas, Chassang, and Snowberg in defining respondents as high-value if they received 3 or more total votes. In the *Cash* and *Task* treatment arms, treatment is random conditional on being high-value. In the *Group Vote* arm, treatment probability is an increasing function of vote count such that treatment is approximately random after controlling for high-value status ¹

2.2.4 Village Selection and Household Sampling

Since the RCT was interested in the adoption of an irrigation pump that required households to have access to a water source, the project focused on sub-county districts that were known to have water sources in most villages. Villages were randomly selected from an exhaustive district-level village list and the team ensured that no sampled villages were directly neighboring one another in order to prevent treatment contamination.

Prior to baseline data collection in the first 2 waves, a partial village census was conducted to determine household eligibility and collect basic household characteristics to be used for stratification. In the third wave, the census was collected simultaneously with a shortened baseline survey.

Households were eligible for selection if they farmed and had land close enough to a water source that pump use was viable. Any water source was allowed including streams, rivers, lakes, ponds, and borehole wells. In each village, the project selected up to 25 eligible households. Beginning with the household in the geographic center of the village, enumerators used snowball sampling around the central household to select the households to be included. If the village had fewer than 25 eligible households, all eligible households

¹In future drafts, I will control for the precise treatment probability in each arm.

were sampled. In the event that a village had fewer than 8 eligible households, a neighboring replacement village was selected. On average, there are 22 sampled households per village. The eligibility criterion and the cap on the number of households together result in few villages in which all households were sampled. In some of the largest villages, the sample of 25 households represents less than 25% of the total village population. A discussion of the implications of this sampling methodology for the social network data can be found below.

2.3 Data

The project was rolled out in 3 waves: the first took place in Busia county, the second in Bungoma county, and the third jointly in Bungoma and Siaya counties. There were 190 sampled villages in Waves 1 and 2 and 200 villages in Wave 3, for a total of 390 villages. A map of the villages can be found in Appendix Figure B.1.2. Census and baseline data collection took place between Spring 2014 and Spring 2015. The baseline surveys included a battery of questions on household characteristics, farming practices, income, social networks, and measures of willingness to pay for or experiment with new technology. After the baseline surveys, villages were stratified by farm size and household income, and randomly assigned to treatments as described above. A few days before baseline data collection, all villages received a 2 hour information session about the irrigation pump. Interventions occurred in the Summer and Fall of 2014 for Waves 1 and 2 respectively and in Summer 2015 for Wave 3.

After the treatments, in late 2015, there was a mid-line survey for households in Waves 1 and 2. During this time, a subset of farmers in Wave 3 villages answered an extended baseline survey. A second mid-line was conducted in late 2016 for farmers in all villages. The end-line survey for all villages took place in Spring 2018. In addition to many of the questions asked at baseline, mid-line and end-line surveys included additional questions about respondents' interactions with pump owners.

2.3.1 Social Network Data

Unusually rich social network data was collected at multiple points throughout the project. This data records not only agricultural links but also other types of social interactions and measures of link strength. As will be discussed later in detail, this allows me to look at changes in network structure along multiple dimensions.

Waves 1 & 2: For Waves 1 and 2, names collected during the census were entered into a table such that respondents were asked questions explicitly about all other households in a given village during the baseline survey. After the baseline, full network modules were asked

for all pump owners during the mid-line survey. Finally, the full network module was asked for all respondents at end-line such that there is a complete, two-wave network panel for all respondents with a third, intermediate wave for pump owners.

Wave 3: Since Wave 3 censuses and baselines surveys were administered simultaneously, full baseline social networks could not be collected. Instead, only a few key questions about close farming links and "top experimenters" in the village were asked. Responses to these questions were matched to respondent identifiers when possible. Just after the interventions, full social network modules were administered to 7 respondents per village: the two pump winners, two high-value respondents who did not win a pump, and three other randomly selected respondents. The full social network module was administered at endline for all respondents. Thus, there is a complete baseline network for a subset of respondents and a full endline.

To ask a respondent explicitly about their relationships with all other respondents in the network is uncommon. Network data is typically collected by asking a respondent to list all of the people in a village with whom a respondent interacts in particular manner. For example, researchers would ask, "Please name all of the people in this village with whom you discuss farming." One may worry that method of network elicitation leads respondents to censor their full set of connections. The omission of an individual would lead the researcher to conclude that no link exists. Recent work shows that censored networks may attenuate peer effect estimates (Griffith (2019)). The concern over "forgotten links" is one of the primary reasons that research using dyadic network data tends to focus on undirected graphs since a link only needs to be mentioned by one of the two respondents to be recorded. Network data elicitation in this project avoids this issue by asking specifically about each other person in the village sample. Thus, non-connections are observed directly and not imputed.

2.3.2 Discussion of Network Data Limitations

While the data used in this paper is unusually rich, this richness comes at the cost of village-level coverage and the limitations of using this data must be acknowledged. First, in most villages the observed social networks do not represent full village-level networks since (1) the networks only include eligible farming households and (2) funding and survey time constraints prevented the project from conducting full village censuses. As mentioned before, a maximum of 25 households per village were included in the study. In any study that relies on self-reported dyadic link data, the researcher must necessarily limit the set of possible connections that can be matched. Typically in the development economics literature, the village is defined to be the outer bound for the set of possible links (Beaman et al. (2018); Banerjee et al. (2019); Dar et al. (2020)), while this paper relies on a geographically concentrated subset of the village. In either case, two individuals who appear to be socially

distant may be connected by another individual outside of the sampling frame. This issue will be more pronounced when the sampling frame is reduced, as in this paper when relying on a subset of the total village. It’s well known that social networks, even for rural and relatively unconnected farmers, extend far beyond the village (Rosenzweig and Stark (1989)). This paper focuses on studying changes in the *observed* network. I cannot rule out that treatment induces changes outside of the observed network, just as other papers in this literature cannot rule out effects or social channels that lie outside of their respective sampling frames. Consequently, one can think of my results on network change as lower bounds of the true effect.

Second, Chandrasekhar and Lewis (2010) show that sampled networks may bias estimates of network and household link statistics. One way to think of the data used in this paper is as a sampled network with a high sampling rate. That is, the observed sub-village network comprised of the ~ 22 sampled households can be thought of as the full network that is subsequently “sampled” due to attrition. Framed in this way, I can apply network statistic corrections derived in Chandrasekhar and Lewis (2010)². In this draft, I follow Banerjee et al. (2019) and calculate all network statistics using induced graphs. Next, the Wave 3 villages can only be included in a subset of the analysis since I only observe partial baseline social networks³. Outcomes and out-degree for Wave 3 winners can still be compared to those of the randomly selected high-value non-winners and when possible, I include Wave 3 endline networks when calculating simple differences. This leverages the random village assignment to treatment.

Finally, one may worry that changes in the network sub-graphs that I focus on in this paper do not reflect changes in the true social network *per se* but rather the temporary activation of a latent connection. Put differently, if the treatment only induces one-time discussions about the pump, and nothing else about the network changes, then it is likely too strong to argue that the results reflect social network change. However, given the 3 to 4 year gap between network collection at baseline and endline, any changes in network characteristics documented in this paper are persistent. Thus, it seems unlikely that one-time interactions shortly after the treatment would be driving the results.

²In this draft I do not use the corrections outlined in Chandrasekhar and Lewis. The observed network does not represent a random subset of the overall village network. There are only minor reductions in network statistic bias if we can consider “sampling” due to attrition where the observed network is considered to be the full network.

³In future work, I hope to identify network structure from the selected Wave 3 samples that I observe. To do so, I will re-create the sampling methodology used for the Wave 3 network baseline in the Waves 1 and 2 baseline networks. Bootstrap sampling from these Waves 1 and 2 baseline networks will allow me to build a sampled network comparable to those from the Wave 3 villages. I will compare this sampled network to the village’s complete baseline network to understand what is missed by using the sampled network

2.3.3 Attrition

Considering the 3-4 year gap between baseline and endline, attrition was relatively low overall at 18.2%. In total, 3,447 respondents completed both baseline and endline in the first 2 waves while 765 completed only the baseline. A substantial portion of this attrition was due to households moving out of the region or passing away such that they could not be contacted. However, there were some who refused to continue to take part in the study.

Attrition regressions are presented in Appendix Table B.1.1. Reassuringly, column (2) shows that treatment status does not predict whether the respondent was missing at endline. We do see that attrition seems to be somewhat higher in *Cash* and *Task* villages, however it's unlikely that this is a meaningful difference - it represents the attrition of less than one additional person per village on average. These coefficients for *Cash* and *Task* are not statistically different from the coefficient on *Random* which is useful for my household-level specifications. There, I can exclude respondents and control villages and use only the variation in household-level treatment to identify treatment effects. The selective trial design makes the application of Lee Bounds challenging since treatment is only random in selective trial arms conditional on high-value status. In future work, I hope to adapt Lee bounds to this context, and to the context of the model presented in Section 5. In column (3) we see that few household characteristics predict attrition. Wealthier (as proxied by house quality) and more educated households are somewhat more likely to be missing at endline. Overall, there is minimal evidence that differential attrition is driving the results in this paper.

2.4 Results

The primary goal of this paper is to understand whether and how networks changed in response to the introduction of a new technology. One can consider changes observable in the overall village-level network as well as changes in interactions for individuals and paired dyads. Since my data records a variety of different types of interactions (sub-graphs), I document network change independently for each sub-graph. While I expect any changes in interactions caused by the introduction of the pump to emerge in agriculturally oriented interactions, as a placebo test I can also check for changes in networks of purely social or other non-agricultural interactions. The ability to separate and independently consider various sub-graphs is a unique benefit of the rich data used in this paper. This relates to the literature on network link strength (Granovetter (1977)) and allows for analysis of changes along the network's "intensive" margin.

In this paper I focus on three sub-graphs, always using directed graphs unless otherwise

specified.⁴ The first sub-graph is whether a farmer has received farming help or advice from another individual. I consider this to be the “strongest” agricultural connection type and reflects an inherently unilateral (directed) link. The second is whether a farmer reports discussing agriculture with another person. While a discussion would generally be considered bilateral (undirected), I think that there is value in considering the directed link as it likely reflects whether the conversations were considered significant. Lastly, I consider the sub-graph of whether a farmer knows the other at all.

2.4.1 Network Statistics

Network structure and individual network position are summarized with a set of standard statistics. This paper focuses on seven such statistics, each of which reflects a different, but potentially correlated, feature of structure or network location. “Out-degree” is the number of people an individual reports linking to; “in-degree” is the number of other people in the network who report linking to an individual. These statistics are identical in undirected graphs since a link missing in one direction is imputed, yet need not be the same in directed graphs. That there would be inequality between in-degree and out-degree is intuitive when thinking about links that reflect learning, information flows, and support. Eigenvector centrality is a measure of node influence that reflects the influence of a node’s connections; betweenness centrality reflects the number of shortest paths between two nodes of which a given node is a part; closeness centrality reflects the distance of a node to all other nodes in the network. Finally, density refers to the total proportion of network ties that are realized relative to the total number that were possible while clustering is a measure of the degree to which nodes in a graph tend to cluster together. Refer to Chandrasekhar and Lewis (2010) for a detailed discussion of these statistics and challenges related to their computation.

One might expect that receiving a pump may influence any one of these measures. For instance, if winning a pump leads more people to ask the pump winner for farming help or support, this would be reflected in an increased in-degree for the pump winner. Similarly, if winning a pump leads other influential farmers to link with the winner, the winner’s eigenvector (and/or closeness) centrality will increase. Averaging these statistics across all nodes in a village allows us to learn something about overall network connectivity. If treated villages generally become more connected, this would be seen in increased network density.

⁴I separately conduct all analysis using undirected graphs.

2.4.2 Who Wins a Pump?

To begin, I document the baseline characteristics of pump winners. Table 2.1 shows the results. Each row is a regression of a network or household characteristic on winner-by-treatment, treatment, and strata dummies.

While the RCT did not explicitly target based on network position, winners in the *Cash* and *Group Vote* arms tend to be more central within a village-level network. They also have higher farming knowledge as reported by other farmers in the village. While winners in the *Cash* and *Task* arms do not appear significantly different from non-winners based on non-network household characteristics, the winners in the *Group Vote* treatment have more farming income, a higher self-reported baseline WTP for a pump, and are more likely to be a member of a community group.

Table 2.1 groups high-value and low-value winners in the selective trial arms. Respondents who are high-value are positively selected across a network and household characteristics as can be seen in Appendix Table B.1.2. I leave a more thorough discussion of the RCT's selection of high-value individuals to the forthcoming paper by Dupas, Chassang, and Snowberg. Finally, Appendix Table B.1.3 incorporates the Wave 3 winners. While the general patterns remain the same, the *Cash* and *Group Vote* winners are more positively selected on measures of education, spending, and income when selected via the quantile lotteries.

2.4.3 Network Composition

In Panel A of Table 2.2, we document balance in network-level statistics and across treatment arms at baseline. Further, we observe substantial variation across network sub-graphs. Consistent with the argument that providing farm help or advice is the strongest link type, it is the least dense of the three networks, while knowing someone at all is the densest. Farmers seem to give and receive farming help and advice to only a subset of those with whom they have discussed agriculture. This supports the assertion that aggregating across all link types, as is done in Banerjee et al. (2019), may miss important sub-graph variation.

Comparing Panel A to Panel B in 2.2, I document changes in network composition over time. There is a secular decrease in network connectivity across all treatment types between baseline and endline. While I cannot rule out that this is mismeasurement due to survey fatigue, Banerjee et al. (2019) show slight decreases in network connectivity in Indian villages over a similar 3-4 year period. This may reflect substitution away from intra-village connections driven by increased mobile phone penetration and market integration during this period. In the analysis that follows, I include period dummies to control for general time trends. Table 2.2 does not lend itself to easily showing changes in network structure attributable to the intervention which is addressed in the next section. Network-

level measures may also obscure intra-village network reshuffling which will be addressed in the sections on household-level and dyadic effects below. Distributions of average degree, betweenness, closeness, and eigenvector centrality by survey round are displayed in Figure 2.1.

To understand the importance of using directed graphs, the analogous undirected statistics can be found in Table B.1.4. Measures of average network density and degree increase by over 50% moving from directed to undirected graphs; average household-level measures of clustering, closeness, and betweenness also increase substantially. This trend does not hold, however, in the sub-graph of whether respondents know each other at all. This is reassuring as we would expect this sub-graph to contain links that are the most bilateral in nature.

Given the different context and nature of network elicitation, it is difficult to compare these networks to others in the literature. However, it is worth noting that average degree for the undirected, agricultural discussion sub-graph is similar to the value in Beaman et al. (2018), though these networks appear to be much denser than those described elsewhere in the literature (Fernando and Sharma (2019); Dar et al. (2020); Banerjee et al. (2019)). It is likely that asking explicitly about each other network member recorded links that would have been missed using traditional methods of network elicitation. Together, these tables support the argument that focusing only on undirected graphs masks substantial heterogeneity seen in their undirected counterparts and that imputing an undirected link may not be justified when considering interactions that need not be bilateral (Comola and Fafchamps (2014)).

2.4.4 Village-level Network Effects

I now consider a set of results where the unit of analysis is the village-level network. Leveraging random village assignment to treatment, my primary specification uses an ANCOVA estimator which increases statistical power relative to a difference-in-differences specification (McKenzie (2012)). With a single baseline and endline, the two estimate the same average treatment effect while the ratio of the variances of the D-i-D and ANCOVA estimator is $2/(1 + \rho)$ where ρ is the auto-correlation of the outcome. The estimating equations are as follows:

$$y(\mathbf{g}_{vs,1}) = \alpha + \beta \text{Treat}_v + \delta X_{v,1} + \theta y(\mathbf{g}_{vs,0}) + \tau_s + \epsilon_{v,1} \quad (2.1)$$

$$y(\mathbf{g}_{vs,1}) = \alpha + \sum_{i=1}^4 \beta_i \mathbb{I}\{\text{Treat}_v = i\} + \delta X_{v,1} + \theta y(\mathbf{g}_{vs,0}) + \tau_s + \epsilon_{v,1} \quad (2.2)$$

Here, $\mathbf{g}_{vs,1}$ is the graph of the network for village v in strata s in post-treatment period. The outcome, $y(\cdot)$ is either network density, average closeness, or average clustering. Treat_v

is an indicator of whether the village was in any treatment arm, while $\mathbb{I}\{Treat_v = i, 1\}$ is an indicator for the village’s specific treatment: *Random, Cash, Task, or Group Vote*. In each specification I include a vector of village-level controls, $X_{v,1}$ and a village’s baseline level of the outcome variable, $y(\mathbf{g}_{vs,0})$.⁵ While treatment was randomly assigned, these will control for village characteristics that could cause networks to change differentially even absent the treatment. Finally, τ_s are strata fixed effects from the village-level stratification into treatment. Standard errors are clustered at the village level.

The results can be found in Table 2.3. Panel A is the agricultural discussion network while Panel B is the farming help or advice network. Across both networks and for each outcome variable, there are precisely estimated null effects on overall network structure. Results are similar for the analogous difference-in-differences specification which can be found in Appendix Table B.1.5.

In Table B.1.6 I incorporate the network statistics from the Wave 3 endline and find no effect on network structure using simple differences with strata fixed effects and robust standard errors. Taken together, these results show that the seeding of a new technology with 2 farmers was insufficient to cause changes in overall network structure.

2.4.5 Household-level Effects

In this subsection, I explore how household-level network position is affected by the intervention. The design of the RCT allows me to recover average treatment effects for three subsets of respondents: *Random* winners vs. non-winners, low-value selective trial winners vs. non-winners, and high-value selective trial winners vs. non-winners. Treatment was purely random for *Random* and low-value winners. For the second set of lotteries, conditional on being high-value, treatment was random.⁶ Fully exploiting the richness of the selective trials framework allows me to separate effects by farmer treatment and high-value status across the whole sample. This heterogeneity is of particular interest since the selective trials were designed to target high-value experimenters who would be more inclined to share and discuss the technology.

As above, I use an ANCOVA estimator to improve power, taking advantage of the random

⁵These controls are village size, share of the village who is an elder, aged 55+, grows maize, grows vegetables, is a member of a community group, the share of female-headed households, average male and female education, land size, income from non-agricultural sources, an index of house quality, baseline WTP for a pump, share able to access funds in an emergency, and a measure of average geographic proximity.

⁶The probability of winning varies slightly for low-value and high-value *Group Vote* respondents since it is a function of the number of votes received. I can control explicitly for the probability of winning for all treatment groups.

allocation of the pumps through the lotteries.⁷ The estimating equations take the following form:

$$y_{iv,1} = \alpha + \beta \text{Win}_{iv} + \gamma \text{HV}_{iv} \times \text{Win}_{iv} + \delta \text{HV}_{iv} + \psi X_{iv,1} + \xi y_{iv,0} + \tau_v + \epsilon_{iv,1} \quad (2.3)$$

$$y_{iv,1} = \alpha + \sum_{k=2}^4 \beta_k \mathbb{I}\{\text{Treat}_v = k\} \times \text{Win}_{iv} \times \text{HV}_{iv} + \sum_{k=2}^4 \gamma_k \{\text{Treat}_v = k\} \times \text{Win}_{iv} \\ + \sum_{k=2}^4 \delta_k \{\text{Treat}_v = k\} \times \text{HV}_{iv} + \eta \text{Win}_{iv} + \psi X_{iv,1} + \xi y_{iv,0} + \tau_v + \epsilon_{iv,1} \quad (2.4)$$

where $y_{iv,1}$ is an outcome variable (network statistic) for individual i in village v in the post-treatment period. Let Win_{iv} be an indicator that individual i won a pump in village v , and HV_{iv} is an indicator for an individual's high-value status. $X_{iv,1}$ is a set of household controls and $y_{iv,0}$ is the value of the pre-intervention outcome. Finally, τ_v are village fixed effects and standard errors are clustered at the village level.

The results for the two agricultural networks are presented in Table 2.4. Panel A is the agricultural discussion network while Panel B is the farming help/advice network. Odd numbered columns are simple differences controlling for high-value status, while even numbered columns include household-level controls and the baseline value of the outcome variable. Outcomes across the two networks are very similar. The results are consistent with pump winners becoming more central within a village social network. In Panel B, the number of people who mention receiving help or advice from winners increases by 19.4% (column (2)) relative to low-value non-winners, betweenness centrality increases by 49.8% (column (6)), and closeness centrality increases by 5.2%, all significant at the 1% level. Columns (3) and (4) show that there is also a significant effect on the number of individuals from whom a winner reports *receiving* farm help or advice.

Across all outcomes and specifications, we similarly see that the coefficient on being high-value is highly significant, even after controlling for the baseline outcome level. While high-value status is not randomly assigned, this is suggestive evidence that publicly signalling one's propensity to experiment with a new technology may make other households more likely to reach out and for help or agricultural discussion.

The negatively and occasionally significant coefficient on the interaction term reflects the fact that after controlling for a household's treatment and high-value status, there is no additional increase in network centrality for high-value winners. If anything, there is a slight decrease.

⁷Autocorrelation for in-degree, betweenness centrality, and closeness are 0.566, 0.281, and 0.356 respectively in the directed farm help/advice graph.

The full heterogeneity analysis by treatment arm and high-value status can be found in Appendix Table B.1.7. Estimates are noisier, but consistent with the grouped treatment results. Effects are concentrated in the *Task* and *Group Vote* arms, while there seem to be minimal effects on network centrality for *Cash* winners and high-value households. For a full discussion of heterogeneity by treatment arm, see the forthcoming Dupas et al. paper. Average treatment effects for subsets of the sample can be found in Appendix Table B.1.8. They are similarly consistent with winners becoming more central irrespective of high-value or low-value status.

The household-level results are consistent with the finding that there is no overall network change in network density or average statistics. First, only 2 people per village were treated, so even though treated individuals became more central, this doesn't have a large effect on overall network density or the average measures. Second, as can be seen in Figure B.1.3, pump winners still saw decreases, on average, in out-degree, in-degree, and closeness centrality.⁸ However, these decreases were significantly smaller than those experienced by non-winners on average.

2.4.6 Dyadic Results & Changes in Links

The final set of reduced-form results explore link survival and dissolution over time. Any directed dyad can be of four types: winner linked to another winner (WW), a winner linked to a non-winner (WN), a non-winner linked to a winner (NW), and non-winner linked to another non-winner (NN). Following Banerjee et al. (2019), I can use a set of dyadic regressions (Fafchamps and Gubert (2007)) to estimate:

$$g_{ij,v,1} = \alpha + \beta_1 WW_{ij,v} + \beta_2 WN_{ij,v} + \beta_3 NW_{ij,v} + \delta' X_{ij,v} + \tau_v + \epsilon_{ij,v,1} \quad (2.5)$$

where $g_{ij,v,1}$ is an indicator that a link is present between individuals i and j in village v in the post-intervention period. $X_{ij,v}$ is a vector of household-level controls interacted at the dyad-level and τ_v are village fixed effects. Following Fafchamps and Gubert (2007) and Cameron and Miller (2014), I use dyadic cluster robust standard errors that account for the dyadic error correlations.

First, limiting to the set of links that *existed* at baseline ($g_{ij,v,0} = 1$), I can test whether the introduction of the pump differentially changes the probability that the link still exists at endline. Similarly, by limiting to the set of links that *did not* exist a baseline ($g_{ij,v,0} = 0$), I can test for whether the introduction of a new technology differentially changes the probability

⁸There was a slight increase in average eigenvector centrality.

of link formation. I note here that the estimation of (2.5) requires that I limit my sample to only include treated villages since control villages contain no pump winners or losers.

Table 2.5 presents the results. Panel A is the agricultural discussion network while Panel B is the farm help and advice network. Columns (1)-(3) restrict to the set of links that existed at baseline and columns (4)-(6) are the set of links that did not exist at baseline. Within the set of links that existed at baseline, the probability that a WW link exists at endline increases by 14.7pp in the agricultural discussion network and 22.8pp in the farm help/advice network relative to LL links off bases of 47.5 and 39.1 respectively. There are also significant increases for NW links in both networks: 8.3pp and 8.5pp. So conditional on the link existing at baseline, pump winners are much more likely to still be linked with other pump winners at endline and non-winners are much more likely to report a link with a winner at endline. Directed WN links do not see a significant change in either network.

Now considering the set of links that did not exist at baseline, there is a similar pattern. WW links are more likely to have been created in both agricultural networks with increases of 18.5pp and 16.9pp off bases of 24.9 and 20.2 respectively. Additionally, NW links are significantly more likely to exist at EL with 5.9pp and 5.4pp increases in the agricultural discussion and farm help/advice networks. Again, directed WN links are not statistically more likely to have been formed between baseline and endline relative to an NN link.

Together these results show that pump winners are much more likely to maintain connections or create new links with other pump winners. Non-winners seem to be similarly incentivized to link with pump winners regardless of whether they were previously linked at baseline. This story is consistent with the results in Fernando and Sharma (2019) that find non-treated individuals may “seek the treated” when treated individuals have information or resources that the non-treated individuals find valuable. Table 2.5 also displays the dynamic nature of social networks in this setting. Less than 50% of NN links that existed at baseline still exist at endline, 25% of NN links that did not exist at baseline appear at endline. The formation of previously non-existent links means that these results cannot be explained by the secular trend between baseline and endline. This implies that the assumption of static networks is a poor approximation of reality, even in a rural and agricultural setting where one would likely expect networks to be more stable.

In Appendix Table B.1.9 and Appendix Table B.1.10 I incorporate a second dyad feature of the members’ high or low-value status. This creates four new dyad categories: high-value linked to another high-value (HH), high-value linked to low-value (HL), low-value linked to high value (LH), and low-value linked to another low-value. I interpret these results with caution since the large number of estimated coefficients would make it likely that one or more would be statistically significant by chance, and high-value status is an endogenous characteristic.

The conclusions above generally hold. Effect magnitudes for WW and NW link remain

largely the same, though the estimates are less precise. Across networks and regardless of baseline link status, HH and LH links are substantially more likely to exist (either persist or be created) at endline. These results may be explained by individuals signalling their type (as high-value) in a selective trial changing the desirability of linking with that person.

Summing up, these results provide striking evidence of strategic network formation (see e.g. Graham (2017)) in response to treatment. Additionally, networks in this setting are dynamic - many links ceased to exist between baseline and endline while many others were formed. One only needs to consider the simplest model of link formation, where an agent's utility derived from a link is increasing in a partner's pump ownership, to rationalize the results.

$$L_{ij,t} = \begin{cases} 1 & \text{if } B(d_{ij,t}, T_{it}, T_{jt}) - C(d_{ij,t}) + e_{ij,t} > 0 \\ 0 & \text{otherwise} \end{cases}$$

Here, agent i chooses to link with agent j in period t as long as the utility from doing so is positive. The linking decision is determined by a benefit function, $B(\cdot)$, which depends on a measure of network distance, d_{ij} and the agents' respective treatment status (pump ownership), T_{it} and T_{jt} . The results in Table 2.5 imply that $\partial B / \partial T_j > 0$ unconditionally and that there may be an additional utility benefit conditional on own treatment, $T_{it} = 1$.

Documenting this type of strategic network behavior and dynamism has rarely been possible in the literature. As shown in Comola and Prina (2019) and expanded upon in the following section, failure to account for strategic network change in response to an intervention or the introduction of a new technology may significantly bias treatment and peer effect estimates.

2.5 A Model with Dynamic Peer Effects

Considering respondents strategically maintain or form network links in response to the intervention and that there is substantial link churn over time, we would like measures of treatment and peer effects that account for these endogenous changes. In general, diffusion and peer effect models hold the network structure fixed. Recent theoretical work (Goldsmith-Pinkham and Imbens (2013); Graham (2017)) has made progress on models of endogenous network formation, but they have generally lacked longitudinal network data and have not been oriented around interventions or policy that may drive network change. The innovation in Comola and Prina (2019) is to leverage panel network data in order to relax the fixed network assumption when estimating treatment and peer effects. Using data from an RCT in Nepal they show that traditional peer effect models that fail to account for changes in network structure dramatically underestimate treatment and peer effects.

To fix ideas, consider the experiment in this paper where receiving a pump may have an effect on an outcome of interest (e.g. crop income). Treatment effects may come through (1) the direct treatment effect (one’s own treatment status) or (2) the indirect treatment effect (peer effects dependent on peer treatment status). We can further separate this second channel into what the Comola and Prina (2019) call an “outcome peer effect” which reflects the change in one’s partners’ outcomes holding connections constant, and a “network peer effect” which accounts for the shift in partner outcomes due to network change. That is, the network peer effect explicitly accounts for the change in an individual’s peer group over time. Since these two peer effects are likely to be correlated, omitting the latter may bias estimates. This is particularly relevant in the context of this paper where many of the benefits from a pump may come from rental, sharing, and hands-on experimentation. These benefits appear to push non-treated individuals to seek out pump owners despite even if they were not closely connected before the intervention. In Appendix B I work through the model in detail, following closely to Comola and Prina (2019). I extend their model to use directed graphs and incorporate the selective trial experimental design.

2.5.1 Set-up

Consider a directed, linear-in-means peer effects model with N agents, $n \in \{1, \dots, N\}$ and two time periods $t \in \{0, 1\}$. y_t is an $N \times 1$ column vector of an outcome of interest indexed by period, and \mathbf{G}_t is an $N \times N$ semi row-standardized directed adjacency matrix. \mathbf{W} is an $N \times 1$ row vector indicating treatment status (winning a pump) and \mathbf{H} is an $N \times 1$ row vector indicating a high-value status. Then the equation for each period can be written as:

$$y_0 = \beta_1 \mathbf{G}_0 y_0 + \mu + \epsilon_0 \tag{2.6}$$

$$y_1 = (\beta_1 \mathbf{G}_0 + \beta_2 \mathbf{G}_{1-0}) y_1 + \gamma \mathbf{W} + (\delta_1 \mathbf{G}_0 + \delta_2 \mathbf{G}_{1-0}) \mathbf{W} + (\eta_1 \mathbf{G}_0 + \eta_2 \mathbf{G}_{1-0}) \mathbf{H} + \psi \mathbf{H} \mathbf{V} + \mu + \epsilon_1 \tag{2.7}$$

where $\mathbf{G}_{1-0} = \mathbf{G}_1 - \mathbf{G}_0$ is the observed change in the network and μ is a vector of unobserved individual-level heterogeneity. Here, I control for the interaction of \mathbf{G}_0 and \mathbf{G}_{1-0} with \mathbf{H} to reflect the fact that in selective trials, treatment is random only after conditioning on high-value status. Stacking the equations to use block matrices and transforming by $\mathbf{J} = [\mathbf{I}_{2 \times 2} - \frac{1}{2} \mathbf{1}_{2 \times 1} \mathbf{1}'_{2 \times 1}] \otimes \mathbf{I}_{N \times N}$ we obtain the main estimating equation in first differences:

$$\begin{aligned} (\mathbf{y}_1 - \mathbf{y}_0) &= \beta_1 \mathbf{G}_0 (\mathbf{y}_1 - \mathbf{y}_0) + \beta_2 \mathbf{G}_{1-0} \mathbf{y}_1 + \gamma \mathbf{W} + \delta_1 \mathbf{G}_0 \mathbf{W} + \delta_2 \mathbf{G}_{1-0} \mathbf{W} \\ &\quad + \eta_1 \mathbf{G}_0 \mathbf{H} + \eta_2 \mathbf{G}_{1-0} \mathbf{H} + \psi \mathbf{H} \mathbf{V} + (\epsilon_1 - \epsilon_0) \end{aligned} \tag{2.8}$$

Here, β_1 is the outcome peer effect, β_2 is the network peer effect, and δ_1 and δ_2 are contextual peer effects.⁹ The contextual peer effects can be interpreted as the direct effects of peers' treatment status after accounting for their crop income. In this setting, contextual peer effects may reflect benefits of being connected to pump owners such as the ability to rent or borrow the pump.

Here, note that setting $\mathbf{G}_{1-0} = 0$ returns the standard peer-effects model and setting both $\mathbf{G}_{1-0} = 0$ and $\mathbf{G}_0 = 0$ is a treatment response model with no peer effects. Relative to the peer effects model in Bramoullé et al. (2009) this model allows for heterogeneity over time and with respect to link type: I separately estimate peer effects from baseline links (\mathbf{G}_0) and those formed at endline (\mathbf{G}_{1-0}).

2.5.2 Identification

Identification in this model relies on two primary assumptions.

Assumption 1. Conditional exogeneity¹⁰:

$$\mathbb{E}[\epsilon_t | \mathbf{G}_0, \mathbf{G}_1, \mathbf{W}, HV, \mu] = 0 \text{ for } t = 0, 1$$

This assumption is standard in the literature (Bramoullé et al. (2009)), while conditioning the exogeneity on individual-level effects remedies selection bias from homophily as long as correlated unobservables are time invariant .

Assumption 2. Fully observed structure:

Since the identification strategy will rely on lagged partner characteristics, identification requires that spillovers spread only through the observed structure of interactions. Peer effect estimates will be biased upwards if spillover spread over other network dimensions than those that are measured or if the data systematically underestimates connections. The richness of the data in this paper makes it particularly well-suited to address concerns about this assumption.

Comola and Prina (2019) show that if the above assumptions and two weaker, supplementary conditions hold¹¹, then both the outcome and network peer effects are identified. Of course, both the outcome peer effect, $\mathbf{G}_0(\mathbf{y}_1 - \mathbf{y}_0)$, and the network peer effect, $\mathbf{G}_{1-0}\mathbf{y}_1$, are endogenous and must be instrumented. The instrumentation strategy adapts methods, standard in the peer effects literature, that leverage changes in the exogenous characteristics of peers of peers, also referred to as lagged or second-order peers (Bramoullé et al. (2009); Calvó-Armengol et al. (2009)). With the conditional exogeneity assumption, one can generate a

⁹Manski refers to contextual peer effects as exogenous social effects.

¹⁰Exogeneity of \mathbf{G}_1 can be relaxed by using the predicted change in the network as an instrument for the observed change

¹¹See Appendix B

finite set of instruments using a series expansion of $\tilde{\mathbf{S}}(\beta)^{-1} = \left[\mathbf{I}_{2N} - \beta_1 \tilde{\mathbf{G}}_0 - \beta_2 \tilde{\mathbf{G}}_{1-0} \right]^{-1}$.¹² The instruments account for an individual’s network centrality and lagged-peer characteristics. They reflect the idea that an agent’s outcome is not affected directly by the characteristics of their friends’ friends (i.e. peers of peers who are not linked to the agent) except through their own friends.

There are four second order (lagged-peer) instruments that satisfy the minimal identification conditions. The intuition is that peer effects may come through the old network peers (\mathbf{G}_0) as well through new peers (\mathbf{G}_{1-0}). Each of these groups, old and new peers, in turn have their own set of old and new peers that may affect their outcomes. The four primary instruments correspond to the share of treated individuals among the old peers of old peers, new peers of new peers, old peers of new peers, and new peers of old peers. Slightly abusing notation, these can be written as:

$$(\mathbf{G}_0)^2 \mathbf{W}, (\mathbf{G}_{1-0})^2 \mathbf{W}, \mathbf{G}_0 \mathbf{G}_{1-0} \mathbf{W}, \mathbf{G}_{1-0} \mathbf{G}_0 \mathbf{W}$$

The instruments are valid if the lagged-network characteristic changes do not affect an agent’s outcomes except through the outcomes of their direct peers. In the authors’ analysis and what follows, the set of 8 additional third-order (second-lagged) IVs are included¹³.

Finally, I can use the above framework to calculate a total treatment effect that incorporates the indirect spillover of the intervention on outcomes using a methodology adapted from the spatial econometrics literature. This incorporates not only the peer effects due to changes in the treatment status in baseline peers, but also peer effects from the intervention-driven changes in the network. I estimate $\frac{\partial E(y_i | \mathbf{W}, HV)}{\partial W_k}$ which is an $N \times N$ matrix of partial derivatives. The k^{th} column of $\frac{\partial E(y_i | \mathbf{W}, HV)}{\partial W_k}$ is an $N \times 1$ vector that represents the effect of agent k ’s treatment on the outcomes of all other agents.

2.5.3 Estimation

I apply this framework to my data, focusing on the directed farming help/advice graph. I choose this graph to be consistent with what has been used throughout the paper but could have chosen any sub-graph that I believe to satisfy Assumption 2. My outcome of interest is crop income (unit: 1000Ksh \approx USD10). Due to a large number of zeros, I use the inverse hyperbolic sine transformation. The results can be found in Table 2.6 which mimics Table 1 from Comola and Prina (2019).

¹² $\tilde{\mathbf{G}}_0$ and $\tilde{\mathbf{G}}_{1-0}$ are the stacked adjacency matrices in the system of equations before taking first differences.

¹³See Appendix B

Column (1) estimates the model with no peer effects, Column (2) estimates the model with static peer effects, and Column (3) estimates the model with dynamic peer effects. Focusing on columns (2) and (3), the coefficient for the outcome peer effect is statistically significant and suggests that a 10% increase in baseline partner crop income increases an individual's own crop income by 4.9%. Similarly, the coefficient on the network peer effect (\mathbf{G}_{1-0}) implies that a 10% increase in new link crop income, relative to old links, increases own crop income by 2.5%. In neither column (2) nor (3) are the contextual peer effects significant. This is somewhat surprising in this setting as one would expect peer pump ownership may directly impact crop income through rental or sharing. In future work, I plan to investigate the creation of rental and sharing sub-graphs in detail.

The direct treatment effect is large in economic magnitude and significant in the first two specifications. However, the incorporation of dynamic peer effects decreases the coefficient on the treatment variable, following the same pattern as in Comola and Prina (2019). As shown in the previous section, individuals strategically adjust their links after the intervention. Thus, treatment and peer effects will be correlated through network change and the coefficient on the direct effect will be biased upwards for the static and no peer effect models.

In Table 2.7 I calculate the total treatment effects under different modeling assumptions, decomposing the effect into the direct treatment effect and indirect treatment effect. Column (1) shows the base specification with no peer effects and Column (2) incorporates static peer effects. Columns (3) and (4) incorporate dynamic peer effects, where the latter includes the estimated μ vector of individual heterogeneity. The results show that failure to account for dynamic peer effects may dramatically underestimate the overall effect of treatment: estimates increase by nearly 70% moving from the model with static PEs to the full model in column (4). This result is driven by the large effect seen in Table 2.6 on the network peer effect coefficient which implies a large increase in crop income due to new peers and is not accounted for in the base or static peer effects models.

2.6 Conclusion

In this paper, I studied how the introduction of a new irrigation technology changes social networks in rural Kenya. Using a panel of dyadic network data collected in conjunction with an RCT, I showed both the network and household-level effects of winning a pump. First, I show that lotteries for two pumps in a village does not change overall network structure 3-4 years later. However, lack of changes in overall network structure masks substantial intra-network reshuffling. In particular, I show that pump winners become more centrally located

within a network along a number of measures of centrality. Substantially more farmers report receiving farming help or advice from pump winners in the post-treatment period.

Next, through a set of dyadic regressions I showed that this change in network centrality for pump winners is driven by non-pump winners differentially maintaining or creating links with pump winners. Additionally, pump winners are much more likely to maintain or create a link with the other pump winner in the village, relative to a link with a non-winner. These results imply a model of strategic network formation, where linking decisions respond to treatment status. These results also show that networks in this context are dynamic, with substantial link turnover and formation over time, which has not been previously documented in the literature. This implies that the common assumption of static networks may not be empirically supported.

In the final portion of the paper, I adapted the peer-effects model from Comola and Prina (2019) to my setting. I show that both a standard outcome peer effect ($\mathbf{G}_0\mathbf{y}$) as well as a dynamic contextual peer effect ($\mathbf{G}_{1-0}\mathbf{W}$) are significant for farmer crop income. The latter term is not accounted for in standard peer effect models. Calculating the overall treatment effect, I show that failure to incorporate dynamic peer effects substantially underestimates the total effect of the intervention.

In future work, I hope to expand upon the model of dynamic peer effects to relax the linearity requirements, incorporate a formal network formation model, and expand on welfare assessment. I view this paper as a starting point for analysis of a rich new dataset that will allow me to say more about the creation of peer borrowing and rental sub-graphs, as well as say something about methods of network elicitation.

Table 2.1: Winner Characteristics

Network Stats Farm Help/Advice, Directed	Mean	Coefficient on “Winner” for:			Obs.
	[Std Dev]	Cash	Task	Vote	
	in Random & Control Villages				
Perceived Farming Knowledge	3.052 [0.558]	0.23 (0.065)***	0.005 (0.059)	0.239 (0.059)***	4211
Out-degree	6.478 [5.379]	0.021 (0.612)	0.39 (0.702)	-0.248 (0.68)	4212
In-degree	6.327 [3.466]	0.787 (0.323)**	-0.436 (0.317)	1.467 (0.31)***	4212
Eigenvector Centrality	0.198 [0.078]	0.018 (0.01)*	0.009 (0.01)	0.027 (0.01)**	3694
Betweenness Centrality	18.154 [25.082]	7.093 (3.49)**	-2.601 (2.105)	6.257 (3.886)	4212
Closeness Centrality	0.656 [0.116]	0.012 (0.012)	0.015 (0.013)	0.02 (0.012)*	4212
Household Characteristics					
Aged 55 or more	0.233 [0.423]	0.085 (0.059)	-0.062 (0.046)	-0.022 (0.043)	4198
Female Headed Household	0.607 [0.488]	0.007 (0.032)	-0.018 (0.028)	-0.041 (0.032)	4211
Male Yrs of Education	7.95 [3.365]	0.184 (0.315)	-0.148 (0.271)	0.276 (0.375)	4212
Female Yrs of Education	6.478 [3.564]	0.241 (0.4)	-0.186 (0.345)	1.291 (0.404)***	4212
Land Size (acres)	2.148 [2.383]	0.366 (0.267)	-0.091 (0.134)	0.124 (0.23)	4203
Spending on ag inputs (1000Ksh)	6.506 [12.709]	-0.073 (1.097)	-1.142 (0.772)	1.557 (1.061)	4206
Crop Income (1000Ksh)	17.564 [32.409]	5.334 (3.514)	-3.196 (3.488)	12.73 (4.644)***	4212
Other Income (1000Ksh)	32.294 [66.023]	3.658 (5.486)	-10.419 (6.111)*	-4.485 (4.922)	4171
Index of House Quality	0.363 [0.274]	0.037 (0.031)	-0.024 (0.029)	0.011 (0.03)	4171
Currently Irrigates	0.373 [0.484]	0.051 (0.052)	-0.023 (0.058)	-0.066 (0.055)	4212
Grows Vegetables	0.744 [0.437]	-0.055 (0.05)	0.001 (0.049)	-0.03 (0.041)	4212
Baseline WTP for pump (1000Ksh)	2.554 [1.819]	-0.064 (0.194)	-0.222 (0.189)	0.413 (0.187)**	4212
Geographic Centrality	1.294 [3.676]	0.026 (0.429)	-0.079 (0.295)	-0.27 (0.208)	4212
Participates in a Community Group	0.834 [0.364]	0.01 (0.04)	-0.032 (0.033)	0.107 (0.037)***	4212

The data are from all respondents from the 190 villages in Waves 1 and 2. Each row represents a regression of the network or household characteristic on treatment-by-winner, treatment, and strata dummies. Standard errors are clustered at the village level. Standard deviations for the pooled control and random villages are in brackets. Standard errors are in parentheses. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table 2.2: Directed Network Statistics

Panel A. Baseline Data	Discuss Planting and/or Agriculture					Provided Info, Help, Advice w/ Agriculture					Know at All				
	Random	Cash	Task	Vote	Control	Random	Cash	Task	Vote	Control	Random	Cash	Task	Vote	Control
Clustering	.452 (.1)	.446 (.11)	.479 (.123)	.437 (.091)	.421 (.094)	.35 (.107)	.325 (.11)	.349 (.112)	.349 (.108)	.34 (.066)	.931 (.048)	.916 (.048)	.921 (.068)	.907 (.062)	.917 (.056)
Closeness	.722 (.06)	.715 (.06)	.733 (.068)	.722 (.05)	.703 (.066)	.662 (.072)	.658 (.063)	.675 (.058)	.669 (.056)	.645 (.057)	.982 (.022)	.977 (.027)	.97 (.048)	.971 (.034)	.965 (.046)
Betweenness	14.346 (5.315)	14.702 (3.626)	13.646 (4.417)	14.437 (3.944)	15.178 (5.787)	17.018 (6.656)	18.93 (5.344)	15.996 (5.784)	17.399 (5.905)	18.014 (4.691)	1.744 (1.427)	2.162 (1.461)	2.087 (1.959)	2.391 (1.705)	2.448 (2.232)
Density	.391 (.091)	.383 (.082)	.407 (.1)	.39 (.074)	.366 (.081)	.298 (.083)	.292 (.073)	.314 (.07)	.308 (.07)	.288 (.056)	.919 (.06)	.901 (.066)	.897 (.097)	.888 (.077)	.89 (.09)
Out-degree	8.288 (2.456)	8.432 (2.052)	8.423 (2.101)	8.491 (2.129)	7.852 (1.834)	6.332 (2.158)	6.432 (1.855)	6.569 (1.816)	6.683 (1.84)	6.167 (1.408)	19.863 (4.207)	20.12 (3.158)	19 (4.265)	19.466 (3.508)	19.39 (3.375)
Households	22.744 (4.483)	23.488 (3.158)	22.279 (4.205)	23.023 (3.535)	23.056 (3.539)	22.744 (4.483)	23.488 (3.158)	22.279 (4.205)	23.023 (3.535)	23.056 (3.539)	22.744 (4.483)	23.488 (3.158)	22.279 (4.205)	23.023 (3.535)	23.056 (3.539)
Clusters	43	43	43	43	18	43	43	43	43	18	43	43	43	43	18

Panel B. Endline Data	Discuss Planting and/or Agriculture					Provided Info, Help, Advice w/ Agriculture					Know at All				
	Random	Cash	Task	Vote	Control	Random	Cash	Task	Vote	Control	Random	Cash	Task	Vote	Control
Clustering	.346 (.149)	.343 (.142)	.369 (.146)	.351 (.126)	.337 (.118)	.249 (.121)	.267 (.13)	.281 (.147)	.242 (.131)	.25 (.126)	.857 (.09)	.859 (.085)	.878 (.067)	.88 (.059)	.826 (.103)
Closeness	.682 (.087)	.682 (.078)	.699 (.09)	.685 (.064)	.673 (.101)	.61 (.09)	.613 (.116)	.634 (.1)	.606 (.078)	.595 (.121)	.963 (.045)	.958 (.045)	.964 (.044)	.975 (.028)	.94 (.055)
Betweenness	12.333 (4.585)	12.748 (5.084)	10.87 (4.796)	12.908 (4.034)	14.833 (6.774)	14.849 (6.743)	13.101 (7.036)	12.955 (7.676)	15.169 (6.423)	16.092 (7.178)	2.663 (1.657)	3.032 (2.187)	2.548 (1.621)	2.363 (1.335)	3.833 (2.322)
Density	.34 (.1)	.332 (.101)	.36 (.12)	.331 (.082)	.323 (.108)	.252 (.084)	.259 (.097)	.277 (.107)	.241 (.074)	.239 (.101)	.838 (.107)	.83 (.102)	.845 (.096)	.864 (.072)	.795 (.117)
Out-degree	6.95 (2.516)	6.745 (2.081)	6.894 (2.414)	6.716 (1.928)	6.65 (2.413)	5.082 (1.926)	5.214 (1.985)	5.307 (2.159)	4.9 (1.778)	4.809 (1.735)	17.872 (4.491)	17.957 (3.011)	17.489 (3.963)	18.496 (3.486)	16.912 (3.468)
Households	22.744 (4.483)	23.465 (3.157)	22.233 (4.253)	23.023 (3.535)	22.944 (3.572)	22.744 (4.483)	23.465 (3.157)	22.233 (4.253)	23.023 (3.535)	22.944 (3.572)	22.744 (4.483)	23.465 (3.157)	22.233 (4.253)	23.023 (3.535)	22.944 (3.572)
Clusters	43	43	43	43	18	43	43	43	43	18	43	43	43	43	18

Table 2.3: Village-level Effects - ANCOVA

Panel A. Network: Agricultural Discussion, Directed						
Dependent Variable:	Density		Clustering		Closeness	
	(1)	(2)	(5)	(6)	(9)	(10)
Treated	0.0078 (0.0174)		0.0071 (0.0245)		0.0088 (0.0244)	
Random		0.0082 (0.0192)		0.0036 (0.0272)		0.0116 (0.0267)
Cash		0.0158 (0.0208)		0.0190 (0.0285)		0.0057 (0.0295)
Task		0.0223 (0.0202)		0.0276 (0.0300)		0.0230 (0.0269)
Vote		-0.0145 (0.0188)		-0.0201 (0.0271)		-0.0050 (0.0254)
Constant	-0.3563 (0.2717)	-0.4345 (0.2761)	-0.9021 (0.3443)***	-1.0041 (0.3706)***	0.0242 (0.3469)	-0.0508 (0.3572)
N	190	190	190	190	190	190
Mean of Dep Var	0.256	0.256	0.259	0.259	0.614	0.614
Controls	Y	Y	Y	Y	Y	Y
Strata FE	Y	Y	Y	Y	Y	Y

Panel B. Network: Farm Help/Advice, Directed						
Dependent Variable:	Density		Clustering		Closeness	
	(1)	(2)	(5)	(6)	(9)	(10)
Treated	0.0073 (0.0238)		0.0028 (0.0264)		0.0107 (0.0218)	
Random		0.0063 (0.0277)		0.0009 (0.0314)		0.0057 (0.0246)
Cash		0.0078 (0.0263)		-0.0007 (0.0322)		0.0110 (0.0240)
Task		0.0246 (0.0258)		0.0071 (0.0310)		0.0235 (0.0237)
Vote		-0.0080 (0.0259)		0.0039 (0.0305)		0.0036 (0.0234)
Constant	-0.3689 (0.3626)	-0.4462 (0.3682)	-0.5755 (0.5613)	-0.5907 (0.5718)	-0.0435 (0.3176)	-0.0897 (0.3223)
N	190	190	190	190	190	190
Mean of Dep Var	0.339	0.339	0.351	0.351	0.686	0.686
Controls	Y	Y	Y	Y	Y	Y
Strata FE	Y	Y	Y	Y	Y	Y

ANCOVA specification. Standard errors are in parentheses clustered at the village level. All specifications include strata fixed effects. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table 2.4: Household-level Effects - ANCOVA - Grouped Treatment

Dependent Variable:	Panel A. Network: Ag Discussion, Directed			Panel B. Network: Farm Help/Advice, Directed				
	In Degree	Out Degree	Closeness	In Degree	Out Degree	Closeness		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Win	1.364 (0.222)***	1.119 (0.211)***	0.657 (0.366)*	0.551 (0.372)	6.897 (1.872)***	5.719 (1.973)***	0.036 (0.008)***	0.030 (0.008)***
HV x Win	-0.450 (0.366)	-0.024 (0.352)	-1.240 (0.542)**	-1.231 (0.529)**	-5.839 (2.647)**	-4.827 (2.733)*	-0.031 (0.013)**	-0.023 (0.012)*
HV	1.360 (0.146)***	0.487 (0.134)***	1.205 (0.219)***	0.604 (0.209)***	7.329 (0.810)***	4.533 (0.776)***	0.047 (0.005)***	0.022 (0.005)***
N	3136	2962	3136	2962	3136	2962	3136	2962
Mean of Dep Var	6.145	6.180	6.150	6.229	13.000	13.083	0.688	0.689
BL Outcome Control	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y

Dependent Variable:	Panel A. Network: Ag Discussion, Directed			Panel B. Network: Farm Help/Advice, Directed				
	In Degree	Out Degree	Closeness	In Degree	Out Degree	Closeness		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Win	0.985 (0.219)***	0.898 (0.192)***	0.938 (0.322)***	0.698 (0.321)**	8.292 (2.231)***	7.581 (2.361)***	0.038 (0.008)***	0.032 (0.008)***
HV x Win	-0.045 (0.368)	0.124 (0.327)	-1.457 (0.458)***	-1.142 (0.431)***	-7.221 (3.168)**	-6.087 (3.192)*	-0.031 (0.012)**	-0.022 (0.011)**
HV	1.301 (0.129)***	0.430 (0.109)***	0.798 (0.185)***	0.408 (0.187)**	7.349 (1.223)***	4.716 (1.262)***	0.044 (0.005)***	0.024 (0.005)***
N	3136	2962	3136	2962	3136	2962	3136	2962
Mean of Dep Var	4.599	4.626	4.601	4.665	15.047	15.222	0.616	0.617
BL Outcome Control	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y

All specifications include village fixed effects. Even numbered specifications include a vector of household controls including the baseline value of the outcome variable. Standard errors reported in parentheses are clustered at the village level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table 2.5: Dyadic Regressions

Panel A. Network: Ag Discussion, Directed						
Dep Var:	Linked at Endline					
	(1)	(2)	(3)	(4)	(5)	(6)
WW	0.150 (0.049)***	0.144 (0.047)***	0.147 (0.050)***	0.184 (0.051)***	0.184 (0.054)***	0.185 (0.055)***
WN	0.020 (0.021)	0.015 (0.020)	0.010 (0.021)	0.018 (0.016)	0.018 (0.017)	0.020 (0.016)
NW	0.094 (0.014)***	0.093 (0.013)***	0.083 (0.014)***	0.060 (0.010)***	0.060 (0.011)***	0.059 (0.009)***
N	25068	25068	22916	36899	36899	36899
Mean of Dep Var	0.472	0.472	0.475	0.249	0.249	0.249
Linked at BL	Yes	Yes	Yes	No	No	No
Cluster FE		Y	Y		Y	Y
Controls			Y			Y

Panel B. Network: Farm Help/Advice, Directed						
Dep Var:	Linked at Endline					
	(1)	(2)	(3)	(4)	(5)	(6)
WW	0.190 (0.050)***	0.193 (0.049)***	0.228 (0.057)***	0.172 (0.049)***	0.172 (0.053)***	0.169 (0.051)***
WN	0.026 (0.024)	0.028 (0.022)	0.034 (0.023)	0.022 (0.014)	0.022 (0.014)	0.021 (0.014)
NW	0.085 (0.015)***	0.084 (0.014)***	0.085 (0.015)***	0.056 (0.011)***	0.056 (0.011)***	0.054 (0.009)***
N	19771	19771	18043	36948	36948	36948
Mean of Dep Var	0.388	0.388	0.391	0.202	0.202	0.202
Linked at BL	Yes	Yes	Yes	No	No	No
Cluster FE		Y	Y		Y	Y
Controls			Y			Y

Dyadic regressions. Dyadic cluster robust standard errors are reported in parentheses. Controls are the interaction of sender and received household characteristics. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table 2.6: Peer Effects - Treatment Response Model

Dep Var:	Asinh(Crop Income)		
Model:	No PE	Static PE	Dynamic PE
	(1)	(2)	(3)
W	0.248** (0.108)	0.233* (0.128)	0.171 (0.119)
<u>Peer Effects:</u>			
G₀y		-0.016 (0.281)	0.511* (0.294)
G₁₋₀y			0.260*** (0.073)
<u>Contextual Effects:</u>			
G₀W		0.326 (0.230)	0.321 (0.284)
G₁₋₀W			-0.002 (0.206)
<u>HV Controls:</u>			
G₀H		0.214* (0.120)	0.182 (0.118)
G₁₋₀H			-0.277 (0.192)
HV	0.157* (0.084)	0.034 (0.079)	-0.018 (0.082)
N	3107	3107	3107
Mean of Dep Var	0.188	0.188	0.188

Bootstrap standard errors in parentheses clustered at the village level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table 2.7: Total Treatment Effects

Dep Var:	Asinh(Crop Income)			
Model:	No PE	Static PE	Dyanmic PE ($\mu = 0$)	Dynamic PE (μ)
	(1)	(2)	(3)	(4)
Direct	0.248	0.232	0.201	0.208
Indirect	–	0.485	0.991	1.006
Total	0.248	0.717	1.192	1.214

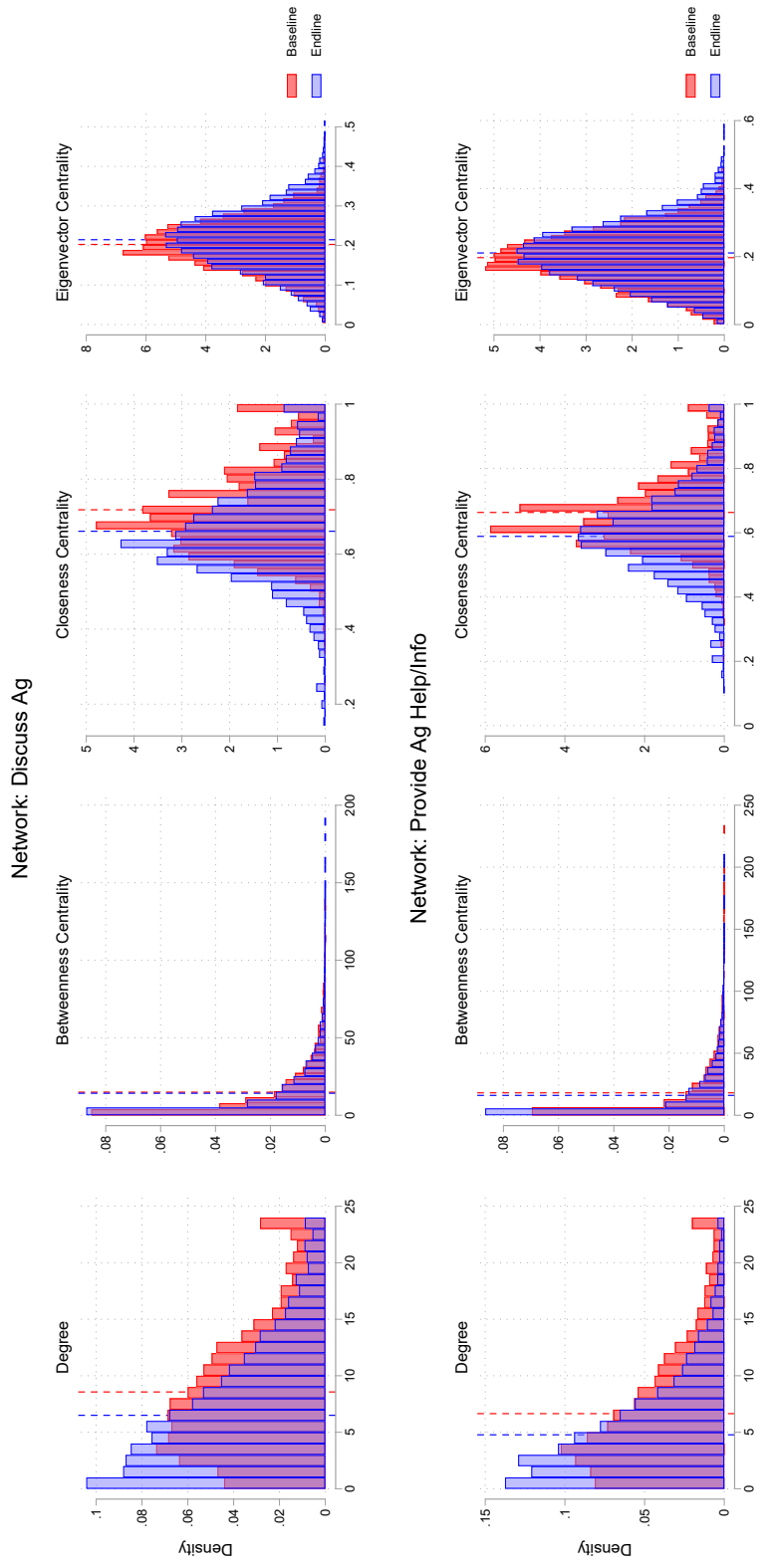


Figure 2.1: Network Statistics - Baseline v. Endline

Chapter 3

Valuing the Time of the Self-Employed

3.1 Introduction

Many development interventions aim to increase the profitability of small owner-operated businesses and farms, the primary source of income for the vast majority of poor households (Merotto et al., 2018). Accurately measuring the value that the self-employed assign to their own time is essential for evaluating the profitability and welfare impacts of most such interventions. The majority of such evaluations ascribe a value of zero to the time of the self-employed.¹ A minority use the prevailing market wage, which likely overstates the value of time in the presence of the labor-market frictions endemic to developing economies (Kaur, 2019; Breza et al., 2021; Jones et al., 2022).² Directly assessing participants' value of time—by, for example, eliciting the minimum wage they would accept for comparable labor—may be unreliable, as the frictions that distort labor markets may originate in individual choices.

We create a method that pairs multiple choices with structural estimation to recover individuals' value of their own time in the presence of labor and credit market imperfections, as well as a broad array of behavioral phenomena. We elicit the preferences of self-employed farmers in western Kenya over trade-offs involving three things: money, time, and lottery tickets for an irrigation pump. The choices over these alternatives show that many farmers

¹See Section 3.6.2 for a survey of studies in economics. It is worth noting that, in addition to the majority that value time at zero, an additional 24% do not attempt to value time at all. Of these 24%, several note that they would like to use *some* value of time, but believe it is too difficult, in their setting, to measure one.

²Putting this another way, de Janvry et al. (2017, p. 458) note, “It is well known that a large number of family farms do not seem economically viable when family labor is valued at the observed market wage rate in the casual labor market, implying that this is not the correct way to value family labor.”

in our study have intransitive preferences, confirming that direct trade-offs between money and time may produce unreliable results. Still, these choices alone bound the average value of time between 40–100% of the *market wage*—the average wage for casual labor in our sample. We then use a structural model that adopts a reduced-form approach to behavioral phenomena (Mullainathan et al., 2012; Gabaix, 2019) by modeling them as *wedges* that may separately affect each choice. This produces a more precise estimate of the average value of time: 60% of the market wage. The results of the structural estimation indicate that wedges only appear in choices that involve money, rather than choices between time and a good. This finding is consistent with a class of behavioral models in which behavioral phenomena manifest only in transactions involving cash.

Our findings imply that common methods for valuing the time of the self-employed are likely to be inaccurate, and we offer several methods for researchers to obtain better measures. The common undervaluing of the time of the self-employed overstates the value of technologies or interventions that increase time commitments, and understates the value of those that save time.³ This may explain why some technologies that appear profitable in evaluations are not adopted, and why labor-saving interventions attract relatively less attention (Suri, 2011; de Janvry et al., 2017). This is unfortunate, as more free time is associated with large improvements in mental and physical health, female labor-force participation, and education.⁴ Our findings can be easily applied in different ways depending on the setting, allowing researchers to more accurately value interventions. Finally, our results suggest an additional explanation for the persistence of self-employment in places with relatively informal labor markets: the wedges driving choices in our data may hinder casual labor market transactions. Behavioral phenomena may cause workers to undervalue wages obtained through one-on-one negotiation, and employers to ration jobs. We find shading when wages are paid in cash, but not in goods. The former finding is consistent with the theory of efficiency wages (Hart and Moore, 2008; Fehr et al., 2011).

Our study augments an elicitation that directly measures participants’ value of time—their reservation wage for temporary jobs—with two others that allow for an indirect assessment of the value of time, as described in Section 3.2. Those additional elicitations allow participants to express the value of a good—lottery tickets with a 1/10 chance of winning an irrigation pump—in both money and hours of casual labor. By dividing these two quantities, we obtain an indirect assessment of participants’ value of time. Each elicitation is based on a standard Becker-DeGroot-Marschak (BDM) mechanism, which has been widely used to obtain valuations, including in low-income contexts (Becker et al., 1964; Crockett and Oprea,

³Valuing time using the market wage would tend to have the opposite effect.

⁴See for example Xiao et al. (2013); Albanesi and Olivetti (2016); Schilbach (2019); Bessone et al. (2021); Whillans and West (2021).

2012; Holt and Smith, 2016; Azrieli et al., 2018; Berry et al., 2020; Burchardi et al., 2021).

Under a benchmark expected-utility model that allows for labor market rigidities and credit constraints, the direct and indirect values of time should be the same, but, in our choice data, they are not, as described in Section 3.3. The average value of time measured directly is similar to the market wage, while the value of time measured indirectly is 40% of the market wage. This difference is caused by a large proportion of our participants making intransitive choices.⁵ Despite these intransitivities, the direct and indirect measures bound the average value of time between 40–100% of the market wage. These bounds may be sufficient for some studies; however, others may require a point estimate.

We show how a model with wedges can be structurally estimated on our experimental data to recover an un-wedged value of time in Section 3.4, and find it is, on average, 60% of the market wage. The structural model uses data from all three elicitation to identify—under assumptions supported by our data—the relative magnitude of the wedge present in each trade-off. Once identified, the effect of the wedges can be removed to produce estimates of individuals’ value of time. As this model nests the benchmark model, this estimate is robust to credit constraints or labor rigidities, in addition to a broad class of behavioral features. The model estimation shows that wedges affect choices in which money is either spent on goods or received for labor, but not when labor is exchanged for goods.

The un-wedged value of time is identified regardless of the source of the wedges in farmers’ choices. That is, the economic interpretation of wedges is only important when a researcher seeks to apply a structural parameter in a different setting. As many interventions evaluate naturalistic trade-offs made by farmers between time and a good—for example, working longer for additional crop yield—wedges generated by either behavioral phenomena or features of the elicitation design are unlikely to be present, and therefore an un-wedged value of time is likely to be appropriate across a broad range of settings.

Our results are consistent with a behavioral model, described in Section 3.5, in which decision makers deflate the value of cash they receive as wages, and inflate the value of cash they pay for goods. That is, our results can be explained by a self-serving bias, or loss aversion, that applies only to cash transactions. We show that wedges are smaller for two groups in which these phenomena are likely to be muted—experienced casual laborers, and those experienced with paying for goods in cash. We then consider several potential alternative sources of our findings—including a tightening of credit constraints, stigma for

⁵As described in the AEA RCT Registry (AEARCTR-0004110), our prior was that wedges between the direct and indirect values of time and the market wage might arise from characteristics of the labor market, or from characteristics of laborers—for example, norms against accepting lower wages (Agness et al., 2019). As described in Appendix C.5.4, we did not find that norms surrounding low-wage work are influential in this setting.

accepting low wages, and present bias. These alternatives are ruled out by either our study design, or by examining additional data from within our study.

We conclude with a discussion of the broader implications of our results, including how our data improve the understanding of labor markets in developing countries in Section 3.6. We methodically review the economic literature from 2016–2021, and show that it uses relatively extreme values of time, with the majority of studies using a value of zero. We then describe how researchers can best make use of our results, and offer guidance for bounding the value of time based on differences in labor market conditions—which can be measured with a brief survey. Finally, we apply our results to some prior studies to illustrate when more reliable estimates for the value of time are likely to affect program evaluations.

Our results inform a broad literature evaluating the welfare impacts of interventions. For example, providing agricultural inputs—such as fertilizer or seeds—increases hours worked on the farm (Dufflo et al., 2011; Emerick et al., 2016), while supporting mechanization decreases hours worked (Caunedo and Kala, 2021). Similarly, improving tenancy contracts (Burchardi et al., 2018) or property rights (Goldstein et al., 2018) affects work hours. Measuring the welfare effects of these interventions requires an estimate of workers’ value of time, but market wages are often a poor proxy for this value, as incomplete factor markets drive a wedge between shadow and market prices (Benjamin, 1992; LaFave and Thomas, 2016).

Difficulty assigning a value to workers’ time has consequently led to widely varying methodologies. For example, Goldstein et al. (2018) assume the household does not face an opportunity cost of supplying labor when studying the effect of a change in property rights. In contrast, Emerick et al. (2016) value all labor at the market wage when estimating the profitability of a flood-resistant type of rice in India.⁶

Mas and Pallais (2019) offer the first experimental estimates of the value of time among job-seekers in the U.S., but do not consider behavioral phenomena.⁷ Instead, they use estimates obtained by simply offering a choice between time and money, a choice that we show produces unreliable estimates. In their study of the gains from mechanization in agriculture, Caunedo and Kala (2021) estimate the shadow cost of family labor in India to be approximately 90% of the market wage in rural India. In contrast to their approach, our

⁶A similar issue arises among researchers testing for labor misallocation: evaluating welfare gaps requires an estimate of the value of time gained or lost when workers transition across sectors. There is a substantial wage premium in the non-agricultural sector of most low-income countries—possibly owing to migration barriers, such as inadequate information (Baseler, 2023) and financial constraints (Bryan et al., 2014)—but non-agricultural workers also work longer hours on average (Caselli, 2005; Restuccia et al., 2008; Gollin et al., 2014). When measuring this agricultural productivity gap, Gollin et al. (2014) control for hours worked, while Pulido and Świącki (2018) do not.

⁷As behavioral phenomena, such as self-serving bias, are common in high-income contexts (see, for example, Babcock et al., 1995; Babcock and Loewenstein, 1997), the market wage and other standard valuation techniques may also produce unreliable estimates of the value of time in high-income economies.

method can be directly applied without relying on noisy measurements of farm inputs or structural assumptions required to identify smallholder production functions.

A related, but methodologically distinct, literature uses travel-cost-based estimates of household time valuation as inputs for welfare analysis, benefit-cost analysis, and value of statistical life calculations (Jeuland et al., 2010; Kremer et al., 2011; Jeuland and Pattanayak, 2012). Studies in this literature measure the value of travel time using either stated willingness to pay—which we show is inaccurate—or a revealed preference approach using variation in observed, non-work travel times—which biases estimates in the presence of credit constraints, as faster modes of transport are usually more expensive. Our approach improves on these methods by identifying the marginal value of work time—the relevant input for most economic cost-benefit analyses—at the individual level, while accounting for a broad class of market imperfections and behavioral phenomena.

Our approach also contributes to the growing literature in structural behavioral economics (see Conlin et al., 2007; Laibson et al., 2007; DellaVigna et al., 2012, 2016; DellaVigna, 2018, for prominent examples). Our proposed interpretation of the patterns in our data—namely, that decision makers treat choices over cash differently than choices between goods and time—builds on models of self-serving bias (see Loewenstein et al., 1993; Babcock et al., 1995; Babcock and Loewenstein, 1997). We see our explanation of the intransitivities in our choice data—wedges that enter decisions involving cash—as a natural application of these models in an environment in which cash transactions are relatively rare.

3.2 Study Design and Choice Data

In this section we describe our study setting, before turning to a more detailed description of the choices offered to farmers.

3.2.1 Setting

The study took place in rural Kenya in April and May, 2019, with a sample of farming households that had first been enumerated in 2014 for a separate randomized controlled trial (Chassang et al., 2023). In that trial, KickStart irrigation pumps were distributed to some farmers in “treatment” villages. For the present study, we focus on the “control” villages from Chassang et al. (2023), in which no pumps were distributed. Villages in Chassang et al. (2023) were selected to ensure there were a sufficient number of farmers with land suitable for manual pump irrigation—that is, close to a water source. In each village, an “anchor farmer” was identified who lived close to a water source, and a snowball sampling technique was used to generate a list of 10 to 25 neighboring farmers with land suitable for pump

irrigation. Focusing on control villages from the earlier study gave a list of 411 potential households for our study, out of which we were able to find and complete activities with 332, or 81%. Appendix C.2.1 provides further details about sampling.

Households in our study all did at least some agricultural work, and had land suitable for manual irrigation. On average, nearly 40% of households' income came from selling crops they had grown. Most households also engaged in micro-entrepreneurship, or provided casual labor on neighbors' farms.

To mimic a setting in which households endogenously choose how to allocate labor supply across individuals, we allowed each household to choose a single adult member to participate after the household learned about the study. We required that this individual participate in all activities. Ninety-five percent of households chose either the female or male head of household. As shown in Table C.3.1, average values of time were consistent across various demographic groups, suggesting that households did, indeed, allocate time similarly regardless of the identity of the person chosen. Table 3.1 displays sample summary statistics. The average participant was 47.7 years old and had 6.8 years of education. Women comprised 69% of our sample. Importantly, men and women in our sample had very similar values of time; see Table C.3.1. The average household in our study earned about 50,000 KSh (\$461) per year.

The jobs we offered—weeding and preparing land—were designed to mimic paid casual labor that most households engage in. Casual labor is, by far, the second most common source of income (after farming) for participants. In our sample, 42% of participants had performed casual labor—and 46% of households had hired casual laborers—in the prior 3 months. These participants had worked an average of 13 days in the prior 3 months, with an average workday of 4.2 hours. Average wages were 82 KSh (about \$0.77) per hour.⁸

Farmers in our sample reported struggling to find paid work. While most farmers (53%) reported that they could definitely find one day of work with a week's notice, only 27% believed they could find a full week (six days) of work. Only 34% believed they could find a day of work with one day's notice. Moreover, farmers believed that working hours would be limited: of those who believed they could find work with a day's notice, the maximum amount of work they said they could find was 4.3 hours, on average. This suggests that farmers in this setting cannot flexibly choose how much labor to supply to the market—a widespread feature of rural labor markets (Breza et al., 2021)—and that market wages may not accurately measure the value that individuals assign to their time.

⁸These wages are high relative to average daily household earnings of 135 KSh. This is because average working hours are low—about 4 hours per week among those who worked—consistent with labor rationing. In line with the literature, we use the term *labor rationing* to describe situations in which qualified workers would like to work additional hours at the market wage, but cannot find employment.

Table 3.1: Summary Statistics

	Mean	Std. Dev.	N
Panel A: Demographics			
Age	47.7	14.3	328
Years of education	6.8	3.6	307
Female = 1	0.69	0.46	332
No male head in household = 1	0.14	0.35	332
Number of adults (age 18 or over) in household	2.7	1.3	324
Number of children (under 18 years) in household	4.0	2.4	324
Panel B: Household income and wealth			
Land area under cultivation (acres)	2.3	2.0	324
Household income (KSh, past year)	49,122	68,358	330
Income share from sale of crops	0.41	0.38	330
Does not have 5,000 KSh saved	0.76	0.43	326
Micro-entrepreneur = 1	0.44	0.50	330
Panel C: Casual labor			
Performed or hired casual labor within past 3 months = 1	0.72	0.45	332
Performed casual labor within past 3 months = 1	0.42	0.50	332
of which, days worked in last 3 months	13.1	16.5	141
during which, hours worked per day	4.2	1.4	141
among which, hourly earnings	82	66	129
Hired casual labor within past 3 months = 1	0.46	0.50	332
of which, days hired in last 3 months	6.5	8.5	154
during which, number of workers hired	3.2	3.5	154
among which, hours hired per day	4.0	1.3	154
among which, hourly wage paid	60	33	137
Could find 6 days of work next week	0.27	0.44	332
Could find 1 day of work next week	0.53	0.50	332
Could find 2 hours of work next week	0.43	0.50	332
Could find work tomorrow	0.34	0.47	332
if so, maximum hours available	4.3	2.0	113
Panel D: Exposure to irrigation pump			
Owns a MoneyMaker irrigation pump	0.01	0.09	332
Has used a MoneyMaker irrigation pump	0.11	0.32	332
Familiar with the MoneyMaker irrigation pump	0.99	0.09	332
Has considered buying a MoneyMaker irrigation pump	0.59	0.48	332
Self-reported valuation of pump (KSh)	4,432	3,318	303

Note: Each observation is a single farmer. Data are taken from multiple rounds of household surveys between 2014–2019. Values are coded as missing if: the farmer was not surveyed when the relevant information was collected; they answered “Don’t Know;” or if the question is not applicable. All monetary units are expressed in 2019 Kenyan shillings (KSh).

The irrigation pump used in this study approximates a common impact-evaluation environment: adoption of a technology with low baseline usage rates. Our prior work had identified a sample of farmers with suitable land for pump irrigation, and who were familiar with, but had not widely adopted, the pump.

Our analysis in Section 3.3.1 relies on the good in the experimental choices having a small value compared to the farmers’ overall budgets. The pump is expensive compared to farmers’ budgets, so we used lottery tickets offering a 1-in-10 chance of winning a pump. As expected, these tickets had a relatively small average subjective value of 111 KSh, about what the average participant could earn from 1.4 hours of casual labor.⁹

The manually powered irrigation pumps we used (branded as “MoneyMaker” by KickStart) are specifically designed for smallholder farmers. An experiment that allocated these pumps to women in Kenya found that they increase net farm revenue by 13%, offsetting their purchase cost after 3 years (Dyer and Shapiro, 2023), although the study did not account for farmers’ value of time. However, at baseline, only 11% of farmers in our study had tried a KickStart pump themselves. The main reasons given for this low uptake are the pumps’ expensiveness (they retail for 9,500 KSh, or about \$89), and the fear that the pumps may be uncomfortable to operate.

3.2.2 Choices

Each farmer in our sample was given three choices that used the BDM design (Becker et al., 1964), as implemented in Berry et al. (2020).¹⁰ This implementation made the choices relatively simple and naturalistic. Participants were asked to state their preferences for some object—for example a lottery ticket for a pump—in some unit of payment—for example, hours of labor. After stating their preferences, a random price was drawn, and if their stated value was higher than the price, that is what they paid for the object. If their value was lower than the price, no transaction occurred.¹¹ Burchardi et al. (2021) implement similar

⁹The average subjective value for a lottery ticket is well below 950 KSh—one-tenth of the pump’s retail price—likely due to risk aversion and low willingness to pay for productive technologies in general (see Footnote 20). Importantly, risk aversion does not affect the predictions of Section 3.3.1, as discussed in Section 3.5.3.

¹⁰Specifically, the surveyor read a description of the procedure, emphasizing that no negotiation would be allowed, and played practice rounds to ensure comprehension.

¹¹Thus, the BDM design is like a second-price auction with a single participant and a random reserve price. Like a second-price auction, the BDM design is incentive compatible, and revelation of true values is a dominant strategy. Complete implementation details are provided in Appendix C.2. Full scripts are available here.

BDMs in rural Uganda, and find high comprehension across several design variations.

Choice RW: Reservation Wage. In the *reservation wage* (RW) choice, farmers were offered the option to receive a cash payment for casual labor.

We explained to each farmer that we were offering one-time, 2-hour jobs performing casual agricultural labor in a different village. We asked each farmer whether they would be willing to accept the job at 120 KSh per hour. If they answered “no,” we asked about their reservation wage directly. If they answered “yes,” we asked whether they would accept the job at incrementally lower wages until they changed their answer to “no.” The minimum amount of money the farmer was willing to accept for the job is denoted by m^{RW} .

Choice CB: Cash Bid. In the *cash bid* (CB) choice, farmers were offered the option to obtain a lottery ticket for the MoneyMaker pump in exchange for money.

We explained to each farmer that we were selling lottery tickets offering 1-in-10 odds of winning a MoneyMaker pump. We collected willingness to pay in cash by asking the farmer whether they would be willing to pay a low price of 20 KSh, and then asking the same question for increasingly higher prices, until the farmer declined the offer.¹² The maximum amount of money the farmer was willing to pay for the lottery ticket is denoted by m^{CB} .

Choice TB: Time Bid. In the *time bid* (TB) choice, farmers were offered the option to obtain a lottery ticket for the MoneyMaker pump in exchange for casual labor.

As in Choice CB, we explained to each farmer that we were offering lottery tickets with 1-in-10 odds of winning a MoneyMaker pump. We collected willingness to pay in time by asking the farmer whether they would be willing to work 30 minutes for the ticket, and then asking the same question for increasingly higher amounts of time, until the farmer declined the offer. The maximum amount of time the farmer was willing to work for the lottery ticket is denoted by h^{TB} .

Offer Revelation and Payment. Choices CB and TB occurred at the beginning of the survey, in random order, and Choice RW came next. Farmers were told they would receive a random price for either Choice CB or TB, but not both, to minimize interactions across these choices. Prices were drawn at the end of the three elicitations. Scripts read to each farmer explained that there could be absolutely no bargaining once the prices were drawn. Work days as a result of choices in RW were scheduled about 1 week after either work days for choices in TB or payments for choices in CB, in order to further reduce interactions across choices.

¹²We chose descending wages in RW, and ascending prices in CB and TB, so that in all choices participants would start by answering “Yes” until switching to “No.”

We implemented the random draws such that farmers could be sure their choices did not influence the drawn prices. Before the survey, we assigned each farmer a random ticket price in either cash or time (but not both), and a random cash wage. Cash wages were assigned independently of ticket price. This information was written on a card and inserted into a sealed envelope, which was shown to the farmer at the beginning of the survey. After the farmer had made their three choices, the envelope was opened, and the ticket price, payment denomination (cash or time), and wage were revealed.

Cash winners—farmers who drew a cash price weakly lower than m^{CB} —were asked to make a down payment of 20 KSh (\$0.19), and were given about one week to collect the remainder. This ensured that farmers were not limited by their cash-on-hand the day of the survey. Time winners—farmers who drew a time price weakly lower than h^{TB} —were scheduled for casual work approximately one week from the date of the survey. Casual jobs for eligible wage workers—farmers who drew an hourly cash wage weakly greater than $m^{RW}/2$ —were scheduled approximately two weeks from the date of the survey.¹³ We provided transportation to and from job sites, and transport time counted towards work commitments.¹⁴

Direct and Indirect Value of Time. Our design lets us compute two measures of each farmer’s value of time: an hourly *direct value of time* (DVT)— $m^{RW}/2$ —reflecting preferences over direct trade-offs between time and money; and an hourly *indirect value of time* (IVT)— m^{CB}/h^{TB} —reflecting trade-offs between money and the lottery, and time and the lottery.

In the next section, we show that these two different values of time should be approximately equal under our benchmark model.

3.3 The Benchmark Model and Evidence Against It

We model farmers’ choices in a framework that allows for credit constraints and *labor rationing*. Labor rationing implies that a farmer’s reservation wage may be strictly less than the market wage. The literature discusses a number of mechanisms that may result in workers being off their labor supply curve, for example, downward wage rigidity resulting from social norms or effort retaliation (Kaur, 2019), or workers acting as a cartel to withhold work

¹³Compliance rates were 88% for cash payments and 75% for casual labor tasks. We discuss implications of non-compliance in Section C.5.5.

¹⁴We told every respondent a specific time on a specific day when we would meet them to begin the work. The relevant part of the script was, “We will provide transport to and from the job site. This will happen on [DATE] starting at [TIME]. Someone from IPA would come and get you (and possibly other workers from your village) at that time.” We set the work day 1–2 weeks out from the initial survey, giving farmers substantial time to reschedule tasks. Section C.5.3 shows it is unlikely that unobserved fixed costs associated with the casual jobs are influencing our results.

from the market and increase wages (Breza et al., 2019). While our model is agnostic as to the source of labor rationing, in Section 3.6.1 we discuss possible mechanisms that are consistent with our data.

A farmer makes decisions over bundles $b \equiv (\tau, h, m)$ corresponding to:

- obtaining or not the lottery ticket $\tau \in \{0, 1\}$,
- time spent on work $h \in \mathbb{R}^+$,
- a monetary transfer m that can be sent ($m > 0$ for symmetry with h) or received ($m < 0$).

Preferences are represented by the indirect utility function

$$\begin{aligned} V(\tau, h, m) &= \max_{c, l} u(c, l + h) + \mathbb{E}_\theta[v(I + wl + \tau\theta - c - m)] & (3.1) \\ l, c \text{ s.t. } & l \leq \bar{l} \\ & I + wl - c - m \geq \underline{k} \end{aligned}$$

Choice variables c and l denote current consumption and labor supply, respectively. Utility function u captures preferences over consumption and labor. The continuation value of next period wealth is captured by v . Non-labor income is denoted by I , w is the wage per unit of labor, and $\theta \in [0, \bar{\theta}]$ is a random variable capturing the returns to the lottery. Labor rationing is imposed through \bar{l} , while credit constraints are modeled with \underline{k} —the lower bound on remaining capital after decisions are made. The Lagrange multipliers associated with the labor and capital constraints are denoted by λ and κ , respectively.

Without loss of generality, we normalize $V(0, 0, 0) = 0$ and assume:

Assumption 1 (smooth preferences). *u and v are strictly concave, and continuously differentiable.*

An immediate implication is that consumption and labor choices c and l , as well as Lagrange multipliers κ and λ , are continuous functions of experimental bundle b .¹⁵

Lemma 1. *Given $b = (\tau, h, m)$, optimal choices $c|_b$ and $l|_b$ in (3.1) are unique and continuous in b . Lagrange multipliers $\kappa|_b$ and $\lambda|_b$ are also unique and continuous in b .*

The fact that the Lagrange multipliers are continuous plays a central role in our interpretation. Small changes in choice variables τ , h , and m parameterizing optimization problem

¹⁵We extend V to values of τ in $(0, 1)$ using the right-hand side of (3.1), capturing scaled-down returns $\tau\theta$ to owning a pump.

(3.1) have a small impact on the shadow value of capital and labor.¹⁶ This appears to be a reasonable assumption; in Section 3.5.3, we explore the possibility that purchasing the lottery ticket has second-order effects on credit constraints, and can rule this out with our data.

Lemma 1 and the Envelope Theorem for Lagrange multipliers (Milgrom and Segal, 2002) imply that the following first order approximation (using the familiar Big-O notation) holds.

Jibberish 1 (first-order approximation). *Under Assumption 1,*

$$V(\tau, h, m) = \tau V_\tau + h V_h + m V_m + O\left(\bar{\theta}^2 + h^2 + m^2\right) \quad (3.2)$$

with

$$V_\tau = \mathbb{E}_\theta[\theta v'(I + wl|_0 - c|_0)], \quad V_h = u_l(c|_0, l|_0), \quad V_m = -v'(I + wl|_0 - c|_0) - \kappa|_0.$$

Where $l|_0$, $c|_0$, and $\kappa|_0$ denote the values of $l|_b$, $c|_b$, and $\kappa|_b$ at $b = (\tau, h, m) = (0, 0, 0)$.

Theorem 1 shows that the indirect utility function V is a locally linear function of experimental choices (τ, h, m) , weighted by preference parameters reflecting the marginal indirect utility value of those choices (V_τ, V_h, V_m) . The fact that credit constraints enter (3.2) only through the value of money, V_m , is useful in examining the potential second-order effects of credit constraints in Section 3.5.3.

We refer to parameter V_h/V_m , the value of time expressed in the numeraire KSh, as the *structural value of time* (SVT).

3.3.1 Testable Implication of the Benchmark Model

Importantly, we believe that the choices in our study satisfy the requirements of Theorem 1: farmers are making decisions over bundles with values that are small compared to the total value of their overall optimization problem. Choice RW (reservation wage) involved 2 hours of work. The average cash bid m^{CB} for lottery tickets in choice CB was 111 KSh (equivalent to about 1.4 times the hourly market wage). The average time bid h^{TB} for lottery tickets in choice TB was 4 hours—roughly equivalent to an average day of casual labor. As a result, the remainder of this section attempts to interpret choice data using linearized preferences (3.2). We show that this leads to a contradiction.

¹⁶Work days were scheduled 1 to 2 weeks in advance so that farmers could reshuffle tasks across days, implying that within-day changes in working hours should be marginal. Lottery tickets had a relatively small average subjective value of 111 KSh, representing roughly what the average participant could earn from 1.4 hours of casual labor, implying that purchasing a ticket is unlikely to significantly change returns to capital.

Direct Value of Time. A farmer's optimal choice m^{RW} corresponds to the amount of money for which the farmer is indifferent between performing two hours of work for an amount m^{RW} , and the status quo:

$$V(\tau = 0, h = 2, m = -m^{RW}) = V(\tau = 0, h = 0, m = 0).$$

Using the first-order approximation (3.2), this implies that $2V_h - m^{RW}V_m = 0$. Thus, the direct value of time (DVT), defined as $DVT \equiv \frac{m^{RW}}{2}$, correctly estimates SVT:

$$DVT \equiv \frac{m^{RW}}{2} = \frac{V_h}{V_m} = SVT.$$

Indirect Value of Time. The indirect value of time (IVT), defined as $IVT \equiv \frac{m^{CB}}{h^{TB}}$, can also be interpreted using (3.2). A farmer's optimal choices m^{CB} and h^{TB} satisfy

$$V(\tau = 1, h = 0, m = m^{CB}) = V(0, 0, 0) \quad \text{and} \quad V(\tau = 1, h = h^{TB}, m = 0) = V(0, 0, 0),$$

respectively. Theorem 1 implies that

$$m^{CB} = -\frac{V_\tau}{V_m} \quad \text{and} \quad h^{TB} = -\frac{V_\tau}{V_h}.$$

Hence,

$$IVT \equiv \frac{m^{CB}}{h^{TB}} = \frac{V_h}{V_m} = DVT = SVT. \quad (3.3)$$

Thus, under our benchmark model, the direct and indirect values of time should be equal. The next subsection shows that, in our choice data, they are not. This implies that at least one of IVT and DVT, and possibly both, incorrectly estimate SVT.

3.3.2 Evidence of Preference Intransitivity

The data clearly reject the benchmark model, as shown in Table 3.2. The average direct value of time, DVT, elicited through choice RW, is 83 KSh/hour. This is close to the average reported wage for casual labor (82 KSh/hour). In contrast, the average indirect value of time, IVT, inferred from choices CB and TB, is 30 KSh/hour, substantially below the mean DVT (difference = 53 KSh/hour; p -val < 0.01). Moreover, the distribution of DVT first-order stochastically dominates the distribution of IVT, as shown in Figure 3.1. Indeed, 81% of farmers expressed a DVT strictly above their IVT.

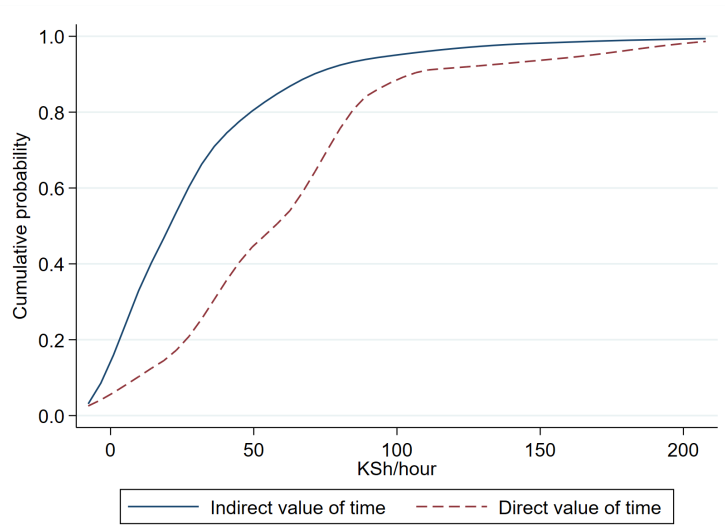
At the individual level, these data suggest that a majority of farmers have cyclical, non-transitive preferences. For instance, one of the farmers in our study, from the village of

Table 3.2: Choice Data (N=332 Farmers)

	Mean	Std. Dev.	p25	p50	p75
Direct value of time ($DVT = m^{RW}/2$)	83	54	50	80	100
Indirect value of time (IVT)	30	35	3	20	40
Cash bid (m^{CB})	111	126	20	100	155
Time bid (h^{TB})	4.0	2.2	3.0	4.0	5.0
DVT-IVT wedge ($\hat{\omega}$)	0.30	1.22	0.28	0.71	0.98

Each observation is a farmer. Currency units are Kenyan shillings (1 USD = 107 KSh). Cash bids, time bids, and DVT elicited through BDM. IVT = cash bid / time bid. DVT-IVT wedge = $1 - IVT/DVT$. p25, p50, and p75 are the 25th, 50th, and 75th percentiles.

Figure 3.1: The value of time is smaller when estimated indirectly through bids in money and time for the same good than when estimated directly through reservation wages.



Kernel-smoothed cumulative distribution functions (van Kerm, 2012) estimated on all farmers.

Turumba A, expressed $m^{RW}/2 = 80$ KSh, $m^{CB} = 100$ KSh, and $h^{TB} = 4$ hours (which matches the average values of these choices). This farmer would then exhibit the following choice behavior:

- 150 KSh \prec 3 hours (as $m^{RW}/2 = 80$),
- $\tau = 1 \prec$ 150 KSh (as $m^{CB} = 100 < 150$), and

- 3 hours labor $\prec \tau = 1$ (as $h^{TB} = 4$).

Examining these choices starting from the bottom reveals a cycle: 3 hours $\prec \tau = 1 \prec 150$ KSh \prec 3 hours.

For each farmer, we define

$$\hat{\omega}_i = 1 - \frac{IVT_i}{DVT_i} \quad (3.4)$$

as a measure of preference intransitivity, which we term the *DVT-IVT wedge*.¹⁷ The average value of $\hat{\omega}_i$ is 0.3, substantially higher than the benchmark prediction $\hat{\omega}_i = 0$ ($p\text{-val} < 0.01$).¹⁸

Credit and Labor Constraints. Although our model explicitly builds in credit and labor constraints, describing why they are unlikely to be driving the wedge between IVT and DVT provides a deeper understanding of Theorem 1. The important condition underlying this result is that the choices we offer have only second-order effects on the shadow value of money or time.

If a farmer is credit constrained, then they will have a high shadow value of money, but this will be reflected in both their IVT and DVT. In particular, a higher shadow value of money will lower both a farmer’s willingness to pay for a lottery ticket, m^{CB} , as well as their reservation wage, m^{RW} .¹⁹ This will lower both IVT and DVT equally, resulting in no wedge between the two. The only way that credit constraints could create such a wedge would be if the decision to buy a lottery ticket significantly tightened credit constraints, or if working for two hours significantly loosened them. In Section 3.5.3, we consider a model in which purchasing the lottery ticket ($\tau = 1$) significantly tightens credit constraints, and show that it is inconsistent with our data. This is not surprising, as many farmers were probably already credit constrained before facing the choices we offered. Moreover, the impact of investing in a lottery ticket is very minor compared to other investment opportunities.²⁰

¹⁷The hat emphasizes that $\hat{\omega}_i$ is empirically observable from choice data.

¹⁸Note that the median value of $\hat{\omega}_i$, 0.71, is much larger than the mean of 0.3. This is due to a long left tail in the distribution, with 17% of farmers exhibiting a $\hat{\omega}_i < 0$. Estimating SVT does not require that $\hat{\omega}_i$ be positive. Moreover, our results are robust to truncating these negative values—see Appendix Table C.5.4. We can also reject that the median of $\hat{\omega}_i$ is equal to 0 ($p\text{-val} < 0.01$).

¹⁹We gave farmers one week to pay, so that they were not constrained by their cash on hand the day they made their bid.

²⁰Examples of high-return investment opportunities with low take-up rates include grain storage facilities (Burke et al., 2018), irrigation (Jones et al., 2022), or, outside the realm of agriculture, antimalarial bed nets (Cohen and Dupas, 2010). Similar logic applies to labor constraints.

3.4 Structural Estimation of a Model With Wedges

In this section, we add wedges to the benchmark model of Section 3.3 that can explain the observed difference between DVT and IVT. We then estimate this extended model on our choice data to recover SVT, which—as we argue in Section 3.5.1—is the appropriate parameter for researchers to use in most settings. We then interpret the results of this estimation in terms of behavioral and other factors.

3.4.1 A Model With Wedges

To account for choice intransitivities, we allow farmers' choice problems to exhibit three separate wedges: under reservation wage choice RW, the size of monetary benefit is reduced by a factor $1 - \omega^{RW}$; under cash bid CB, the returns θ to owning the pump are scaled down by a factor $1 - \omega^{CB}$; under time bid TB, the returns θ to owning the pump are scaled down by a factor $1 - \omega^{TB}$. Thus, if $\omega^j = 0$, this implies that the associated choice j is not affected by a wedge. Choices RW, CB, and TB are characterized by the indifference conditions

$$\begin{aligned} V(0, 2, -(1 - \omega^{RW})m^{RW}) &= 0 & 2V_h - (1 - \omega^{RW})V_m m^{RW} &= 0, \\ V(1 - \omega^{CB}, m^{CB}, 0) &= 0 & \Rightarrow (1 - \omega^{CB})V_\tau + V_m m^{CB} &= 0, \\ V(1 - \omega^{TB}, 0, h^{TB}) &= 0 & (1 - \omega^{TB})V_\tau + V_h h^{TB} &= 0, \end{aligned} \quad (3.5)$$

where the equations on the right-hand side follow from linearizing using (3.2).

Note that there is a symmetry between shrinking the value of one object of choice and inflating the value of the other object: for example, shrinking the value of the monetary payment in Choice RW (reservation wage) by an amount $1 - \omega^{RW}$ is equivalent to inflating the value of the number of hours worked in that choice by $1/(1 - \omega^{RW})$. Using this structure, we can solve for m^{RW} , m^{CB} , and h^{TB} in the three choices and obtain:

$$DVT \equiv \frac{m^{RW}}{2} = \frac{V_h}{(1 - \omega^{RW})V_m} \quad \text{and} \quad IVT \equiv \frac{m^{CB}}{h^{TB}} = \frac{(1 - \omega^{CB})V_h}{(1 - \omega^{TB})V_m},$$

leading to an empirically observable DVT–IVT wedge $\hat{\omega}$ defined as

$$\hat{\omega} \equiv 1 - \frac{IVT}{DVT} = 1 - \frac{(1 - \omega^{RW})(1 - \omega^{CB})}{(1 - \omega^{TB})}. \quad (3.6)$$

Bounding SVT. The preference parameter V_h/V_m —the structural value of time (SVT)—is not identified by choice data alone, as any triplet $(\omega^{RW}, \omega^{CB}, \omega^{TB})$ that satisfies (3.6) rationalizes the wedge between DVT and IVT. For example, note that a wedge in only

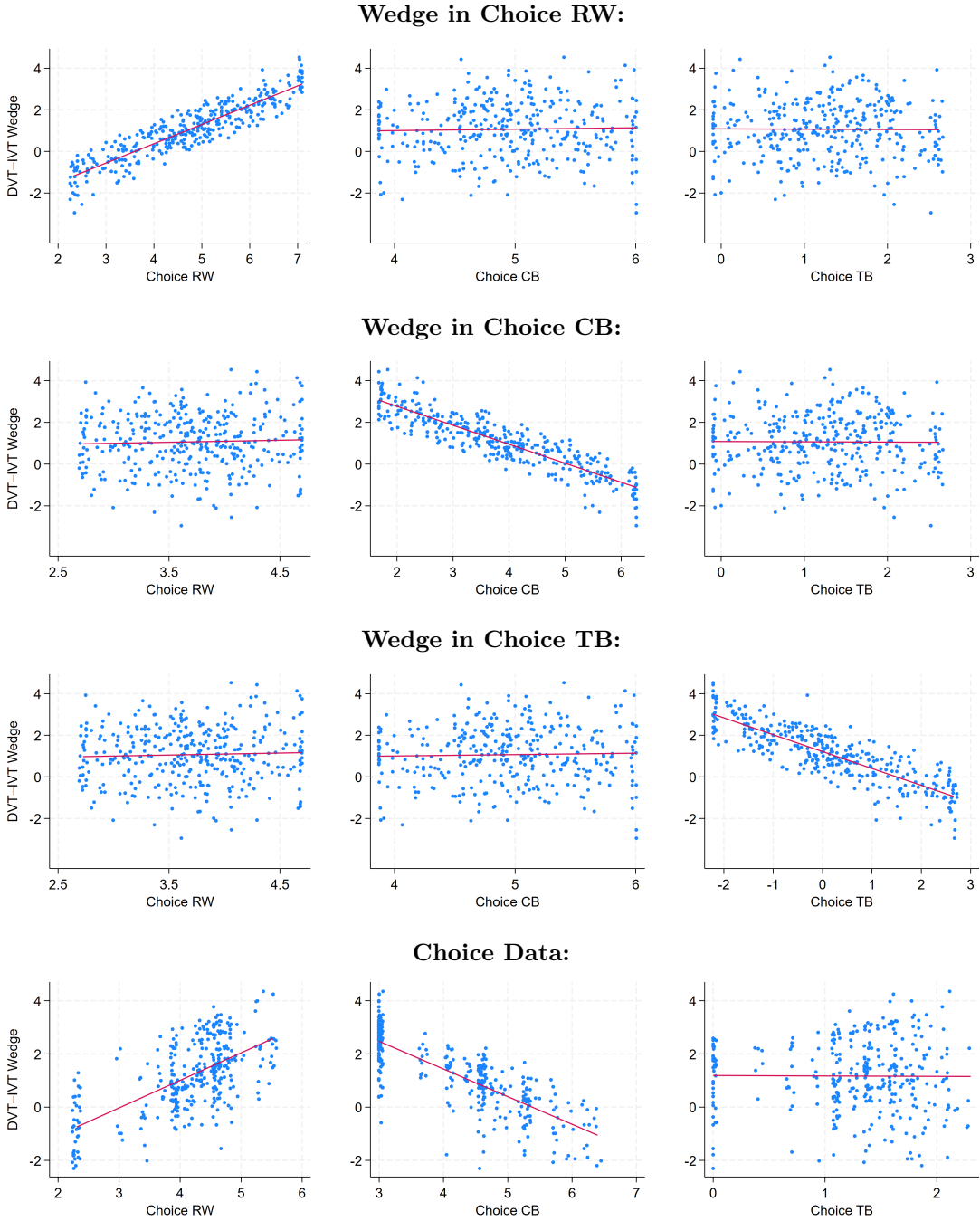
Choice RW ($\omega^{RW} = \widehat{\omega}$ and $\omega^{CB} = \omega^{TB} = 0$) would lead to IVT=SVT, and DVT>SVT. A wedge in only Choice CB ($\omega^{CB} = \widehat{\omega}$ and $\omega^{RW} = \omega^{TB} = 0$) would lead to DVT=SVT, and IVT<SVT. For interior values of the wedges ω^{RW} , ω^{CB} , and ω^{TB} that satisfy (3.6), SVT will be a weighted average of DVT and IVT, with weights determined by the (unknown) values of the wedges. Assuming that $\omega^{CB} \geq \omega^{TB}$ —which holds in our estimation results—we can bound SVT in [IVT, DVT] without additional assumptions—see Appendix C.1 for a proof of this statement. In our data, those bounds correspond to about 40% and 100% of the market wage. As we show in Section 3.6 by re-examining the conclusions of prior evaluations, knowing that the value of time is somewhere in this broad range may be sufficient to draw conclusions about whether or not a particular intervention is beneficial.

Point Identification of SVT. There are also interventions where more precise estimates are necessary. In the next subsection, we use the fact that different combinations of wedges do not predict the same patterns of correlation across choices m^{RW} , m^{CB} , and h^{TB} to identify, under some assumptions, the distribution of preference parameters ω^{RW} , ω^{CB} , and ω^{TB} in the population. This yields a precise estimate of SVT.

Before we estimate the model, it is useful to provide an intuitive argument for why identification of specific wedges may be possible. In our model, individuals with a large aggregate wedge will exhibit more distorted choices, on average. Thus, the correlations between the aggregate wedge and individual decisions tells us which of those decisions is more or less distorted. We show this graphically in the first three rows of Figure 3.2, which simulates the relationship between choice data m^{RW} , m^{CB} , h^{TB} and the log-linearized DVT–IVT wedge $-\log(1 - \widehat{\omega})$, with only one wedge present per panel. The fourth panel presents choice data from our study.

In our data, farmers’ time bids h^{TB} are uncorrelated with the DVT–IVT wedge $\widehat{\omega}$, whereas $\widehat{\omega}$ is positively correlated with m^{RW} , and negatively correlated with choice m^{CB} . Taken together, these correlations can be explained by positive wedges in the RW and CB choices, and no wedge in the TB choice—that is, $\omega^{RW} > 0$, $\omega^{CB} > 0$, and $\omega^{TB} = 0$. In the next subsection, we formalize this intuitive argument.

Figure 3.2: Aggregate choice data allow us to identify wedges and the structural value of time.



Rows 1–3 show the relationships between choices Choices RW ($m^{RW}/2$), CB (m^{CB}), and TB (h^{TB}) and the DVT–IVT wedge $\hat{\omega}$ that would arise if a wedge is present in only Choice RW, CB, or TB, respectively. The fourth row shows the same relationships observed between choices in our data. Each observation is a farmer with a 3% jitter. OLS line in red. All variables are log transformed.

3.4.2 Framework and Data-Generating Process

We return to the general model in (3.5), which contains parameters ω^{RW} , ω^{CB} , and ω^{TB} that can affect each choice in a distinct way. We use this model to specify variation in preferences across farmers. We index farmers by $i \in \{1, \dots, N\}$, and allow for farmer-level heterogeneity so that (3.5) takes the form

$$2V_{h,i} - (1 - \omega_i^{RW})V_{m,i}m_i^{RW} = 0, \quad (1 - \omega_i^{CB})V_{\tau,i} + V_{m,i}m_i^{CB} = 0, \quad (1 - \omega_i^{TB})V_{\tau,i} + V_{h,i}h_i^{TB} = 0. \quad (3.7)$$

It is convenient to re-express farmer i 's wedges ω_i^{RW} , ω_i^{CB} , and ω_i^{TB} as

$$1 - \omega_i^{RW} = \exp(-\rho_i\gamma_i^{RW}), \quad 1 - \omega_i^{CB} = \exp(-\rho_i\gamma_i^{CB}), \quad 1 - \omega_i^{TB} = \exp(-\rho_i\gamma_i^{TB})$$

with γ parameters normalized so that $\gamma_i^{RW} + \gamma_i^{CB} + \gamma_i^{TB} = 1$.

Thus, parameter ρ_i is an index of farmer i 's aggregate wedge, while parameters γ_i^{RW} , γ_i^{CB} , and γ_i^{TB} capture the relative intensity with which that wedge manifests across choice problems.

Using the following assumption:

Assumption 2. *Farmers vary in their aggregate wedge (ρ_i), but not in the relative intensity of each wedge (γ_i^X fixed across all i for $X \in \{RW, CB, TB\}$),*

we can rewrite (3.7) as

$$\begin{aligned} \log(m_i^{RW}/2) &= \log(V_{h,i}/V_{m,i}) + \rho_i\gamma^{RW} \\ \log m_i^{CB} &= \log(-V_{\tau,i}/V_{m,i}) - \rho_i\gamma^{CB} \\ \log h_i^{TB} &= \log(-V_{\tau,i}/V_{h,i}) - \rho_i\gamma^{TB}. \end{aligned} \quad (3.8)$$

Recall that a farmer's empirical DVT-IVT wedge $\widehat{\omega}_i$ is

$$1 - \widehat{\omega}_i = \frac{IVT_i}{DVT_i} = \frac{2m_i^{CB}}{m_i^{RW}h_i^{TB}}.$$

Hence, it follows from (3.8) that

$$\log \frac{1}{1 - \widehat{\omega}_i} = \log(m_i^{RW}/2) - \log(m_i^{CB}) + \log(h_i^{TB}) = \rho_i(\gamma^{RW} + \gamma^{CB} - \gamma^{TB}). \quad (3.9)$$

Note that ρ_i can only be estimated if $\gamma^{RW} + \gamma^{CB} - \gamma^{TB} \neq 0$. As $\widehat{\omega}_i \neq 0$ for many farmers, (3.9) implies this condition holds.

Let $\widehat{\delta}^{RW}$, $\widehat{\delta}^{CB}$, and $\widehat{\delta}^{TB}$ denote the OLS estimates obtained from the linear model:

$$\begin{aligned}\log(m_i^{RW}/2) &= c_A + \widehat{\delta}^{RW} \log \frac{1}{1 - \widehat{\omega}_i} + \epsilon_i^{RW} \\ \log m_i^{CB} &= c_B - \widehat{\delta}^{CB} \log \frac{1}{1 - \widehat{\omega}_i} + \epsilon_i^{CB} \\ \log h_i^{TB} &= c_C - \widehat{\delta}^{TB} \log \frac{1}{1 - \widehat{\omega}_i} + \epsilon_i^{TB}.\end{aligned}\tag{3.10}$$

With the following assumption, we can identify the main parameters of the structural model:

Assumption 3. *Conditional on observable characteristics, behavioral parameter ρ_i is uncorrelated with the logarithms of preference parameters $-V_{\tau,i}/V_{m,i}$, and $V_{h,i}/V_{m,i}$.*

Jibberish 2 (identification). *With probability one as the sample size N gets large:*

- For all $X \in \{RW, CB, TB\}$,

$$\widehat{\gamma}^X \equiv \frac{\widehat{\delta}^X}{\widehat{\delta}^{RW} + \widehat{\delta}^{CB} + \widehat{\delta}^{TB}} \rightarrow \gamma^X;$$

- For all $i \in \{1, \dots, N\}$,

$$\widehat{\rho}_i \equiv (\widehat{\delta}^{RW} + \widehat{\delta}^{CB} + \widehat{\delta}^{TB}) \log \frac{1}{1 - \widehat{\omega}_i} \rightarrow \rho_i.$$

Moreover, the OLS estimates of δ^X are as efficient as those estimated from a seemingly unrelated regressions model.

Simulations show that these estimators perform well for sample sizes similar to that of our data.²¹ Standard errors are obtained using the bootstrap with 10,000 draws.

To understand the role of Assumption 3 in identifying the model, it is useful to write down the structural analogues of the estimation equations (3.10)—which come from combining

²¹Across 10,000 simulations, estimating model (3.10) on data simulated with a single wedge produces estimates of the corresponding γ parameter that are always greater than 0.987, and of the other γ parameters that are always less than 0.013. Simulating data with the estimated parameters— $\gamma^{RW} = 0.39$, $\gamma^{CB} = 0.61$, $\gamma^{TB} = 0.00$ —produces estimates that are at most 0.015 away from the true values of those parameters.

(3.8) and (3.9):

$$\begin{aligned}
\log(m_i^{RW}/2) &= \log(V_{h,i}/V_{m,i}) + \frac{\gamma^{RW}}{\gamma^{RW} + \gamma^{CB} - \gamma^{TB}} \log \frac{1}{1 - \widehat{\omega}_i} \\
\log m_i^{CB} &= \log(-V_{\tau,i}/V_{m,i}) - \frac{\gamma^{CB}}{\gamma^{RW} + \gamma^{CB} - \gamma^{TB}} \log \frac{1}{1 - \widehat{\omega}_i} \\
\log h_i^{TB} &= \log(-V_{\tau,i}/V_{h,i}) - \frac{\gamma^{TB}}{\gamma^{RW} + \gamma^{CB} - \gamma^{TB}} \log \frac{1}{1 - \widehat{\omega}_i}.
\end{aligned} \tag{3.11}$$

Consistent estimation of the first and second equation in (3.10) requires the omitted variables $\log(V_{h,i}/V_{m,i})$ and $\log(-V_{\tau,i}/V_{m,i})$ to be uncorrelated with $\log \frac{1}{1 - \widehat{\omega}_i}$, which is a linear function of ρ_i . This is exactly Assumption 3. To see that Assumption 3 also gives consistent estimation of the third equation, it is helpful to note that $\log(-V_{\tau,i}/V_{h,i}) = \log(-V_{\tau,i}/V_{m,i}) - \log(V_{h,i}/V_{m,i})$, which are both uncorrelated with ρ_i by assumption.

While $\log(V_{h,i}/V_{m,i})$ and $\log(-V_{\tau,i}/V_{m,i})$ are not directly observable, we show that survey-based proxies can be constructed. These proxies can be used to test for the influence of omitted variable bias arising from a violation of Assumption 3, a point we return to in Section 3.4.4.

Consistent estimates of the structural value of time of farmer i , $\widehat{\text{SVT}}_i$, can be recovered using (3.8) and Theorem 2:

$$\widehat{\text{SVT}}_i = \widehat{V_{h,i}/V_{m,i}} \equiv \frac{m_i^{RW}}{2} \exp \left(-\widehat{\delta}_i^{RW} \log \left(\frac{m_i^{RW} h_i^{TB}}{2m_i^{CB}} \right) \right). \tag{3.12}$$

This formula represents the process described intuitively in the introduction: data from all three choices are used to estimate the extent to which choice RW is impacted by a wedge, and then to remove that effect.²²

3.4.3 Estimation Results

Across the specifications and sub-populations in Table 3.3, all estimated using Theorem 2, choice TB shows no evidence of distortions ($\widehat{\gamma}^{TB} = 0$), while those choices that involve cash

²²As consistently estimating $\widehat{\text{SVT}}_i$ requires only a consistent estimate of $\widehat{\delta}^{RW}$, it requires only that $\log(V_{h,i}/V_{m,i})$ is uncorrelated with ρ_i —see the first equation of (3.11)—a subset of Assumption 3.

are the source of distortions ($\hat{\gamma}^{RW}, \hat{\gamma}^{CB} > 0$).²³ This pattern is the same as that shown in Figure 3.2: distortions are consistent with non-zero wedges only in choices involving cash.

Fitting data from the full sample, in Column 1, results in a mean structural value of time equal to 49 KSh/hour, or 60% of the average wage for casual labor. As expected, this lies inside the range of estimates produced by IVT and DVT (40% to 100% of the market wage).

3.4.4 Threats to Identification

Our strategy produces valid estimates of all our model parameters as long as identifying Assumptions 2 and 3 hold in our data. We thus examine a number of different specifications and subgroups that provide support for these assumptions.

3.4.4.1 Stability of Estimates Across Subgroups

To investigate whether both Assumptions 2 and 3 are reasonable, we estimate our model separately within groups of economically similar farmers.²⁴ There is likely to be less confounding variation in preferences within these groups, so that independence between the DVT–IVT wedge $\hat{\omega}$ and the parameters $\log(-V_{\tau,i}/V_{m,i})$ and $\log(V_{h,i}/V_{m,i})$ is more likely to hold. Estimating our model separately also provides a check of whether γ^{RW} , γ^{CB} , and γ^{TB} are stable across heterogeneous subgroups. We form four groups using Partitioning Around Medoids (PAM) cluster analysis, which is described in Appendix C.4. We characterize these four groups—sorted from lowest to highest average DVT–IVT wedge $\hat{\omega}$ —as consisting of the low-skill self-employed, low-skill employees, hirers of casual labor, and older, low-education households. These characterizations are based on the strongest predictors of membership in each group, as shown in Table C.4.1.

Estimated parameters γ^{RW} , γ^{CB} , and γ^{TB} are stable across groups, as shown in Columns 2–5 of Table 3.3. This supports Assumption 2: that the relative intensities γ are fixed across the sample. The estimated structural value of time is also stable, varying from 54–67% of the market wage.²⁵ This is true despite substantial variation in the average DVT–IVT wedge

²³As we bottom code cash and time bids that are outside the range of allowed prices—bids below 20 KSh or 1 hour, respectively—and top code DVT above 250 KSh/hour, we test for sensitivity to recoding in Columns 1–4 of Appendix Table C.5.4. The estimated relative intensities $\hat{\gamma}^{RW}$, $\hat{\gamma}^{CB}$, $\hat{\gamma}^{TB}$ change little across specifications, and the estimated mean structural value of time is very stable at 57–60% of the market wage.

²⁴Table C.3.1 shows how our estimates of the SVT vary based on respondent gender, age, education, income, the presence of a child under 3, and whether someone in the household operates a micro-enterprise. Estimates of SVT are highly stable across subgroups, varying from 54–67% of the market wage.

²⁵As another way of describing the relative stability of estimates of SVT_i/\bar{w}_i in our data, the standard deviation of SVT_i/\bar{w}_i —0.52—is low relative to the standard deviation of DVT_i/\bar{w}_i —0.92.

Table 3.3: Structural Estimates of the Value of Time

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Cluster breakdown					
	Full sample	Low-skill self-employed	Low-skill employees	Hires casual workers	Older, low-edu households	Full sample +controls	Casual laborers	Considered buying pump
Structural estimation								
Reservation wage	0.39	0.39	0.40	0.36	0.42	0.39	0.39	0.40
relative intensity ($\hat{\gamma}^{RW}$)	(0.023)	(0.034)	(0.049)	(0.049)	(0.060)	(0.023)	(0.039)	(0.031)
Cash bid	0.61	0.59	0.61	0.63	0.58	0.62	0.61	0.60
relative intensity ($\hat{\gamma}^{CB}$)	(0.025)	(0.041)	(0.049)	(0.058)	(0.063)	(0.025)	(0.039)	(0.032)
Time bid	0.003	0.025	0.000	0.010	0.000	0.000	0.000	0.000
relative intensity ($\hat{\gamma}^{TB}$)	(0.014)	(0.029)	(0.010)	(0.029)	(0.018)	(0.013)	(0.007)	(0.017)
Structural value of time (SVT)	49	45	46	58	46	49	46	44
	(2.4)	(3.8)	(4.2)	(5.8)	(6.4)	(2.4)	(3.5)	(2.7)
Market wage (\bar{w})	82	84	77	86	81	82	73	82
	(1.8)	(3.8)	(3.4)	(3.4)	(3.5)	(1.8)	(3.1)	(2.4)
Relative value of time (SVT/\bar{w})	0.60	0.54	0.59	0.67	0.57	0.60	0.63	0.54
	(0.033)	(0.052)	(0.060)	(0.077)	(0.082)	(0.033)	(0.055)	(0.037)
Choices								
Direct value of time (DVT)	83	79	70	92	98	83	72	73
	(3.0)	(5.8)	(4.1)	(6.2)	(8.4)	(3.0)	(3.9)	(3.2)
Indirect value of time (IVT)	30	31	31	35	17	30	31	29
	(1.9)	(4.0)	(3.4)	(4.1)	(3.2)	(1.9)	(3.1)	(2.4)
Cash bid	111	127	125	117	50	111	129	123
	(6.9)	(16.2)	(12.6)	(13.2)	(7.6)	(6.9)	(11.4)	(9.6)
Time bid	4.0	4.2	4.5	3.8	3.3	4.0	4.6	4.4
	(0.1)	(0.2)	(0.2)	(0.3)	(0.2)	(0.1)	(0.2)	(0.2)
DVT-IVT wedge ($\hat{\omega}$)	0.30	0.12	0.18	0.33	0.74	0.30	0.19	0.17
	(0.067)	(0.169)	(0.132)	(0.115)	(0.048)	(0.067)	(0.113)	(0.103)
Observations	332	75	108	93	56	332	141	189

Each observation is a farmer. Currency units are Kenyan shillings (1 USD=107 KSh). Columns (2)–(5) show results estimated separately within clusters of similar farmers. Column (6) controls for unincorporated proxies of the value of time and the valuation of the lottery ticket. Column (7) shows results estimated on farmers who performed casual labor within the past 3 months. Column (8) shows results estimated on farmers who report having considered buying a MoneyMaker irrigation pump. Cash and time bids bottom-coded at 20 KES and 1 hour respectively. Bootstrap standard errors in parentheses.

$\widehat{\omega}$ across clusters—from 0.12 to 0.74. This provides some evidence that 60% of the market wage is a reasonable rule of thumb for the SVT, even across heterogeneous subgroups.

3.4.4.2 Robustness to Controlling for Proxies of V_τ and V_h

A further test of the plausibility of Assumption 3—that farmers’ aggregate wedges ρ_i are uncorrelated with $\log(-V_{\tau,i}/V_{m,i})$ and $\log(V_{h,i}/V_{m,i})$ —comes from examining the estimates of $\widehat{\rho}_i$, $\widehat{\gamma}^{RW}$, $\widehat{\gamma}^{CB}$, and $\widehat{\gamma}^{TB}$ after controlling for the logs of $-V_{\tau,i}/V_{m,i}$ and $V_{h,i}/V_{m,i}$ in (3.10). As shown in (3.8), choices in our model are determined solely by the logs of $-V_{\tau,i}/V_{m,i}$ and $V_{h,i}/V_{m,i}$, ρ_i , and parameters γ^{RW} , γ^{CB} , and γ^{TB} . While $-V_{\tau,i}/V_{m,i}$ and $V_{h,i}/V_{m,i}$ cannot be observed directly, our survey data offer proxies. If our model estimates are unaffected by controlling for the log of such proxies, this implies that ρ_i is uncorrelated with $\log(-V_{\tau,i}/V_{m,i})$ and $\log(V_{h,i}/V_{m,i})$.

We have two such proxies. First, we use stated willingness to work—in hours—for a lottery ticket for an irrigation pump (collected as part of a baseline survey conducted five years earlier, in 2014) as a proxy for $-V_{\tau,i}/V_{m,i}$. Second, we use the stated minimum amount of money for which the respondent would be willing to travel one hour (collected during our main 2019 survey) as a proxy for $V_{h,i}/V_{m,i}$. We find that these unincentivized proxies are strongly correlated with farmers’ choices, but uncorrelated with wedges, suggesting that they are good proxies for $-V_{\tau,i}/V_{m,i}$ and $V_{h,i}/V_{m,i}$.²⁶

Controlling for the log of the unincentivized proxies of $-V_{\tau,i}/V_{m,i}$ and $V_{h,i}/V_{m,i}$, in Column 6 of Table 3.3, has very little effect on our estimates. In particular, $\widehat{\rho}_i$ changes very little between Columns 1 and 6—from an average of 1.18 to 1.17—and $\widehat{\gamma}^{RW}$, $\widehat{\gamma}^{CB}$, and $\widehat{\gamma}^{TB}$ are also highly stable. This suggests that, indeed, $\log(-V_{\tau,i}/V_{m,i})$ and $\log(V_{h,i}/V_{m,i})$ are uncorrelated with ρ_i , which is exactly Assumption 3.²⁷

²⁶The p -value from the bivariate regression of $-\log(1-\widehat{\omega}_i)$ on the logarithm of the unincentivized willingness to work for the ticket is 0.50; on the logarithm of the unincentivized reservation payment for traveling one hour, it is 0.29. The p -values from bivariate regressions of $\log(m_i^{CB})$ and $\log(h_i^{TB})$ on the logarithm of the unincentivized willingness to work for the ticket are 0.03 and 0.00, respectively, and the p -value from the bivariate regression of $\log(m_i^{RW}/2)$ on the logarithm of the unincentivized reservation payment for traveling one hour is 0.01.

²⁷Additionally, if $-V_{\tau,i}/V_{m,i}$ and $V_{h,i}/V_{m,i}$ are uncorrelated with ρ_i , then the DVT among farmers exhibiting no wedges should approximate the average value of time in the sample. Consistent with this prediction, we find that farmers with $|\widehat{\omega}| < 0.15$ have an average DVT of 54 KSh/hour, close to the average SVT of 49 KSh/hour in the full sample ($p = 0.47$).

3.5 Interpretation and Robustness

In this section, we consider potential economic interpretations of the decision wedges. We first explain why the SVT is relevant for evaluating welfare in many settings, regardless of the specific mechanisms driving the wedges. We then outline behavioral models that can rationalize our results in Section 3.5.2. Finally, we describe other possible interpretations of our results that we can reject by our design or data in Section 3.5.3.

3.5.1 When Is the SVT Welfare Relevant?

Under the assumptions discussed in Section 3.4.3, and checked in Section 3.4.4, SVT is identified. In this section, we provide guidance to researchers interested in using the SVT to assess the welfare impacts of interventions.

Our results show that wedges do not affect choices that trade off time for a good: across several heterogeneous subgroups, and regardless of whether we estimate our model with or without control variables, our estimate of the wedge ω^{TB} is a precisely estimated zero. Applying this finding can help researchers decide when the SVT is the appropriate parameter for welfare evaluation. An intervention that changes time spent working on one’s own farm or small business is best modeled as a trade-off between time and goods, and thus one where the “unwedged” value of time—the SVT—correctly reflects the opportunity cost of time. As many interventions evaluate similarly naturalistic trade-offs, the SVT is appropriate across a broad range of settings. In contrast, an intervention that leads farmers to trade-off time for money—for example, one that increases hiring by reducing labor market frictions—would require the researcher to take a stand on whether to incorporate wedges into welfare evaluations. In cases where the intervention is likely to evoke a behavioral response, using the DVT to evaluate welfare may be appropriate. If a researcher is unsure, they can consider using SVT and DVT as bounds.

3.5.2 Interpreting Wedges: Potential Models

Explaining the wedge between DVT and IVT requires a steep change, or “kink,” in the indirect utility function (3.1). The estimation results in Section 3.4.3 indicate that this kink arises in our study whenever transactions involve cash. Cash-specific wedges could arise in an environment where farmers regularly make opportunity cost calculations in terms of goods and time—for example, deciding how much time to work on their field in order to obtain a greater yield—but rarely use cash. However, researchers interested in understanding the surplus generated by a new technology often wish to translate changes in yield or time use into a single numeraire by assigning them a cash value. Making this translation—either by

offering cash for work or by selling a good for both cash and time—could cause farmers to make trade-offs that do not represent their underlying value of time or of the good. In this subsection, we discuss potential behavioral models that could drive this cash-specific kink. Distinguishing between these models is not necessary for identifying SVT, but may be relevant for researchers applying our estimates in different environments.

Cash-Specific Self-Serving Bias. The results of our estimation can be explained by a *self-serving bias* that arises only in transactions that involve cash. In models with self-serving bias, people discount the value of goods obtained from other parties (Loewenstein et al., 1993; Babcock et al., 1995; Babcock and Loewenstein, 1997). Using this frame to interpret our results suggests that farmers over-value their labor when compensated in money (but not goods), and under-value goods when paying in money (but not time). To give this a more succinct, but less precise, interpretation: farmers fear being taken advantage of—or think negotiation is more important—when transactions involve cash.

Cash-Specific Loss Aversion. Our results can also be explained by a model of loss aversion (Kahneman et al., 1991; Kahneman and Tversky, 1979). As with a self-serving bias, loss aversion would need to arise only in transactions involving cash. This distinction is particularly natural in the case of loss aversion, which was originally identified in monetary gambles (Kahneman and Tversky, 1979).

Cash-Specific Risk Aversion. As explained in Section 3.5.3, standard models of risk aversion will not generate a wedge between DVT and IVT. For risk aversion to explain our results, farmers would need to be differentially averse to risk when paying in cash compared to time.²⁸

Evidence for Behavioral Models. We find some support for a cash-specific behavioral bias in our data. Under the assumption that behavioral phenomena will be less pronounced when individuals are experienced with specific choices (List, 2003; Feng and Seasholes, 2005; Kőszegi and Rabin, 2006; Carney et al., 2019), we can analyze the choices of those who have performed or hired casual labor within the past three months, and those who have experience exchanging their cash for goods. These are all proxies for experience transacting in cash, and were measured in a baseline survey.²⁹ In these three groups, the DVT–IVT wedge $\hat{\omega}$ is

²⁸In theory, a similar result could arise if farmers are averse to spending cash, but not time, on an unfamiliar good. However, farmers in our study seem familiar with the pump; see Section 3.5.3.

²⁹Specifically, we compute the first principal component of eight indicators for whether the farmer purchased (or rented) agricultural equipment or inputs, home durables, land, buildings, cattle, chickens, or other livestock; or made business investments. We split the sample based on the median value of this component.

smaller than in the full sample, as shown in Appendix Table C.3.2, which presents formal regression analysis showing the predictive power of these three, and other, covariates.³⁰

3.5.3 Interpreting Wedges: Models Rejected by Our Data or Design

In this section, we discuss and summarize evidence against several potential alternative interpretations of the wedges. While identification does not depend on the specific model generating wedges, the source of wedges may be relevant when applying our estimates in different environments. Appendix C.5 expands on each model listed in this section.

First-Order Effects of Credit or Labor Constraints. First-order effects of credit or labor constraints are incorporated into our benchmark model, and thus, cannot explain a wedge between DVT and IVT. If a farmer is credit constrained, they will have a high shadow value of money, but this will be reflected in both their IVT and DVT equally through the value of money $V_{m,i}$.

Second-Order Effects of Credit or Labor Constraints. Second-order effects of credit and labor constraints can explain the DVT–IVT wedge; however, this explanation gives rise to an additional testable prediction that is inconsistent with our data, as we show in Appendix C.5.2. Specifically, this explanation predicts that reservation wages should be negatively correlated with $\hat{\omega}$, because the value of money will be higher for farmers facing tightened credit constraints, thereby decreasing reservation wages and increasing the DVT–IVT wedge. However, as shown in Panel 4 of Figure 3.2, the DVT–IVT wedge is strongly positively correlated with the reservation wage.

Uncompensated Costs of the Work Activity. We provided transportation to and from job sites, and the time this took was credited towards farmers’ work commitments. However, farmers needed to make room in their schedule to attend the work session, and spend time traveling between their home and the pickup location in the village center. This could appear as a wedge in Choice RW or TB. Work days were scheduled 1 to 2 weeks in advance so that farmers could reshuffle tasks across days, implying that within-day changes in working hours should be small. Additionally, if some component of transport costs is not observed—for example, some people live farther than others—the benchmark model implies restrictions on farmers’ choices that are rejected in our data. Appendix C.5.3 formalizes this argument.

³⁰The relative intensities γ are similar in these groups to those in the full sample, implying that differences in the choice-specific wedges ω_i^{RW} and ω_i^{CB} are driven by differences in ρ_i rather than by differences in γ .

Stigma of Accepting Low Wages. If accepting low-wage work is stigmatized, as in (Breza et al., 2019), this could inflate DVT above SVT. To test for these norms, we elicited survey reactions to a story about a farmer accepting a wage 50% below the market rate, and found that positive reactions were much more common than negative ones to both the worker and the hirer. This points to a limited scope for low-wage stigma in our setting. Additionally, these survey reactions are not significantly correlated with DVT, suggesting that their influence on our results is minimal (see Appendix C.5.4). More general versions of an aversion to low-cash wages are possible—for example, if self-image is tied to hourly wages but not to a low implied wage in Choice TB, possibly because the implied wage is more opaque than a cash wage.

Non-Compliance. If farmers inflate their cash or time bids above their willingness to pay—or deflate their reservation wages below their willingness to accept—while intending to later renege by not making the payment or completing work, this could appear as a wedge. Reneging was possible, as our design gave farmers 1–2 weeks before their full cash payment was due, or before they completed casual work for a lottery ticket or a payment. The rate of follow-through for cash payments was high. Among farmers who drew a random cash price below their willingness to pay (so were eligible to buy a ticket), 88% paid the correct price on or before collection day. Follow-through in choices TB and RW was lower: among farmers who drew a time price below their willingness to pay, 75% completed their work on the scheduled work day. Among farmers selected for wage work who had a reservation wage weakly below their wage draw, 74% completed their work on the scheduled work day.³¹ As we discuss in Appendix C.5.5, the correlations between compliance and choices suggest that most farmers were not planning on reneging when making their choices. Finally, restricting estimation to farmers with high predicted compliance does not significantly affect our results.

BDM Comprehension. The BDM elicitation method we use is common in studies of the self-employed (Berry et al., 2020; Burchardi et al., 2021). Four pieces of evidence, described in Appendix C.5.6, suggest that features which may be present in the BDM design are not driving the intransitivities we observe. First, we find no significant order effects when we randomize the sequence of Choices CB and TB. Second, we find no evidence that farmers are anchoring their choices either to the prevailing wage or to the starting points of the BDM procedure. Third, very few farmers took the opportunities we offered them to revise

³¹Our compliance rate for cash payments is in line with other studies using BDM: see Maffioli et al. (2023) for a discussion of reneging after BDMs. The lower compliance rate when paying in time is likely due to the down payment used in choice CB, which is difficult to mimic for choices that involve a time commitment. A multivariate test of means rejects equality of compliance rates with $p = 0.03$. As discussed in Appendix C.5.1, compliance does not depend on the amount of time a farmer had to obtain cash.

their bids. Fourth, and finally, very few farmers expressed regret about their choice after the random price was drawn. While these facts are reassuring, it is worth noting that any technique for eliciting the value of time may introduce wedges, which would need to be estimated in order to recover SVT.

Familiarity With the Work Activity and Good. The specific work activity or good used in our choices—casual labor and a lottery for an irrigation pump—are unlikely to drive the wedges we observe. Casual labor is very common in this setting, and nearly all farmers were familiar with the pumps, with most having considered purchasing one in the past. To test whether familiarity with the BDM activities matters for our results, we re-estimate our model separately within the set of farmers who have recently performed casual labor, and within those who have considered purchasing the irrigation pump in the past. The SVT for these subgroups as a fraction of the market wage, shown in Columns 7 and 8 of Table 3.3, is 63% and 54%, respectively, close to the 60% estimate in the overall sample.

Present Bias. Standard models of time discounting, in which decision makers value a good less the longer they have to wait for it, cannot explain our findings. Workers received payment in Choice RW immediately after work was completed. Lotteries were held as soon as payment and work were complete. None of the choices in our study involved trade-offs between the present and the future (with the exception of the 20-KSh down payment for the lottery ticket when paid in cash). As such, present bias cannot contribute to our results.

Intra-Household Decision-Making. Our study design mimicked real-world decisions by allowing the household to choose which member participated in the study. If farmers who participated in our study are expected to consult their family members about cash purchases, but not time spent on work, this could potentially generate a wedge between DVT and IVT. All surveys were held at participants' homes, and spouses were permitted to sit in, so consulting with them was possible.³² We find that single-headed and smaller households exhibit a greater DVT–IVT wedge on average, which is difficult to reconcile with intra-household decision-making dynamics driving our results.

Risk Aversion. Farmers whose preferences exhibit risk aversion will be willing to pay—in cash or in time—less than the expected value of the lottery ticket in Choices CB and TB. However, this will affect choices only through the farmer's value of the ticket $V_{\tau,i}$, which does not enter IVT or DVT.

³²We did not observe significantly different wedges when spouses sat in on the activities. We observe larger wedges for women than for men, but not different values of time, as shown in Appendix Table C.3.1. The gender difference in wedges disappears when controlling for other characteristics, as shown in Appendix Table C.3.2.

3.6 Discussion

This paper seeks to better understand how to measure people’s value of time in policy evaluations. We show that a direct, incentivized elicitation in which participants perform casual labor for money may not produce a valid estimate of the value of time due to behavioral wedges. In particular, participants seem to overvalue their time when exchanging it for cash. Using a design involving choices between time, money, and a good, we are able to identify the effects of wedges, and recover a welfare-relevant structural value of time. This value of time is roughly 60% of both the value elicited through a direct BDM mechanism and the market wage for casual labor. Figure 3.3 displays these facts visually. Market wages and reservation wages elicited through a direct BDM mechanism are fairly similar. However, the structural value of time is much lower than either the market wage or the BDM elicitation.

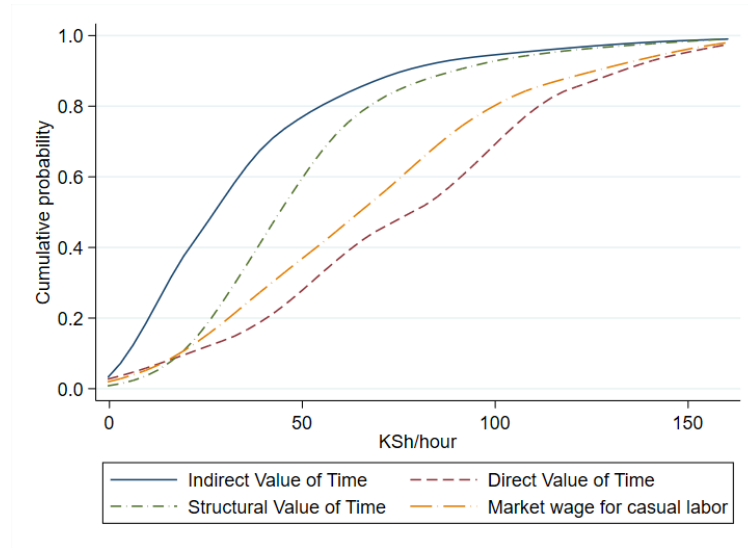
3.6.1 Implications for Labor Markets

Self-employment in the informal sector accounts for the majority of work in Africa (O’Higgins et al., 2020). Self-employment may be disguised excess labor supply (Breza et al., 2021) generated by frictions such as wage rigidity (Kaur, 2019) or other labor market constraints (Benjamin, 1992; Jones et al., 2022). Our results suggest an additional factor contributing to high self-employment levels: behavioral responses to negotiations involving cash, such as a cash-specific self-serving bias. As this phenomenon can cause an impasse in negotiations even when information is complete (Babcock and Loewenstein, 1997), it may lead workers to opt for self-employment over higher-paying casual jobs.³³ Further, this phenomenon may make maintaining norms of not accepting low-wage jobs easier, which Breza et al. (2019) identify as a source of labor-market distortions. Finally, survey questions requiring farmers to estimate the cash value of in-kind payments or of agricultural production may be inaccurate in settings where goods and time are typically transacted without cash.

Alternatively, if this phenomenon does not extend to most negotiations, then the finding that market wages for casual labor first-order stochastically dominate the structural value of time suggests that wages are higher than the market-clearing rate, and that casual jobs are rationed. Labor rationing may be a response to shading of job performance due to wage deviations below a laborer’s reference point (Hart and Moore, 2008; Fehr et al., 2011). We are able to test for this in our setting using the random variation in hourly wages paid

³³It could also cause those who hire casual labor to undervalue it relative to cash during negotiations. Unfortunately, we do not observe willingness to pay for labor in any of our activities. Note that our analysis does not imply that behavioral phenomena are welfare reducing in equilibrium, even for a given individual. In strategic contexts, like wage bargaining, behavioral phenomena can influence the behavior of other parties, helping individuals to obtain better terms.

Figure 3.3: The structural value of time is lower than wages and the direct value of time.



Kernel-smoothed cumulative distribution functions (van Kerm, 2012) estimated on all farmers. All variables top coded at 150 KSh/hour.

for casual work in choices RW and TB. Specifically, we test whether the quality of work performed—as evaluated by field staff after work was completed—depends on the random wage paid. For example, in the RW choice, the wage paid for day work is random, and—because only those who drew a wage higher than their DVT were eligible to work—eligibility is random conditional on DVT. We find significant evidence of shading at lower wages, but only for wages below reference wages—the amount farmers told us they thought they could earn for casual labor—as shown in Appendix Table C.6.1. Moreover, shading only occurred when the farmer was working for a cash wage, as opposed to a set reward. This suggests that, when paying cash, employers may find it worthwhile to pay a higher wage to increase the average quality of work, leading to fewer jobs.

3.6.2 Value of Time Assumptions in the Literature

In this section, we survey the extant literature to understand how it accounts for the value of time of the self-employed. We searched top economics journals for any study from 2016–2021

of the self-employed in a low-income country, in which revenue or profits were measured.³⁴ This search resulted in a total of 106 studies, of which only 42 had collected enough information, in theory, for us to reinterpret their results in light of our findings.³⁵

As shown in the top-left bar of Figure 3.4, 24% of the 106 studies do not attempt to use profit as an outcome, instead only reporting output-oriented measures, such as yields or revenue, that do not account for changing costs. Many of these papers justify their focus on output with the fact that it is difficult to measure the value of time for the self-employed (see, for example, Suri, 2011; Ahmed et al., 2021; Beaman et al., 2021). An additional 50% of the studies compute profit estimates using zero as the value of time. That is, together, 74% of the studies either avoid evaluating welfare impacts, or omit participants' value of time when doing so. The remaining studies (23%) use the market wage to value the time of the self-employed. A subset of these (8% of all studies) use both zero and the market wage to bound profit estimates under a range of values of time, similar to our first simple strategy above—although we recommend a lower bound of 40% of the market wage.

Studies that collected sufficient information to, in principle, calculate profits under different values of time ($N = 42$) were more likely to value the time of the self-employed, with 57% assigning a positive value in at least some specifications, as shown in the center bar of Figure 3.4. Among those studies where we could obtain the necessary data for these calculations ($N = 18$), 61% assigned a positive value in at least some specifications.³⁶

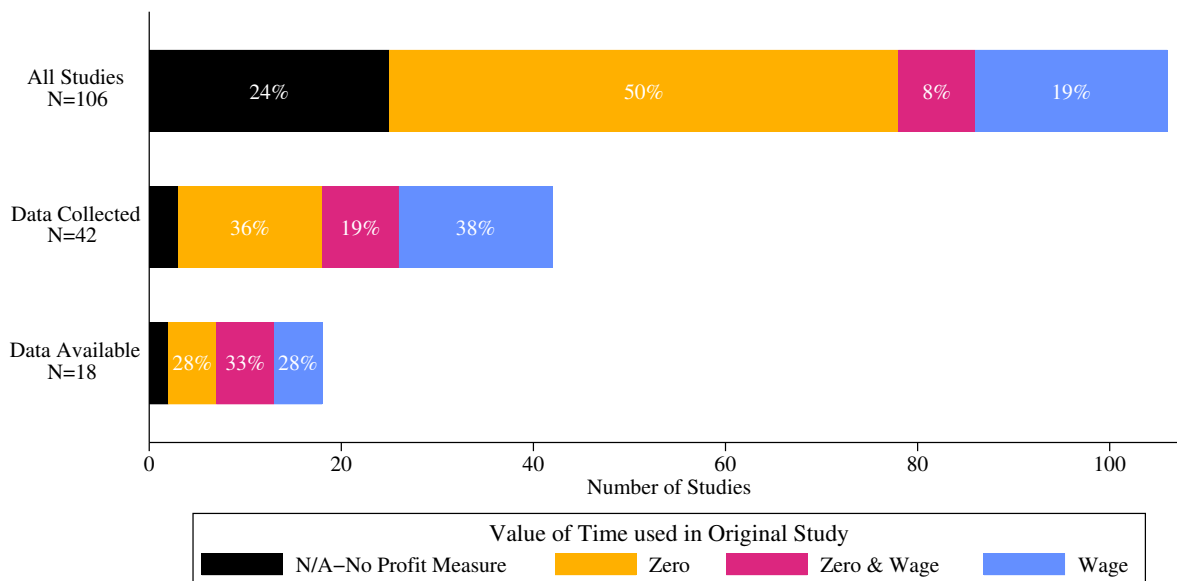
The fact that many recent studies do not measure input costs, even though they consider profits as a primary outcome, may be surprising. This may stem, in part, from the findings of De Mel et al. (2009), which suggest that asking the self-employed to self-report accounting profits is more accurate than eliciting revenues and costs, and computing profits from these quantities. However, that study does not consider the hours worked by the self-employed as

³⁴In particular, we searched Top-5 journals, plus top applied journals (*Journal of Development Economics* and *American Economic Journal: Applied Economics*), and top ag-econ journals (*American Journal of Agricultural Economics* and *European Review of Agricultural Economics*) for papers with 45 *JEL* codes during the years 2016–2021. The reviewed *JEL* codes can be found in Appendix C.7. The papers that resulted from this search were then read to find those about the self-employed that measured revenue or profits.

³⁵Analyzing the sensitivity of results to assumptions about the value of time requires three pieces of information: household labor hours, the locally prevailing market wage, and revenue net of other input costs. From what we could gather, 64 of the 106 studies did not collect all necessary data. In particular, only 8 (12.5%) of these 64 studies appear to have collected data on household labor supply, and 14 (22%) on market wages.

³⁶Of the 42 studies that collected the data needed to re-calculate profits, 6 contained sufficient information in the paper itself for us to re-evaluate their results, 12 had replication datasets with sufficient information available online, and an additional 15 studies required us to gather the source data for the paper. We received a complete replication dataset for 2 of those 15. We thank the authors who provided these data.

Figure 3.4: Value of Time Used in Prior Literature on the Self-Employed



a cost in their profit measure.³⁷ Yet, two programs that impact accounting profits equally, but affect work hours for the business owner differently, will clearly have different welfare impacts. Even if one were to only ask the self-employed about accounting profits, as De Mel et al. (2009) suggest, our results indicate that one should additionally ask about the hours worked by the self-employed, and use this information in calculating profits.

3.6.3 Practical Implications for Researchers

Overall, our findings suggest the need for more understanding of how the self-employed value their own time. However, they also suggest approaches that can be immediately applied. In this subsection, we describe some rules of thumb and their limitations, and, in the next, apply these simple techniques to prior studies in order to illustrate their potential usefulness.

We begin with two simple strategies for valuing the time of the self-employed:

³⁷When eliciting profits directly, they ask: “What was the total income the business earned during the month of [March] after paying all expenses including the wages of employees, *but not including any income you paid yourself?* That is, what were the profits of your business during [March]?” (emphasis ours).

Use a range of 40–100% of the market wage. This does not require committing to a particular model or choice(s) as “correct,” consistent with the approach in Bernheim and Rangel (2009). As we illustrate below, in Figure 3.5, this approach is sometimes sufficient for evaluating whether or not a particular intervention is beneficial. However, for some applications, a point estimate may be necessary, in which case we suggest:

Use 60% of the market wage. Researchers evaluating interventions in similar contexts as ours could opt to rely on our estimate that the value of time is close to 60% of the market wage for casual labor; see Acampora et al. (2022) for a recent example of an application of this rule of thumb. This follows the “parametric tradition” of welfare evaluation: see Sadoff et al. (2020) for a brief summary and other examples.

A more complex strategy, but one that might be useful for large-scale studies that need a precise value of time, would be to replicate our activities and associated analysis.³⁸ Interventions that are likely to substantially increase or decrease family labor supply are the most likely to meet this criterion. If the study is large enough, adding a replication of our method may have a relatively low marginal cost. This does present some challenges—it requires scheduling workdays and transporting workers to and from work sites—so conducting this exercise within a subset of participants may be optimal.

External Validity. The main limitation of our two simple approaches is external validity: factors that keep wages above the value of time are likely to be context specific. For example, because our estimates are local to the season in which our activities took place—in this case, the end of sowing season—we cannot rule out that labor is increasingly rationed during lean seasons, as in Breza et al. (2021). Nevertheless, we observe a striking robustness in the relative value of time across subgroups in our data, lending some credibility to a rule of thumb approach, especially in similar environments.³⁹ We recommend that researchers applying the 60% rule of thumb also present bounds on estimated impacts if working in a dissimilar environment.

Researchers who are concerned that the degree of labor rationing may be different in their setting can consider adjusting or bounding our rule of thumb. Doing so would require

³⁸Unincentivized choices are likely to be seen as an attractive alternative, but should be used with extreme caution. In particular, unincentivized survey-based measures modeled on our choices are likely to produce unreliable results. In our sample, farmers’ reservation wages elicited through an unincentivized survey question are significantly higher than the incentivized reservation wage m^{RW} —although the incentivized and unincentivized quantities are highly correlated, as described in Section 3.4.3.

³⁹Beyond the subgroup analysis presented in Table 3.3, we find relatively little variation in the relative value of time across villages: the 25th, 50th, and 75th percentiles of the village-level averages are 0.52, 0.59, and 0.73, respectively. The minimum and maximum are 0.39 and 0.77.

a measure of labor rationing in the new setting. In Appendix C.8.1, we show that a proxy can be computed from two survey questions: each worker’s recent market wage, and their potential wage if they were to seek work tomorrow. This proxy is strongly correlated with the individual-level measure of labor rationing λ_i (the Lagrangian on the labor constraint) identified by our model. In Appendix Figure C.8.1, we offer rules of thumb specific to bands of this proxy, which researchers can use depending on which band appears to best represent their setting. Additionally, for researchers anticipating non-uniform labor supply responses to an intervention—which may be correlated with SVT—we recommend using an unincentivized measure to capture the relevant heterogeneity, and adjusting the rule of thumb as described in Appendix C.8.2.

Identification in Other Settings. Researchers setting up similar choices to those used in this paper, in order to produce their own estimate of SVT, will need to impose Assumption 2. However, SVT can be estimated without Assumption 3, if the researcher has a proxy for the logarithm of V_h/V_m . A hypothetical question about willingness to travel for cash is easy to measure, and appears, in our data, to serve as a good proxy. In cases where estimates of relative intensities γ are not stable across subgroups, the researcher could opt to estimate our model separately within groups of economically similar farmers.

Variation in the Cost of Time Across Settings. The opportunity cost of time for a given worker is likely to vary across tasks and periods of time. When benchmarking the value of time against a market wage—or when designing a task to serve as a benchmark—researchers should choose benchmarks that are comparable to the labor changes induced by their intervention. For example, workers are likely to require higher wages to work on a fixed schedule than on a flexible one: the market wage for flexible casual work would thus be too low of a benchmark for a technology that requires labor input at a specific hour every day. Because the task used in this study was typical of the casual jobs commonly performed in settings like ours, our measure of the SVT is likely appropriate for a broad set of activities in similar environments. Relatedly, technologies leading to large changes in daily time use would need to be handled with care. A researcher may need to elicit marginal values for different lengths of the work day. In these cases, our rule of thumb cost may be useful as a lower bound when an intervention increases workload, or an upper bound when an intervention leads to decreases in workload.

3.6.4 Applying Our Results to the Literature

Finally, we apply our bounding and rule of thumb strategies to prior studies. We calculate treatment impacts under four values of time of the self-employed: 0%, 40%, 60%, and 100%

of the market wage. Figure 3.5 shows results for six studies selected for their illustrative value. Results for the full set of studies that we could reevaluate are shown in Table C.7.1. To standardize outcome measures across studies, we report treatment effects on profits, normalized by mean profits in the control group. Note that most of these papers treat the value of time conservatively: valuing it at zero for time-saving interventions, and w for those that increase time use.

Impact assessments are most sensitive to assumptions about the value of time when the intervention significantly changes participants' labor. A few examples are Jones et al. (2022), which estimates the impact of irrigation by small-scale farmers; Baird et al. (2016), which finds long-run labor supply effects of de-worming; and Karlan et al. (2014), which studies the introduction of rainfall index insurance. In each case, treatment effect estimates vary dramatically depending on the assumed value of time. In particular, for Jones et al. (2022), as the authors themselves point out, impacts are negative when valuing time at the market wage, but very large when the labor is valued at zero. A similar pattern can be seen in Baird et al. (2016).

For interventions producing modest changes in labor supply, the assumed value of time remains important, though its effects are less dramatic. Two examples are de Mel et al. (2019), which subsidizes paid employees of micro-enterprises, and Fink et al. (2020), which subsidizes loans to farmers during the lean season. In each study, estimated treatment effects are positive when valuing time using our rule of thumb of 60% of the market wage, but negative when valuing time at the market wage. For de Mel et al. (2019), estimated treatment effects are statistically significant using the authors' assumed value of time of 0, but statistically insignificant when time is valued at 60% of the market wage.

For interventions that do not meaningfully change labor supply, the assumed value of time of the self-employed is less important when calculating treatment impacts, even when labor represents a large share of costs. For example, in Schilbach (2019), the increase in household labor associated with the sobriety incentives is small (0.4%). Consequently, the normalized change in profits varies from 2.6% when household labor is valued at zero, to 2.0% when household labor is valued at the market wage. Note, however, that valuing time appropriately is still likely to be important for researchers measuring profit levels.

Finally, for labor saving technologies, using a more reliable value of time can increase their apparent efficacy. For example, Ahmed et al. (2021) studies the introduction of genetically modified eggplant in Bangladesh, which reduces the amount of time farmers spend weeding and applying pesticides. Note that profit estimates for this study, in Figure 3.5, are in reverse order—highest when time is most highly valued. In particular, valuing time at zero leads to an estimate that is too low, as it fails to account for the saved farmer labor. This highlights a general point: relative to more appropriate assumptions about the value of time, valuing participants' time at zero overestimates the efficacy of interventions that

Figure 3.5: Sensitivity of Estimated Profit Impacts to the Assumed Value of Time



Diamonds represent the value of time assumed by the authors. Note the jump in the x-axis.

increase participants' time use, and underestimates the efficacy of those that save time.

3.6.5 Conclusion

Consistent with researchers often focusing on yield or revenue maximization rather than costs, reviews of technology adoption in low-income countries indicate there has been little study of labor-saving technologies (de Janvry et al., 2017; Magruder, 2018; Macours, 2019). The failure to properly account for labor—often a primary cost—may explain adoption failures for some technologies that appear welfare-improving. Further, technologies that could improve welfare by saving users' time may appear less useful in evaluations, and thus may not be deployed by development agencies.

Under the principle that we only value what we measure, accounting for the labor of self-employed workers may help redirect efforts to improve the lives of the poor in novel and useful ways. There are many channels by which labor-saving technologies can improve welfare: increased leisure (Devoto et al., 2012); increased female labor participation (Albanesi and Olivetti, 2016); increased school participation;⁴⁰ improved mental health (Whillans and West, 2021); improved cognitive capability (Bessone et al., 2021); reduced pain (Xiao et al., 2013), and reduced pain management through alcohol (Schilbach, 2019).

⁴⁰Pinker (2018, p. 231) cites this tractor advertisement from 1921: “By investing in a Case Tractor and Ground Detour Plow and Harrow outfit now, your boy can get his schooling without interruption, and the Spring work will not suffer by his absence. Keep the boy in school—and let a Case Kerosene Tractor take his place in the field. You’ll never regret either investment.”

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Appendices

Appendix A

Housing and Human Capital: Condominiums in Ethiopia – Appendix

A.1 Tables and Figures

Table A.1.1: Condominium Openings

	Total	Studio	1 BR	2 BR	3 BR
2006	18972	4087	5677	8091	1117
2007	15031	2592	5070	6263	1106
2009	11005	2965	3679	3626	735
2010	25775	5882	11459	6131	2303
2011	9981	1255	4457	2742	1527
2012	7300	2952	3594	433	321
2013	4991	-	-	-	-
2015	31178	6573	14293	6695	3617
2016	11695	2103	5392	2723	1477
2018	2602	246	1041	123	1192
2019	32653	1248	18823	7127	5455
Total	171183	29903	73485	43954	18850

Figure A.1.1: Condominium Openings

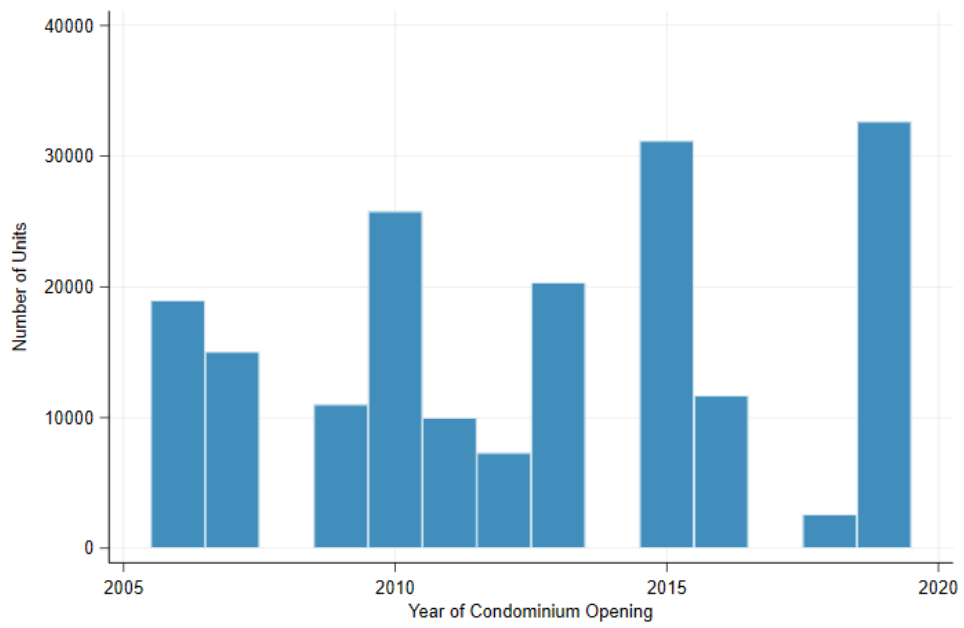


Figure A.1.2: Neighborhood Quality

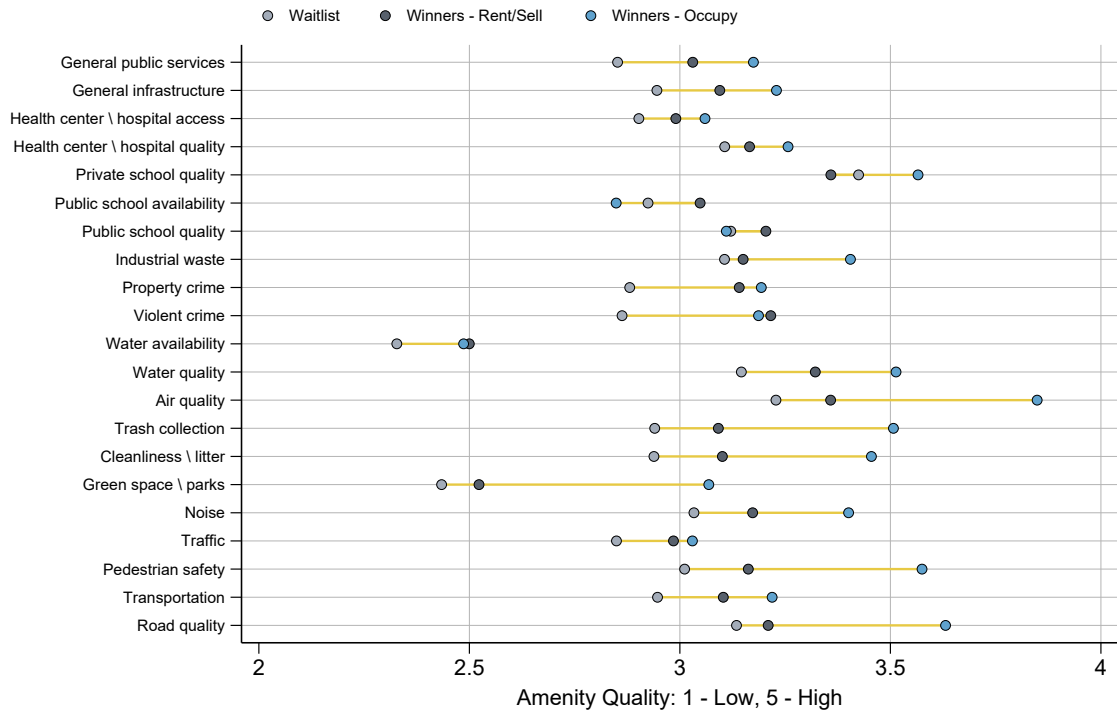


Figure A.1.3: Neighborhood Distance

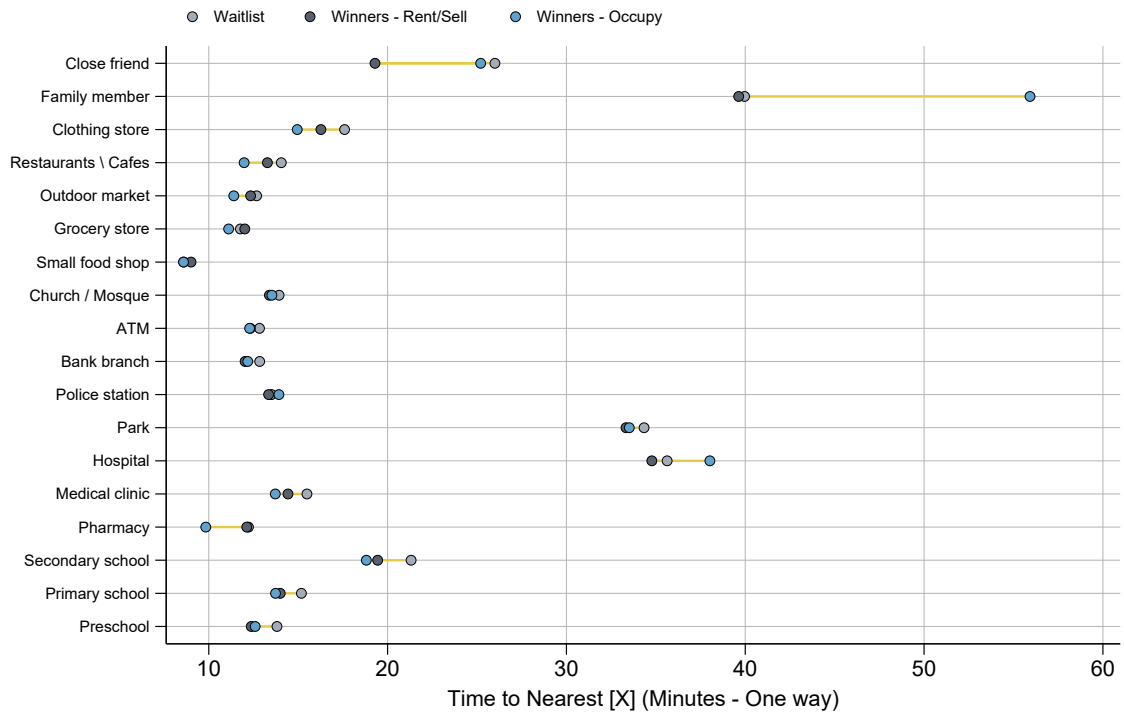


Table A.1.2: Neighborhood School Quality and Density

	Avg School Quality		Num Primary Schools		Num Secondary Schools		Num Public Schools		Num Private Schools	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1(Won Condo)	-0.008*** (0.003)	-0.002 (0.003)	1.322*** (0.344)	0.567 (0.409)	0.080 (0.128)	0.281* (0.164)	0.074 (0.085)	0.043 (0.112)	1.266*** (0.399)	0.716 (0.492)
1(Occupy)		0.010 (0.007)		0.218 (0.580)		-0.504*** (0.192)		-0.242* (0.145)		-0.083 (0.709)
Winner X Occupy		-0.022*** (0.008)		1.717** (0.744)		-0.113 (0.263)		0.264 (0.178)		1.441 (0.903)
Constant	0.637*** (0.011)	0.634*** (0.011)	6.771*** (1.296)	7.139*** (1.257)	1.955*** (0.480)	1.885*** (0.474)	2.707*** (0.321)	2.738*** (0.321)	5.874*** (1.443)	6.158*** (1.442)
N	2234	2234	2234	2234	2234	2234	2234	2234	2234	2234
Wait/Non-Dwell Mean	0.648	0.645	6.574	6.803	1.685	1.831	2.636	2.627	5.631	6.000
Sampl Weights	X	X	X	X	X	X	X	X	X	X
HHH Controls	X	X	X	X	X	X	X	X	X	X

Table A.1.3: Neighborhood Spending Per Capita

	Education Spending Pc		Women/Children Spending Pc		Health Spending Pc	
	(1)	(2)	(3)	(4)	(5)	(6)
1(Won Condo)	-8.002*** (1.333)	-4.623*** (1.251)	-7.899*** (1.340)	-4.439*** (1.329)	-4.501*** (1.043)	-2.880*** (0.978)
1(Occupy)		-4.691 (3.698)		-4.808 (3.117)		-1.911 (2.128)
Winner X Occupy		-4.806 (3.940)		-4.918 (3.486)		-2.569 (2.431)
Constant	34.671*** (4.458)	33.254*** (4.185)	32.490*** (4.889)	31.040*** (4.504)	19.964*** (3.595)	19.263*** (3.547)
N	2234	2234	2234	2234	2234	2234
Wait/Non-Dwell Mean	35.271	34.993	31.664	31.569	20.824	20.691
Samp Weights	X	X	X	X	X	X
HHH Controls	X	X	X	X	X	X

Figure A.1.4: Variation in Neighborhood Characteristics

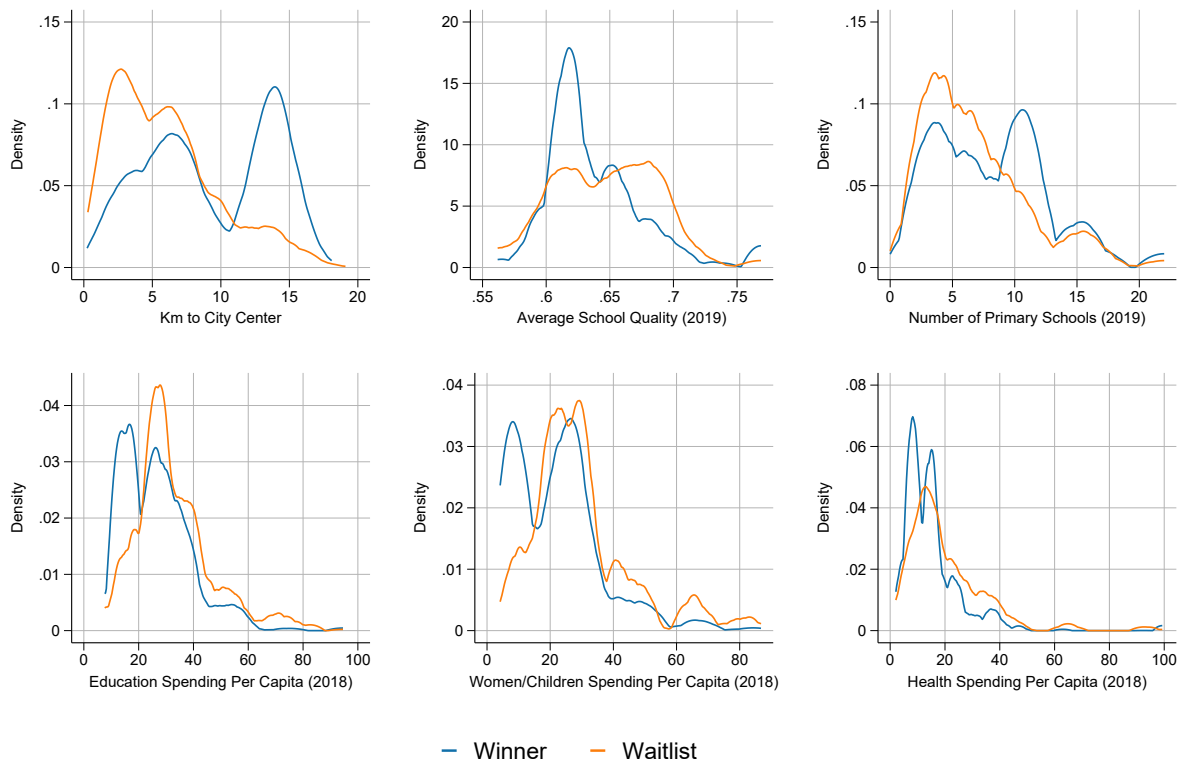


Figure A.1.5: Educational Attainment

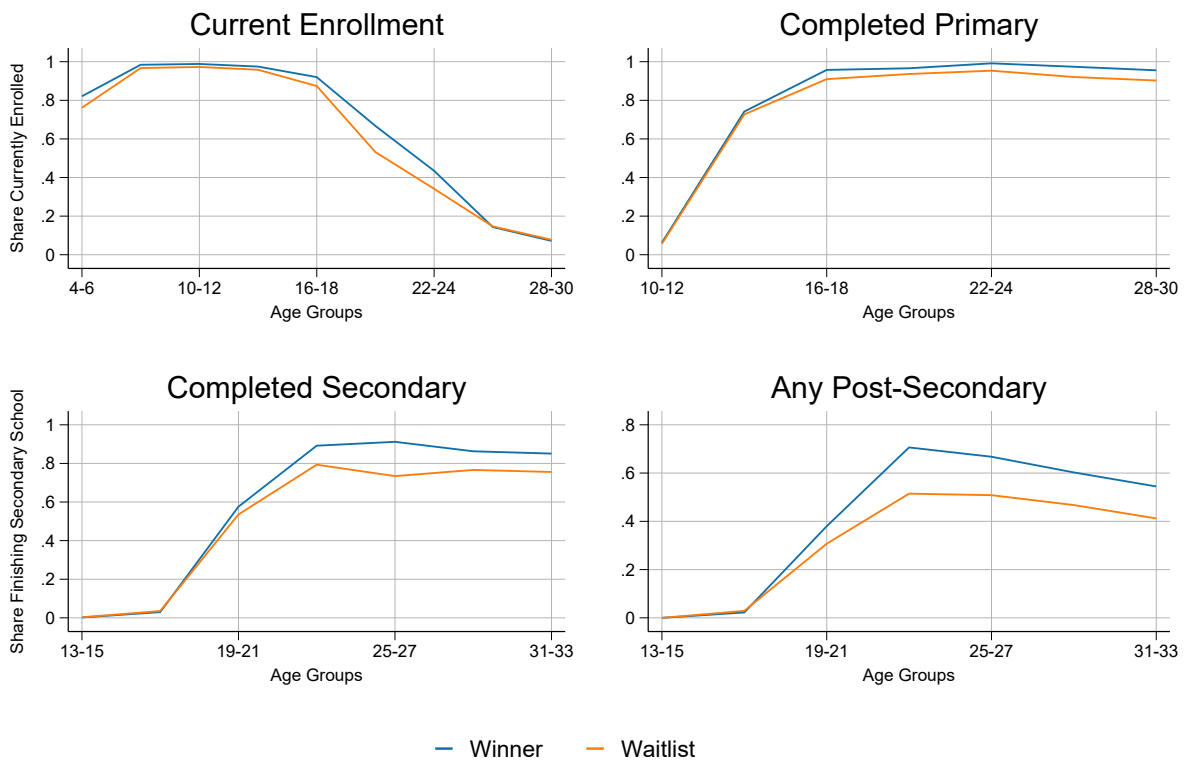


Figure A.1.6: Years of Childhood Exposure

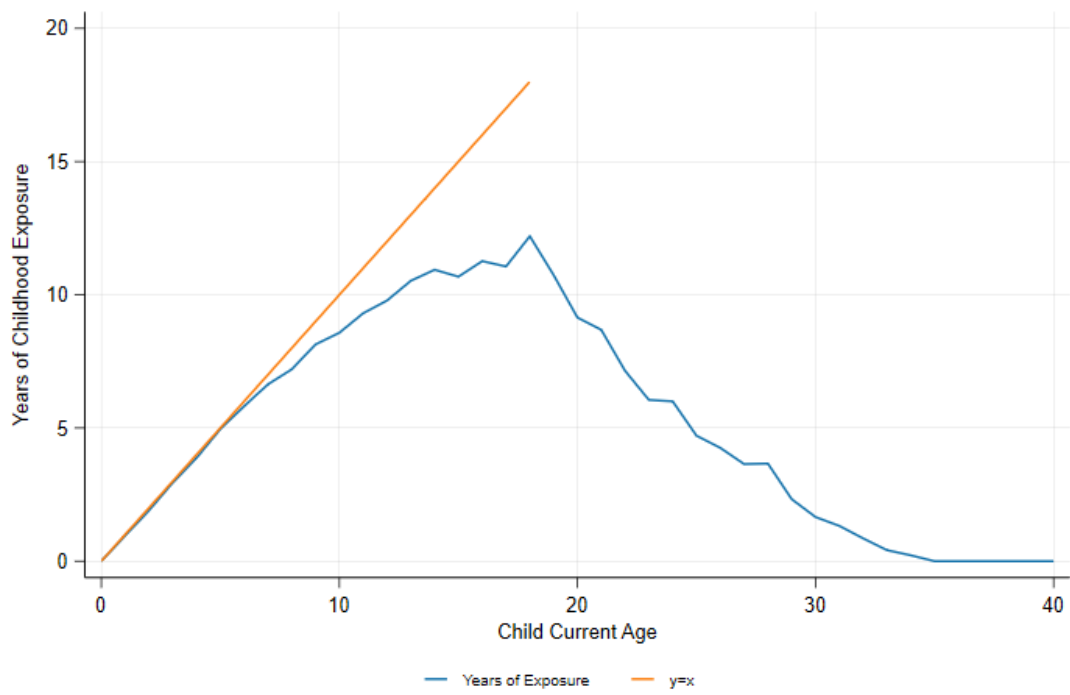


Table A.1.4: Cognitive Tests - Components

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Math	Num Patterns	Counting	Math Index	Writing	Sentences	Words	Letters	Literacy Index
1(Won Lottery)	0.812** (0.394)	-0.109 (0.326)	0.213 (0.281)	0.331 (0.364)	-0.002 (0.336)	0.240 (0.263)	0.319 (0.208)	0.069 (0.150)	0.164 (0.385)
1(Male)	0.741** (0.309)	0.272 (0.274)	0.282 (0.258)	0.609** (0.302)	0.521* (0.289)	0.272 (0.251)	0.349 (0.216)	0.316* (0.178)	0.522 (0.349)
Constant	2.391*** (0.422)	1.933*** (0.294)	4.890*** (0.309)	-0.049 (0.337)	1.792*** (0.336)	1.644*** (0.326)	2.115*** (0.244)	2.324*** (0.239)	-0.780* (0.432)
N	127	127	127	127	127	127	127	127	127
Waitlist Mean	2.513	1.600	5.350	-0.261	2.100	2.050	2.406	2.688	-0.303
Birth Cohort FEs	X	X	X	X	X	X	X	X	X
Sample	6-12	6-12	6-12	6-12	6-12	6-12	6-12	6-12	6-12

Table A.1.5: Child Educational Aspirations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	School Satis	School Dissat	Aspire Adv Deg	Ed Aspir Likely	Read Better than most	Math Better than most	English Better than most	Science Better than most
1(Won Lottery)	-0.238 (0.346)	-0.005 (0.233)	0.101 (0.107)	-0.350** (0.143)	0.373*** (0.123)	0.329* (0.186)	0.340*** (0.117)	0.393*** (0.192)
1(Male)	-0.162 (0.283)	0.491** (0.204)	-0.347*** (0.112)	-0.016 (0.123)	-0.203 (0.142)	0.007 (0.166)	-0.358*** (0.107)	-0.068 (0.167)
Constant	3.631*** (0.385)	0.989*** (0.231)	0.748*** (0.130)	1.143*** (0.128)	0.496*** (0.172)	0.161 (0.187)	0.541*** (0.115)	0.209 (0.215)
N	225	220	98	98	98	98	98	98
Waitlist Mean	2.545	1.058	0.762	0.778	0.300	0.253	0.237	0.170
Birth Cohort FEs	X	X	X	X	X	X	X	X

Table A.1.6: Child Occupational Aspirations

	(1)	(2)	(3)	(4)	(5)
	Aspire Adv Occ	Occ Aspir Likely	Aspir Occ Edu Constraint	Apsir Occ Fam Constraint	Likely Adv Occ
1(Won Lottery)	0.115 (0.078)	0.022 (0.060)	-0.241** (0.107)	-0.064 (0.108)	0.083 (0.098)
1(Male)	-0.183** (0.071)	-0.040 (0.057)	0.347*** (0.110)	-0.083 (0.110)	-0.035 (0.101)
Constant	0.827*** (0.075)	0.945*** (0.052)	0.216 (0.130)	0.256** (0.112)	0.787*** (0.083)
N	225	98	98	98	98
Waitlist Mean	0.741	0.857	0.571	0.190	0.794
Birth Cohort FEs	X	X	X	X	X

Table A.1.7: Children's Well-Being

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Ladder Current	Ladder Future	1(Future Better)	Future Diff	Nhood Satis	Nhood Dissat	Num Friends	Num Friends School	1(Treated Well)
1(Won Lottery)	-0.485 (0.470)	0.132 (0.631)	0.187** (0.092)	0.617 (0.647)	0.178 (0.403)	-0.292** (0.124)	-0.077 (0.737)	1.520 (1.595)	0.102 (0.088)
1(Male)	0.044 (0.431)	-0.125 (0.637)	-0.137 (0.110)	-0.169 (0.711)	0.007 (0.292)	-0.118 (0.117)	2.914*** (0.672)	3.012** (1.383)	0.027 (0.061)
Constant	4.989*** (0.423)	7.511*** (0.752)	0.694*** (0.144)	2.521*** (0.863)	2.540*** (0.450)	0.806*** (0.146)	2.322*** (0.713)	3.755*** (1.102)	0.822*** (0.061)
N	97	97	98	97	225	225	225	225	225
Waitlist Mean	4.903	8.194	0.902	3.290	2.042	0.811	4.281	4.536	0.776
Birth Cohort FEs	X	X	X	X	X	X	X	X	X

Table A.1.8: Household Assets

	(1) As Index 1	(2) Com Bldgs	(3) Houses	(4) Apts	(5) Ag Land	(6) As Index 2
1(Won Condo)	0.398*** (0.109)	0.015* (0.008)	0.055** (0.026)	0.300*** (0.028)	-0.005 (0.009)	0.420*** (0.109)
Constant	-1.054*** (0.357)	-0.055 (0.040)	0.113 (0.147)	0.120 (0.123)	-0.033 (0.031)	-1.059*** (0.357)
N	2269	2269	2269	2267	2269	2267
Waitlist Mean	-0.527	0.000	0.004	0.004	0.019	-0.544
Samp Weights	X	X	X	X	X	X
HHH Controls	X	X	X	X	X	X

Table A.1.9: House Quality

	(1) Imp Floor	(2) Iron Roof	(3) Imp Walls	(4) Qual Index 1	(5) Area PP	(6) Qual Index 2
1(Won Condo)	0.083*** (0.023)	-0.103*** (0.032)	0.285*** (0.035)	1.051*** (0.137)	3.834*** (0.691)	1.219*** (0.142)
Constant	0.742*** (0.080)	0.866*** (0.132)	0.197 (0.138)	-1.972*** (0.519)	3.755 (2.430)	-2.099*** (0.522)
N	2269	2269	2269	2269	2269	2269
Waitlist Mean	0.784	0.831	0.275	-0.914	8.196	-1.042
Samp Weights	X	X	X	X	X	X
HHH Controls	X	X	X	X	X	X

Table A.1.10: House Value

	(1)	(2)	(3)	(4)	(5)	(6)
	Ln(Rent) Sim	Ln(Rent) Est	Ln(Rent)	Rent Val All	Ln(Sale) Sim	Ln(Sale) Est
1(Won Condo)	0.494*** (0.088)	0.324 (0.197)	1.294*** (0.163)	1.672*** (0.135)	-0.510* (0.290)	0.383 (0.566)
Constant	7.445*** (0.250)	8.339*** (0.452)	8.576*** (0.493)	7.583*** (0.401)	14.140*** (1.360)	11.329*** (2.158)
N	703	359	1268	1627	235	390
Waitlist Mean	7.909	8.833	6.310	6.365	15.015	14.512
Samp Weights	X	X	X	X	X	X
HHH Controls	X	X	X	X	X	X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ **Table A.1.11: Household Head Employment**

	(1)	(2)	(3)	(4)	(5)
	Any Work	Formal Emp	Self Emp	Casual Emp	Unemployed
1(Won Condo)	-0.009 (0.026)	0.086*** (0.031)	-0.047 (0.029)	-0.044*** (0.014)	-0.012 (0.022)
Constant	1.147*** (0.120)	0.319** (0.129)	0.567*** (0.102)	0.270*** (0.062)	0.190** (0.096)
N	2267	2267	2267	2267	2267
Waitlist Mean	0.813	0.419	0.297	0.083	0.117
Samp Weights	X	X	X	X	X
HHH Controls	X	X	X	X	X

Table A.1.12: Household Income

	(1)	(2)	(3)
	HHH Tot Inc	HHH + Spouse Tot Inc	HH Tot Inc Pc
1(Won Condo)	0.595*** (0.226)	0.747*** (0.238)	0.794*** (0.215)
Constant	4.193*** (0.899)	4.433*** (0.996)	3.670*** (0.879)
N	2265	2269	2269
Waitlist Mean	6.824	6.783	6.320
Samp Weights	X	X	X
HHH Controls	X	X	X

Table A.1.13: Children's Education - Sibling Design

	(1)	(2)	(3)	(4)	(5)	(6)
	1(Enrolled)	1(Enrolled)	Primary	Secondary	Post-Sec Att	Post-Sec Att
Exposure (Years)	0.011* (0.007)	0.018* (0.010)	-0.005 (0.008)	-0.020 (0.020)	0.007 (0.021)	0.003 (0.016)
1(Male)	-0.024 (0.021)	-0.041 (0.036)	0.040 (0.029)	0.060 (0.039)	-0.055 (0.063)	-0.058 (0.051)
Constant	0.701*** (0.028)	0.485*** (0.039)	0.911*** (0.025)	0.733*** (0.051)	0.494*** (0.055)	0.505*** (0.038)
N	3892	1858	1471	1200	1200	1571
No Exposure Mean	0.639	0.387	0.916	0.718	0.500	0.490
HH FEs	X	X	X	X	X	X
Birth Cohort FEs	X	X	X	X	X	X
Sample	5-30	14-30	14-30	18-30	18-30	18-35

A.2 Sampling

A.2.1 Site Sampling

To sample winning households, we used a two-step sampling procedure. In the first step, we sampled condominium sites. A condominium site is the physical location of the condominium and the year of opening dual: Since some locations were opened in a staggered fashion over the course of years, each of those openings would be considered a separate “site” in our sampling. This means that site locations could be sampled multiple times.

The cleaning algorithm for the site sampling is as follows:

1. Exclude condominium for which household contact data is always missing (round 5, 8, 9).
2. Exclude lottery round 13 which took place too recently for many impacts to be observed.
3. Combine physical proximate sites that opened in the same year if one of the sites had fewer than 50 new condominiums dispersed.
4. Keep only sites that had at least 80 new units dispersed in a given location-year (~ 4 condominium blocks).
5. Within each condominium round (year), split sites at the median based on size of new condominiums dispersed.
6. Sample an approximately equal number of sites above and below the median in each condominium round, with a larger share of sampled sites drawn from earlier condominium rounds.

Site Re-sampling – Due to political instability in Ethiopia, some sites were unsafe for our enumerators to visit or had been repurposed by the government during the state of emergency that coincided with our fieldwork. In these instances, we drew new sites from the same site strata (round-median size) to replace the inaccessible sites.

A.2.2 Household Sampling

In the second sampling step, we sampled the winning households within selected sites. These households were drawn from the administrative data received from the AAHDA and supplemented with updated phone numbers collected by our team from condominium areas

and local public officials. All winning households were included in the administrative data, irrespective of whether they decided to move into their unit after winning.

The algorithm used for sampling households from the administrative data was as follows:

1. Keep only households that had a valid phone number in the data (90%).
2. Exclude 3-bedroom winners, as nearly all 3-bedroom applicants during the first lottery registration period have been given a unit.
3. Target 50 sampled households for each site, drawing proportionally across strata (registration subcity \times house type \times household head gender) within site.
4. Generate a randomly ordered list of backups within the strata and site.

In Table A.2.1, comparing columns (1) and (2) we see that our sample of winners and waitlist households is balanced along observable baseline household characteristics. The imbalance between the sample of winners in column (1) and the full waitlist in column (3) is due to the sampling algorithm employed by the AAHDA that differentially selected female-headed households and reflects the fact that units (e.g. 1- or 2-bedroom) were not built proportional to the number of applicants for that unit type. That is, relatively more studios were built than units of one and two bedrooms.

A.3 Attrition

In Table A.3.1 we see that across both winning households and waitlist households, we were approximately 5pp more likely to contact male-headed households, 6pp less likely to contact 2-bedroom applicants, and 6pp less likely to contact lottery winners. There were minimal differences in contact rates based on households subcity at the time of their application.

In column (6) we see that conditional on being contacted, lottery winners were approximately 3pp less likely to be eligible for the survey which required that they be living in Addis Ababa and have a child less than 35 years old.

A.4 Soft-Skills and Cognitive Tests

Strengths & Difficulties We administer the Strengths and Difficulties Questionnaire (SDQ) to parents for a randomly selected 50% sample of their children. The SDQ was developed is a behavioral screening tool, developed by child psychologists (Goodman, 1997), about 2-17 year olds. It can be administered directly to children, to their parents, or to

Table A.2.1: Sample Balance - Admin Data

	(1) Winner Samp	(2) Waitlist Samp	(3) Full Waitlist	(4) T-test (1)-(2)	(5) T-test (1)-(3)
Share Female	0.51 (0.50)	0.50 (0.50)	0.21 (0.41)	0.52	0.00***
Reg: Num BR	1.76 (0.43)	1.75 (0.43)	1.86 (0.34)	0.83	0.00***
Reg: Studio	0.24 (0.43)	0.25 (0.43)	0.14 (0.34)	0.83	0.00***
Reg: 1BR	0.39 (0.49)	0.39 (0.49)	0.47 (0.50)	0.88	0.00***
Reg: 2BR	0.36 (0.48)	0.36 (0.48)	0.40 (0.49)	0.96	0.01***
Reg: 3BR	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	.	.
Reg: SC 1	0.04 (0.19)	0.03 (0.18)	0.03 (0.16)	0.64	0.07*
Reg: SC 2	0.11 (0.31)	0.11 (0.31)	0.10 (0.31)	0.77	0.77
Reg: SC 3	0.09 (0.29)	0.08 (0.28)	0.09 (0.29)	0.49	0.72
Reg: SC 4	0.07 (0.25)	0.07 (0.26)	0.08 (0.27)	0.81	0.18
Reg: SC 5	0.13 (0.34)	0.13 (0.33)	0.13 (0.33)	0.80	0.73
Reg: SC 6	0.13 (0.34)	0.16 (0.36)	0.16 (0.37)	0.07*	0.00***
Reg: SC 7	0.11 (0.32)	0.10 (0.30)	0.10 (0.31)	0.35	0.38
Reg: SC 8	0.11 (0.31)	0.12 (0.33)	0.12 (0.33)	0.22	0.11
Reg: SC 9	0.11 (0.32)	0.10 (0.30)	0.09 (0.29)	0.14	0.01***
Reg: SC 10	0.09 (0.29)	0.09 (0.29)	0.09 (0.28)	0.74	0.31
Observations	1648	1500	47710	3148	49358

their teachers. Given the difficulty tracking children in our context, we chose to administer the questionnaire to parents. The survey has been globally validated, including in Ethiopia (Hoosen et al., 2018; Mekonnen et al., 2020).

The questionnaire consists of 25 questions about children's attributes, some positive and some negative, that are grouped into 5 indices:

1. Emotional symptoms
2. Conduct problems
3. Hyperactivity / Inattention
4. Peer relationship problems
5. Prosocial behavior

Respondents answer using a 3-step Likert scale (Not True/Somewhat True/Certainly True). The questionnaires are scored using a standard scoring methodology. "Somewhat True" is always scored as 1, but the scoring of "Not True" and "Certainly True" varies with the item, with higher scores indicating more behavioral problems.¹

The SDQ questions are included below.

- Considerate of other people's feelings
- Restless, overactive, cannot stay still for long
- Often complains of headaches, stomach-aches or sickness
- Shares readily with other children, for example toys, treats, pencils
- Often loses temper
- Rather solitary, prefers to play alone
- Generally well behaved, usually does what adults request
- Many worries or often seems worried
- Helpful if someone is hurt, upset or feeling ill
- Constantly fidgeting or squirming
- Has at least one good friend

¹In our analysis we flip the scale such that a higher score is associated with fewer behavioral problems to ease interpretation.

- Often fights with other children or bullies them
- Often unhappy, depressed or tearful
- Generally liked by other children
- Easily distracted, concentration wanders
- Nervous or clingy in new situations, easily loses confidence
- Kind to younger children
- Often lies or cheats
- Picked on or bullied by other children
- Often offers to help others (parents, teachers, other children)
- Thinks things out before acting
- Steals from home, school or elsewhere
- Gets along better with adults than with other children
- Many fears, easily scared
- Good attention span, sees work through to the end

Stroop Test To children aged 13 - 17, we administer a version of the numerical Stroop Test, previously validated in Ethiopia and adapted from Abebe et al. (2021). First proposed in Mani et al. (2013), our enumerator shows a string of digits to the respondent (e.g., 2222) and the respondent is asked to report the number of digits shown. For the string '2222', the correct response would be '4'. Respondents are shown 20 strings in total, and their performance is measured by the number of correct responses and the total time required to answer.

Raven's Matrices Again following the Ethiopian validation of Abebe et al. (2021), we administer Raven's matrices to children aged 6-17 (Raven, 2000). Respondents are given instructions on the test, which consists of pattern matching, and are administered 12 matrices. Performance is based on the respondent's number of errors.

A.5 Additional Household Outcomes

Table A.3.1: Contact & Eligibility

	Contacted			Eligible		
	(1)	(2)	(3)	(4)	(5)	(6)
HH Male	0.054*** (4.17)	0.047*** (3.68)	0.048*** (3.81)	-0.007 (-0.58)	-0.007 (-0.56)	-0.006 (-0.49)
1 Bed Room	0.000 (0.01)	-0.000 (-0.02)	0.001 (0.09)	-0.008 (-0.53)	-0.008 (-0.53)	-0.007 (-0.48)
2 Bed Room	-0.094*** (-5.49)	-0.056*** (-3.26)	-0.063*** (-3.67)	-0.017 (-0.97)	-0.019 (-1.12)	-0.023 (-1.33)
SC2	0.029 (0.77)	0.032 (0.85)	0.021 (0.57)	-0.097*** (-3.35)	-0.097*** (-3.36)	-0.102*** (-3.53)
SC3	-0.056 (-1.51)	-0.007 (-0.19)	-0.009 (-0.25)	-0.106*** (-3.65)	-0.109*** (-3.71)	-0.113*** (-3.82)
SC4	-0.003 (-0.07)	0.035 (0.92)	0.034 (0.88)	-0.064** (-2.24)	-0.067** (-2.30)	-0.070** (-2.40)
SC5	-0.049 (-1.29)	-0.047 (-1.24)	-0.056 (-1.47)	-0.090*** (-3.12)	-0.090*** (-3.12)	-0.095*** (-3.29)
SC6	-0.002 (-0.07)	-0.004 (-0.10)	-0.013 (-0.37)	-0.109*** (-3.99)	-0.109*** (-3.99)	-0.115*** (-4.19)
SC7	-0.024 (-0.66)	0.028 (0.77)	0.020 (0.55)	-0.101*** (-3.52)	-0.104*** (-3.64)	-0.110*** (-3.82)
SC8	-0.068* (-1.84)	-0.027 (-0.74)	-0.036 (-0.97)	-0.056** (-2.09)	-0.059** (-2.16)	-0.066** (-2.39)
SC9	0.000 (0.00)	0.004 (0.10)	0.001 (0.01)	-0.078*** (-2.76)	-0.079*** (-2.77)	-0.082*** (-2.88)
SC10	0.001 (0.02)	0.003 (0.08)	-0.005 (-0.12)	-0.091*** (-3.12)	-0.091*** (-3.13)	-0.096*** (-3.30)
Landline		-0.608*** (-15.43)	-0.584*** (-14.77)		0.170*** (7.12)	0.162*** (6.71)
Early Cell		-0.263*** (-10.95)	-0.227*** (-8.94)		0.028 (0.98)	0.045 (1.53)
Lotto Winner			-0.060*** (-4.45)			-0.027** (-2.00)
Constant	0.654*** (19.14)	0.651*** (19.04)	0.686*** (19.52)	0.953*** (40.38)	0.954*** (40.33)	0.971*** (38.99)
N	5657	5657	5657	2977	2977	2977
DV Mean	0.593	0.593	0.593	0.859	0.859	0.859

Table A.5.1: Household Employment - Exposure

	(1)	(2)	(3)	(4)	(5)
	Any Work	Formal Emp	Self Emp	Casual Emp	Unemployed
1(Won Condo)	-0.044 (0.057)	-0.067 (0.069)	0.143** (0.072)	-0.075*** (0.017)	-0.037 (0.036)
Yrs Since Lotto	0.005 (0.008)	0.020** (0.009)	-0.025*** (0.010)	0.004 (0.003)	0.003 (0.005)
Constant	1.144*** (0.121)	0.307** (0.130)	0.583*** (0.102)	0.267*** (0.061)	0.188** (0.096)
N	2267	2267	2267	2267	2267
Waitlist Mean	0.782	0.411	0.271	0.085	0.141
Samp Weights	X	X	X	X	X
HHH Controls	X	X	X	X	X

Table A.5.2: Household Savings

	(1)	(2)	(3)	(4)	(5)
	1(Bank Acct)	Any Sav 12 Mo	Asinh(Sav 12 Mo)	Asinh(Tot Sav)	Sav Index
1(Won Condo)	-0.000 (0.011)	-0.038 (0.034)	-0.378 (0.334)	-1.116*** (0.387)	-0.219* (0.120)
Constant	1.015*** (0.033)	0.398*** (0.119)	4.306*** (1.164)	5.541*** (1.486)	0.516 (0.425)
N	2269	2269	2213	2213	2213
Waitlist Mean	0.983	0.315	2.872	5.109	0.088
Samp Weights	X	X	X	X	X
HHH Controls	X	X	X	X	X

Standard errors in parentheses

Index 1 is the first principal component from the other outcome variables in the table.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.5.3: Parental Aspirations

	Age Child Des Adv Degree Likely: Ed Aspir Stud Top Acad Des Adv Occ				Par Aspir Index			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1(Won Lottery)	0.031 (0.100)	0.040* (0.021)	0.031* (0.017)	0.053*** (0.017)	-0.043*** (0.016)	0.061 (0.047)	0.091 (0.057)	0.051 (0.053)
Child Age	0.050*** (0.005)	-0.005*** (0.001)	-0.005*** (0.001)	-0.003*** (0.001)	-0.005*** (0.001)	-0.017*** (0.002)	-0.019*** (0.003)	-0.021*** (0.004)
1(Male)	1.231*** (0.084)	-0.029** (0.013)	-0.024** (0.012)	-0.012 (0.012)	-0.060*** (0.011)	-0.131*** (0.029)		
Constant	25.807*** (0.167)	0.537*** (0.031)	0.730*** (0.025)	0.598*** (0.027)	0.902*** (0.021)	0.121* (0.064)	0.061 (0.082)	0.323*** (0.092)
N	5128	5128	5128	5128	5128	5128	2582	2546
Waitlist Mean	27.155	0.561	0.702	0.587	0.811	0.037	-0.018	0.092
Strata FEs	X	X	X	X	X	X	X	X
Sample	All	All	All	All	All	All	Male	Female

Appendix B

New Technology and Network Change: Experimental Evidence from Kenya - Appendix

B.1 Additional tables and figures

Table B.1.1: Attrition Regressions

Dependent Variable:	Missing Endline		
	(1)	(2)	(3)
Random	0.019 (0.022)		
Cash	0.062*** (0.022)		
Task	0.050** (0.022)		
Group Vote	0.035 (0.022)		
Winner		0.004 (0.022)	0.004 (0.021)
Aged 55 or more			-0.021 (0.015)
Female Headed Household			0.038 (0.023)
Male Yrs of Education			0.004* (0.002)
Female Yrs of Education			0.008*** (0.002)
Land Size (acres)			-0.007* (0.004)
Spending on ag inputs (1000Ksh)			0.000 (0.001)
Crop Income (1000Ksh)			-0.000 (0.000)
Other Income (1000Ksh)			0.000** (0.000)
Index of House Quality			0.087*** (0.026)
Currently Irrigates			0.014 (0.014)
Grows Vegetables			-0.043** (0.017)
Baseline WTP for pump (1000Ksh)			0.005 (0.003)
Geographic Centrality			-0.001 (0.002)
Participates in a Community Group			-0.075*** (0.018)
Constant	0.144*** (0.018)	0.181*** (0.006)	0.129*** (0.032)
N	4212	4212	4108
Mean of Dep Var	0.182	0.182	0.181
R-squared	0.006	0.059	0.085

The data are from all respondents from the 190 villages in Waves 1 and 2. The outcome variable is whether the respondent is missing at endline. Robust standard errors are included in parentheses. Column (1) includes strata fixed effects while columns (2) and (3) include cluster fixed effects. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table B.1.2: High-Value Characteristics

Network Stats Farm Help/ Advice, Directed	Mean [Std Dev]	Coefficient on “High Value” for:			Obs.
	in Random & Control Villages	Cash	Task	Vote	
Perceived Farming Knowledge	3.034 [0.548]	0.386 (0.049)***	0.074 (0.034)**	0.528 (0.038)***	4211
Out-degree	6.563 [5.465]	0.106 (0.44)	2.273 (0.424)***	1.164 (0.426)***	4212
In-degree	6.413 [3.574]	1.836 (0.301)***	1.132 (0.227)***	3.03 (0.257)***	4212
Eigenvector Centrality	0.198 [0.075]	0.037 (0.006)***	0.036 (0.006)***	0.054 (0.006)***	3694
Betweenness Centrality	17.981 [25.428]	10.803 (2.629)***	6.71 (1.634)***	13.662 (1.775)***	4212
Closeness Centrality	0.661 [0.118]	0.026 (0.009)***	0.06 (0.008)***	0.061 (0.008)***	4212
Household Characteristics					
Aged 55 or more	0.23 [0.421]	0.101 (0.045)**	-0.064 (0.034)*	-0.067 (0.028)**	4198
Female Headed Household	0.585 [0.493]	-0.06 (0.026)**	-0.016 (0.021)	-0.048 (0.022)**	4211
Male Yrs of Education	7.882 [3.398]	0.781 (0.246)***	-0.262 (0.252)	1.152 (0.227)***	4212
Female Yrs of Education	6.453 [3.573]	0.565 (0.396)	-0.005 (0.279)	0.982 (0.285)***	4212
Land Size (acres)	2.097 [2.55]	0.569 (0.221)**	0.14 (0.116)	0.363 (0.118)***	4203
Spending on ag inputs (1000Ksh)	6.637 [14.404]	4.288 (1.572)***	-0.01 (0.64)	4.111 (1.016)***	4206
Crop Income (1000Ksh)	16.29 [30.388]	9.764 (2.872)***	-0.759 (2.415)	14.605 (2.815)***	4212
Other Income (1000Ksh)	30.952 [65.485]	20.56 (9.106)**	-14.216 (6.584)**	7.561 (5.131)	4171
Index of House Quality	0.357 [0.28]	0.153 (0.031)***	-0.059 (0.022)***	0.073 (0.018)***	4171
Currently Irrigates	0.384 [0.487]	0.063 (0.039)	0.073 (0.031)**	0.122 (0.036)***	4212
Grows Vegetables	0.74 [0.439]	0.093 (0.027)***	0.042 (0.024)*	0.079 (0.022)***	4212
Baseline WTP for pump (1000Ksh)	2.561 [1.794]	0.56 (0.182)***	0.058 (0.12)	0.395 (0.123)***	4212
Geographic Centrality	1.33 [3.945]	0.118 (0.691)	0.48 (0.45)	0.104 (0.338)	4212
Participates in a Community Group	0.827 [0.37]	0.117 (0.024)***	0.05 (0.022)**	0.085 (0.029)***	4212

The data are from all respondents from the 190 villages in Waves 1 and 2. Each row represents a regression of the network or household characteristic on treatment-by-high value, treatment, and strata dummies. Standard errors are clustered at the village level. Standard deviations for the pooled control and random villages are in brackets. Standard errors are in parentheses. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table B.1.3: Winner Characteristics - All Waves

Household Characteristics	Mean	Coefficient on “Winner” for:			Obs.
	[Std Dev]	Cash	Task	Vote	
	in Random & Control Villages				
Aged 55 or more	0.286 [0.452]	0.039 (0.038)	-0.056 (0.034)	-0.016 (0.033)	8681
Female Headed Household	0.76 [0.427]	0.001 (0.014)	-0.01 (0.012)	-0.02 (0.014)	8893
Male Yrs of Education	8.268 [3.239]	0.172 (0.22)	-0.176 (0.21)	0.482 (0.254)*	8935
Female Yrs of Education	6.685 [3.505]	0.407 (0.243)*	-0.143 (0.232)	0.84 (0.256)***	8935
Land Size (acres)	2.15 [2.374]	0.401 (0.184)**	0.039 (0.109)	0.034 (0.179)	8686
Spending on ag inputs (1000Ksh)	6.327 [11.388]	0.782 (0.735)	-0.475 (0.508)	1.528 (0.66)**	8924
Crop Income (1000Ksh)	18.187 [32.216]	3.399 (2.692)	0.948 (2.813)	8.428 (3.344)**	5537
Other Income (1000Ksh)	37.433 [73.63]	15.885 (6.125)***	-4.145 (4.874)	1.247 (4.114)	8888
Index of House Quality	0.374 [0.261]	0.041 (0.02)**	-0.018 (0.018)	0.024 (0.02)	8535
Currently Irrigates	0.385 [0.487]	0.047 (0.037)	0.046 (0.039)	0.02 (0.04)	5540
Grows Vegetables	0.8 [0.4]	0.002 (0.023)	0.028 (0.024)	0.024 (0.02)	8935
Baseline WTP for pump (1000Ksh)	3.538 [4.401]	-0.498 (0.284)*	0.138 (0.357)	0.228 (0.383)	8935
Geographic Centrality	1.065 [3.513]	0.211 (0.365)	-0.023 (0.257)	-0.305 (0.108)***	8935
Participates in a Community Group	0.834 [0.36]	0.029 (0.027)	-0.025 (0.027)	0.058 (0.029)**	5542

The data are from all respondents from the 390 villages in Waves 1, 2, and 3. Each row represents a regression of the network or household characteristic on treatment-by-winner, treatment, and strata dummies. Standard errors are clustered at the village level. Standard deviations for the pooled control and random villages are in brackets. Standard errors are in parentheses. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table B.1.4: Undirected Network Statistics

Panel A. Baseline Data		Discuss Planting and/or Agriculture						Provided Info, Help, Advice w/ Agriculture						Know at All							
Network Type:		Random	Cash	Task	Vote	Control	Random	Cash	Task	Vote	Control	Random	Cash	Task	Vote	Control	Random	Cash	Task	Vote	Control
Clustering	.715 (.084)	.709 (.078)	.741 (.076)	.732 (.068)	.702 (.091)	.637 (.102)	.639 (.081)	.673 (.083)	.667 (.087)	.637 (.073)	.983 (.021)	.978 (.025)	.974 (.036)	.973 (.032)	.969 (.041)						
Closeness	.722 (.06)	.715 (.06)	.733 (.068)	.722 (.05)	.703 (.066)	.662 (.072)	.658 (.063)	.675 (.058)	.669 (.056)	.645 (.057)	.982 (.022)	.977 (.027)	.97 (.048)	.971 (.034)	.965 (.046)						
Betweenness	8.821 (3.565)	9.411 (3.053)	8.532 (3.175)	8.992 (2.664)	9.986 (3.912)	11.272 (4.29)	12.008 (3.695)	10.541 (3.354)	11.328 (3.064)	12.089 (3.533)	.457 (.619)	.582 (.725)	.761 (1.185)	.739 (1.003)	.968 (1.504)						
Density	.586 (.112)	.576 (.106)	.603 (.119)	.586 (.093)	.559 (.113)	.478 (.12)	.475 (.104)	.501 (.099)	.487 (.097)	.456 (.085)	.98 (.026)	.974 (.033)	.962 (.065)	.966 (.044)	.957 (.062)						
Degree	12.331 (3.326)	12.614 (2.679)	12.457 (2.916)	12.638 (2.848)	11.871 (2.667)	10.069 (3.141)	10.397 (2.642)	10.43 (2.673)	10.502 (2.675)	9.719 (2.274)	21.151 (4.373)	21.695 (3.07)	20.359 (4.351)	21.125 (3.499)	20.867 (3.484)						
Households	22.744 (4.483)	23.488 (3.158)	22.279 (4.205)	23.023 (3.535)	23.056 (3.539)	22.744 (4.483)	23.488 (3.158)	22.279 (4.205)	23.023 (3.535)	23.056 (3.539)	43 (4.483)	43 (3.158)	43 (4.205)	43 (3.535)	43 (3.539)						
Clusters	43	43	43	43	18	43	43	43	43	18	43	43	43	43	18						

Panel B. Endline Data

Network Type:		Discuss Planting and/or Agriculture						Provided Info, Help, Advice w/ Agriculture						Know at All							
Network Type:		Random	Cash	Task	Vote	Control	Random	Cash	Task	Vote	Control	Random	Cash	Task	Vote	Control	Random	Cash	Task	Vote	Control
Clustering	.662 (.132)	.667 (.115)	.667 (.148)	.664 (.086)	.663 (.128)	.572 (.122)	.577 (.159)	.582 (.168)	.529 (.125)	.545 (.155)	.964 (.041)	.96 (.04)	.967 (.038)	.976 (.028)	.942 (.056)						
Closeness	.682 (.087)	.682 (.078)	.699 (.09)	.685 (.064)	.673 (.101)	.61 (.09)	.613 (.116)	.634 (.1)	.606 (.078)	.595 (.121)	.963 (.045)	.958 (.045)	.964 (.044)	.975 (.028)	.94 (.055)						
Betweenness	8.219 (3.127)	8.806 (3.198)	7.742 (3.222)	8.581 (3.025)	9.25 (3.796)	10.8 (4.275)	10.675 (5.006)	10.113 (4.368)	11.464 (4.006)	11.546 (3.78)	.682 (.762)	.933 (1.193)	.676 (.825)	.503 (.607)	1.386 (1.338)						
Density	.528 (.13)	.519 (.13)	.552 (.143)	.523 (.107)	.512 (.154)	.414 (.119)	.426 (.144)	.451 (.144)	.401 (.101)	.399 (.152)	.955 (.061)	.949 (.06)	.956 (.058)	.971 (.034)	.927 (.071)						
Degree	10.303 (3.625)	9.982 (2.732)	10.101 (3.353)	10.057 (2.522)	10.054 (3.402)	7.892 (2.806)	8.082 (2.852)	8.203 (3.205)	7.678 (2.432)	7.672 (2.733)	19.896 (4.703)	20.049 (2.97)	19.388 (4.289)	20.374 (3.542)	19.314 (3.475)						
Households	22.744 (4.483)	23.465 (3.157)	22.233 (4.253)	23.023 (3.535)	22.944 (3.572)	22.744 (4.483)	23.465 (3.157)	22.233 (4.253)	23.023 (3.535)	22.944 (3.572)	43 (4.483)	43 (3.157)	43 (4.253)	43 (3.535)	43 (3.572)						
Clusters	43	43	43	43	18	43	43	43	43	18	43	43	43	43	18						

Table B.1.5: Village-level Effects - Difference-in-Differences

Dependent Variable:	Density			Clustering				Closeness				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A. Network: Agricultural Discussion, Directed												
Treated x Post	-0.009 (0.026)	0.002 (0.025)			-0.017 (0.029)	-0.006 (0.030)			-0.006 (0.024)	0.001 (0.023)		
Treated	0.023 (0.019)	0.024 (0.014)*			0.024 (0.018)	0.027 (0.018)			0.018 (0.015)	0.020 (0.012)*		
Post	-0.043 (0.025)*	-0.028 (0.028)	-0.043 (0.025)*	-0.052 (0.030)*	-0.085 (0.028)***	-0.080 (0.034)**	-0.085 (0.028)***	-0.132 (0.039)***	-0.030 (0.023)	-0.015 (0.025)	-0.030 (0.023)	-0.041 (0.027)
Random x Post			-0.008 (0.031)	0.004 (0.031)			-0.021 (0.036)	-0.013 (0.038)			-0.010 (0.027)	0.003 (0.028)
Cash x Post			-0.007 (0.028)	0.008 (0.030)			-0.019 (0.034)	-0.009 (0.037)			-0.002 (0.026)	0.014 (0.027)
Task x Post			-0.004 (0.028)	0.005 (0.03)			-0.026 (0.033)	-0.026 (0.037)			-0.004 (0.026)	0.005 (0.026)
Vote x Post			-0.016 (0.029)	-0.006 (0.030)			-0.002 (0.034)	0.018 (0.036)			-0.007 (0.026)	0.002 (0.027)
N	380	380	380	380	380	380	380	380	380	380	380	380
Mean of Dep Var	0.365	0.365	0.365	0.365	0.401	0.401	0.401	0.401	0.703	0.703	0.703	0.703
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Panel B. Network: Farm Help/Advice, Directed												
Treated x Post	0.003 (0.021)	0.003 (0.022)			0.007 (0.032)	0.007 (0.034)			-0.000 (0.027)	-0.003 (0.027)		
Treated	0.014 (0.014)	0.017 (0.013)			0.001 (0.017)	0.005 (0.017)			0.020 (0.013)	0.023 (0.013)*		
Post	-0.048 (0.020)**	-0.011 (0.026)	-0.048 (0.021)**	-0.046 (0.028)*	-0.090 (0.030)***	-0.025 (0.037)	-0.090 (0.030)***	-0.055 (0.044)	-0.050 (0.026)*	-0.000 (0.031)	-0.050 (0.026)*	-0.024 (0.034)
Random x Post			0.003 (0.024)	0.009 (0.026)			-0.011 (0.035)	-0.002 (0.039)			-0.002 (0.029)	0.002 (0.030)
Cash x Post			0.0157 (0.024)	0.020 (0.026)			0.033 (0.036)	0.036 (0.040)			0.005 (0.031)	-0.001 (0.032)
Task x Post			0.011 (0.024)	0.009 (0.027)			0.022 (0.037)	0.024 (0.043)			0.009 (0.029)	0.000 (0.030)
Vote x Post			-0.018 (0.024)	-0.017 (0.027)			-0.017 (0.037)	-0.013 (0.041)			-0.013 (0.029)	-0.021 (0.031)
N	380	380	380	380	380	380	380	380	380	380	380	380
Mean of Dep Var	0.279	0.279	0.279	0.279	0.301	0.301	0.301	0.301	0.639	0.639	0.639	0.639
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Standard errors are in parentheses clustered at the village level. All specifications include strata fixed effects. Even columns include treatment dummies that are not reported. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table B.1.6: Village-Level Effects - All Waves - Simple Differences

Dependent Variable:	Panel A. Network: Agricultural Discussion, Directed																
	Density						Clustering						Closeness				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)					
Treated	0.0133 (0.0241)	0.0235 (0.0210)			0.0067 (0.0231)	0.0190 (0.0230)	0.0112 (0.0263)	0.0186 (0.0261)	0.0111 (0.0232)	0.0190 (0.0205)							
Random			0.0211 (0.0257)	0.0275 (0.0231)			0.0112 (0.0263)	0.0186 (0.0261)			0.0171 (0.0245)	0.0227 (0.0220)					
Cash			0.0121 (0.0255)	0.0260 (0.0226)			0.0067 (0.0252)	0.0219 (0.0252)			0.0127 (0.0242)	0.0233 (0.0217)					
Task			0.0106 (0.0264)	0.0205 (0.0229)			0.0014 (0.0258)	0.0144 (0.0255)			0.0037 (0.0249)	0.0113 (0.0220)					
Vote			0.0092 (0.0253)	0.0202 (0.0219)			0.0075 (0.0252)	0.0209 (0.0246)			0.0108 (0.0241)	0.0186 (0.0213)					
Constant	0.2989 390	0.1389 (0.2627)***	0.2994 (0.0236)***	0.1459 (0.2644)***	0.3163 (0.0219)***	-0.0018 (0.3671)***	0.3165 (0.0220)***	0.0047 (0.3717)***	0.6514 (0.0228)***	0.6021 (0.2447)**	0.6517 (0.0229)***	0.6165 (0.2477)**					
N	390	390	390	390	390	390	390	390	390	390	390	390					
Mean of Dep Var	0.312	0.312	0.312	0.312	0.323	0.323	0.323	0.323	0.662	0.662	0.662	0.662					
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y					

Dependent Variable:	Panel B. Network: Farm Help/Advice, Directed																
	Density						Clustering						Closeness				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)					
Treated	0.0152 (0.0207)	0.0219 (0.0165)			0.0047 (0.0263)	0.0143 (0.0231)	0.0036 (0.0280)	0.0082 (0.0246)	0.0171 (0.0237)	0.0238 (0.0217)							
Random			0.0187 (0.0218)	0.0229 (0.0177)			0.0036 (0.0280)	0.0082 (0.0246)			0.0254 (0.0249)	0.0304 (0.0230)					
Cash			0.0163 (0.0222)	0.0248 (0.0185)			0.0090 (0.0278)	0.0194 (0.0251)			0.0152 (0.0257)	0.0220 (0.0240)					
Task			0.0185 (0.0225)	0.0247 (0.0179)			0.0056 (0.0288)	0.0178 (0.0254)			0.0151 (0.0254)	0.0217 (0.0229)					
Vote			0.0070 (0.0214)	0.0157 (0.0169)			0.0007 (0.0279)	0.0125 (0.0242)			0.0124 (0.0244)	0.0210 (0.0221)					
Constant	0.2170 390	0.1675 (0.1840)***	0.2172 (0.0203)***	0.1611 (0.1822)***	0.2326 (0.0255)***	-0.3334 (0.2459)***	0.2325 (0.0256)***	-0.3478 (0.2466)***	0.5734 (0.0232)***	0.6743 (0.2154)***	0.5739 (0.0233)***	0.6843 (0.2153)***					
N	390	390	390	390	390	390	390	390	390	390	390	390					
Mean of Dep Var	0.231	0.231	0.231	0.231	0.237	0.237	0.237	0.237	0.590	0.590	0.590	0.590					
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y					

Effects on network statistics using all 390 villages across Waves 1-3 using simple differences. Standard errors are in parentheses heteroskedasticity robust. All specifications include strata fixed effects. Even columns include treatment dummies that are not reported. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table B.1.7: Household-level Effects - ANCOVA

Panel A. Network: Ag Discussion, Directed								
Dependent Variable:	In Degree		Out Degree		Betweenness		Closeness	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Random x Win	1.144*** (0.326)	0.814*** (0.286)	0.109 (0.561)	0.405 (0.602)	5.807** (2.748)	5.957** (2.922)	0.025** (0.012)	0.022 (0.013)
HV Cash x Win	0.198 (0.561)	0.563 (0.540)	0.375 (0.871)	0.288 (0.874)	2.895 (4.185)	2.240 (4.250)	0.020 (0.023)	0.026 (0.021)
HV Task x Win	1.042** (0.417)	1.420*** (0.371)	-1.568*** (0.564)	-1.567*** (0.501)	-0.164 (2.753)	0.528 (2.717)	-0.019 (0.013)	-0.019 (0.013)
HV Vote x Win	1.549*** (0.479)	1.294*** (0.495)	0.006 (0.806)	-0.267 (0.710)	2.027 (2.944)	1.366 (2.944)	0.028* (0.016)	0.027* (0.014)
LV Cash x Win	2.048*** (0.540)	1.559*** (0.464)	0.373 (0.859)	0.129 (0.822)	5.524 (4.042)	1.810 (4.170)	0.047** (0.019)	0.036** (0.018)
LV Task x Win	0.150 (0.538)	0.523 (0.585)	1.077* (0.602)	0.499 (0.688)	2.723 (2.878)	2.479 (3.084)	0.028 (0.018)	0.019 (0.018)
LV Vote x Win	2.107*** (0.370)	1.921*** (0.473)	2.046*** (0.779)	1.499* (0.784)	14.495*** (5.090)	12.779** (5.620)	0.061*** (0.015)	0.052*** (0.018)
HV Cash	1.161*** (0.298)	0.195 (0.290)	0.439 (0.387)	0.010 (0.466)	4.185*** (1.533)	1.044 (1.706)	0.029*** (0.010)	0.005 (0.011)
HV Task	0.855*** (0.227)	0.364* (0.195)	1.528*** (0.319)	0.792** (0.310)	7.134*** (0.997)	5.138*** (0.935)	0.044*** (0.008)	0.020*** (0.007)
HV Vote	1.964*** (0.225)	0.823*** (0.228)	1.316*** (0.386)	0.692** (0.334)	9.249*** (1.479)	5.846*** (1.324)	0.060*** (0.009)	0.032*** (0.008)
N	3136	2962	3136	2962	3136	2962	3136	2962
Mean of Dep Var	6.145	6.180	6.150	6.229	13.000	13.083	0.688	0.689
BL Outcome Control		Y		Y		Y		Y
Controls		Y		Y		Y		Y

Panel B. Network: Farm Help/Advice, Directed								
Dependent Variable:	In Degree		Out Degree		Betweenness		Closeness	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Random x Win	0.679** (0.336)	0.625** (0.282)	0.474 (0.484)	0.599 (0.486)	6.898* (3.892)	8.524** (4.214)	0.022** (0.011)	0.019* (0.011)
HV Cash x Win	0.137 (0.606)	0.457 (0.538)	-0.038 (0.695)	0.136 (0.640)	0.831 (3.792)	0.472 (4.024)	0.004 (0.020)	0.011 (0.018)
HV Task x Win	0.990** (0.461)	1.204*** (0.407)	-0.626 (0.435)	-0.569 (0.359)	2.042 (3.233)	3.555 (3.013)	-0.001 (0.013)	-0.002 (0.012)
HV Vote x Win	1.562*** (0.486)	1.283*** (0.419)	-0.581 (0.748)	-0.635 (0.613)	1.934 (4.510)	1.682 (4.254)	0.026 (0.017)	0.025* (0.015)
LV Cash x Win	1.350** (0.548)	0.997** (0.476)	0.730 (0.777)	0.500 (0.810)	7.239* (3.783)	4.541 (4.272)	0.047** (0.022)	0.041** (0.021)
LV Task x Win	0.135 (0.507)	0.742 (0.549)	1.454*** (0.496)	0.926* (0.548)	2.932 (3.041)	2.870 (3.240)	0.030 (0.022)	0.028 (0.022)
LV Vote x Win	1.989*** (0.308)	1.622*** (0.302)	1.967*** (0.707)	1.017 (0.700)	17.300*** (4.569)	13.070*** (4.549)	0.078*** (0.011)	0.059*** (0.012)
HV Cash	1.490*** (0.277)	0.484* (0.280)	0.097 (0.326)	-0.142 (0.387)	2.805 (2.634)	-0.114 (2.896)	0.033*** (0.010)	0.014 (0.011)
HV Task	0.683*** (0.191)	0.314* (0.162)	0.817*** (0.277)	0.307 (0.291)	5.499*** (1.563)	3.612** (1.690)	0.032*** (0.008)	0.015* (0.008)
HV Vote	1.831*** (0.189)	0.586*** (0.173)	1.213*** (0.320)	0.802*** (0.276)	11.801*** (2.089)	8.520*** (2.018)	0.063*** (0.008)	0.038*** (0.007)
N	3136	2962	3136	2962	3136	2962	3136	2962
Mean of Dep Var	4.599	4.626	4.601	4.665	15.047	15.222	0.616	0.617
BL Outcome Control		Y		Y		Y		Y
Controls		Y		Y		Y		Y

All specifications include village fixed effects. Even numbered specifications include a vector of household controls including the baseline value of the outcome variable. Standard errors reported in parentheses are clustered at the village level. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table B.1.8: Household-level Effects - ANCOVA by Sample Subset

Panel A. Network: Ag Discussion, Directed																									
Dep Var:	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)		(9)		(10)		(11)		(12)		
	In Deg	Out Deg	In Deg	Out Deg	Betw.	In Deg	Out Deg	Betw.	In Deg	Out Deg	Betw.	In Deg	Out Deg	Betw.	In Deg	Out Deg	Betw.	In Deg	Out Deg	Betw.	In Deg	Out Deg	Betw.	In Deg	Out Deg
Win	0.749 (0.296)**	0.475 (0.622)	6.294 (2.949)**	1.132 (0.211)**	0.564 (0.380)	5.882 (1.992)**	1.030 (0.338)**	-0.952 (0.468)**	0.446 (2.430)	1.207 (0.286)**	-0.508 (0.423)	1.830 (1.958)													
Constant	-0.012 (0.721)	5.182 (1.417)**	7.405 (5.466)	1.049 (0.626)*	5.950 (2.976)**	5.066 (6.401)	5.407 (0.934)**	-1.329 (1.565)	-2.505 (6.257)	4.879 (0.757)**	0.629 (1.461)	2.611 (5.556)													
N	749	749	749	2266	2266	2266	452	452	452	689	689	689													
Dep Var Mean	6.566	6.570	13.259	5.920	6.042	11.983	6.894	6.790	15.133	7.045	6.829	16.669													
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y													
Sample	Rand	Rand	Rand	All LV	All LV	All LV	C/T HV	C/T HV	C/T HV	All HV	All HV	All HV													

Panel B. Network: Farm Help/Advice, Directed																									
Dep Var:	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)		(9)		(10)		(11)		(12)		
	In Deg	Out Deg	In Deg	Out Deg	Betw.	In Deg	Out Deg	Betw.	In Deg	Out Deg	Betw.	In Deg	Out Deg	Betw.	In Deg	Out Deg	Betw.	In Deg	Out Deg	Betw.	In Deg	Out Deg	Betw.	In Deg	Out Deg
Win	0.623 (0.293)**	0.525 (0.504)	8.175 (4.301)*	0.905 (0.193)**	0.732 (0.334)**	7.821 (2.386)**	0.855 (0.338)**	-0.448 (0.371)	0.959 (2.501)	1.094 (0.272)**	-0.313 (0.330)	2.320 (2.260)													
Constant	0.043 (0.675)	5.242 (1.189)**	10.373 (6.057)*	0.307 (0.575)	6.094 (2.321)**	8.306 (5.927)	3.162 (0.779)**	-0.501 (1.207)	-1.874 (6.003)	3.418 (0.611)**	0.603 (1.129)	11.901 (6.707)*													
N	749	749	749	2266	2266	2266	452	452	452	689	689	689													
Dep Var Mean	4.658	4.681	16.195	4.353	4.528	13.992	5.438	5.091	16.575	5.522	5.118	19.257													
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y													
Sample	Rand	Rand	Rand	All LV	All LV	All LV	C/T HV	C/T HV	C/T HV	All HV	All HV	All HV													

ANCOVA specifications. All specifications contain a vector of household controls, the baseline value of the outcome variable, and village fixed effects. Standard errors reported in parentheses are clustered at the village level. Asterisks indicate statistical significance at the 1% **, 5% **, and 10% * levels.

Table B.1.9: Dyadic Regressions - Farm Help/Advice

Dep Var:	Linked at Endline					
	(1)	(2)	(3)	(4)	(5)	(6)
WW	0.224 (0.073)***	0.231 (0.079)***	0.260 (0.093)***	0.151 (0.102)	0.138 (0.101)	0.175 (0.111)
WN	0.044 (0.034)	0.052 (0.031)*	0.057 (0.033)*	0.046 (0.021)**	0.044 (0.021)**	0.048 (0.023)**
NW	0.079 (0.023)***	0.075 (0.022)***	0.069 (0.022)***	0.039 (0.014)***	0.037 (0.013)***	0.036 (0.014)***
HH	0.089 (0.022)***	0.119 (0.020)***	0.103 (0.020)***	0.050 (0.017)***	0.064 (0.016)***	0.056 (0.016)***
HL	0.024 (0.020)	0.051 (0.020)**	0.041 (0.021)**	0.004 (0.012)	0.017 (0.012)	0.013 (0.012)
LH	0.059 (0.015)***	0.088 (0.013)***	0.081 (0.014)***	0.027 (0.010)**	0.039 (0.008)***	0.035 (0.009)***
WW x HH	0.055 (0.118)	0.029 (0.123)	0.053 (0.123)	-0.057 (0.130)	-0.052 (0.131)	-0.072 (0.142)
WW x HL	-0.274 (0.134)**	-0.298 (0.135)**	-0.308 (0.159)*	-0.069 (0.132)	-0.055 (0.130)	-0.042 (0.146)
WW x LH	-0.059 (0.125)	-0.057 (0.129)	-0.061 (0.151)	0.223 (0.147)	0.246 (0.141)*	0.255 (0.149)*
WN x HH	-0.071 (0.056)	-0.081 (0.053)	-0.071 (0.052)	-0.051 (0.036)	-0.055 (0.035)	-0.060 (0.036)*
WN x HL	-0.059 (0.057)	-0.077 (0.052)	-0.065 (0.055)	-0.062 (0.030)**	-0.065 (0.030)**	-0.071 (0.031)**
WN x LH	0.045 (0.057)	0.039 (0.056)	0.019 (0.060)	0.028 (0.042)	0.036 (0.040)	0.030 (0.045)
NW x HH	-0.013 (0.043)	-0.016 (0.039)	-0.008 (0.041)	0.054 (0.031)*	0.053 (0.028)*	0.051 (0.028)*
NW x HL	0.038 (0.053)	0.023 (0.056)	0.081 (0.055)	-0.028 (0.035)	-0.014 (0.036)	-0.023 (0.037)
NW x LH	-0.038 (0.035)	-0.036 (0.033)	-0.026 (0.034)	0.007 (0.024)	-0.001 (0.021)	-0.003 (0.022)
N	19771	19771	18043	36948	36948	33629
Mean of Dep Var	0.388	0.388	0.391	0.202	0.202	0.205
Linked at BL	Yes	Yes	Yes	No	No	No
Cluster FE		Y	Y		Y	Y
Controls			Y			Y

Dyadic regressions. Dyadic cluster robust standard errors are reported in parentheses. Controls are the interaction of sender and received household characteristics. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table B.1.10: Dyadic Regressions - Ag Discussion Network

Dep Var:	Linked at Endline					
	(1)	(2)	(3)	(4)	(5)	(6)
WW	0.167 (0.093)*	0.141 (0.101)	0.187 (0.107)*	0.094 (0.087)	0.082 (0.092)	0.095 (0.096)
WN	0.030 (0.031)	0.026 (0.030)	0.036 (0.031)	0.032 (0.023)	0.033 (0.023)	0.028 (0.024)
NW	0.093 (0.021)***	0.094 (0.018)***	0.081 (0.019)***	0.058 (0.015)***	0.054 (0.012)***	0.056 (0.013)***
HH	0.119 (0.021)***	0.147 (0.020)***	0.129 (0.020)***	0.057 (0.019)***	0.086 (0.018)***	0.075 (0.019)***
HL	0.013 (0.018)	0.041 (0.019)**	0.033 (0.019)*	0.006 (0.013)	0.034 (0.014)**	0.030 (0.014)**
LH	0.046 (0.015)***	0.074 (0.013)***	0.063 (0.013)***	0.006 (0.011)	0.032 (0.009)***	0.028 (0.009)***
WW x HH	-0.051 (0.138)	-0.071 (0.137)	-0.091 (0.139)	0.091 (0.141)	0.085 (0.144)	0.085 (0.149)
WW x HL	-0.059 (0.140)	-0.035 (0.144)	-0.114 (0.160)	-0.006 (0.130)	0.022 (0.131)	0.017 (0.142)
WW x LH	-0.074 (0.137)	-0.020 (0.136)	-0.117 (0.151)	0.269 (0.134)**	0.277 (0.138)**	0.303 (0.141)**
WN x HH	-0.082 (0.049)*	-0.085 (0.046)*	-0.090 (0.047)*	-0.072 (0.041)*	-0.079 (0.041)*	-0.068 (0.043)
WN x HL	-0.025 (0.048)	-0.033 (0.046)	-0.066 (0.048)	-0.038 (0.036)	-0.053 (0.035)	-0.048 (0.037)
WN x LH	0.023 (0.052)	0.039 (0.048)	0.024 (0.051)	0.036 (0.042)	0.050 (0.039)	0.053 (0.042)
NW x HH	-0.029 (0.038)	-0.035 (0.034)	-0.027 (0.036)	0.032 (0.035)	0.026 (0.032)	0.015 (0.034)
NW x HL	-0.006 (0.043)	0.003 (0.042)	0.026 (0.043)	-0.046 (0.037)	-0.025 (0.035)	-0.038 (0.043)
NW x LH	-0.039 (0.033)	-0.054 (0.030)*	-0.044 (0.032)	-0.013 (0.024)	-0.019 (0.021)	-0.017 (0.022)
N	25068	25068	22916	36899	36899	33485
Mean of Dep Var	0.472	0.472	0.475	0.249	0.249	0.252
Linked at BL	Yes	Yes	Yes	No	No	No
Cluster FE		Y	Y		Y	Y
Controls			Y			Y

Dyadic regressions. Dyadic cluster robust standard errors are reported in parentheses. Controls are the interaction of sender and received household characteristics. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

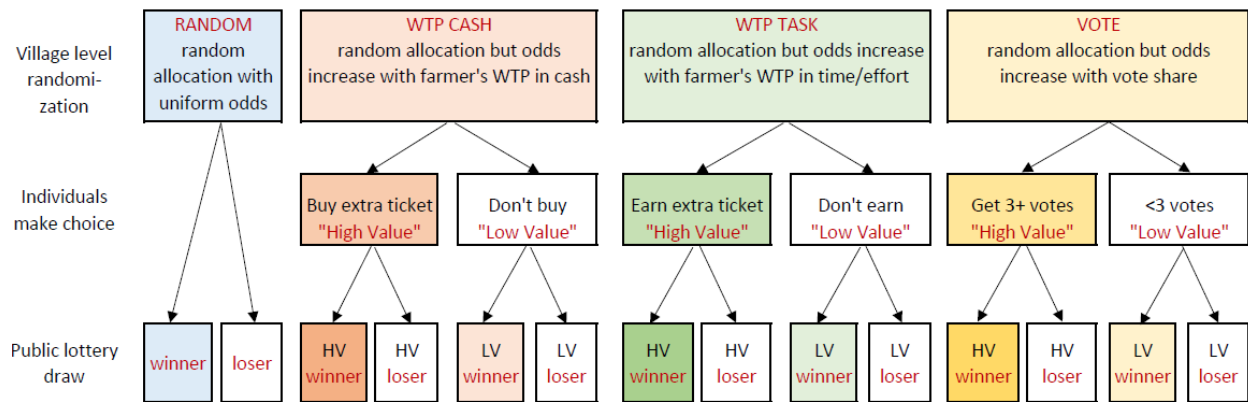


Figure B.1.1: Experimental Design

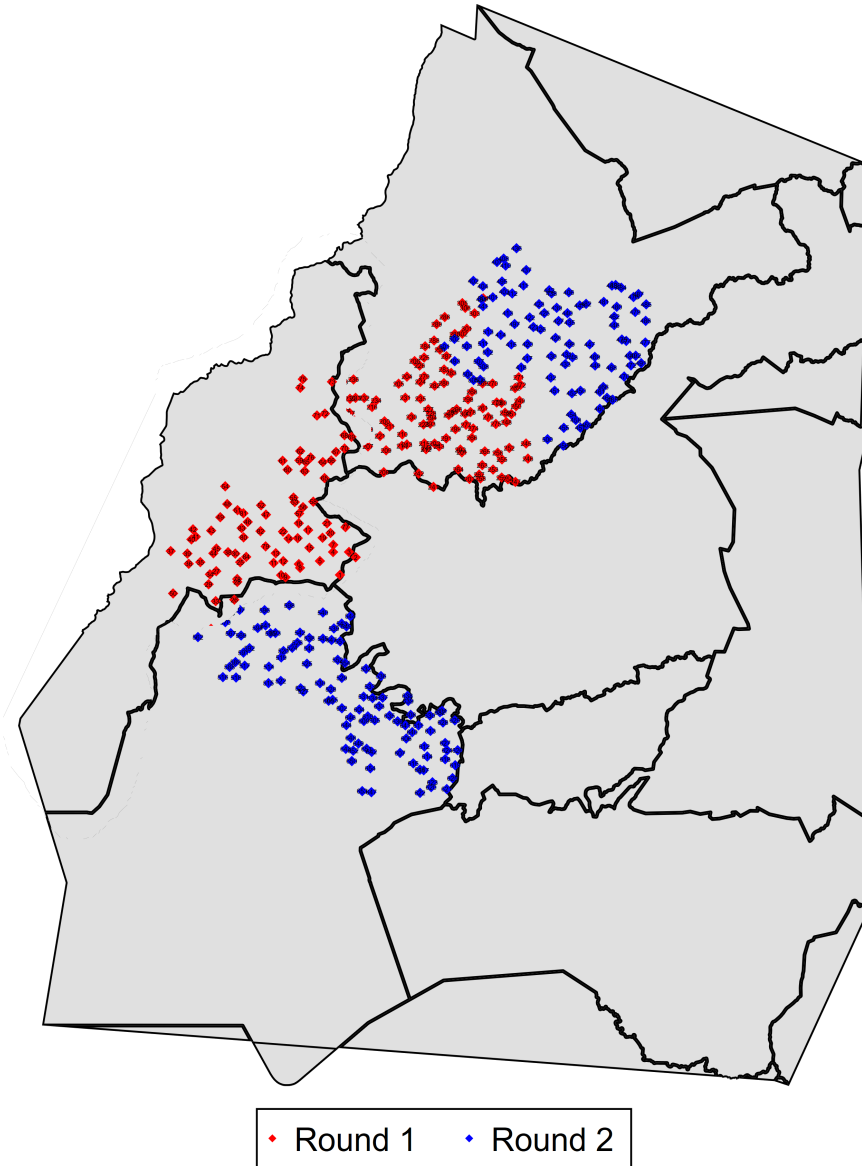


Figure B.1.2: Map of Villages

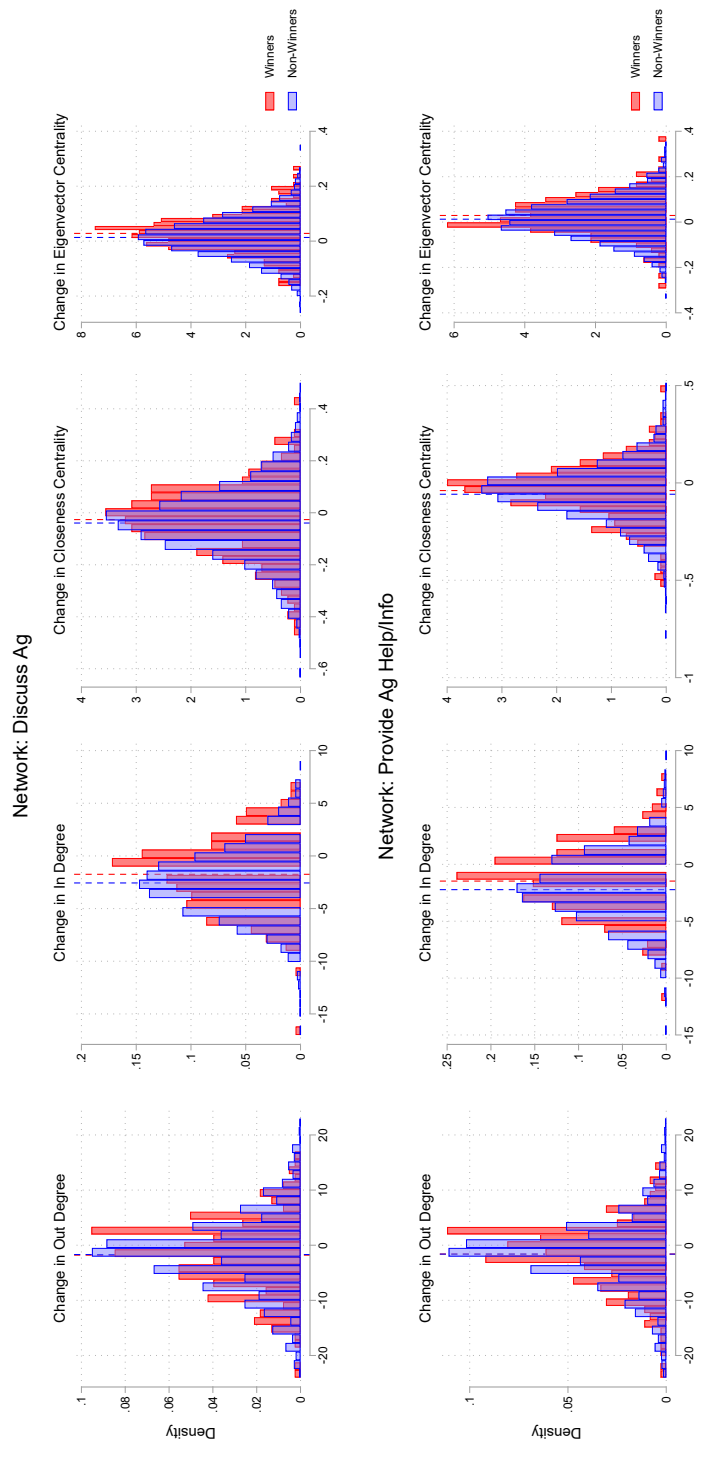


Figure B.1.3: Change in Network Stats - Winners v. Non-Winners

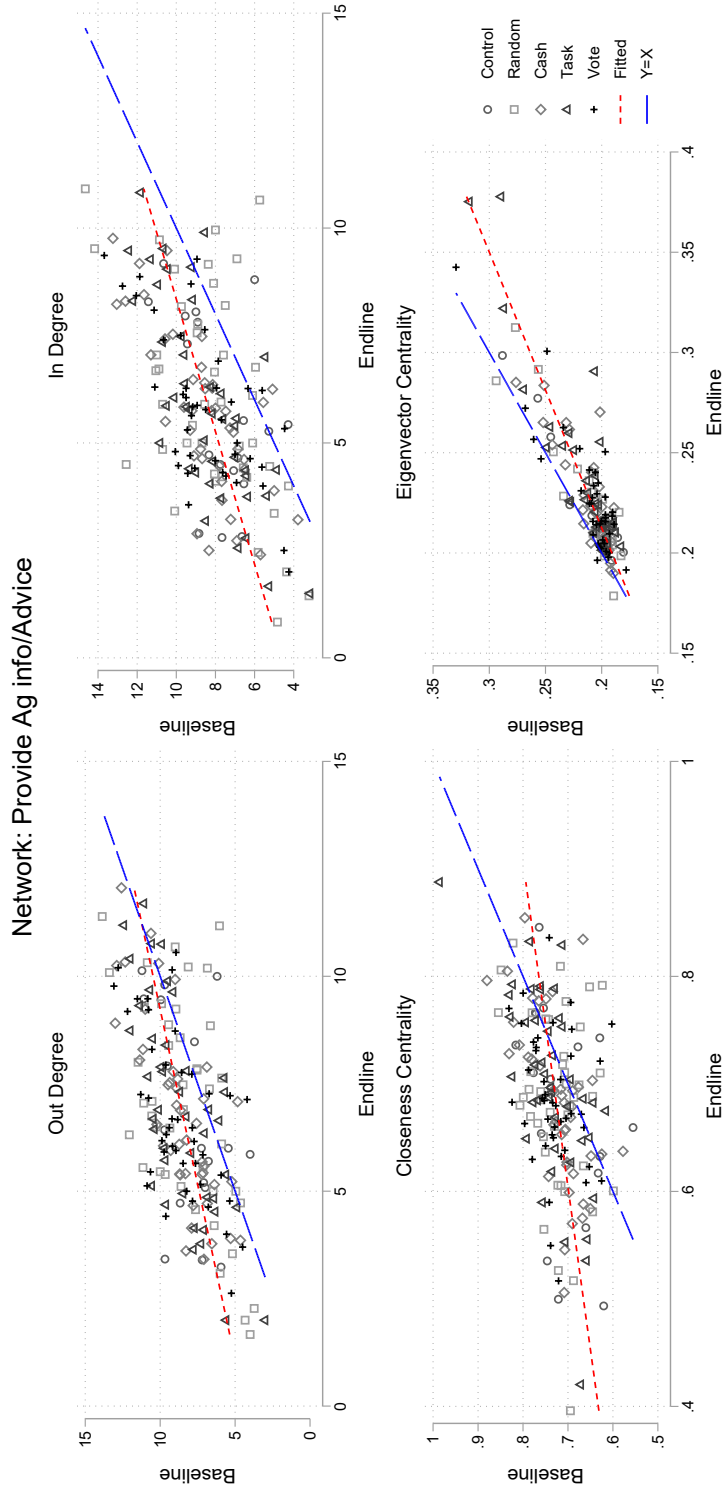


Figure B.1.4: Village-Level Network Stats - Baseline v. Endline

B.2 Model Details

I begin by defining the primary assumption and subsequent propositions required for identification. I then describe in more detail the derivation of the estimating equations. This adheres closely to the model in Comola and Prina (2019) and I encourage the reader to consult that paper for more details on the model.

B.2.1 Assumption 1.

Conditional exogeneity¹:

$$\mathbb{E}[\epsilon_t | \mathbf{G}_0, \mathbf{G}_1, \mathbf{W}, HV, \mu] = 0$$

common in the literature (Bramoullé et al. (2009)). This accounts for correlated unobservables at the individual level, so long as they are time invariant.

B.2.2 Proposition 1.

If $|\beta_1| < 1$, $|\beta_2| < 1$, and $|\beta_1 - \beta_2| < 1$ then $\tilde{\mathbf{S}}(\beta)$ is invertible. Where

$$\tilde{\mathbf{S}}(\beta) = \left[\mathbf{I}_{2N} - \beta_1 \tilde{\mathbf{G}}_0 - \beta_2 \tilde{\mathbf{G}}_{1-0} \right]$$

similar to stationarity conditions in time-series/spatial economics.

B.2.3 Proposition 2.

² If:

- a. $\tilde{\mathbf{S}}(\beta)$ is invertible
- b. $(\gamma\beta_1 + \delta_1) \neq 0$
- c. $(\gamma\beta_2 + \delta_2) \neq 0$
- d. \mathbf{I} , $\tilde{\mathbf{G}}_0$, $\tilde{\mathbf{G}}_{1-0}$, $(\tilde{\mathbf{G}}_0)^2$, $(\tilde{\mathbf{G}}_{1-0})^2$, $\tilde{\mathbf{G}}_0 \tilde{\mathbf{G}}_{1-0}$, $\tilde{\mathbf{G}}_{1-0} \tilde{\mathbf{G}}_0$ are linearly independent

then the peer effects are identified.

¹Exogeneity of \mathbf{G}_1 can be relaxed by using the predicted change in the network as an instrument for the observed change

²See Bramoullé et al. (2009); Arduini (2014); Dieye and Fortin (2016)

B.2.4 Main Estimating Equation - Treatment Response Model

We stack equations (2.6) and (2.7) to obtain:

$$\mathbf{y} = \tilde{\mathbf{S}}(\beta)^{-1} \left[\left(\gamma \mathbf{I}_{2N} + \delta_1 \tilde{\mathbf{G}}_0 + \delta_2 \tilde{\mathbf{G}}_{1-0} \right) \tilde{\mathbf{W}} + \left(\eta_1 \tilde{\mathbf{G}}_0 + \eta_2 \tilde{\mathbf{G}}_{1-0} \right) \tilde{\mathbf{H}} + \psi HV + \tau \mu \right] + \tilde{\mathbf{S}}(\beta)^{-1} \epsilon \quad (\text{B.1})$$

Next we remove the individual effects by pre-multiplying by the transformation matrix \mathbf{J} to obtain the main estimating equation:

$$\mathbf{J}\mathbf{y} = \beta_1 \mathbf{J}\tilde{\mathbf{G}}_0 \mathbf{y} + \beta_2 \mathbf{J}\tilde{\mathbf{G}}_{1-0} \mathbf{y} + \mathbf{J} \left(\gamma \mathbf{I}_{2N} + \delta_1 \tilde{\mathbf{G}}_0 + \delta_2 \tilde{\mathbf{G}}_{1-0} \right) \tilde{\mathbf{W}} + \mathbf{J} \left(\eta_1 \tilde{\mathbf{G}}_0 + \eta_2 \tilde{\mathbf{G}}_{1-0} \right) \tilde{\mathbf{H}} + \psi \mathbf{J}HV + \mathbf{J}\epsilon \quad (\text{B.2})$$

where where $\mathbf{y} = \begin{bmatrix} y_0 \\ y_1 \end{bmatrix}$, $\tilde{\mathbf{G}}_0 = \begin{bmatrix} \mathbf{G}_0 & 0 \\ 0 & \mathbf{G}_0 \end{bmatrix}$, $\tilde{\mathbf{G}}_{1-0} = \begin{bmatrix} 0 & 0 \\ 0 & \mathbf{G}_{1-0} \end{bmatrix}$, $\tilde{\mathbf{W}} = \begin{bmatrix} 0 \\ \mathbf{W} \end{bmatrix}$, $\tilde{\mathbf{H}} = \begin{bmatrix} 0 \\ \mathbf{H} \end{bmatrix}$, $\tau = \mathbf{1}_{2 \times 1} \otimes \mathbf{I}_{N \times N}$ and $\mathbf{J} = \left[\mathbf{I}_{2 \times 2} - \frac{1}{2} \mathbf{1}_{2 \times 1} \mathbf{1}'_{2 \times 1} \right] \otimes \mathbf{I}_{N \times N}$. This extends the base model to account for the structure of the selective trials in which treatment is random after conditioning on high-value status. When $T = 2$, (10) is equivalent to estimating the first differenced equation (2.8) from the main text.

B.2.5 Series Expansion for Instruments

We know that the $\mathbf{J}\tilde{\mathbf{G}}_0$ and $\mathbf{J}\tilde{\mathbf{G}}_{1-0}$ are endogenous as they are simultaneously determined and must be instrumented for.

The series expansion of $\tilde{\mathbf{S}}(\beta)^{-1}$ can be written as:

$$\begin{aligned} \tilde{\mathbf{S}}(\beta)^{-1} &= \sum_{k=0}^{\infty} \tilde{\mathbf{S}}(\beta) \\ &= \mathbf{I}_{2N} + \beta_1 \tilde{\mathbf{G}}_0 + \beta_2 \tilde{\mathbf{G}}_{1-0} \sum_{k=2}^{\infty} \tilde{\mathbf{S}}_k(\beta) \end{aligned}$$

where $\tilde{\mathbf{S}}_k(\beta) = \sum_{i=0}^k \binom{k}{i} \left(\beta_1 \tilde{\mathbf{G}}_0 \right)^{k-i} \times \left(\beta_2 \tilde{\mathbf{G}}_{1-0} \right)^i$

By first inserting the series expansion into (9), pre-multiplying by $\tilde{\mathbf{G}}_0$, taking the conditional expectation with respect to $\tilde{\mathbf{W}}$, and then pre-multiplying by \mathbf{J} (defined above), the

full set of instruments can be written as:

$$\begin{aligned} \mathbf{Q}_\infty = \mathbf{J} \left[\widetilde{\mathbf{W}}, \widetilde{\mathbf{G}}_0 \widetilde{\mathbf{W}}, \widetilde{\mathbf{G}}_{1-0} \widetilde{\mathbf{W}}, \right. \\ \left. \widetilde{\mathbf{G}}_0 \widetilde{\mathbf{S}}(\beta)^{-1} \widetilde{\mathbf{W}}, \widetilde{\mathbf{G}}_0 \widetilde{\mathbf{S}}(\beta)^{-1} \widetilde{\mathbf{G}}_0 \widetilde{\mathbf{W}}, \widetilde{\mathbf{G}}_0 \widetilde{\mathbf{S}}(\beta)^{-1} \widetilde{\mathbf{G}}_{1-0} \widetilde{\mathbf{W}}, \widetilde{\mathbf{G}}_0 \widetilde{\mathbf{S}}(\beta)^{-1} \boldsymbol{\tau}, \right. \\ \left. \widetilde{\mathbf{G}}_{1-0} \widetilde{\mathbf{S}}(\beta)^{-1} \widetilde{\mathbf{W}}, \widetilde{\mathbf{G}}_{1-0} \widetilde{\mathbf{S}}(\beta)^{-1} \widetilde{\mathbf{G}}_0 \widetilde{\mathbf{W}}, \widetilde{\mathbf{G}}_{1-0} \widetilde{\mathbf{S}}(\beta)^{-1} \widetilde{\mathbf{G}}_{1-0} \widetilde{\mathbf{W}}, \widetilde{\mathbf{G}}_{1-0} \widetilde{\mathbf{S}}(\beta)^{-1} \boldsymbol{\tau} \right] \end{aligned}$$

The proofs for this expansion can be found in Appendix A of Comola and Prina (2019). Further developing the series expansion, one can build lagged partner instruments of higher order (e.g. 3rd order instruments used in the empirical portion). The minimal set of excluded instruments based on Proposition 2 are:

$$(1) \mathbf{J} \left(\widetilde{\mathbf{G}}_0 \right)^2 \widetilde{\mathbf{W}}, (2) \mathbf{J} \left(\widetilde{\mathbf{G}}_{1-0} \right)^2 \widetilde{\mathbf{W}}, (3) \mathbf{J} \widetilde{\mathbf{G}}_0 \widetilde{\mathbf{G}}_{1-0} \widetilde{\mathbf{W}}, (4) \mathbf{J} \widetilde{\mathbf{G}}_{1-0} \widetilde{\mathbf{G}}_0 \widetilde{\mathbf{W}}$$

See the text for a discussion of these instruments.

B.2.6 Calculating the Total Treatment Effect

First, let:

$$\mathbf{M} = \left[\left(\gamma \mathbf{I}_{2N} + \delta_1 \widetilde{\mathbf{G}}_0 + \delta_2 \widetilde{\mathbf{G}}_{1-0} \right) \widetilde{\mathbf{W}} + \left(\eta_1 \widetilde{\mathbf{G}}_0 + \eta_2 \widetilde{\mathbf{G}}_{1-0} \right) \widetilde{\mathbf{H}} + \psi HV + \tau \boldsymbol{\mu} \right]$$

from equation (9).

The goal is to solve for the total effect of treating individual k , $\frac{\partial E(y_1|W, HV)}{\partial W_k}$ which is an $N \times 1$ vector that represents the k^{th} column of the full $N \times N$ matrix of partial derivatives. Comola and Prina (2019) show that this can be written as:

$$\begin{aligned} \frac{\partial E(y_1|W, HV)}{\partial W_k} &= \frac{\partial S(\beta)^{-1}}{\partial W_k} \mathbf{M} + S(\beta)^{-1} \frac{\partial \mathbf{M}}{\partial W_k} \\ \frac{\partial S(\beta)^{-1}}{\partial W_k} &= S(\beta)^{-1} \beta_2 \frac{\partial \mathbf{G}_{1-0}}{\partial W_k} S(\beta)^{-1} \\ \frac{\partial \mathbf{M}}{\partial W_k} &= \gamma \mathbf{e}_k + \delta_1 \mathbf{G}_0 \mathbf{e}_k + \delta_2 \frac{\partial \mathbf{G}_{1-0}}{\partial W_k} \mathbf{W} + \eta_1 \mathbf{G}_0 \mathbf{e}_k + \eta_2 \frac{\partial \mathbf{G}_{1-0}}{\partial W_k} \mathbf{H} \end{aligned}$$

The matrix, $\frac{\partial E(\mathbf{G}_{1-0})}{\partial W_k}$, that is obtained by differentiating each element of \mathbf{G}_{1-0} is the effect of the treatment on the matrix of social interactions.

Then, following standard practice in spatial econometrics, we obtain the direct treatment effect as the average of the diagonal of $\frac{\partial E(y_1|W, HV)}{\partial W}$ and the indirect treatment effect is the

average of the column sums of the non-diagonal elements of $\frac{\partial E(y_1|W,HV)}{\partial W}$. Thus, we capture peer effects that come through change in the treatment status to baseline peers as well as those from the intervention-driven network change.

Appendix C

Valuing the Time of the Self-Employed - Appendix

C.1 Proofs

Proof of Lemma 1. Let $x = (c, l)$, and $b = (\tau, m, h)$. Maximization problem (3.1) is continuous in x, b and strictly concave in b over a convex and compact domain. It follows that for every b , problem (3.1) admits a unique solution x_b , and it is continuous in b .

The Lagrangian associated with (3.1) takes the form

$$\mathcal{L}(x, b, \lambda) = u(c, l + h) + \mathbb{E}_\theta(v(I + wl + \tau\theta - c - m)) + \lambda \times (\bar{l} - l) + \kappa \times (I + wl - c - m).$$

Given unique values $c|_b$ and $l|_b$ associated with $b = (\tau, h, m)$, Lagrange multipliers $\kappa|_b$ and $\lambda|_b$ are uniquely pinned down by first-order conditions with respect to c and l :

$$\begin{aligned} u_c(c|_b, l|_b + h) - \mathbb{E}_\theta[v'(I + wl|_b + \tau\theta - c|_b - m)] - \kappa|_b &= 0 \\ u_l(c|_b, l|_b + h) + w\mathbb{E}_\theta[v'(I + wl|_b + \tau\theta - c|_b - m)] - \lambda|_b &= 0. \end{aligned}$$

■

Proof of Theorem 1. Theorem 1 follows from Corollary 5 of Milgrom and Segal (2002) and the fact that optimal choices, and Lagrange multipliers associated with (3.1) are unique. Consider $b = (\tau, h, m)$. For any direction $\Delta b \in \mathbb{R}^3$ the directional derivative of V at b along direction Δb is

$$D_{\Delta b}V(b) = D_{\Delta b}\mathcal{L}(x|_b, b, \lambda|_b).$$

Where \mathcal{L} is the Lagrangian introduced in the proof of Lemma 1. ■

Proof of Theorem 2. Equations (3.8) and (3.9) imply that

$$\begin{aligned}\log(m_i^{RW}/2) &= \log(V_{h,i}/V_{m,i}) + \frac{\gamma^{RW}}{\gamma^{RW} + \gamma^{CB} - \gamma^{TB}} \log \frac{1}{1 - \widehat{\omega}_i} \\ \log m_i^{CB} &= \log(-V_{\tau,i}/V_{m,i}) - \frac{\gamma^{CB}}{\gamma^{RW} + \gamma^{CB} - \gamma^{TB}} \log \frac{1}{1 - \widehat{\omega}_i} \\ \log h_i^{TB} &= \log(-V_{\tau,i}/V_{h,i}) - \frac{\gamma^{TB}}{\gamma^{RW} + \gamma^{CB} - \gamma^{TB}} \log \frac{1}{1 - \widehat{\omega}_i}.\end{aligned}$$

Under the assumption that ρ_i —and thus also $-\log(1 - \widehat{\omega}_i)$, a linear transformation of ρ_i —is uncorrelated with $\log(-V_{\tau,i}/V_{m,i})$ and $\log(V_{h,i}/V_{m,i})$, it follows that for all $X \in \{RW, CB, TB\}$, OLS coefficient $\widehat{\delta}^X$ consistently estimates $\gamma^X/(\gamma^{RW} + \gamma^{CB} - \gamma^{TB})$. In turn, the assumption that $\gamma^{RW} + \gamma^{CB} + \gamma^{TB} = 1$ implies that $\widehat{\delta}^{RW} + \widehat{\delta}^{CB} + \widehat{\delta}^{TB} = 1/(\gamma^{RW} + \gamma^{CB} - \gamma^{TB})$. This implies that for all $X \in \{RW, CB, TB\}$, $\widehat{\delta}^X/(\widehat{\delta}^{RW} + \widehat{\delta}^{CB} + \widehat{\delta}^{TB})$ is a consistent estimator of γ^X . As the regression equations in (3.10) form a seemingly unrelated regressions model with identical right-hand-side regressors, the OLS estimators are also as efficient as the seemingly unrelated regressions estimators. ■

Proof of Tightness of [IVT,DVT] as Bounds for SVT.

Claim: There exist $(\log -V_{\tau,i}/V_{m,i}, \log -V_{\tau,i}/V_{h,i}, \gamma^X)_{X \in \{RW, CB, TB\}} \geq 0$ such that (3.8) holds if and only if $SVT_i \in [IVT_i, DVT_i]$. We assume throughout that $\omega^X \in (0, 1)_{X \in \{RW, CB, TB\}}$ and $\omega^{CB} \geq \omega^{TB}$.

Assume there exist $(\log -V_{\tau,i}/V_{m,i}, \log -V_{\tau,i}/V_{h,i}, \gamma^X)_{X \in \{RW, CB, TB\}} \geq 0$ such that (3.8) holds. Then $\log V_{h,i}/V_{m,i} = \log(m_i^{RW}/2) + \log(1 - \omega_i^{RW}) \leq \log(m_i^{RW}/2)$ which implies that $SVT_i \leq DVT_i$. Similarly, $\log V_{h,i}/V_{m,i} = \log(m_i^{CB}) - \log(h_i^{TB}) - \log(1 - \omega_i^{CB}) + \log(1 - \omega_i^{TB})$, so that $SVT_i \geq IVT_i$.

We now turn to the converse. Take as given values DVT_i , IVT_i and SVT_i such that $SVT_i \in [IVT_i, DVT_i]$. There are multiple parameter combinations

$$(\log -V_{\tau,i}/V_{m,i}, \log -V_{\tau,i}/V_{h,i}, \gamma^X)_{X \in \{RW, CB, TB\}} \geq 0$$

such that (3.8) holds. One specification is

$$\begin{aligned}\gamma^{TB} &= 0, & \log -V_{\tau,i}/V_{h,i} &= \log(h_i^{TB}), \\ \gamma^{CB} &= \log SVT_i - \log IVT_i, & \log -V_{\tau,i}/V_{m,i} &= \log -V_{\tau,i}/V_{h,i} + \log SVT_i, \\ \gamma^{RW} &= \log DVT_i - \log IVT_i \geq 0.\end{aligned}$$

■

C.2 Implementation Details

C.2.1 Sample Selection

We selected 18 villages for our sample from a set of villages sampled for a separate project (Chassang et al., 2023) which examined how using different mechanisms to targeting a new technology (a Kickstart irrigation pump) affected experimentation and takeup. The villages in Chassang et al. (2023) were selected from a list of all villages in Bungoma County, Kenya. Villages were aggregated into geographic clusters of 2–8 villages, and up to one village was chosen per geographic cluster. Then, villages were randomly assigned to one of four treatment arms and a control treatment state. Field work for this paper was conducted in all control-group villages from Chassang et al. (2023). In Table C.2.1 we show that households and villages in our analysis sample look similar to those in the full Chassang et al. (2023) sample; that is, randomization was successful.¹

Although 61% of farmers were using some form of irrigation, the overwhelming majority use “bucket irrigation” (which is extremely time consuming and dramatically limits the area that can be irrigated) and only 6% of farmers had used a manual pump in the past 3 years.²

C.2.2 Survey Protocol

Before the survey, our project staff explained the elicitation design and quizzed farmers on hypothetical outcomes to ensure comprehension. If the head of household was unable to perform casual labor, a different household member was selected at the outset. Staff gave farmers information on the irrigation pump, including its market price, hose length, maximum pumping height, and flow rate. Staff explained that casual labor would be performed in groups in a nearby village, and that workers would be monitored by project staff to ensure the work was performed. Because the work was done for a stranger in a different village, we do not expect farmers to internalize the direct value of their work. Additionally, because the work was similar to casual agricultural work that is commonly done throughout all of our villages, there should not be any learning value from completing the work.

¹Households in our sample also look similar to those in Egger et al. (2022), sampled from a neighboring county, in terms of household composition and occupation.

²The majority of the world’s poor lives in sub-Saharan Africa and earns very little money as small-scale farmers. Without irrigation, it is difficult for these farmers to grow multiple cycles of high value crops throughout the year and to harvest and sell their crops in the dry season when prices are higher. Yet, according to a 2010 FAO report, less than 4% of arable land in sub-Saharan Africa is irrigated.

Table C.2.1: Value of Time Sample Balanced with Larger Household Sample.

	(1) VoT	(2) STRIP	(3) T-test (1)-(2)
No male head in household = 1	0.14 (0.35)	0.14 (0.35)	0.85
Number of adults (age 18 or over) in household	2.68 (1.29)	2.66 (1.42)	0.80
Number of children (under 18 years) in household	3.97 (2.37)	3.76 (2.24)	0.13
Home: Improved Walls = 1	0.09 (0.29)	0.12 (0.33)	0.11
Home: Improved Roof = 1	0.87 (0.33)	0.83 (0.38)	0.02**
Home: Improved Floor = 1	0.14 (0.35)	0.19 (0.39)	0.02**
Home: Improved Drinking Water = 1	0.67 (0.47)	0.68 (0.47)	0.61
All household members have shoes = 1	0.43 (0.50)	0.44 (0.50)	0.67
Could get 2,500 KSh for emergency	0.26 (0.44)	0.28 (0.45)	0.49
Years in Dwelling	20.14 (15.29)	19.43 (15.11)	0.43
Luhya ethnicity = 1	0.94 (0.25)	0.88 (0.33)	0.00***
Land area under cultivation (acres)	2.30 (1.96)	2.17 (4.40)	0.29
Land distance to water source (meters)	5.29 (39.93)	7.97 (52.41)	0.26
Household income ('000 KSh, past year)	66.91 (95.63)	68.76 (109.56)	0.74
Household income from crops ('000 KSh, past year)	26.87 (47.76)	23.37 (42.38)	0.20
Income share from sale of crops	0.38 (0.38)	0.39 (0.37)	0.62
Spending on Inputs ('000 KSh, past year)	8.71 (9.45)	9.00 (10.69)	0.62
Used agricultural inputs = 1	0.90 (0.30)	0.93 (0.26)	0.07*
Used fertilizer = 1	0.80 (0.40)	0.85 (0.35)	0.02**
Irrigates = 1	0.32 (0.47)	0.35 (0.48)	0.29
Used irrigation pump = 1	0.02 (0.15)	0.03 (0.16)	0.65
Discussed irrigation pump = 1	0.60 (0.49)	0.59 (0.49)	0.72
WTP for manual pump (KSh)	369.30 (302.81)	371.66 (302.50)	0.89
Altruism: Share to village	0.25 (0.17)	0.24 (0.17)	0.21
Altruism: Share to household	0.32 (0.19)	0.33 (0.18)	0.84
Observations	330	3881	4211
Joint Test <i>p</i> -Value			0.827

Observations in Column (1) are those used in the main analysis of the paper. Observations in Column (2) are those from the larger sample. Column (3) reports the p-value of the two-sided t-test comparing mean values from Columns (1) and (2). The Joint Test *p*-value is the p-value from the joint test that the coefficients of all covariates are equal to zero, with standard errors clustered at the village level.

Eliciting Willingness to Pay / Willingness to Accept

Choices CB and TB occurred at the beginning of the survey, in random order. Choice RW came next. Prices were drawn at the end of the three elicitation. Scripts read to each farmer explained that there could be absolutely no bargaining once the prices were drawn.

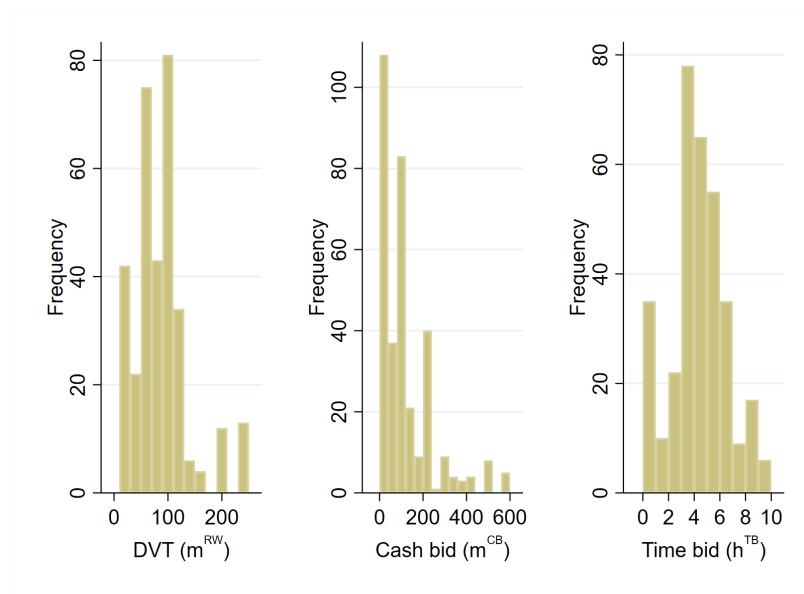
Choice RW (Reservation Wage): Time vs. Money. Each farmer was asked whether they would be willing to perform casual labor for a series of decreasing wages, beginning from 120 KSh/hour and decreasing in 10-KSh/hour increments down to 10 KSh/hour. If the farmer was not willing to work at 120 KSh/hour, we asked for their reservation wage in a single question. Once their reservation wage was determined, it was explained once more that if the wage drawn were 10 KSh lower than their stated reservation wage, they would be unable to take the job. At this point, they were given the option to revise their answer.³

Choice CB (Cash Bid): Money vs. Lottery Ticket. Each farmer was asked whether they would be willing to purchase the lottery ticket for a series of increasing prices, beginning from 20 KSh and increasing in 20-KSh increments up to 500 KSh. If the farmer was willing to pay 500 KSh, we asked for their maximum willingness to pay (WTP) in a single question. Farmers were not aware that there was a price ceiling during the elicitation. Once their WTP was determined, it was explained once more that if the price drawn were 20 KSh higher than their stated WTP, they would be unable to purchase the ticket. At this point, they were given the option to revise their answer.

Choice TB (Time Bid): Time vs. Lottery Ticket. Each farmer was asked whether they would be willing to perform casual labor for the lottery ticket for a series of increasing hours, beginning from 30 minutes and increasing in 30-minute increments up to 6 hours. If the farmer was willing to work for 6 hours, we asked for their maximum WTP (in hours) in a single question. Farmers were not aware that there was an hours ceiling during the elicitation. Once their WTP was determined, it was explained once more that if the price drawn were 30 minutes greater than their stated WTP, they would be unable to purchase the ticket. At this point, they were given the option to revise their answer. Figure C.2.1 shows the distribution of choices in the RW, CB, and TB elicitation.

³Twenty-five percent of farmers declined to place a cash bid for a lottery ticket. We code these as bids of 0 KSh. Ten percent of farmers declined to place a time bid for a lottery ticket. We bottom code these as bids of 1 hour so that the wedge $\hat{\omega}$ is defined. Results are not sensitive to excluding these bids. Nine percent of farmers declined to participate in the day work activity, as we told farmers ahead of time that the maximum possible wage was 120 KSh/hour. For these farmers, we ask their reservation wage directly and top code them at 250 KSh/hour.

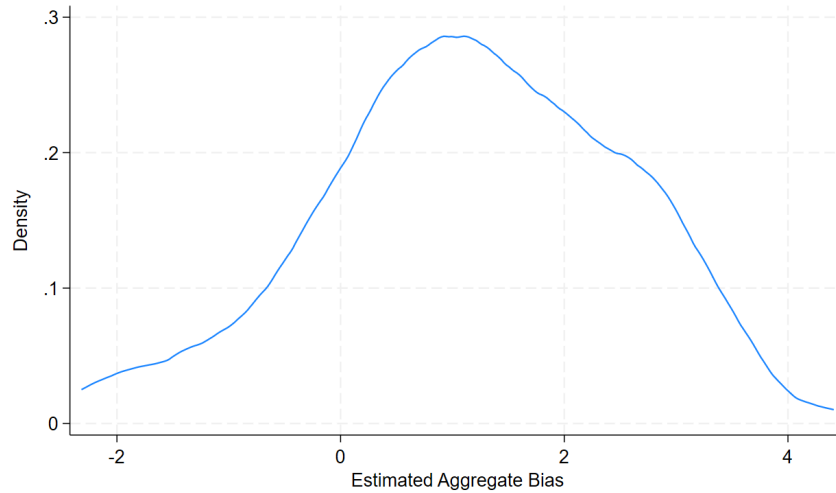
Figure C.2.1: BDM Choices



Currency units are KSh; time units are hours.

Correlations Between Choices. In our data, choices CB and TB are positively correlated, while choice RW is negatively correlated with both CB and TB; see Figure C.2.3.

Figure C.2.2: Distribution of Estimated Aggregate Bias



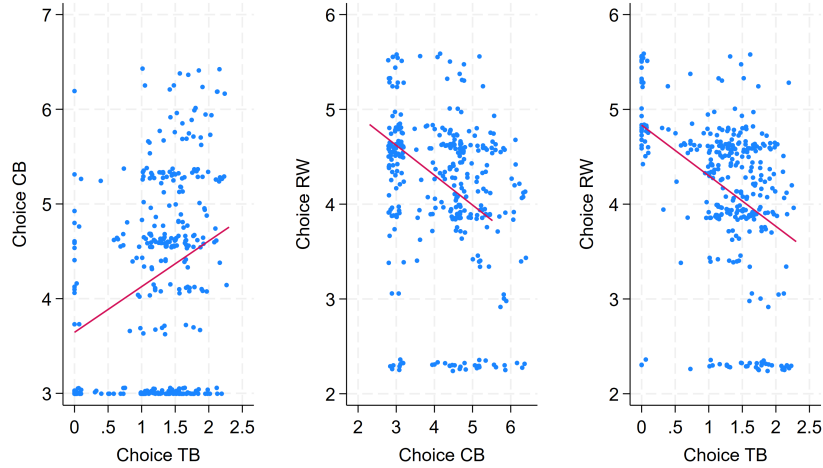
Notes: Kernel density estimate of $\hat{\rho}$. See Theorem 2 for estimation details.

Assignment of Prices

Each village was randomly assigned (by a pseudo-random number generator) to one of three groups: Cash, Cash + Day Work, or Task. Farmers in *Cash* villages received a lottery ticket price payable in cash only, and were not eligible for wage work. Farmers in *Cash + Day Work* villages received a lottery ticket price payable in cash only, and were eligible for wage work. Farmers in *Task* villages received a lottery ticket price payable in hours of work only, and were not eligible for day work. We randomized at the village level to simplify logistics, as this reduced the number of work sites we needed to set up. In practice, the randomization was conducted on a computer prior to the field visit, but farmers did not learn about their assignment until their lottery ticket price was drawn (see the subsection below). Farmers were not told the sample space of assignment types or the level of assignment, only that there was some positive probability that each choice would be used. To reduce the possibility that farmers might share information with each other, we completed all surveys within each village in the same day.⁴

⁴Note that even if farmers did talk during the survey day, in principle this should not affect their choices. Without seeing the results of a high number of price draws, farmers should not infer that price denomination

Figure C.2.3: Correlations Between BDM Choices



Each observation is a farmer with a 3% jitter. OLS line in red. All variables are log transformed.

Lottery Ticket Price and Wage Draws

Each farmer received a random ticket price and a random day work wage. Prices and wages were drawn independently from distributions stratified at the village level. In particular, each farmer was assigned two pseudo-random numbers (one for ticket price and one for wage), and price and wage assignment were based on the within-village percentile of the random price and wage numbers.

Before the survey, we assigned each farmer a random ticket price in either cash or time, and a random cash wage. Farmers were assigned a single ticket price in either cash or time, but not both. Cash wages were assigned independently of ticket price. This information was written on a card and inserted into a sealed envelope, which was shown to the farmer at the beginning of the survey. After the farmer had made their three decision choices, the envelope was opened and the ticket price, payment denomination (cash or time), and wage revealed. Farmer could thus be sure that their choices did not influence the drawn prices.

assignment occurred at the village level.

Cash Collection and Day Work

Cash winners—farmers who drew a cash price weakly lower than m^{CB} —were asked to make a down payment of 20 KSh (\$0.19) at the end of the survey. Approximately one week later, enumerators returned to the village to collect the remaining amount owed. Time winners—farmers who drew a time price weakly lower than h^{TB} —were scheduled for casual work approximately 1 week from the date of the survey. Enumerators returned approximately one week later to transport time winners to and from the job site and monitor their work. Casual jobs for eligible wage workers—farmers who drew an hourly cash wage weakly greater than $m^{RW}/2$ —were scheduled approximately 2 weeks from the date of the survey. Enumerators returned at this time to provide transport and monitoring. Wages were paid immediately upon completion of work.

Compliance was high: 88% of farmers paying cash and 75% of farmers performing casual labor completed their payments or work (see Section C.5.5 for details on compliance). After payments and work were complete, lotteries were held publicly. Farmers who were eligible for a lottery ticket or day work but did not complete payment or show up for work were ineligible for the rest of the study. This was made salient to farmers throughout the elicitation to discourage bids that farmers were not truly willing to accept.

Lotteries

In *Cash* and *Task* villages, lotteries were conducted immediately following collection, at which point farmers were informed that their village had not been selected for day work. In *Cash + Day Work* villages, enumerators returned to the village approximately one week after collection to take eligible day workers to the job site. Lotteries were held immediately following the day work.

Lotteries were held in groups with all present ticket winners. Farmers were ordered randomly from position $n \in \{1, \dots, N\}$, and given a lottery card numbered $c = \text{mod}(n, 10)$. For villages with $\geq N$ ticket winners, a single number between 1 and 10 was drawn and all farmers holding that card won a pump. For villages with fewer than N ticket winners, a single number between 1 and N was drawn to determine the winner. The minimum number of winners per village was therefore 1, and the maximum was $\text{ceiling}(N/10)$.

C.3 Correlates of Wedges

We assess how our estimates of the structural value of time vary based on respondent gender, age, education, income, the presence of a child under 3, and whether someone in the

household operates a micro-enterprise in Table C.3.1. Estimates are highly stable across subgroups, varying from 63–74% of the market wage.

Table C.3.1: The Structural Value of Time Across Subgroups

	Structural value of time (\widehat{SVT})	Market wage (\bar{w})	Relative value of time (\widehat{SVT}/\bar{w})	DVT–IVT wedge ($\widehat{\omega}$)	N
Female participant	47.8 (1.9)	76.0 (2.0)	0.72 (0.04)	0.38 (0.07)	228
Male participant	51.0 (3.4)	93.8 (3.4)	0.63 (0.05)	0.13 (0.15)	104
Under 45 years old	48.6 (2.1)	76.2 (2.5)	0.74 (0.04)	0.38 (0.08)	170
Over 45 years old	48.4 (2.6)	87.2 (2.6)	0.64 (0.04)	0.24 (0.11)	158
Graduated from primary	49.5 (2.7)	89.5 (2.9)	0.64 (0.04)	0.10 (0.12)	133
Did not graduate from primary	47.8 (2.2)	75.3 (2.4)	0.73 (0.04)	0.46 (0.08)	174
Income above median	49.0 (2.3)	81.4 (2.5)	0.69 (0.04)	0.25 (0.10)	166
Income below median	48.6 (2.4)	81.8 (2.6)	0.70 (0.04)	0.35 (0.09)	166
Has child under 3	51.0 (3.2)	85.5 (3.5)	0.73 (0.06)	0.24 (0.13)	103
Has no children under 3	48.6 (2.3)	80.1 (2.6)	0.70 (0.04)	0.28 (0.10)	168
Operates small business	48.1 (2.3)	79.6 (2.7)	0.71 (0.04)	0.23 (0.11)	145
Does not operate small business	49.3 (2.4)	83.0 (2.5)	0.69 (0.04)	0.35 (0.09)	185

Each observation is a farmer. Currency units are Kenyan shillings (1 USD=107 KSh). See Section 3.4 for details on the structural model. Structural value of time is estimated using the full sample. Standard error of the mean in parentheses.

To understand which farmers exhibit larger wedges, and thus what the sources of the wedges likely are, we estimate regressions of the form:

$$y_i = \alpha + X_i' \Gamma + \epsilon_i, \quad (\text{C.1})$$

where y_i is a choice such as the DVT–IVT wedge $\widehat{\omega}$, X_i is a vector of predictor variables, and ϵ_i is an error term. To account for censoring in choices, we estimate (C.1) using Tobit

Table C.3.2: Farmers with greater wedges tend to be younger, less educated, and inexperienced in transacting with cash.

	DVT-IVT wedge ($\hat{\omega}$)	Direct value of time ($m^{RW}/2$)	Indirect value of time (m^{CB}/h^{TB})	Cash bid (m^{CB})	Time bid (h^{TB})
Age	-0.184** (0.092)	-5.4 (4.1)	0.6 (2.9)	-3.6 (9.6)	-0.180 (0.148)
Years of education	-0.286*** (0.094)	-9.7** (4.6)	7.2*** (2.8)	20.2** (8.8)	0.011 (0.152)
Household size	-0.128 (0.085)	1.3 (3.5)	1.6 (2.6)	5.9 (9.0)	-0.012 (0.139)
Female = 1	0.009 (0.196)	-12.5 (8.1)	0.0 (6.1)	-8.9 (20.7)	-0.427 (0.313)
Total income	0.127 (0.084)	5.5 (4.3)	-2.0 (2.5)	-6.5 (9.2)	-0.266* (0.150)
Experience paying in cash	-0.132* (0.076)	-1.9 (3.7)	5.6** (2.6)	24.6*** (9.2)	0.179 (0.154)
Supplies casual labor = 1	-0.357** (0.170)	-19.6*** (6.8)	6.3 (5.4)	40.5** (17.8)	0.944*** (0.257)
Hires casual labor = 1	-0.110 (0.162)	4.9 (7.1)	7.7 (4.9)	19.8 (17.4)	0.203 (0.263)
Considered buying pump = 1	-0.258 (0.184)	-20.9*** (7.5)	-4.3 (5.7)	11.1 (18.6)	0.665** (0.292)
Cash scarce = 1	0.356 (0.216)	-3.4 (8.6)	-6.8 (5.8)	-36.6 (22.2)	0.153 (0.351)
Altruism	-0.072 (0.092)	-6.4** (2.9)	-0.4 (2.2)	3.9 (9.7)	0.130 (0.104)
Overconfidence	-0.051 (0.093)	3.6 (2.9)	4.1* (2.2)	16.5** (7.7)	-0.007 (0.121)
Observations	332	332	332	332	332
Dep. Var. Mean	0.30	82.8	29.8	110.8	4.0

Each observation is a farmer. Currency units are Kenyan shillings (1 USD \approx 107 KSh). Time units are hours. Each column is estimated from a Tobit regression of an auction outcome on a vector of predictors. All non-binary predictors are standardized to mean 0, standard deviation 1. Robust standard errors in parentheses.

models. Table C.3.2 shows results. Table C.3.3 displays bivariate estimates of (C.1). Results are overall very similar across these two specifications.

The characteristics we analyze are not randomly assigned, and so estimates of Γ should not be interpreted as causal. However, recall that in the benchmark model of Section 3.3,

the DVT–IVT wedge $\hat{\omega}$ is invariant to both observed and unobserved farmer characteristics. Characteristics that are non-behavioral—including the farmer’s value of time, valuation of the pump, risk aversion, wealth, and effort cost of providing casual labor—influence both IVT and DVT proportionately. We therefore view estimates of (C.1) as informative of the characteristics of farmers that exhibit larger wedges.

Table C.3.3: Estimates of choice correlations are very similar in bivariate regressions.

	DVT–IVT wedge ($\hat{\omega}$)	Direct value of time ($m^{RW}/2$)	Indirect value of time (m^{CB}/h^{TB})	Cash bid (m^{CB})	Time bid (h^{TB})
Age	-0.035 (0.079)	1.3 (3.6)	-3.1 (2.5)	-19.9** (8.8)	-0.284** (0.134)
Years of education	-0.299*** (0.085)	-7.3* (4.1)	8.3*** (2.6)	30.2*** (8.0)	0.114 (0.147)
Household size	-0.146 (0.089)	0.3 (3.7)	1.7 (2.7)	7.9 (9.4)	0.028 (0.145)
Female = 1	0.263 (0.201)	-5.0 (8.0)	-3.3 (5.5)	-18.3 (19.4)	-0.406 (0.304)
Total income	-0.009 (0.077)	2.8 (4.3)	1.3 (2.5)	8.1 (9.6)	-0.192 (0.148)
Experience paying in cash	-0.213*** (0.076)	-5.3 (3.5)	7.1*** (2.3)	35.4*** (8.9)	0.346*** (0.133)
Supplies casual labor = 1	-0.295* (0.175)	-20.8*** (6.8)	5.3 (5.1)	42.6** (18.0)	1.130*** (0.258)
Hires casual labor = 1	-0.264 (0.170)	3.8 (6.9)	12.5** (5.0)	39.5** (17.7)	0.165 (0.265)
Considered buying pump = 1	-0.489*** (0.177)	-23.2*** (7.3)	4.3 (5.4)	48.8*** (18.4)	0.877*** (0.269)
Cash scarce = 1	0.414** (0.209)	-4.3 (8.8)	-10.5* (5.4)	-50.3** (21.6)	0.188 (0.331)
Altruism	-0.120 (0.093)	-8.7*** (3.2)	0.9 (2.3)	10.9 (9.9)	0.241** (0.104)
Overconfidence	-0.083 (0.093)	1.6 (2.9)	4.6** (2.3)	20.4** (8.1)	0.090 (0.127)
Observations	332	332	332	332	332
Dep. Var. Mean	0.30	82.8	29.8	110.8	4.0

Each observation is a farmer. Currency units are Kenyan shillings (1 USD \approx 107 KSh). Time units are hours. Each column is estimated from a Tobit regression of an auction outcome on a single predictor variable. All non-binary predictors are standardized to mean 0, standard deviation 1. Robust standard errors in parentheses.

To account for experience with the choices in our study—which may reduce self-serving bias—we include an indicator for whether the farmer has recently provided casual labor (a choice similar to Choice RW), and a measure of each farmer’s experience paying for goods in cash (a choice similar to Choice CB).⁵ We find that both measures of experience predict significantly smaller wedges. We also include age, education, and an indicator for hiring casual labor as proxies for experience. We find that older and more educated farmers exhibit smaller wedges. Buyers of casual labor have slightly smaller wedges, but the difference is not statistically significant.

Behavioral phenomena may be amplified by choices which are not well-integrated into reference expectations (Kőszegi and Rabin, 2006; Carney et al., 2019). We include information from prior surveys on whether the farmer’s household had considered buying an irrigation pump in the past. These farmers may have thought more carefully through their willingness to pay for the lottery tickets in our choices, and therefore to be less subject to behavioral phenomena. Indeed, we find smaller wedges on average among these farmers.

A large body of work finds that scarcity affects decision-making (see Mullainathan and Shafir, 2013, for a review). We use a survey-based measure of cash scarcity—whether the farmer reports that they do not have savings to cover a 5,000 KSh (\$47) emergency (Dupas et al., 2018)—to test whether farmers facing scarcity exhibit larger wedges. We find that these farmers do have larger wedges. We also include a measure of total household income, and find that farmers with more income exhibit a slightly larger wedge, though the coefficient is not statistically significant in either specification.

Scarcity can potentially affect decision-making in many ways. One interpretation, following the framework of Shah et al. (2012), is that scarcity focuses attention on immediate needs and away from other economic decisions, making it more difficult to overcome behavioral phenomena. Persistent scarcity—or poverty—may also influence the formation of human capital (see Dean et al., 2017, for a review). Another possibility is that scarcity increases present bias (Schofield, 2014). We do not believe that our results are driven by changes in present bias. In our design, transactions occurred at least one week after the elicitations, with no substantial differences in wait times for cash payments, work, or wages paid.

Scarcity may also create behavioral features by increasing decision stakes (relative to income). Fehr et al. (2022) randomize which of two equally valued items was given to households as compensation for their time. The authors find evidence of exchange asymmetries: when households are offered the opportunity to trade the endowed item for the alternative item at the end of the survey, only 35% of households (far fewer than the 50% predicted by neoclassical theory) trade the endowed item. These asymmetries are lower along several

⁵We compute the first principal component of eight indicators for purchasing capital, such as agricultural inputs, in cash

measures of scarcity. We also find a modestly attenuated wedges among poorer farmers, but much greater wedges among cash scarce farmers.

There is some evidence that women exhibit greater loss aversion than men (Rau, 2014). We find a larger average DVT–IVT wedge among women in a bivariate specification, but the difference is not statistically significant and disappears in the multivariate regression.

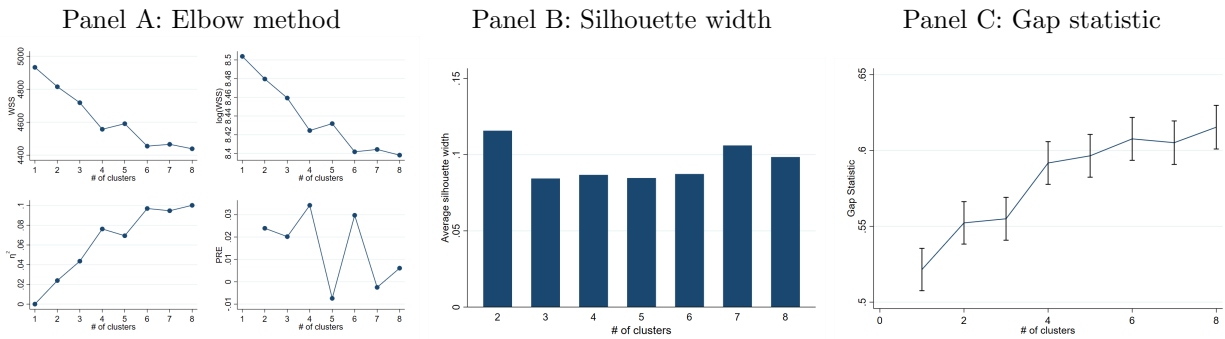
Altruism may mitigate self-serving bias (Di Tella et al., 2015). We find that a measure of altruism—the share donated to an unspecified person in the farmer’s village in a hypothetical dictator game—is associated with smaller wedges, though the difference is not statistically significant.

C.4 Clustering Analysis

To divide our sample into groups of economically similar farmers, we conduct clustering analysis using the partition around medoids (PAM) method with the Gower dissimilarity coefficient (Gower, 1971) implemented by the Stata command *clpam*. We first solve for the optimum number of clusters by inspecting the within sum of squares function, the average silhouette width (see Rousseeuw, 1987), and the gap statistic (Tibshirani et al., 2001) for between 1 and 8 clusters. Figure C.4.1 presents results for each of our 3 criteria. Four clusters is a local maximum of the average silhouette width, produces a kink in the within-cluster sum of squares criterion, and is suggested by the gap statistic method. The following variables are used for clustering: age, years of education, a female dummy, a dummy for having no male head of household, household size, the number of children under 18 in the household, area of land cultivated, farming income, non-farm income, a dummy for whether the household irrigates, a measure of uncertainty aversion, measures of intra-household and intra-village altruism, a cash-scarcity dummy, two dummies for supplying or hiring casual labor, a dummy for being experienced paying for capital in cash, 6 occupation dummies, a measure of overconfidence, and a measure of network centrality.

To describe these clusters, we characterize them using post-LASSO OLS regressions (Tibshirani, 1996; Belloni and Chernozhukov, 2013) of membership in the cluster on the set of control variables used to construct the clusters (see Table C.4.1). We also show results from the same process for the the two subgroups of interest in Table 3.3.

Figure C.4.1: Optimal clustering criteria suggest $n = 4$ groups for cluster analysis.



Cluster analysis performed using partition around medoids (PAM) using the Gower dissimilarity coefficient. See Rousseeuw (1987) for a description of the silhouette method, and Tibshirani et al. (2001) for a description of the gap statistic method. “WSS” is the within sum of squares. $\eta_k^2 = 1 - \frac{WSS(k)}{WSS(1)}$. “PRE” is the proportionate reduction in error, given by $PRE_k = \frac{WSS(k-1) - WSS(k)}{WSS(k-1)}$.

Table C.4.1: Characteristics of Farmer Subgroups

	(1)	(2)	(3)	(4)	(5)	(6)
	Cluster breakdown					
	Low-skill self- employed	Low-skill employees	Hires casual workers	Older, low-edu households	Casual laborers	Considered buying pump
Farmer characteristics						
Years of education			0.08	-0.11		
Age				0.07		
No male head in household						-0.10
Crop income						0.09
Casual laborer		0.21	-0.06	-0.09	-	
Hires casual labor	0.07	-0.12	0.16	-0.08		
Often pays in cash	0.09	-0.14	0.12	-0.07		-
Irrigates	0.10	0.14	-0.16	-0.09		
Agricultural employee		0.04			0.14	
Low-skill self-employed	0.20		-0.12			
Within-household altruism		0.10				
Network centrality						0.12
Observations	332	332	332	332	332	332

Each observation is a farmer. Columns (1)–(4) show results estimated separately within clusters of similar farmers (see Section 3.4.3). Column (5) shows results estimated on farmers who performed casual labor within the past 3 months. Column (6) shows results estimated on farmers who report that they have considered buying a MoneyMaker irrigation pump. Each column shows OLS coefficients from LASSO regressions of a dummy variable equal to 1 if the farmer is a member of the corresponding subgroup. All variables are standardized to have mean 0, standard deviation 1.

C.5 Details on Alternative Potential Models

This appendix provides additional details on the alternative models discussed in Section 3.5.3.

C.5.1 First-Order Effects of Credit or Labor Constraints

First-order effects of credit or labor constraints are incorporated into our benchmark model, and thus, cannot explain a wedge between DVT and IVT. If a farmer is credit constrained, they will have a high shadow value of money, but this will be reflected in both their IVT and DVT equally through the value of money $V_{m,i}$. In particular, a higher shadow value of money will lower both a farmer’s willingness to pay for a lottery ticket m^{CB} , as well as their reservation wage m^{RW} . This will lower both IVT and DVT equally, resulting in no wedge between the two. Moreover, we gave farmers about one week to pay, so that they were not constrained by their cash on hand the day they made their bid. Villages where farmers had longer to raise their cash payment—due to quasi-random variation in the scheduling of our field visits—do not exhibit significantly different compliance or willingness to pay in cash, suggesting that cash-on-hand constraints are not significantly affecting farmers’ choices.⁶

C.5.2 Second-Order Effects of Credit or Labor Constraints

A possible way to explain the DVT–IVT wedge ($\hat{\omega}$) relies on the second-order effects of credit constraints. This explanation leads to a testable implication in our data—namely that reservation wages should be negatively correlated with $\hat{\omega}$. However, as shown in Panel 4 of Figure 3.2, the DVT–IVT wedge is positively correlated with the reservation wage. This subsection formalizes a model of second-order credit constraints that drives $\hat{\omega}$, and the (falsified) implication above. This finding is not surprising, as farmers have many opportunities for useful investment, and are likely already credit constrained when we offer them our choices. Additionally, farmers had 1–2 weeks to plan their schedules and find cash needed for payment, implying that average within-day changes in working hours are likely to be small, and that farmers were not constrained by cash-on-hand on the day of the choices.

A second-order effect of credit constraints implies that the shadow cost of capital changes in response to the choice to purchase (or not) the lottery ticket. To model this, we allow for different Lagrange multipliers associated with the credit constraint κ , depending on whether

⁶The coefficient from a regression of a dummy for compliance on the number of days the farmer had to obtain cash is -0.006 (p -val = 0.67). The coefficient from a regression of willingness to pay in cash for the ticket on the number of days the farmer had to obtain cash is 1.9 KSh (p -val = 0.65).

the decision maker can purchase a lottery ticket $\tau \in \{0, 1\}$, denoted by $\kappa(\tau)$. Our main analysis implicitly assumes that $\kappa(\tau = 0) \simeq \kappa(\tau = 1)$. Here, we entertain the possibility that $\kappa(\tau = 0) \neq \kappa(\tau = 1)$. That is, we allow the marginal value of money to depend on τ because of a large change (or jump) in how binding the borrowing constraint is:

$$V_{m,i}(\tau) = -v'_0 - \zeta_i \kappa(\tau)$$

in which $\zeta_i \geq 0$ represents how tight credit constraints are, in general, for person i . As a reminder, $v'_0 \equiv v'(I + wl|_0 - c|_0)$, with the 0 subscript indicating that these are the choices and values for $(\tau, h, m) = (0, 0, 0)$.

DVT and *IVT* are then

$$DVT \equiv \frac{V_h}{V_m(\tau = 0)} \quad \text{and} \quad IVT \equiv \frac{V_h}{V_m(\tau = 1)}$$

so that

$$\widehat{\omega} = 1 - \frac{IVT}{DVT} = 1 - \frac{V_m(\tau = 0)}{V_m(\tau = 1)} = \zeta_i \frac{\kappa|_1 - \kappa|_0}{v'_0 + \zeta_i \kappa|_1}. \quad (\text{C.2})$$

As noted in many places in the text, $\widehat{\omega}$ tends to be significantly larger than zero. This can be rationalized by $\kappa|_0 < \kappa|_1$.⁷

Can population level variation in the tightness of credit constraints rationalize patterns of choice at the population level? An implication of (C.2) is that $\widehat{\omega}$ is increasing in ζ_i , the tightness of credit constraints. This is consistent with our data: the value of $\widehat{\omega}$ is greater on average for respondents who self-report being unable to find enough cash to cover an emergency, as shown in Table C.3.2.

A further implication of this model is that a participant's reservation wage,

$$\frac{m_i^{RW}}{2} = \frac{V_h}{V_m^i(\tau = 0)} = \frac{-V_h}{v'_0 + \zeta_i \kappa|_0}$$

should be decreasing in ζ_i (as $V_h < 0$): participants who are more credit constrained should also have lower reservation wages.

As $\widehat{\omega}$ is increasing in ζ_i , and m_i^{RW} is decreasing in ζ_i this implies that the DVT–IVT wedge and reservation wages should be negatively correlated. This is not the case empirically: as highlighted in Panel 4 of Figure 3.2, the DVT–IVT wedge is strongly positively correlated with a participant's reservation wage.

⁷In order for a participant to have $\widehat{\omega} < 0$ in this model, they would need $\kappa|_0 > \kappa|_1$; that is, the shadow value of capital is higher when they have not purchased the ticket, which is extremely difficult to rationalize. Our data suggest that $\widehat{\omega} < 0$ instead represents people who have a very high value of the lottery ticket, but do not have a correspondingly high value of choice TB due to time cost convexities at very high hours.

C.5.3 Uncompensated Costs of the Work Activity

Conceptually, the value of time is a comparison of the values of two possible activities, and thus depends on which activities are being compared. For example, if work effort is costly, farmers will require a lower payment to sit idly than they would to work for the same amount of time. Applied to interventions that affect working hours, the correct measure of the value of time is thus the one that accounts for the real-world disutility of effort. With this in mind, we designed the work activity to be as commonplace as possible: work involved casual agricultural tasks which are extremely common in this context. The short-term nature of the contract was also typical: in our data, the median real-world casual labor contract lasts for 12 hours spread over 3 days.

One possible explanation for the observed wedge between the direct and indirect values of time is that farmers viewed the two task activities differently. We do not think this can explain our results. The two activities were designed to be as similar as possible: they involved the same type of work and were monitored the same way. If effort costs are convex in labor supply (for example, because of increasing marginal fatigue), then the average effort cost per hour of work for a wage may differ than the effort cost of work for the lottery ticket. However, time bids for the ticket were on average greater than the fixed length of the day-work contract (4 hours versus 2 hours), so any convexity in effort costs will cause us to underestimate the true wedge. In principle, farmers may anticipate expending less effort on the task than on the wage activity, which would deflate our measure of IVT relative to DVT. However, we do not find significant differences in work quality—as measured by field staff (see Section C.6 for more details)—across the task and wage activities: 31% of farmers in both groups performed work rated as a 5 out of 5 on the quality scale, and the share performing work rated 4 or better was 90% for the wage activity and 82% for the task activity.

Uncompensated scheduling and transportation costs may also matter: farmers must make room in their schedule to attend the task day, and spend time traveling between their home and the pickup site in the village center (transport between villages was provided, and included in total work time). Work days were scheduled 1 to 2 weeks in advance so that farmers could reshuffle tasks across days, implying that within-day changes in working hours should be marginal.⁸

If some component of transport costs is not observed—for example, some people live farther than others—the benchmark model implies restrictions on farmers’ choices. To see this, denote by f the unobserved fixed cost, in hours, of participating in the work activity.

⁸Task days for lottery tickets were scheduled on average one week out from the elicitations; task days for a wage were scheduled on average two weeks out from the elicitations. Assuming that rescheduling is more costly the sooner the event, differential scheduling costs should lead us to underestimate the true wedge.

Then, for any f , farmers with time bids $h^{TB} = 2$ will satisfy $DVT = IVT$, because the fixed cost distributed over total work time is identical across the two work activities.⁹ Our data clearly reject this constraint. The average value of $1 - \hat{\omega} = IVT/DVT$ among farmers who bid exactly $h^{TB} = 2$ is $1 - \hat{\omega} = 0.25$; among farmers who bid $h^{TB} \in [1.5, 2.5]$ it is $1 - \hat{\omega} = 0.32$; and among farmers who bid $h^{TB} \in [1, 3]$ it is $1 - \hat{\omega} = 0.27$. These are all very close to the unconditional average $1 - \hat{\omega} = 0.30$.

More generally, the indifference conditions defining optimal bids become

$$\begin{aligned} m^{CB} V_m &= -(h^{TB} + f) V_h \\ m^{RW} V_m &= -(2 + f) V_h. \end{aligned}$$

This implies, for f within $[0, \bar{f}]$,

$$\frac{m^{CB}}{m^{RW}} = \frac{h^{TB} + f}{2 + f} \begin{cases} \in \left[\frac{h^{TB} + \bar{f}}{2 + \bar{f}}, h^{TB}/2 \right] & \text{if } h^{TB} > 2 \\ \in \left[h^{TB}/2, \frac{h^{TB} + \bar{f}}{2 + \bar{f}} \right] & \text{if } h^{TB} < 2 \\ = 1 & \text{if } h^{TB} = 2. \end{cases}$$

This restriction is violated for many farmers even for large values of \bar{f} :

- Most homes are well within a 15 minute walk from the center of the village. Setting $\bar{f} = 0.5$, the restriction is violated for 90% of farmers.
- Setting $\bar{f} = 1$, the restriction is violated for 87% of farmers.
- Setting $\bar{f} = 2$, the restriction is violated for 84% of farmers.

That is, even very large unobservable fixed costs of work cannot explain the choices of a vast majority of the farmers in our study.

A related possibility is that the relevant unit for selling casual labor is longer than an hour (in our data, 3-, 4-, and 5-hour contracts are particularly common), implying that the RW activity indirectly requires a sacrifice of more than 2 work hours. However, note from Table 3.1 that selling casual labor is not common on a day-to-day basis: the average farmer sells casual labor on only 6% of days. The remaining time is largely spent on tasks that are more easily divisible, such as household chores and own-farm work.

⁹Recall this is because the work activity in choice RW is two hours for all farmers.

Table C.5.1: We find no evidence that stigma against low wage offers affects DVT.

	<i>Dep. Var: DVT ($m^{RW}/2$)</i>		
Belief about low-wage worker			
Should feel ashamed = 1	-0.3 (5.9)		
Angry at worker = 1		0.1 (6.5)	
Proud of worker = 1			10.9 (5.5)
Belief about low-wage employer			
Should feel ashamed = 1	-1.9 (6.2)		
Angry at hirer = 1		6.9 (6.9)	
Proud of hirer = 1			0.8 (5.9)
Observations	332	332	332
Dep. Var. Mean	82.8	82.8	82.8

An observation is a farmer. Currency units are Kenyan shillings (1 USD \approx 107 KSh). Each coefficient estimated from a regression of a farmer’s DVT on an indicator for whether they answered that a hypothetical worker accepting (or an employer offering) a low wage should feel shame, anger, or pride. Robust standard errors in parentheses.

C.5.4 Stigma of Accepting Low Wages

We do not find that low-wage work is commonly stigmatized in this context. In our survey questions eliciting emotional responses to low-wage work, 81% of respondents said that they did not think the worker should feel any shame at all and 83% said that they did not feel any anger at all toward the worker. Positive responses were more common: 67% report feeling “very proud” of the worker. Responses about the employer are similar. Responses are generally uncorrelated with the DVT (see Table C.5.1): the only statistically significant coefficient appears on those who report feeling proud of the low-wage worker (coeff = 11 KSh ; p -val = 0.05).

C.5.5 Non-Compliance and Censoring

If farmers inflate their cash or time bids above their willingness to pay—or deflate their reservation wages below their willingness to accept—while intending to later renege by not making the payment or completing work, this could appear as a wedge. Table C.5.2 presents regression estimates testing for systematic differences in the choices of farmers who reneged on a transaction (that is, did not comply). Compliance is uncorrelated with Choices in TB (coeff. = 0.16 hours on a base of 4.8; p -val = 0.76) and RW (coeff. = 0.6 KSh/hour on a base of 46; p -val = 0.95), but positively correlated with Choice CB (coeff. = 49 KSh on a base of 184; p -val = 0.02). The positive correlation between choices in CB and compliance is the opposite of what we expect to see if participants were giving artificially high bids with the intention of not honoring them. These correlations—together with our finding from our estimation results that cash bids appear deflated and reservation wages inflated—suggest that most farmers are not planning on renegeing when making their choices. To further test whether our estimates are influenced by renegeing, we restrict our sample to farmers with high predicted compliance in all 3 activities in Table C.5.3.¹⁰ This restriction results in minimal change to our estimates.

Allowable choices in our sample were bounded above and below, raising concerns that censoring may be influencing our estimation results. A quarter of farmers chose 0 KSh in Choice CB, 10% chose 0 hours in Choice TB, and 3% express an extremely high reservation wage (more than 10x the sample median). These choices may, in part, reflect transaction costs of participating in the activities. In our main analysis, we bottom code cash and time bids at 20 KSh and 1 hour respectively, and top code reservation wages at 250 KSh/hour (the 97th percentile).¹¹ Our estimates are not sensitive to this recoding, as shown in Appendix Table C.5.4, which presents estimates of $\hat{\gamma}^{RW}$, $\hat{\gamma}^{CB}$, and $\hat{\gamma}^{TB}$ under alternative recoding strategies. Our estimates of SVT vary from 57–60% of the market wage across these strategies.

¹⁰We do not observe compliance for every farmer. We only observe compliance in cash and task for those with a sufficiently high bid given the random price, and who were randomly offered a price in cash or hours of work. We only observe compliance in the reservation wage activity for those with sufficiently low reservation wages given the random wage, and whose villages we visited for work—a random subset of all villages. We therefore predict compliance with a probit regression of compliance on the three choices m^{RW} , m^{CB} , h^{TB} fitted on those for whom we observe compliance, and then re-estimate our results on the restricted sample of farmers with at least 50% predicted compliance in all three choices.

¹¹These bids of 0 likely represent truly low willingness to spend cash on the ticket, as opposed to an implicit decision to opt out of the study, meaning that our imputations represent a very small adjustment to true willingness to pay. Very few farmers (11/332) placed a bid of zero in cash and task, and expressed a reservation wage above 120, the highest wage we offered. Of these, 8 expressed a reservation wage between 120 and 250.

Table C.5.2: Non-compliance does not explain our results.

	Cash bid (m^{CB})	Time bid (h^{TB})	Direct value of time ($m^{RW}/2$)
Complied = 1	48.9 (21.2)	0.16 (0.50)	0.6 (9.4)
Observations	118	83	39
Dep. Var. Mean	184.49	4.76	46.41
Compliance rate	0.88	0.75	0.74

An observation is a farmer who was eligible for a ticket to be paid in cash or time, or who was eligible and randomly selected for day work. Currency units are Kenyan shillings (1 USD \approx 107 KSh). Time bid measured in hours. Each column reports estimates from a regression of an auction choice on a dummy for compliance, defined as completing payment or work. Robust standard errors in parentheses.

Table C.5.3: Value of time estimates are robust to excluding farmers with low predicted compliance.

	Mean	Std. Dev.	p25	p50	p75	N
Direct value of time ($m^{RW}/2$)	86	54	50	80	100	298
Indirect value of time (m^{CB}/h^{TB})	32	36	7	24	44	298
Cash bid (m^{CB})	122	127	20	100	180	298
Time bid (h^{TB})	4.2	2.1	3.0	4.0	5.5	298
DVT–IVT wedge ($\hat{\omega}$)	0.29	1.17	0.20	0.67	0.92	298

Each observation is a farmer with a predicted compliance above 50% for all three auctions. Currency units are Kenyan shillings (1 USD \approx 107 KSh). p25, p50, and p75 are the 25th, 50th, and 75th percentiles.

Table C.5.3 shows BDM choices in the restricted sample of farmers with high predicted compliance in all 3 activities. The effects of this restriction on our estimates are generally very small.

Table C.5.4 shows that our estimates of $\hat{\gamma}^{RW}$, $\hat{\gamma}^{CB}$, and $\hat{\gamma}^{TB}$ are not sensitive to alternative recoding strategies to handle censoring.

Table C.5.4: Value of time estimates are not sensitive to recoding of choices.

	(1)	(2)	(3)	(4)	(5)
	Full sample	Farmers with ≥ 1 eligible choice	Recoding DVT only	No recoding	No recoding + exclude negative wedges
Reservation wage	0.39	0.38	0.45	0.44	0.28
relative intensity ($\widehat{\gamma}^{RW}$)	(0.023)	(0.023)	(0.031)	(0.032)	(0.040)
Cash bid	0.61	0.62	0.55	0.56	0.72
relative intensity ($\widehat{\gamma}^{CB}$)	(0.025)	(0.024)	(0.031)	(0.033)	(0.040)
Time bid	0.00	0.00	0.00	0.00	0.00
relative intensity ($\widehat{\gamma}^{TB}$)	(0.014)	(0.011)	(0.005)	(0.005)	(0.004)
Structural value of time (\widehat{SVT})	49	49	47	46	54
	(2.5)	(2.4)	(2.4)	(2.3)	(3.3)
Market wage (\bar{w})	82	82	80	80	77
	(1.8)	(1.8)	(2.2)	(2.2)	(2.5)
Relative value of time (\widehat{SVT}/\bar{w})	0.60	0.60	0.59	0.57	0.71
	(0.034)	(0.032)	(0.034)	(0.033)	(0.048)
Observations	332	329	231	221	166

Each observation is a farmer. Currency units are Kenyan shillings (1 USD \approx 107 KSh). See Section 3.4 for details on the structural model. This table shows sensitivity of our results to recoding of lottery choices, with increasing strictness moving from left to right. Column 5 shows results among farmers with non-negative wedges and no recoding of bids. Column 4 adds farmers with negative discount rates. Column 3 adds farmers with stated DVT greater than 120 KSh/hour. Column 2 bottom-codes cash and time bids and 20 KSh and 1 hour respectively, restricting to the set of farmers who placed at least 1 eligible bid (defined as a positive cash or time bid, or a DVT less than or equal to 120 KSh/hour) across the three lotteries. Column 1 recodes bids for all farmers. All regressions include controls for unincentivized proxies of the value of time and the valuation of the lottery ticket. Bootstrap standard errors in parentheses.

C.5.6 BDM Comprehension

Four pieces of evidence suggest that effects of the BDM design, such as a lack of comprehension by participants, are not driving the intransitivities we observe. We find no significant order effects when we randomize the sequence of the BDM elicitations, we find no evidence that farmers are anchoring their choices either to the prevailing wage or to the starting points of the BDM procedure, very few farmers took the opportunities we offered them to revise their bids, and very few farmers expressed regret about their choice after the random price

was drawn.

Order Effects Across Elicitations. If respondents do not understand the BDM design, we would expect them to lower their expressed willingness to pay. To test for this, we randomized the order of the cash and time BDMs. The wage work BDM always came third. Table C.5.5 shows the effect on choices of the randomized order of the cash BDM. We find no significant order effects. If there are unobserved order effects in the wage work BDM, we expect these to put downward pressure on DVT relative to IVT, since as participants become more familiar with the elicitation, their willingness to accept should approach their structural value of time.

Anchoring Within Elicitation. If respondents do not understand the BDM design—or prefer not to think carefully about their answers—we would expect their responses to be highly influenced by readily available anchors, such as the market wage. To test for anchoring effects, we asked farmers what the typical wage is for casual agricultural work in their village and regress their choices on their perception of the typical wage. Table C.5.5 shows results. Although time bids are modestly lower for those who report a high typical wage, we find no evidence of anchoring effects on either measure of the value of time.

A distinct form of anchoring could arise if farmers anchor their choices to the starting point of the BDM procedure (120 KSh/hour in choice RW, 20 KSh in choice CB, and 0.5 hours in choice TB). We do not find any evidence of this form of anchoring. There is no excess mass around the starting points (8% of farmers choose 120 KSh/hour in choice RW, 8% choose 20 KSh in choice CB, and 1% choose 0.5 hours in choice TB), and the SVT estimated after dropping these farmers is 58% of the market wage, which is very close to the 60% estimated within the full sample.

A less extreme version of anchoring to starting points is *starting point bias*: farmers' choices may be lower in an ascending than a descending multiple price list (Andersen et al., 2006; Jack et al., 2022). Our BDM design used descending wages in choice RW and ascending prices in choices CB and TB so that in all choices, participants would start by answering “Yes” before switching to “No.” Because the IVT is constructed from the ratio of choices in elicitation CB and TB, any starting point bias proportionate to the underlying valuation (for example, a 20% deflation) will not influence the IVT. Relative to an ascending design for all three choices, our design will underestimate DVT—and therefore understate the magnitude of behavioral features—if respondents exhibit starting point bias.

Choice Revision and Regret. In addition to explaining the rules at several points throughout the process, we gave farmers the opportunity to revise each of their choices at the end of the elicitation but before the random price was drawn, after again explaining

Table C.5.5: We find no evidence that risk aversion, order effects, or anchoring to typical wages drive our results.

	DVT-IVT wedge ($\hat{\omega}$)	Direct VoT ($m^{RW}/2$)	Indirect VoT (m^{CB}/h^{TB})	Cash bid (m^{CB})	Time bid (h^{TB})
Risk averse = 1	-0.064 (0.132)	-2.5 (6.1)	3.1 (3.8)	-5.9 (13.5)	-0.50 (0.24)
Cash auction appeared first = 1	0.146 (0.130)	0.2 (6.0)	-0.1 (3.8)	-10.3 (13.6)	-0.31 (0.24)
Perceived typical wage	-0.104 (0.072)	-0.0 (2.9)	2.6 (2.1)	2.7 (6.5)	-0.25 (0.12)
Observations	332	332	332	332	332
Dep Var Mean	0.300	82.75	29.80	110.8	4.012

An observation is a farmer. Currency units are Kenyan shillings (1 USD \approx 107 KSh). Each column reports estimates from a regression of an auction choice on three predictors. “Risk averse” is a dummy = 1 if the farmer reports a willingness to take risks below the sample median. “Cash auction appeared first” is a dummy = 1 if the cash bid was elicited prior to the task bid (the order was randomized prior to the survey). “Perceived typical wage” is the wage the farmer reports as typical for casual agricultural work in their village and is standardized to have mean 0 and standard deviation 1. Robust standard errors in parentheses.

what would happen if the drawn price was higher or lower than their choice. Fewer than 2% of respondents decided to revise their answer at this point—and this share does not vary based on the random order of the choice in the survey—suggesting that farmers understood the choices they were making.

Additionally, we asked farmers whose CB or TB choices were lower than the random price (or whose RW choice was higher) whether they wished they had chosen a different price. Only 6% said that they did, again suggesting that farmers understood the choices they had made. Finally, the robustness of our estimates of the relative value of time across subgroups of farmers (see Table 3.3) is also inconsistent with lack of comprehension driving our results.

C.6 Evidence of Shading

We test for shading in ex-post performance resulting from wage deviations below reference points (Fehr et al., 2011; Hart and Moore, 2008). We rely on the random variation in hourly wages for casual work in choices RW and TB combined with a survey-based measure of

the wages farmers expect they could earn from similar casual contracts. Specifically, we test whether farmers perform lower-quality work—as measured by field staff after work was completed—when they receive a lower random wage, and whether this effect is pronounced below the worker’s expected wage for casual work. For example, in the RW choice, the wage paid for day work is random, and—because only those who drew a wage higher than their reservation wages were eligible to work—eligibility is random conditional on DVT.

We find evidence of shading at lower wages, but only for wages below reference points and only when the farmer is working for a cash wage as opposed to a set reward. We first regress a binary measure for whether the quality of work was above the median rating on the random cash wage, controlling for the DVT and a worksite fixed effect.¹² We find modest shading on average: a 10-KSh per hour increase in the wage increases the probability of high-quality work by 6 percentage points (pp., p -val = 0.10). Next, we control for a binary variable indicating whether the random wage was strictly below the worker’s expected wage for casual work, plus the same binary variable interacted with the random wage, to test whether shading is pronounced below workers’ reference points. We find that it is: for wages below workers’ reference points, a 10-Ksh per hour increase in the wage increases the probability of high-quality work by 38 pp. (p -val < 0.01). Because all workers receiving cash wages above their reference points performed work rated 5/5 by monitors, we cannot distinguish between a level effect and a kink in shading above the reference point. Because the random wage may affect the decision to show up for work, we add controls for predicted compliance (see Section C.5.5).¹³ We find no evidence of shading for farmers performing casual work for the lottery ticket. This suggests that, when paying cash, employers may pay a higher wage to increase the average quality of work, leading to fewer jobs at higher wages.¹⁴

¹²Average wages paid in the RW choice were 70 KSh/hour; the average effective wage—defined as the average ticket valuation V_τ divided by the required number of work hours—in the TB choice was 57 KSh/hour. Work quality was measured on a 5-point scale. The median quality report was a 4.

¹³Work days took place 1–2 weeks after farmers made their choices, so the decision to show up on the work day may be affected by the random wage draw. We see no evidence of this: regressing a dummy for showing up to work on the random wage draw—controlling for the reservation wage and a worksite fixed effect—yields small, insignificant coefficients: 3 percentage point per 10-KSh increase for wage workers, p -val = 0.47; –1 percentage point per 10-KSh increase for task workers, p -val = 0.73.

¹⁴Shirking—which occurs because of imperfect monitoring and can lead employers to pay higher wages (Shapiro and Stiglitz, 1984)—or gift exchange—when norms lead workers to “exchange” a higher work standard for wages above the market rate (Akerlof, 1982)—could also generate the results in the first column. However, it is not clear that that they could generate the heterogeneity based on reference points.

Table C.6.1: Evidence of shading at lower wages

	<i>Dep. Var.: Performed high-quality work = 1</i>					
	Working for cash wage		Working for lottery ticket			
Random wage ($\times 10$ -KSh / hour)	0.062 (0.036) [0.10]	-0.005 (0.015) [0.76]	0.000 (0.015) [0.98]	0.018 (0.016) [0.26]	0.017 (0.022) [0.43]	0.018 (0.022) [0.42]
Random wage \times wage < reference		0.351 (0.055) [0.00]	0.354 (0.054) [0.00]		0.003 (0.040) [0.93]	0.005 (0.041) [0.91]
Wage < reference		-0.379 (0.102) [0.00]	-0.372 (0.105) [0.00]		0.006 (0.065) [0.92]	0.007 (0.066) [0.91]
Observations	29	29	29	62	62	62
Control for predicted compliance?			X			X

An observation is a farmer who showed up to the task day to perform work for wages (first three columns) or for a lottery ticket (next three columns). The dependent variable is a dummy = 1 if the monitor judged the worker to have performed work quality at or above the sample median. “Random wage” refers to the random hourly wage paid in Choice RW (cash wage) or average ticket valuation V_τ divided by the random number of work hours in Choice TB. “Wage < reference” is a dummy = 1 if the worker’s random wage was strictly below the hourly wage they expected they could earn from casual work. All wage units are in 10-KSh-increment deviations from mean. Predicted compliance estimated using choices m^{RW} , m^{CB} , h^{TB} (see Section C.5.5). All regressions control for the DVT (first three columns) or the task bid h^{TB} (next three columns). All regressions control for a worksite fixed effect (each village was assigned one worksite). Robust standard errors in parentheses.

C.7 Literature Review Details

C.7.1 JEL Codes

To relate our work to the existing literature, we reviewed all papers published between 2016 and Spring 2021 in the *American Economic Review*, *Quarterly Journal of Economics*, *Econometrica*, *Review of Economic Studies*, *Journal of Political Economy*, *Journal of Development Economics*, *American Economic Journal: Applied Economics*, *American Journal of Agricultural Economics* and the *European Review of Agricultural Economics* which were listed under the following 45 *JEL* codes: C91, C93, C99, D00, D01, D10, D13, D60, D90, I00, I15, I30, I31, I32, I38, I39, J00, J01, J20, J22, J30, J38, J40, J43, J46, O00, O01, O12, O13, O14, O15, O17, O22, O30, Q00, Q01, Q10, Q11, Q12, Q13, Q14, Q16, Q18, Q19. Papers found through this search were supplemented with other papers the authors were aware of from earlier time periods and/or selected other journals.

C.7.2 Re-Analysis of Prior Studies

Table C.7.1 shows the underlying data we used to generate Figure 3.5. It shows the change in household labor use in each study—as a percentage of household labor in the control group—along with 95% confidence intervals. We then estimate the change in profits at different values of time discussed in the text, from 0 to the market wage \bar{w} .

To understand the sensitivity of the results in the table to estimates in the change of household labor, one can use the confidence interval on estimated household labor to adjust the range of profit estimates. For example, in Callen et al. (2019), the point estimate on the change in household labor is around -1% , and the range of changes in profits is about 3 pp. At the bottom end of the confidence interval on household labor (-9%) the range would thus be 27 pp., implying that their intervention increased profits by 74% if labor is valued at the market wage \bar{w} . On the other hand, if labor changed by an amount similar to the top-end of the confidence interval (8%), then the range of profit estimates would be -24 pp., suggesting that the intervention only increased profits by 23% if labor is valued at the market wage.

Table C.7.1: Literature Sensitivity to Varying Values of Time

	(1)	(2)	(3)	(4)	(5)	(6)
	% Change	Daily Wage	% Δ Profit for Different Values of Time			
	HH Labor	US PPP	0	$0.4\bar{w}$	$0.6\bar{w}$	\bar{w}
Jones, Kondylis, Magruder, Loeser (WP 2021)	731% [310%, 1152%]	2.69	43%	18%	5.2%	-20%
Oyinbo, Chamberlin, Abdoulaye, Maertens (AJAE 2021)	37%	6.04	17%	16%	15%	14%
Vandercasteelen, Dereje, Minten, Taffesse (ERAE 2018)	27% [17%, 37%]	5.26	9.2%	6.4%	5.0%	2.3%
Baird, Hicks, Kremer, Miguel (QJE 2016)	25% [10%, 46%]	3.41	32%	18%	11%	-3.0%
Barrett, Islam, Malek, Pakrashi, Ruthbah (AJAE 2021)	24% [7.8%, 41%]	11.68	28%	25%	24%	22%
Fink, Jack, Masiye (AER 2020)	9.3% [-0.33%, 19%]	6.53	6.0%	2.6%	0.6%	-3.0%
Karlan, Osei, Osei-Akoto, Udry (QJE 2014)	7.5% [-5.2%, 20%]		-15%	-25%	-29%	-39%
Emerick, de Janvry, Sadoulet, Dar (AER 2016)	6.7% [3.1%, 10%]	10.35	16%	14%	14%	12%
Beaman, Karlan, Thuysbaert, Udry (AER P&P 2013)	4.8% [-9.4%, 19%]	2.03	-3.6%	-4.9%	-5.5%	-6.8%
de Mel, McKenzie, Woodruff (AEJ:AE 2019)	3.7% [-3.2%, 11%]	10.30	4.1%	3.2%	2.7%	1.8%
Schilbach (AER 2019)	0.44% [-10%, 10%]	2.69	2.6%	2.3%	2.2%	2.0%
Van Campenhout, Spielman, Lecoutere (AJAE 2020)	-0.45% [-11%, 10%]	4.72	13%	14%	14%	15%
Callen, de Mel, McIntosh, Woodruff (ReStud 2019)	-0.87% [-9.5%, 7.7%]	10.46	47%	48%	48%	49%
Michler, Tjernstrom, Verkaart, Mausch (AJAE 2018)	-3.6% [-8.3%, 1.0%]	6.72	28%	29%	29%	29%
Goldstein, Udry (JPE 2008)	-7% [-28%, 14%]		66%	68%	69%	71%
Ahmed, Hoddinott, Abedin, Hossain (AJAE 2021)	-10% [-21%, 1.3%]	11.30	128%	141%	147%	160%

Column 1 reports the normalized treatment effects on household and self-employed labor relative to the control group. 95% confidence intervals in brackets. Column 2 displays the daily market wage converted from local currency units to USD using PPP conversion factor from the time of the intervention. Columns 3 - 6 show normalized profits under varying values of self-employed time, as in Figure 3.5. Normalized profits are Δ Profits / $\mathbb{E}[\text{Profit} | \text{Treat} = 0, \text{VoT} = 0]$. Bolded estimates represent the value(s) of time used by the authors.

C.8 Adjusting or Bounding SVT in Other Settings

C.8.1 Labor Rationing

Researchers studying settings where workers’ ability to adjust their labor supply in the market is much more or less constrained than in our setting should apply our rule of thumb based on market wages with caution. The SVT is directly related to labor rationing through the Lagrange multiplier on the labor constraint in (3.1) (below we normalize $V_m = -1$):

$$SVT = w - \lambda.$$

While λ is identified by our structural model, it may not be possible to recover from survey data. In this section, we show that a reliable proxy for λ can be constructed from data on workers’ past market wages and potential future earnings.¹⁵ Intuitively, workers whose labor supply is unconstrained will be able to find work that pays close to their market productivity, as measured by past wages; constrained workers would only be able to find low potential wages compared to their market productivity. We compute a survey-based proxy for λ as $1 - (\text{Potential Wage}/\text{Market Wage})$, with higher values indicating tighter constraints, and a value of 0 indicating that the worker could find a job at their past wage.

This proxy is strongly correlated with our structural estimate of λ ($r = 0.50$), suggesting that it is a reasonable measure of the labor rationing workers face. Moreover, this survey-based proxy can be easily measured in other settings and used to adjust our rule of thumb, or add bounds to impact estimates, in cases where researchers are unable to replicate our full BDM exercises.

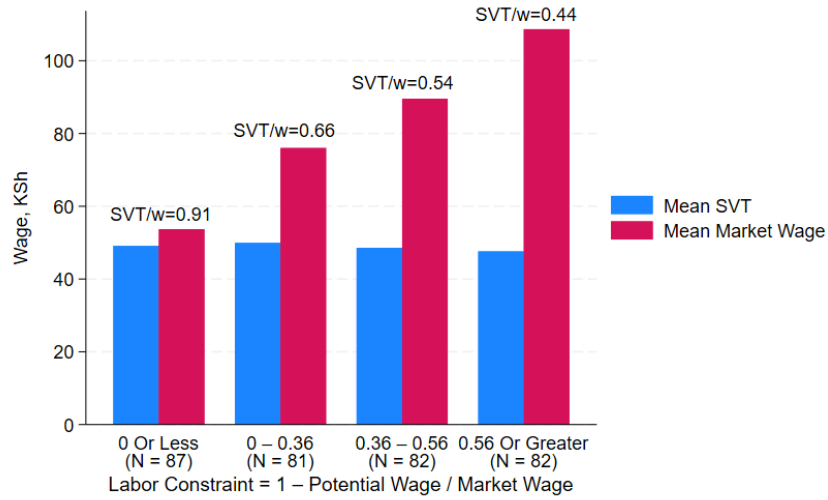
Figure C.8.1 presents the average SVT, market wage, and the ratio of the two separately for farmers with $\lambda^{PROXY} \leq 0$ (indicating no labor rationing), $\lambda^{PROXY} \in (0, 0.36]$, $\lambda^{PROXY} \in (0.36, 0.56]$, or $\lambda^{PROXY} > 0.56$, with cutoffs chosen to equalize group sizes among constrained farmers. The mean SVT expressed as a ratio of the average market wage is 0.91, 0.66, 0.54, and 0.44 in these four groups respectively.

C.8.2 Heterogeneous Labor Supply Responses

For researchers anticipating non-uniform labor supply responses to an intervention—which may be correlated with SVT—we recommend using an unincentivized measure to capture

¹⁵Specifically, we ask farmers “If you wanted to work as much as possible tomorrow, how many hours of work do you think you would be able to find?” followed by “How much do you think you would get paid for those hours of work tomorrow?” and compute the implied hourly wage. For farmers who say they would not be able to find any job, we bottom code their response to be the lowest hourly payment for casual labor they have heard of in their village.

Figure C.8.1: SVT Rule of Thumb Heterogeneity by Labor Rationing



the relevant heterogeneity, and adjusting the rule of thumb as follows:

$$E[\Delta Labor * SVT] = E[\Delta Labor] * E[SVT] + Cov(\Delta Labor, SVT),$$

in which $\Delta Labor$ is the observed labor supply change, $E[SVT]$ is the rule of thumb ($0.6w$), and $Cov(\Delta Labor, SVT)$ can be assessed using the hypothetical SVT survey measure, possibly after deflating the values of the unincentivized measure to match the mean of SVT, if the levels are off. In our data, the unincentivized measures estimate the heterogeneity well, even though the average level is too high.