Urban Public Works in Spatial Equilibrium: Experimental Evidence from Ethiopia

Simon Franklin Clément Imbert Girum Abebe
Carolina Mejia-Mantilla*

July 26, 2021

Abstract

Anti-poverty programs affect not only beneficiaries but also the entire economy, through spillover effects which are often difficult to identify and quantify. This paper evaluates Ethiopia's Urban Productive Safety Net Program, which provides employment on local public works to the urban poor. For identification, we use the random roll-out of the program across neighborhoods of Addis Ababa. We develop a spatial equilibrium model and leverage unique data on local amenities and city-wide commuting flows to account for spillover effects. We show that the program increases public employment, improves local amenities, and reduces private labor supply in program neighborhoods. We then estimate the effect of the program on labor markets across the city: we find that wages increased by 15% in program neighborhoods and 3% in other neighborhoods. Finally, we compute the welfare gains to the poor from the program once fully rolled-out: 26% come from public employment, 12% from improvements in local amenities and 62% from rising private-sector wages. These results suggest that welfare gains are four times larger after taking spillover effects into account.

*Franklin: Queen Mary University London, s.franklin@qmul.ac.uk. Imbert: University of Warwick, BREAD, CEPR, EUDN and JPAL, c.imbert@warwick.ac.uk. Abebe: World Bank. Mejia-Mantilla: World Bank. We would like to thank Stefano Caria, Morgan Hardy, Emanuela Galasso, Ruth Hill, Seema Jayachandran, Gabriel Kreindler, Karthik Muralidharan, Paul Niehaus, Michael Peters, Barbara Petrongolo, Debraj Ray, Marta Santamaria, Gabriel Ulyssea, Eric Verhoogen, Christina Wieser, Yanos Zylberberg, as well as participants at various seminars and conferences for their comments. All remaining errors are ours.

1 Introduction

The effects of social programs are not limited to their direct beneficiaries, but may also spill over to non-beneficiaries and the whole economy. For example, cash and in-kind transfers affect the consumption of non-beneficiaries and local prices (Angelucci and Giorgi, 2009; Cunha et al., 2019). Public works, another popular form of anti-poverty policy in developing countries, can improve local amenities for beneficiaries and non-beneficiaries and affect the labor market equilibrium locally and in other locations (Imbert and Papp, 2015, 2020).[1] Despite the large literature on social programs, there have been few attempts to fully quantify their effect beyond their direct effects on beneficiaries in targeted locations (Egger et al., 2019; Muralidharan et al., 2017). Also, while evaluations of social programs have so far focused on rural policies, spillover effects to untreated locations are likely to be magnified in dense urban settings, which have received much less attention.

Estimating equilibrium effects of programs is challenging: it requires variation in exposure to treatment at the geographic or market level, which is rarely randomized or plausibly exogenous. Even when such variation is present, the assumption of non-interference between treated and untreated units (Stable Unit Value Assumption, or SUTVA) is unlikely to hold if there are spatial spillovers across locations. As a result, estimates of equilibrium effects that rely on comparing treated with untreated locations will be biased. A typical solution to this problem is to make parametric assumptions about the geographic extent of spillovers and to compare among untreated units those that are closer or further away from treated units. This approach may not fully capture spatial spillovers if their radius is misspecified, or if the underlying economic interactions are not only based on distance, e.g. if they follow a gravity model. While justifiable when applied to remote rural locations, it is ill-suited to study strongly inter-connected urban neighborhoods.

This paper provides a comprehensive evaluation of Ethiopia's Urban Productive Safety Net Program, one of the world's largest urban public works programs. We combine random variation in the partial roll-out of the program across neighborhoods of Addis Ababa and a spatial equilibrium model with commuting. We estimate the effects of the program on participants, on local amenities and on labor markets across the city. Our framework then allows us to quantify the welfare effects of the program after it was rolled

[1] A common rationale for these programs is that labor markets in developing countries have "surplus labor" so that hiring workers should have little effect on private sector employment (Lewis, 1954; Harris and Todaro, 1970). However, this is rarely the case since wages are commonly set above the prevailing market wage Ravallion (1987).

out to the entire city. Our approach is at the intersection of randomized program evaluation at scale (Muralidharan and Niehaus, 2017) and quantitative analysis of spatial equilibrium (Redding and Rossi-Hansberg, 2017).

First, we exploit the randomized roll-out of the program across neighborhoods (woredas) of the city of Addis Ababa, combined with precisely georeferenced panel data on households across the city and compare households in woredas with and without the program. We also look separately at eligible and ineligible households.[2] After one year, the reduced form comparison between treatment and control areas suggests that the program generated public employment, but reduced the labor supply of eligible households to to the private sector. The net effect on employment is close to zero and insignificant.[3] The reduction in private labor supply is large enough that it could have equilibrium effect on private sector wages. A reduced form approach to estimating the effect of the program on wages would compare wages in treated and control woredas, without accounting for commuting flows. This approach would assume that spillover effects of the program are only local, which is unlikely given that only 46% of workers work in their local woreda.

To estimate and quantify the equilibrium effects of the program, we then develop a spatial model that borrows from the urban economics literature (Monte et al., 2018; Heblich et al., 2020; Balboni et al., 2020). We leverage the structure of the model (i) to estimate labor market spillovers across the city (ii) to quantify the welfare effects of the program including direct benefits, effect on amenities and labor market effects (iii) to provide counterfactual comparisons of the program under full roll-out and a cash transfer.

To estimate labor market spillovers, we express equilibrium changes in wages in each local labor market as a function of changes in labor supply coming from treated woredas. We implement this model-based equation following the methodology recently developed by Borusyak and Hull (2020) and estimate the causal effect of exposure to the program on wages, defining exposure of a given labor market as a weighted sum of treatment status in all woredas, weighted by the share of commuters to that labor market that come from these woredas. As in Borusyak and Hull (2020), to account for the fact that even if treatment is randomized exposure to the treatment is not randomly assigned, we recenter the exposure measure using potential exposure to 2,000 re-randomizations of the treatment assignment. Our estimate implies that

[2]Eligibility was determined by the local community prior to program implementation. Treatment woredas were determined randomly through a public lottery. Only households that have been residents of the targeted area for at least six months are eligible.

[3]Because the program pays wages well above the level in the private sector, households in the program experience sizeable increases in household income relative to control households.

private sector wages increased by 15% in treated areas and by 3% in control woredas, which confirms that a comparison of wages in treated and control woredas would underestimate the effect of the program on wages.

Turning to the effect of the program on local amenities, we use an index which aggregates five subjective indicators of neighbourhood quality that were specified in a pre-analysis plan, and show that neighbourhood quality increased by 0.6 SDs in treated neighborhoods relative to the control mean. The effect is present both for program beneficiaries and non-beneficiaries who live in treated areas.[4] To quantify the value of improvements in public goods, we correlate these measures of local amenities with private market rents using both data from our sample and using rent data from another survey. Overall, we estimate an effect on amenities equivalent to 2.5% of total local amenity value.[5]

We use two alternative versions of a gravity equation to estimate the Frechet parameter, the key parameter of the model that governs the distribution of the idiosyncratic taste for working in a given location. We first estimate the parameter as the elasticity of commuting with respect to wages at destination, instrumenting these wages by the destination's exposure to the program (through its own commuting networks). This methods yield estimates of 3.36. We also estimate the Frechet parameter as the elasticity of commuting with respect to commuting costs, instrumented by walking distance and find large estimates (4.6 to 5.3), which are similar to papers from using the same method for historical European cities (Ahlfeldt et al., 2015; Heblich et al., 2020).

Finally we use the structure of the model to compute the welfare changes due to the program, combining the direct income effects on participating households, equilibrium wage effects and improvements in local amenities in treated woredas. Our model allows us to consider two scenarios: when the program was partially rolled-out and after it was completely rolled-out across the city. We show that under partial roll-out, the treated areas were the ones who gained the most from the program, but half of the welfare gains were due to rising wages, a fifth to improved amenities, and only a third to program participation. Under partial roll-out, control areas only benefited through labor market spillovers. Under complete roll-out, the welfare gains extended to all neighborhoods and became larger, due to equilibrium effects. Welfare increases by 25.3%, including a 6.5% direct gain from participation, a 3% gain from improved amenities and a 15.8% gain from rising private sector wages

[4]Because all infrastructure projects of the UPSNP were carried out on a small scale within treated neighborhoods, we do not expect spillover effect on amenities in control neighborhoods.

[5]We also estimate a 3% rise in rents, but the effect is imprecisely estimated, as the majority of the poor live in government-owned slums where rents are fixed or zero.

across the city.[6] As a benchmark, we compute the welfare gains from a cash transfer that pays public works wages without affecting labor supply. We show that the cash transfer does better when one considers only the direct benefits from participation, but that public works dominate as soon as effects on amenities and wages are taken into account.

This paper contributes to four main strands of the literature. First, we contribute to the literature on the equilibrium and spillover effects of anti-poverty programs using large cluster-randomized controlled trials (Egger et al., 2019; Muralidharan et al., 2017; Crépon et al., 2013). These papers either assume non-interference between potential treatments units or define exposure to spillovers as a parametric– usually, step-wise – function of euclidean distance to treated areas. While this assumption may be justified in the context of relative remote rural villages, it is unlikely to hold in urban areas that are closely connected by commuting between labor markets. We go further than the existing literature by using a structural model of inter-connected urban labor markets in which exposure to spillovers is determined by a network of locations in spatial equilibrium. The structure of the model guides the estimation of spillover effects, and the quantification of welfare effects from the program during and after its roll-out across the city. This method provides a framework for evaluating urban social programs, an area where empirical evidence is scarce, especially for developing countries.

Second, we contribute to the literature which studies urban change using spatial equilibrium models. Most papers study variations in commuting costs due to changes in the transportation network in historical cities (Heblich et al., 2020; Ahlfeldt et al., 2015) and cities in developing countries today (Tsivanidis, 2018; Balboni et al., 2020). We are the first to study an urban public works program, with its unique combination of effects on income, local amenities, and labor markets. We borrow from other papers (Heblich et al., 2020; Balboni et al., 2020) to model commuting decisions, the spatial labor market equilibrium and the welfare effects of changes in wages and amenities. Our model is a simplified version of theirs, as it does not include migration or trade, since we do not find evidence that the program affects consumption expenditures or residential mobility. We improve on identification by exploiting random variation in the placement of the program across neighborhoods combined with detailed individual data on amenities, commuting, employment and wages. This enables us, for example, to estimate the Frechet parameter as

[6]The evaluation does not include changes in goods prices; our reduced form estimates show no short-term impact on household consumption, and we do not find evidence of price increases in markets more exposed to the program in CPI data.

the elasticity of commuting with respect to exogenous changes in destination wages driven by exposure to the program.

Third, our paper is closely related to the literature on local labor markets, local development policies and the spatial transmission of labor market shocks (Moretti, 2011; Kline and Moretti, 2014; Manning and Petrongolo, 2017; Monte et al., 2018; Monras, 2020; Imbert and Papp, 2020). In particular, Monte et al. (2018) study equilibrium responses to local labor demand shocks in US commuting zones, and emphasize that openness to commuting dissipates the effects of these shocks on local employment. Using a different approach, Manning and Petrongolo (2017) structurally estimate a job search model and find that while the search radius of a given job seeker is small, labor markets largely overlap, so that local shocks are likely to have ripple effects. We contribute to this literature by directly estimating the equilibrium effects of a labor market shock using the randomized program roll-out for identication and detailed information on commuting networks at the individual level. We show that a placed-based policy that is ear-marked for local residents still has large spillover effects on labor markets across the city.

Finally, we contribute to the literature on the evalution of public works programs in the developing world. A large literature has estimated the effects of public works programs on a range of different outcomes for program beneficiaries (Berhane et al., 2014; Beegle et al., 2017; Alik-Lagrange et al., 2017).[7] Quantifying the total welfare effects of these programs has been more challenging, due to the challenges of identifying equilibrium wage effects and public goods benefits of the public works. Closely related to this paper, Imbert and Papp (2015) and Muralidharan et al. (2017) estimate positive equilibrium effects of India's rural public works program on rural wages and Imbert and Papp (2020) estimate spillovers on urban areas due to changes in seasonal migration flows. As compared to these papers, ours combines the advantage of random program placement, detailed information on commuting networks at baseline, and a structural model to estimate labor market spillovers. In addition, we provide the first direct experimental evidence of the effect of public works programs on local amenities.[8] Finally, we make progress towards a comprehensive evaluation of public works programs by constructing a model-based measure of welfare effects, including effects on beneficiaries, on local amenities and labor markets, under partial and complete roll-out.[9]

[7]For a comprehensive review of the literature on the effects of India's employment guarantee on economic and social outcomes see Sukhtankar (2016).

[8]Gazeaud et al. (2020) use a difference-in-differences strategy and find no change in vegetation cover due to the rural PSNP in Ethiopia.

[9]Our paper considers only the contemporaneous effects of the program. Alik-Lagrange

The paper proceeds as follows. In Section 2 we describe the program, the evaluation data and design, and we describe briefly the economic lives of the beneficiaries of the program. Section 3 establishes four headline reduced form results using our experimental design, which motivate our model, presented in 4. In Section 5 we then use the model to quantify the effects of the program in spatial equilibrium, before concluding.

2 Program and setting

2.1 Program

The Urban PSNP takes its name from PSNP (Productive Safety Nets Program) that has been running throughout rural Ethiopia since 2005 (Berhane et al., 2014). The UPSNP was introduced in 2017 in eleven cities in the country (one city from each region), and provides guaranteed public work to targetted households. The number of beneficiary households per city varies depending on the city size and poverty rates. In the capital, Addis Ababa, 18% of households in the city were enrolled in the program, when the program reached full-scale, and, due to the size of the capital, 70% of all beneficiaries in the country are in Addis Ababa. Since the evaluation in this paper focusses exclusively on Addis Ababa, we describe the roll-out and beneficiaries for that city. The program is implemented by local government administrative units or *woredas* within cities, with guidelines and oversight from the Federal Ministry of Urban Development and Construction.

Public work and wages: Each beneficiary households is offered up to 60 days of public works per year per working age member, up to a maximum of four members. Most households are offered up to the maximum of 240 days of work a year. Households are enrolled into the program for three years in total.[10] Households are free to choose whom within the household will do the work, although those individuals need to have been registered as eligible at the time of the household targetting. Conditional on completing the work, households were paid 60 Birr (around $2) per day of work. The average beneficiary household earns roughly 1000 Birr (around $33) per month, or 40% of average household consumption for households in the bottom consumption quintile in representative data.

et al. (2017) and Bertrand et al. (2017) evaluate the effects of public employment on labor market outcomes of beneficiaries *after* they leave the program.

[10] The number of days available to each household decreases incrementally with each year in the program, but this does not occur within the time frame of this evaluation.

Work activities take place for an average of five hours per day, starting in the early morning. All work is done in local communities called *ketenas* , a smaller administrative unit within the woreda, which also conducts the targeting of the program. As a result most public work takes place very close to beneficiary households' place of living. Program wages are paid at the household level, into special bank accounts set up in the name of the head of the household, regardless of who does the work.

The work consists of small-scale activities aimed at neighborhood improvement. The most common activites are: cleaning streets, maintaining drains and ditches, garbage disposal, and greening of public spaces (planting of trees and gardening). Most beneficiaries involved in the program report doing multiple or all of these activities. Construction of small cobbled streets in slum areas took place in a few rare cases.

Direct support treatment arm: In addition to the public works component of the project, there is an additional unconditional cash transfer arm of the program, known as the "direct support" (DS) arm, which provides a cash transfers to poor households with no members able to participate in the public works due to chronic illness, age or disabilities. These transfers is considerably smaller than the wages from public works.[11] Although our study is designed and powered to separately identify the effects of the DS, we do not focus on those results in this paper. Reduced form impacts of the DS are negligible across a range of outcomes, which makes us confident that this component is not driving the equilibrium effects of the program.

Targeting: Households are selected for the program by local *ketena* committees (local communities within woredas). A strict residential requirement was enforced: only households that were resident in the local *ketena* for at least 6 months could be selected for the program. Qualitative work on the community targeting suggests that communities selected households on the basis of asset poverty and a sense of household vulnerability. We compare the characteristics of a representative sample of targetted beneficiary households against a representative household survey from the same year as our program baseline (2016).[12] We find that households with members with disabilities, and female-headed (often widow-) headed households are overrepresented the beneficiary sample, relative to a representative sample of households below

[11]The DS provides ETB 170 per person per month; the average household enrolled into DS receives 350 Birr per month, or roughly a third of public works beneficiaries.

[12]Note that the data used for targeting analysis is separate from and in addition to our evaluation sample, which is representative of poor households in the city. We do not have full consumption modules for the sample of representative beneficiaries, only for our evaluation sample.

the consumption poverty line in Addis Ababa. In terms of asset ownership and housing quality, targeted households are worse off than representative households below the poverty line. We fail to reject a joint significance test of woreda fixed-effects on beneficiary observables; suggesting that the targeting was done in a similar way across woredas in the city.

Take-up: Take up of the program at the household level is almost universal among households that are offered it. We find that fewer than 3% of households in our evaluation sample report being offered the program and declining to be involved. Similarly, take up of the public work is high on the intensive margin. Within households, public works is mostly done by women and, in particular, older women. Figure 1 shows the propensity to engage in the public works by age and gender in our evaluation data.

Figure 1 here.

2.2 Evaluation and data

The program was randomized at the woreda (urban district) level in Addis Ababa. In year 1 of the program, only households residing in woredas with poverty rates above 20% were eligible for the program: specifically, 90 out of 116 woreda in the city. Randomization was conducted by a public draw of woreda names on November 2016, and stratified by sub-city (10 urban sectors within Addis Ababa). Of these 90 eligible woredas, 35 were randomly selected for the program in year 1 (henceforth, treated woredas) and the remaining 55 woredas to receive the program in year 2 (control woredas). Figure 2 shows a map of the randomization outcomes at the woreda level.

Figure 2 here.

We surveyed the households for our evaluation immediately after the randomization of woredas into the program but before targetting and roll-out of the program occurred (see Table 1 below). First, we conducted a screening survey of nearly 30,000 households drawn from a random sample of all households in the city. For this, we used random walk sampling starting from randomly selected points within each of the 90 eligible woredas. This was a short survey focussed on household composition and asset ownership, used to derive a predicted poverty score using a proxy means test (PMT) for consumption poverty. Next, we selected the poorest 28% of households in the distribution of PMT scores, with whom we then conducted a detailed baseline survey. This constitutes our evaluation sample of 6,096 households.

Our baseline sample over-samples treated areas, so that the final household sample includes an equal proportion of households in treatment and control areas, despite only 40% of woredas being treated in the first year.

Table 1 here.

We conducted a detailed endline survey with our sample of these 6,096 households one year later. We want to identify within our sample eligible and non-eligible households (throughout the paper, we use *eligibilty* to refer to whether a household was selected by the local community regardless of the year in which their woreda was treated). For year 1 (treated) woredas we observe this directly from self-reported participation in the main endline survey. For year 2 (control) woredas, we conducted an additional survey with all households in year 2 woredas a few months after the main endline when the program had been rolled out in those woredas one year later. This allows us to estimate the effect of the program on both eligible and ineligible households using year 1 endline data. Furthermore, we determine individual participation in public works at the endline level from the same two surveys, allowing us to compare individuals within households that would select into the work, across treatment and control woredas.

Balance and attrition: Attrition in our endline survey is very low at 2.94% of households from the baseline. Appendix Table A1 shows that there is no significant difference in attrition rates by treatment across treated and untreated in woredas. Very little else is correlated with with response rates; households living in kebele housing (publicly managed and subsidized housing) are slightly more likely to respond, perhaps because these households are less mobile. Table A1 in the Appendix shows no sign of imbalance between households in treated (year 1) and untreated (year 2) woredas at baseline, consistent with the randomization of the program at the woreda level and with identical sampling procedures across treatment and control woredas.

2.3 Beneficiary characteristics

Employment and earnings: The program offers work and remuneration that is better, on average, than beneficiaries' private options. This is partly because the program requires only 5 hours of work per day, relative to 9, on average, for work in the private sector (wage- and self-employment). The daily wage in public works is roughly similar to daily wages in private sector work, but roughly 64% higher than private sector wage work on an hourly basis. These wages are even more attractive for the lower-earning members of targeted households, who are more likely to take up the public works. Figure 3 below shows the distribution of wages paid by public works as compared to private sector wages in the control group at the time of the first endline survey. Women, who are more likely to do public works within households, earn less than men in this context, making the public works relatively more attractive. This is shown even more starkly among individuals in the control group who will later take up the public works earned less than half at the first endline than they later would in the public works.

Figure 3 here.

Commuting: Our survey data captures individual's commuting destinations. Combined with the location of the household, this allows us to study commuting flows at the woreda-pair level, which is essential for our structural estimation. Among private sector workers in this sample, 36% in wage labor work within their own woreda. Self-employment is much more local: 74% of self-employed workers work in their local woreda. Woredas in the city are organised into 10 subcities, which make up the largest administrative units in the city. Only 55% of wage employees work in their local subcity, compared to 80% among the self-employed. Figures 4 and 5 show out- and in-commuting flows at the woreda level in our data. The woredas that send the most commuters tend to be the central woredas, except a few located at the periphery. Central woredas have higher rates of workers who commute in than those further away, but some peripheral woredas also receive substantial flows in-commuters.

Figures 4 and 5 here.

Housing and rents: In our sample, 75% of households live in "kebele" housing: this is government-owned homes where households generally live for free or for a nominal fee paid to local government officials. This housing is usually of very low quality; fewer than 10% of kebele houses have walls made of formal materials. The average rent for households who do pay rent in this type of housing is 11 Birr per month, relative to roughly 660 Birr per month on average in private sector housing. Opportunities to live in kebele housing are rationed, and households cannot move home easily without losing access to these low rents. As a result, mobility rates among households in our sample, and those living in kebele housing, are very low. Only 2.4% of our sample moved between the first and second endline survey (over a 21 month period) and only 1.5% among those in kebele housing.

3 Reduced form results

3.1 Estimation

We first estimate the intention-to-treat (ITT) effect of living in a treated woreda $\mathtt{T}_w$ on outcome Y_{ihw} for individual worker i living in household h in woreda (district) w using the following equation:

$$Y_{ihw} = \alpha + \beta T_w + \gamma \mathbf{X_{ihw}} + \varepsilon_{ihw}. \tag{1}$$

The vector $\mathbf{X_{ihw}}$ includes baseline individual and household level controls, the outcome at baseline where possible and subcity fixed effects. For labor outcomes we restrict the sample to working-age individuals. Equation 1 can also be estimated at the household level to estimate the treatment effect on any household-level outcome Y_{hw}.

We then amend Equation 1 to estimate separately the effect of the program on individuals who belong to eligible and ineligible households:

$$\begin{aligned} Y_{ihw} = \alpha &+ \beta_1 Eligible_h \times T_w + \beta_2 Ineligible_h \times T_w \\ &+ \gamma \mathbf{X_{ihw}} + \delta Eligible_h + \varepsilon_{wit}. \end{aligned} \tag{2}$$

$Eligible_h$ is a dummy equal to one for households eligible to the public works component of the program and $Ineligible_h$ is a dummy equal to one for households not eligible to receive any benefits.[13] Here β_1 estimates the effect of being enrolled in the program, while β_2 estimates the effect of living in a neighborhood with the program, but without directly benefiting from it.

3.2 Results

Table 2 summarizes the main reduced form results at the individual level. Panel A shows the ITT effect of being in a treated woreda when the program is implemented (Equation 1), while Panel B presents separate estimates for eligible and ineligible households (Equation 2).

Table 2 here.

The results in columns 1, 2 and 3 of Panel A suggest that the program generated substantial employment on public works (5.2pp. or 14% of employment in the control), but also decreased labor supply to the private sector

[13]For simplicity of exposition we exclude from the estimation households eligible for the cash-only ("direct support") component of the program.

by 12% (4.49pp. decrease as compared the control mean of 37.4%), so that in net it did not significantly increase total employment (the coefficient is a precisely estimated zero). Turning to Panel B columns 1, 2 and 3, the increase in public employment is as expected concentrated among eligible households, and is substantial: it is equal to 28% of worked hours in the control (10pp. as compared to the control mean of 36%). But the increase in total employment among eligible is only 8% (2.9pp. from a control mean of 36%) because the increase in public employment is offset by a similarly large decrease in private employment (-7.2pp. or 20% of the control mean). Appendix Table A1 presents the effects on private employment by gender and skill level. We find that the program reduces private employment for male and female workers, for workers with and without a high school diploma. Consistent with the information on program take-up discussed in section 2, the effects are larger for women and low-skilled workers.

We also show the effects of the program on households' self-reported neighborhood amenities. The outcome is a standardized and normalized index comprised of five measures of neighborhood quality namely: quality of drainage infrastructure, cleanliness of streets, public toilets, presence of odors from sewerage, presence of odors from trash. See Table A1 for summary statistics of these components. These measures were designed to capture improvements to neighborhoods that were likely to result from the activities conducted under the public works. The program improves self-reported neighborhood quality by roughly 0.6 standard deviations (Column 4 Panel A in Table 2). Importantly, this result is not just driven by eligible households who directly participated in the work, but is present among other residents of the neighborhood who did not do the work (Column 5 Panel B). Since program did small scale neighborhood improvements in beneficiaries' home woredas, these amenity effects are unlikely to spill-over to neighboring woredas.[14]

To conclude, the comparison of household outcomes in treated and control neighborhood suggests that employment generated on public works was almost entirely offset by a fall in private sector work. Since 18% of households in treated areas are in the program, this suggests a large negative labor supply

[14] In Appendix, we test whether the improvement in amenities led to an increase in rents in treated woredas or an decrease in the fraction of households moving out of treated neighborhoods. The results in Appendix Table A1 suggest that rents may have increased by about 3%, but the coefficient is not significant, due to the small fraction of households who actually pay rents (18%). Few households move houses (2%), and the proportion is not different in treated woredas. These results are consistent with the fact that poor households in Addis Ababa benefit from government housing and do not pay rent, but have little scope for residential mobility (see Section 2).

shock to the private sector, which could induce important effects on private sector wages. The program also led to an improvement in local amenities. In the next sections, we will use a spatial equilibrium model to quantify the labor market spillovers of the program and combine the direct and indirect effect of the program into a unified welfare analysis.[15]

4 Model

In this section, we model the effects of a public works program in a spatial equilibrium framework of commuting based on Monte et al. (2018) and Heblich et al. (2020). We consider a city comprising of $i = 1, ..., n$ locations. In each location i live $\overline{R_i}$ residents, each of whom supplied inelastically one unit of labour. Workers can commute (choose where they work) but they cannot migrate (choose where they live). Let π_{ij} denote the proportion of residents from i who work in j. We assume frictionless trade across the city.

4.1 Utility

We assume that utility for a worker ω residing in i and working in neighborhood j is given by:

$$U_{ij}(\omega) = B_i b_{ij}(\omega) \tau_{ij} C_i$$

where C_i denotes consumption of the tradable good, τ_{ij} iceberg commuting costs (≤ 1). B_i is the average amenity from living in i and $b_{ij}(\omega)$ is an idiosyncratic amenity shock drawn from a Frechet distribution with dispersion parameter θ:

$$G(b) = e^{-b^{-\theta}}$$

4.2 Consumption

Workers consume of a single good, which is freely traded across the city. We use its price as numeraire. Utility maximisation implies that workers consume all of their income on goods.

Let $\overline{v_i}$ denote the average income of workers living in i and C_i denote aggregate consumption:

$$C_i = \overline{v_i}$$

[15]Appendix Table A1 provides additional results on household outcomes: household income increases, due to public works wages received by eligible households, but household expenditures do not increase, instead eligible households double their savings.

4.3 Production

We assume that production in each location is made by a representative firm with cobb-douglas production function with constant return to scale.

$$Y_j = a_j L_j^{1-\alpha} \quad \text{where} \quad a_j = A_j K_j^{\alpha} \quad \text{and} \quad \alpha > 0$$

Capital K_j and productivity A_j are assumed to be fixed. All firms produce the same product whose price is one. Profit maximization implies that:

$$w_j = (1-\alpha) a_j L_j^{-\alpha}$$

Optimal labour demand is:

$$L_j = \left((1-\alpha) \frac{a_j}{w_j} \right)^{\alpha}$$

Taking logs and differencing yields the labour demand elasticity:

$$\frac{\partial \ln L_j}{\partial \ln w_j} = -\alpha$$

4.4 Commuting

Utility is linear, and the budget constraint imposes $C_{ij} = w_j$, hence the utility from living in i and working in j is:

$$U_{ij} = B_i b_{ij} \tau_{ij} w_j$$

The utility is a monotonic function of b which follows a Frechet distribution, hence it also follows a Frechet distribution. It cumulative distribution function will be :

$$G_{ij}(u) = e^{-\Phi_{ij} u^{-\theta}} \quad where \quad \Phi_{ij} = (B_i \tau_{ij} w_j)^{\theta}$$

Workers in a given location of residence i choose among the locations of work j the one that gives them the highest utility. The maximum of a series of Frechet distributed random variable is itself Frechet distributed. Let $G_i(u)$ denote the cumulative distribution function of the maximum utility attained by workers from i:

$$G_i(u) = \prod_j G_{ij}(u) = e^{-\Phi_i u^{-\theta}} \quad where \quad \Phi_i = \sum_j (B_i \tau_{ij} w_j)^{\theta}$$

Because there is no mobility, utility is not necessarily equalised across locations of residence. However it is still equal within a location of residence across the different possible destinations. The expected utility of a location of residence i is (see proof in appendix):

$$\forall i \quad U_i = \gamma \left[\sum_{j=1}^{n} (B_i \tau_{ij} w_j)^\theta \right]^{\frac{1}{\theta}} \quad \text{where} \quad \gamma = \Gamma \left(\frac{\theta - 1}{\theta} \right) \tag{3}$$

By the properties of the Frechet distribution, the probability that a worker who lives in i will work in j is:

$$\pi_{ij} = \frac{(B_i \tau_{ij} w_j)^\theta}{\sum_k (B_i \tau_{ik} w_k)^\theta} = \frac{\Phi_{ij}}{\Phi_i} \tag{4}$$

This suggests a commuting gravity equation, with an elasticity of commuting with respect to the wage at destination (and to commuting costs) equal to θ. θ can be estimated in that way.
The expected income of workers from i is:

$$v_i = \sum_j \pi_{ij} w_j$$

4.5 General Equilibrium

Given the endowments A_i, B_i, R_i, and K_i, the commuting costs τ_{ij}, and the two parameters α and θ, an equilibrium is a vector of wages w_i in each location which ensures that the labour markets clear:

$$\forall j \quad L_j = \sum_i \pi_{ij} R_i$$

Monte et al. (2018) show that this equilibrium exist and is unique.

4.6 Public Works

Let T_i be the treatment indicator equal to one if the public works program is implemented in neighbourhood i. If $T_i = 1$, the program offers to workers who live in i the opportunity to work locally (without commuting costs) for p part of their time at a wage w_g:

$$w_g = (1 + g) w_i$$

where g is he wage premium given by the programme and w_i is the local wage pre-programme. We assume that $\forall j, \forall i \quad (1 + g) w_i > \tau_{ij} w_j$ so that there is full take-up of the programme.

We use the "exact hat" algebra, popular in trade (e.g. Arkolakis, Costinot and Rodrigues Clare 2012) and denote with a hat changes between two equilibria. The programme has three effects:

1. A net direct income gain, equal to public works wages minus forgone income from the private sector:

$$Direct\ \ Income\ \ Gain = pT_i \left[(1+g)w_i - \sum_j \pi_{ij} w_j \right] \tag{5}$$

2. A labour market equilibrium effect. The programme reduces the labor endowment in locations in which it is implemented which reduces labor supply in each commuting destination. Given the expression of the labor demand elasticity, the change in wages in each location j is:

$$\ln \widehat{w_j} = -\frac{1}{\alpha} \ln \left(\frac{\sum_i \pi_{ij}(1-pT_i)R_i}{\sum_i \pi_{ij} R_i} \right) > 0 \tag{6}$$

   Wages will rise overall, by more in locations with a higher fraction of commuters from treated locations (including treated locations themselves).

3. An increase in local amenities for all residents. Let $\widehat{B_i}$ denote the relative change in amenities:

$$\widehat{B_i} = (1+\beta T_i)$$

Expected utility for a worker living in i is now:

$$\widehat{U_i} U_i = \gamma \left[pT_i \left((1+g)\widehat{B_i} \right)^{\theta} (B_i w_i)^{\theta} + (1-pT_i) \sum_j (\widehat{B_i}\widehat{w_j})^{\theta} (B_i \tau_{ij} w_j)^{\theta} \right]^{\frac{1}{\theta}} \tag{7}$$

4.7 Welfare Effects

Based on the two equations 3 and 7, we can derive the welfare gains from the public works program (see proof in appendix B):

$$\widehat{U_i} = \underbrace{(1+\beta T_i)}_{Amenity\ Effect} \left[1 + \underbrace{pT_i \left(\pi_{ii}(1+g)^{\theta} - 1 \right)}_{Direct\ Effect} + \underbrace{(1-pT_i) \left(\sum\nolimits_j \pi_{ij} \widehat{w_j}^{\theta} - 1 \right)}_{Wage\ Effect} \right]^{\frac{1}{\theta}} \tag{8}$$

which includes the effect of improved amenities, the direct gains from participation in the program and the gains from rising private sector wages, and can be computed with the knowledge of p (share of the labour supply devoted to the programme), $(1+g)$ the wage premium on public works, $\widehat{w_j}$ the proportional change in the wage, π_{ij} the commuting probabilities at baseline, θ the elasticity of commuting w.r.t. the wage and $(1+\beta)$ the proportional change in the value of local amenities.

As a benchmark, we will compare the welfare gains from the program with the benefits from a cash transfer that provides the same utility as public works wages without any work requirement, and hence no effect on the private labor market (see appendix B for more details):

$$\widehat{U_i^{cash}} = \left[\pi_{ii}(pT_i(1+g))^{\theta} + 1\right]^{\frac{1}{\theta}} \tag{9}$$

4.8 Discussion

The model abstracts from two dimensions that may be potentially important in other contexts: housing and trade. The absence of housing markets in the model is motivated by a context in which poor households receive housing from the government, rarely pay rents and rarely change residence. There is also no empirical evidence that rents or migration respond to the program (see Appendix Table A1). The model does not consider the goods market either, and potential effects on local prices. This is motivated by the fact that goods markets within a city are likely to be well integrated, and also by the evidence that the program did not increase household expenditures (Appendix Table A1). Our setting in this regard is very different from studies of rural social protection programs, which can have large effects on consumption and prices in remote villages (Cunha et al., 2019; Egger et al., 2019). We also test empirically whether the program had any effect on local prices, using official micro data from the Consumer Price Index and do not find evidence of price effects (see Appendix C and Table C1).

5 Quantitative Analysis

5.1 Labor market spillovers

A reduced form estimation of the effects of the program on labor markets would simply compare wages earned by workers from neighborhoods with the program with wages earned by workers from neighborhoods without the program. Following the model notations, let us denote with T_i the treatment dummy for neighborhood i, and w_i the average wage earned by workers who live in i. The reduced form specification is:

$$\ln \widehat{w}_i = \alpha + \beta T_i + \gamma \mathbf{X_i} + \varepsilon_i \tag{10}$$

where $\mathbf{X_i}$ includes baseline characteristics and baseline wages as controls, as well as subcity fixed effects. In order for this specification to provide unbiased estimates of the effect of the program, the Stable Unit Treatment Value Assumption (SUTVA) needs to hold, i.e. wages in a given neighborhood should not be affected by the implementation of the program in other neighborhoods. Given the importance of commuting flows across neighborhoods, this assumption is unlikely to hold. In particular, Equation 6 in the model makes it clear that the wage effects of the program are better captured as changes in wages by place of work, rather than place of residence, and are proportional to changes in labor supply of commuters coming from treated neighborhoods.

To take Equation 6 to the data, we consider as an outcome private sector wages earned by workers who work in a neighborhood j (rather than live in a neighborhood i), and regress it on exposure to the program:

$$\ln \widehat{w_j} = \alpha + \beta Exposure_j + \gamma \mathbf{X_j} + \varepsilon_j \tag{11}$$

where $\mathbf{X_j}$ includes baseline characteristics and baseline wages as controls, as well as subcity fixed effects. Exposure to the program is defined as

$$Exposure_j = \left[\sum_i \lambda_{ij} T_i - \frac{1}{R} \sum_{0 \leq r \leq R} \sum_i \lambda_{ij} \tilde{T_i^r} \right]$$

where T_i is a dummy for the implementation of the program in neighborhood of residence i and λ_{ij} is the probability that work who works in neighborhood j lives in neighborhood i. Note that $i = j$ is one of the elements of the sum, so that the coefficient β captures the effect of the program on local wages as well as its effect on wages in other neighborhoods. Our instrumentation is similar to a shift-share instrument as in the migration literature, e.g. in Imbert et al. (2020). Our setting is a perfect application of Borusyak and Hull (2020), because neighborhoods are non-randomly exposed (through commuting shares) to a randomly allocated shock (the program). To avoid an omitted variable bias, we follow Borusyak and Hull (2020) and recenter actual exposure using average exposure from 2000 simulated independent treatment assignments $\tilde{T_i^r}$ that follow the same (stratified) random allocation.

The reduced form and model based estimates of β are presented in Table 3. In column 1, the reduced form comparison between control and treated neighborhoods suggests that wages earned by workers from treated neighborhoods increased by 11.1%. In contrast, in column 2, the model-based estimates suggests that a labor market who would draw all its labor supply from treated areas would see its wages increase by 19.6%. Treated neighborhoods on average receive 75.4% of their labor supply from treated neighborhoods, against 16% for control neighborhoods: the difference is due to the fact that about half of the workers do not commute. The model-based estimate implies that wages have increased by about 14.8% in treated neighborhoods, and 3.1% in control neighborhoods. Hence, the reduced form estimates that ignore labor market spillovers and the failure of SUTVA do miss a sizeable rise in wages in control neighborhoods, and underestimated the rise in wages in treated neighborhoods.

We also investigate heterogeneity by skill and gender. Specifically, we compute wages, commuting probabilities and exposure separately for men and women, workers with and without a high school diploma, and estimate the spillover effects of the program as if they were on entirely separate labor markets. Appendix Table A1 presents the estimates. Interestingly, the increase in wages is only felt by male, rather than female workers (Columns 1 and 2). Given that the decrease in private sector work is if anything stronger for women (as we saw in section 3 and Appendix Table A1), this suggests that the labor demand elasticity for female labor is much larger than for men. Turning to heterogeneity by skill, the estimates suggest that the wage effects are concentrated on low-skilled workers, with no effect for workers who completed high-school. This result is consistent with the fact that the program had a stronger negative effect on labor supply of low-skilled workers (Appendix Table A1). Another factor is that the poor households in our sample represent a larger share of the low-skilled workforce than the high-skilled force in each neighborhood, so that the same change in labor supply would have larger effects on low-skilled wages.

Table 3 here.

5.2 Effect on local amenities

The reduced form results in Section 3 suggest that the public works program improves local amenities in the neighborhoods where it is implemented. Specifically, we have measured amenities through a standardized index of qualitative assessments on different dimensions of neighborhood quality and shown that the index increases by 0.596 in neighborhoods with the program. In order to take into account the welfare gains from better amenities, we need convert the increase in index quality into a monetary equivalent. For this, we use information on hypothetical rents, i.e. on the value that households think they could expect to pay if they were renting the place they live in, and we compute the correlation between these rents and the quality index. Column 1 in Table 4 presents the raw correlation between index quality and log rents, which is 0.046. One might worry that household or housing characteristics may be correlated both with neighborhood quality and rents (e.g. household income or housing size). To alleviate this concern,we implement a double post-selection lasso procedure to select within a long list of household and housing characteristics those that are the best predictors of either neighborhood quality or rents and include them in the regression. The correlation coefficient, shown in Table 4 Column 2 remains very similar after including these controls (0.043), which is reassuring. We combine this coefficient and the increase in the index to compute the improvement in amenities due to the public works in monetary terms: $0.596 * 0.043 = 0.026$.[16]

Table 4 here.

5.3 Commuting probabilities

To estimate the key model parameter θ, we derive a gravity equation from the expression of the commuting probabilities (equation 4):

$$\ln \pi_{ij} = \theta \ln w_j + \theta \ln B_i - \theta \ln \tau_{ij} + \Phi_i$$

where $\Phi_i = \sum_k (B_i \tau_{ik} w_k)^\theta$ is fixed at the residence level. We use this equation to estimate θ in two ways.

First, we estimate θ as the elasticity of commuting with respect to wages with the following poisson specification:

$$\pi_{ij} = \exp(\theta \ln w_j - \theta \ln \tau_{ij} + \nu_i + \varepsilon_{ij})$$

where πij is the log of the share of residents from i commuting to a destination j, $\ln w_j$ is the log of the wage at destination, $\ln \tau_{ij}$ is the cost of commuting from i to j, and ν_i is a residence fixed-effect which captures residential amenities in

[16] If housing markets were fully functional, one would expect this increase in amenities to be reflected in increase in rents paid by households. Appendix Table A1 shows that the program has an insignificant positive effect on rents paid, but the point estimate is 0.035, which is close to 0.026, our estimate of the monetary value of improved amenities.

i and average expected utility of workers who live in i. This equation allows us to estimate θ, but only if we can deal with the endogeneity of the wage response to changes in commuting, which in the model is described by Equation 6. We use exposure to the program as instrument for changes in the wage $\widehat{w_j}$. Table 5 presents the results. Column 1 presents the OLS estimate for the correlation between changes in the wage at destination and changes in commuting. The correlation is positive, which is expected given that commuters are more likely to go to destination with higher wage growth. This estimate is however likely to be downward biased, because more commuting will decrease at destination. The IV estimate presented in Column 2 is much larger in magnitude and highly significant, and implies that the Frechet parameter $\theta = 3.36$. The first stage presented in Column 3 is positive, confirming that destination most exposed to the program saw their wages increase.

Table 5 here.

Second, we use an alternative strategy, and estimate θ as the elasticity of commuting to commuting costs τ_{ij} in the equation:

$$\ln \pi_{ij} = -\theta \tau_{ij} + \nu_i + \mu_j + \varepsilon_{ij}$$

where ν_i are residence fixed effects which capture expected utility from i and $\mathcal{B}_i$ and μ_j are workplace fixed effects which capture w_j. We use two alternative measures of τ_{ij}, the commuting cost and commuting time reported by the survey respondents. Since transportation networks and hence travel costs may be endogenous, τ_{ij} can be instrumented by walking distance.[17] The results are presented in Appendix Table A1. The two IV estimates are very close to each other and imply estimates of θ (4.7 and 5.3) that are higher than the estimate based on the elasticity of commuting with respect to wages, but very similar with estimates obtained with the same method in literature (e.g. Heblich et al. (2020) find $\theta = 5.25$ for 19th century London). We use 3.36 as our estimate of θ to quantify the welfare effects in the next section.

[17]This approach is similar to Heblich et al. (2020), except that they do not observe commuting costs, but use commuting time d_{ij} instead, and assume $\tau_{ij} = e^{-\kappa d_{ij}}$. This implies that they do not separately identify κ and θ from the gravity equation, but calibrate θ later on.

5.4 Welfare effects

Finally, we combine reduced form and structural estimates to compute the welfare effects of the program, based on Equation 8 from the model:

$$\widehat{U}_i = (1+\beta T_i)\left[1 + pT_i\left(\pi_{ii}(1+g)^{\theta}-1\right) + (1-pT_i)\left(\sum_j \pi_{ij}(\widehat{w_j})^{\theta}-1\right)\right]^{\frac{1}{\theta}} \tag{12}$$

where π_{ij} are commuting probabilities which vary across neighbourhoods. Based on Table 2, the fraction of the labor supply taken away from the private labor market is $p = 4.9/37.4 = 13pp$. The equation includes improvement in amenities by the program, which we have valued at $\beta = 2.45\%$. It includes the changes in wages due to the program, which at the beginning of this section we have estimated to be $\widehat{w_j} = 0.20\sum_i \lambda_{ij}T_i$. It also includes the key Frechet parameter , which we have estimated to be $\theta = 3.36$. There is also the wage premium g, which is the difference between the public and the private sector wage per hour, which we estimate to be 60.3%.

Table 6 here.

We do this first in the context of the partial roll-out of the program, and estimate separately the welfare effects for areas with and without the program, and then in the context of the complete roll-out of the program, in which all neighborhoods are treated. We also sequentially remove part of the welfare effects to show their contribution: first the wage spillover effects, then the improvement in amenity. Figure 6 and Table 6 presents the results. In the partial roll-out, control neighborhoods experience a 5% increase in welfare, which is entirely due to the labor market spillovers. The treated neighborhoods experience a much larger welfare gain (18.9%), of which about a third (6.1%) is due to direct effect from participation, and more than a half to rising private sector wages (10%), and the rest to improvements in amenities (2.7%). We next estimate welfare gains in the complete roll-out scenario. The welfare gains are larger (25.7%) overall, an increase that is driven by stronger labor market spillover effects (16.5%), while the direct benefits (6.2%) and the amenity effects (3%) are basically unchanged. These results make it clear that the labor market spillovers are a very important part of the welfare effects from the program.

As a benchmark, we estimate the welfare gains from a counterfactual policy, a cash transfer which would provide to households the utility equivalent of wages received on public works. As compared to the public works, this hypothetical cash transfer has the advantage of not imposing any work requirements, so that labor supply is unchanged.[18] At the same time, because labor supply is unaffected, there are no equilibrium wage effects. As the results in Figure 6 and Table 6 suggest, the cash transfer does better than the public works only if one focuses on the direct gains from participation. Once indirect effects on amenities and private sector wages are taken into account, the conclusion is overturned, and public works dominate cash.[19]

6 Conclusion

Our paper provides a comprehensive evaluation of the UPSNP, Ethiopia's urban public works program. We exploit the random roll-out of the program across neighborhoods in Addis-Ababa, which we combine with detailed survey data on local amenities, employment and wages. We first present reduced form evidence that the program improves local amenities, increases total employment, crowds out private sector employment and increases private wages. We then develop a spatial equilibrium model and leverage detailed data on commuting flows to compute the labor market spillovers of the program. We show that it increases wages by 20% in program neighborhoods and by 10% in neighborhoods that do not have the program, which suggests that the reduced form effect underestimates the spillover effects of the program. We then rely the structure of the model to compute the welfare effects of the program once completely rolled-out across the city. We show that two thirds of the welfare gains come from rising private wages, a fifth from improved amenities and a quarter come from public employment. Our results emphasize the importance of taking into account spillover effects in the evaluation of anti-poverty programs, and our paper provides a first example of how to do so through a combination of experimentation at scale and structural modelling.

[18] The literature on cash transfers in developing countries suggests that their effects on poor households' labor supply are negligible (Banerjee, Hanna, Kreindler, and Olken, Banerjee et al.)

[19] In Appendix D, we develop a quantification of the income gains from the program which does not rely on any modelling assumption about utility but ignores the gains from improved amenities. The results are very similar: the wage effects are more than two times larger than the direct effects, and taking them into account tips the balance in favor of public works against a cash transfer that would pay the equivalent of public works wages without any work requirement.

Tables and Figures

Figure 1: **Individual particiupation within treated households by men and women**

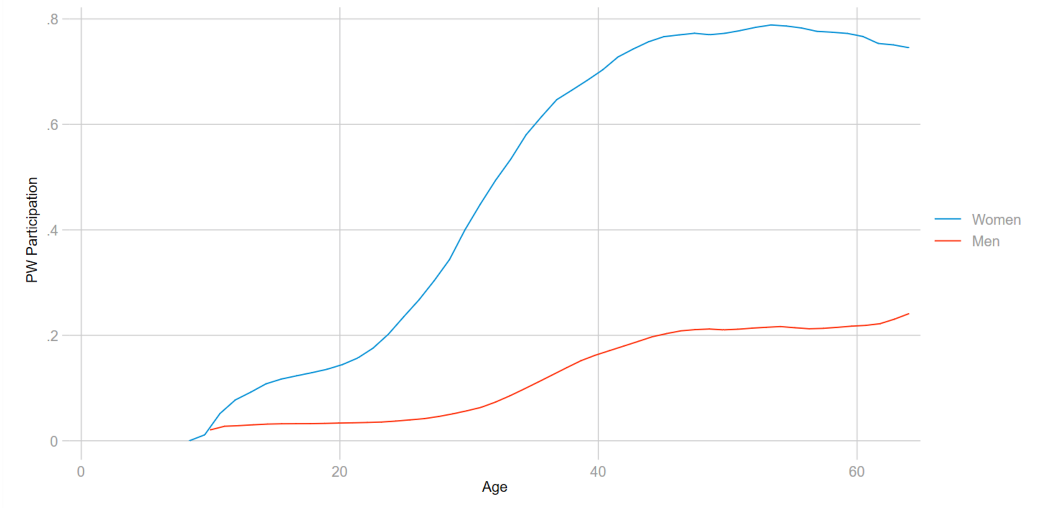

Figure 2: **Randomization outcome of the program across eligible woredas**

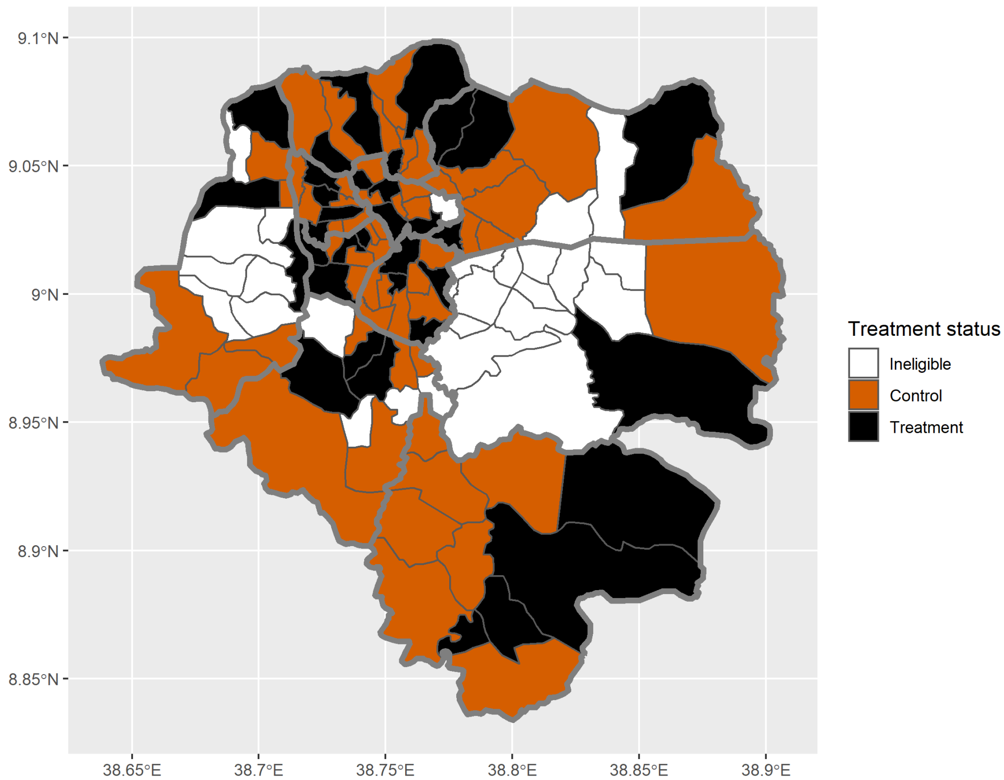

Figure 3: **Distribution of wages in public and private works at the time of the first endline survey**

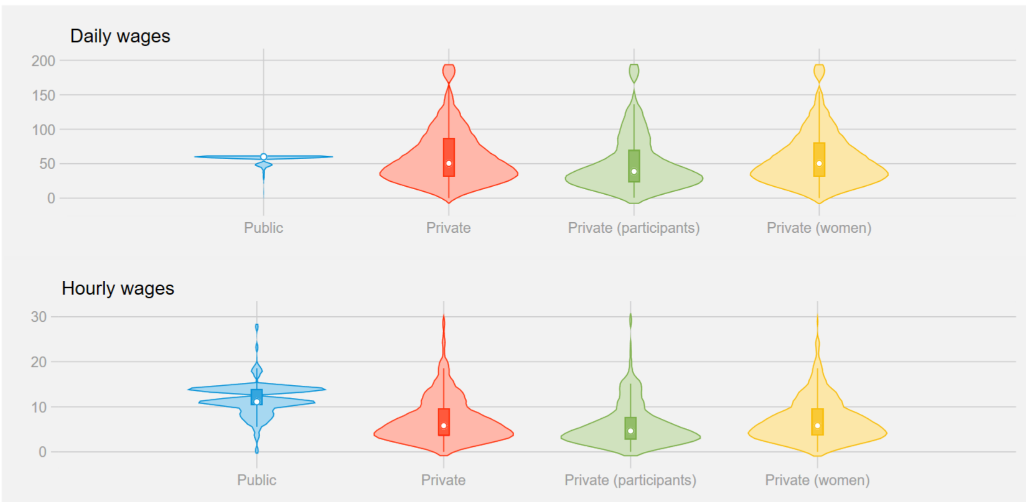

Figure 4: **Out-commuting rates as a percentage of workers by woreda**

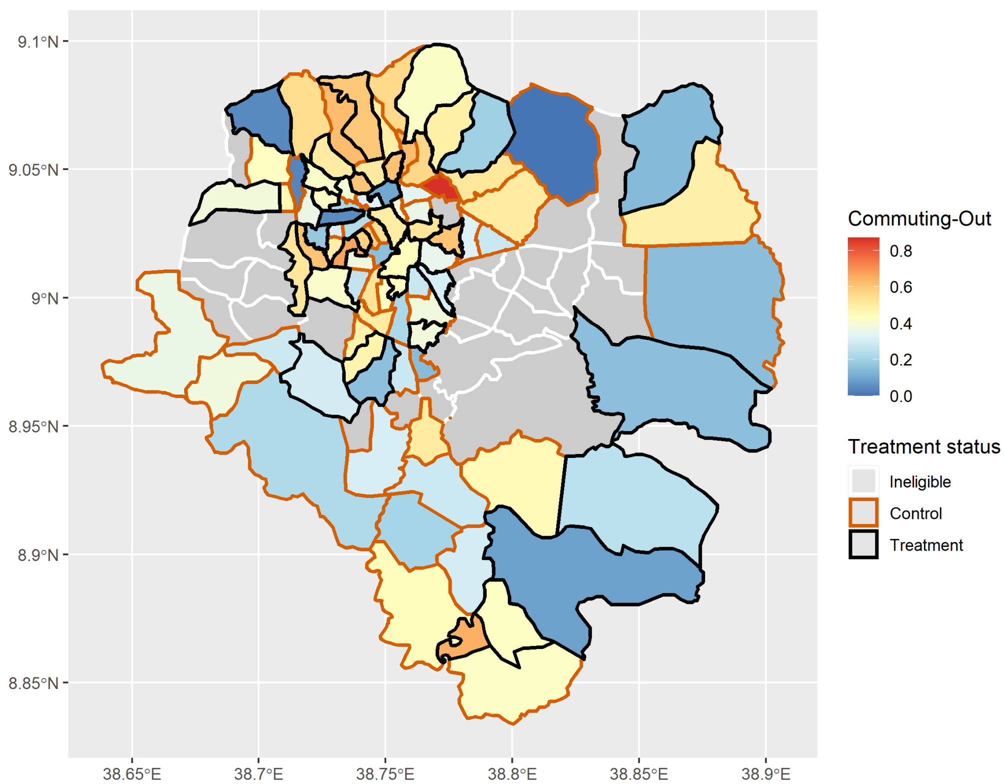

Figure 5: **In-commuting rates as a percentage of workers by woreda**

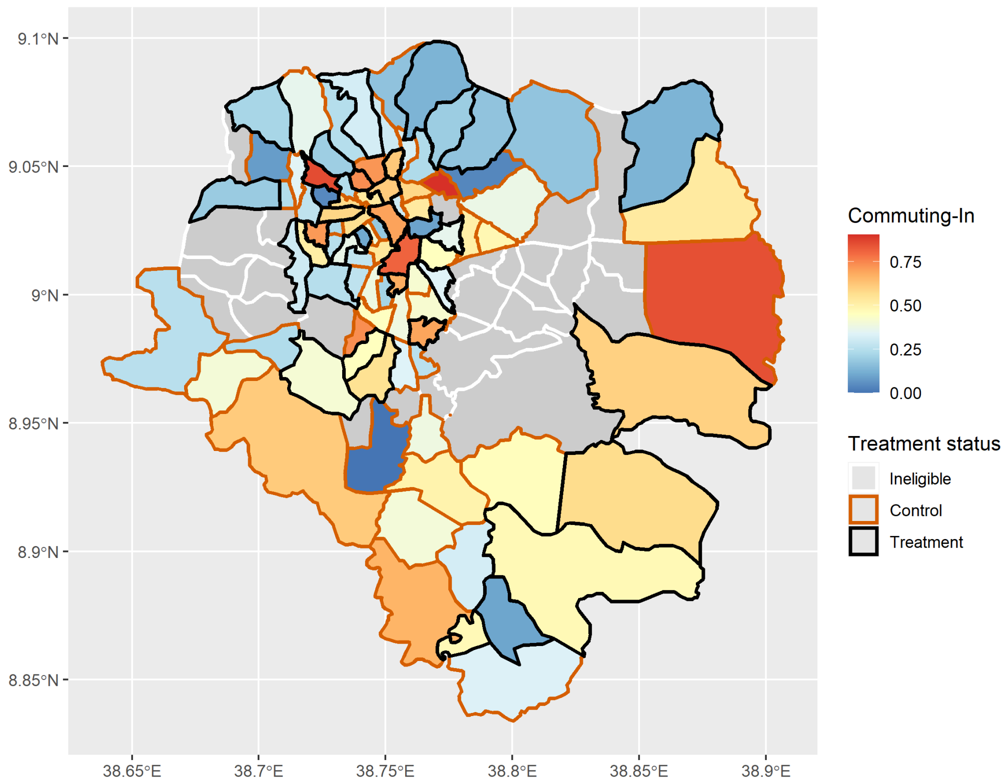

Figure 6: **Welfare effects of the program under partial and full roll-out compared to a cash transfer**

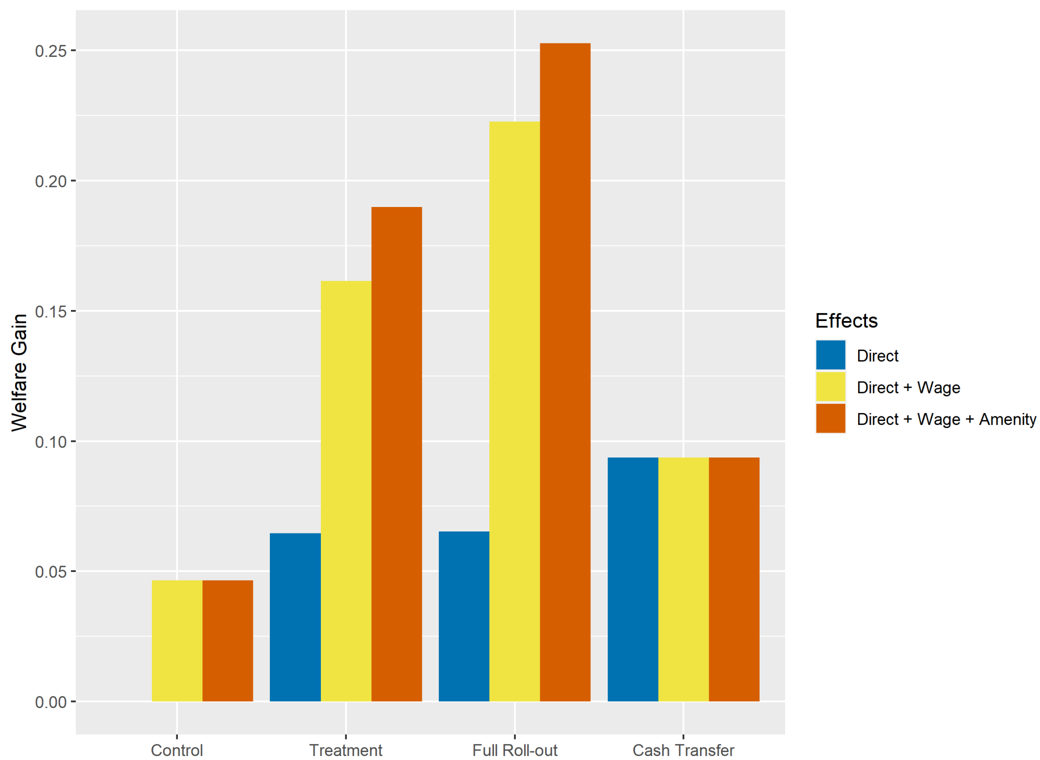

Table 1: Timeline of program roll out and data collection

Months	Year	Event
Oct-Nov	2016	Screening survey
Nov	2016	Woreda randomization
Nov-Jan	2016/17	Baseline survey collection
February	2017	Beneficiary targeting and selection for year 1
April	2017	Start of program in year 1 districts
March	2018	Endline survey 1.
July	2018	Beneficiary selection for year 2 (control woredas)
August	2018	Start of the program in year 2 woredas.
August	2018	Survey of treatment status in year 2 woredas.
December	2019	Endline survey 2.

Table 2: Main Reduced Form Effects

	Employment	Public Employment	Private Employment	Neighbourhood Amenities
	(1)	(2)	(3)	(4)
Panel A: Intention to treat				
Treatment (T)	0.008	0.052	−0.044	0.563
	(0.011)	(0.003)	(0.011)	(0.077)
Control Mean	0.374	0	0.374	-0.006
Observations	17,065	17,065	17,065	4,658
Panel B: Treatment by eligibility				
T×Eligible	0.029	0.101	−0.072	0.617
	(0.015)	(0.003)	(0.014)	(0.090)
T×Ineligible	−0.011	0.001	−0.012	0.513
	(0.012)	(0.001)	(0.011)	(0.085)
Control Mean Eligible	0.36	0	0.359	-0.001
Control Mean Ineligible	0.388	0	0.388	-0.011
Observations	17,065	17,065	17,065	4,658

Note: The unit of observation is an individual survey respondent. In columns 1 to 3 the sample is composed of all adult household members. In column 4 the sample is composed of one adult per household. "Employment" denotes total hours worked divided by 48 hours per week. Public employment denotes hours worked on public works divided by 48 hours per week. "Private employment" denotes hours worked on private sector wage work or self-employment divided by 48 hours per week. "Neighborhood Amenities" is a standardized index of answers to five questions about neighborhood quality describe in Appendix Table A1. "Treatment" is a dummy equal to one for households in treated neighborhoods. "Eligible" is a dummy equal to one for households who are eligible to participate in the public works program, and "Ineligible" a dummy equal to one for non-eligible households. All specifications include controls. Standard error are clustered at the woreda level.

Table 3: Labor Market Spillovers from the Public Works Program

	Log wages at origin	Log wages at destination
	(1)	(2)
Treatment at Origin	0.111	
	(0.040)	
Exposure of Destination		0.196
		(0.074)
RI p-values	0.013	0.009
Observations	90	90

Note: The unit of observation is a neighborhood. In columns 1 the dependent variable is log wages earned by workers who live in that neighborhood. In Column 2 the dependent variable is log wages earned by workers who work in that neighborhood. Treatment is a dummy equal to one if the neighborhood is treated. Exposure of a neighborhood j is defined as the sum of the treatment status of each neighborhood i weighted by the fraction of residents from i who work in neighborhood j. The sum includes neighborhood j itself. Actual exposure is recentered following Borusyak and Hull (2020) using average exposure across 2000 simulated treatment assignments.

Table 4: Correlation between Neighborhood Quality and Hypothetical Rents

	Log Hypothetical Rent	
	(1)	(2)
Neighborhood Quality Index	0.046	0.043
	(0.010)	(0.008)
Controls	No	Yes
Observations	4,694	4,694

Note: The unit of observation is a household. The dependent variable is the log of rents that each household pays for its housing or how much it would pay if it were to rent it (for households who own their housing or do not pay rents). The neighborhood quality index is a standardized index of answers to five questions about neighborhood quality describe in Appendix Table A1. In column 2 the specification includes household and housing controls selected by double lasso. Standard error are clustered at the woreda level.

Table 5: Commuting Elasticity with Respect to Wages

	Commuting Probability		Log Destination Wage
	Poisson	*Poisson-IV*	First Stage *OLS*
	(1)	(2)	(3)
Log Destination Wage	0.513	3.360	
	(0.341)	(1.807)	
Destination Exposure to Program			0.155
			(0.0001)
Log walking time	−2.291	−2.310	0.008
	(0.085)	(0.084)	(0.002)
Observations	7,744	7,744	7,744

Note: All specifications include origin fixed effects

Note: The unit of observation is a neighborhood origin×destination pair. The dependent variable is the commuting probability. Log destination wage is the log of private sector income per hour earned by workers who work in the neighborhood of destination. Destination Exposure to the Program is for each neighborhood of destination j equal to the sum of treatment status of all neighborhoods i weighted by the commuting probability from i to j. Following Borusyak and Hull (2020), we re-center actual exposure using average exposure to 2000 simulated treatment assignment. Log Walking Time is the log of minutes needed to walk between the centroid of the origin and destination neighborhoods according to Google API. In Column 1 the estimation is done with OLS. In Column 2 Log Destination Wage is instrumented with the Destination Exposure to the Program. Column 3 presents the first stage of the estimation. All specifications include origin fixed effects.

Table 6: Welfare Effects of the Public Works Program

Roll-out	Partial		Complete
	Control	Treatment	All
	(1)	(2)	(3)
Treatment	0.00	1.000	1.000
Exposure	0.16	0.752	1.000
Direct Effect	1.00	1.061	1.062
Direct + Wage Effects	1.05	1.161	1.227
Direct+Wage+Amenity	1.05	1.189	1.257
Cash Transfer	1.00	1.091	1.092

References

Ahlfeldt, G. M., S. J. Redding, D. M. Sturm, and N. Wolf (2015, November). The Economics of Density: Evidence From the Berlin Wall. *Econometrica 83*, 2127–2189.

Alik-Lagrange, A., O. Attanasio, C. Meghir, S. Polana-Reyes, and M. Vera-Hernandes (2017). Work pays: Different benefits of a workfare program in colombia.

Angelucci, M. and G. D. Giorgi (2009, March). Indirect Effects of an Aid Program: How Do Cash Transfers Affect Ineligibles' Consumption? *American Economic Review 99*(1), 486–508.

Balboni, C., G. Bryan, M. Morten, and B. Siddiqi (2020). Transportation, gentrification, and urban mobility: The inequality effects of tanzania's brt system. Technical report.

Banerjee, A. V., R. Hanna, G. E. Kreindler, and B. A. Olken. Debunking the Stereotype of the Lazy Welfare Recipient: Evidence from Cash Transfer Programs. *32*(2), 155–184.

Beegle, K., E. Galasso, and J. Goldberg (2017). Direct and indirect effects of Malawi's public works program on food security. *Journal of Development Economics 128*(C), 1–23.

Berhane, G., D. O. Gilligan, J. Hoddinott, N. Kumar, and A. S. Taffesse (2014). Can Social Protection Work in Africa? The Impact of Ethiopia's Productive Safety Net Programme. *Economic Development and Cultural Change 63*(1), 1–26.

Bertrand, M., B. Crepon, A. Marguerie, and P. Premand (2017). Contemporaneous and post-program impacts of a public works program: Evidence from cote d'ivoire.

Borusyak, K. and P. Hull (2020, September). Non-Random Exposure to Exogenous Shocks: Theory and Applications. CEPR Discussion Papers 15319, C.E.P.R. Discussion Papers.

Crépon, B., E. Duflo, M. Gurgand, R. Rathelot, and P. Zamora (2013). Do labor market policies have displacement effects? evidence from a clustered randomized experiment. *The quarterly journal of economics 128*(2), 531–580.

Cunha, J. M., G. D. Giorgi, and S. Jayachandran (2019). The Price Effects of Cash Versus In-Kind Transfers. *Review of Economic Studies 86*(1), 240–281.

Egger, D., J. Haushofer, E. Miguel, P. Niehaus, and M. W. Walker (2019, December). General Equilibrium Effects of Cash Transfers: Experimental Evidence from Kenya. NBER Working Papers 26600, National Bureau of Economic Research, Inc.

Gazeaud, J., V. Stephane, et al. (2020). Productive workfare? evidence from ethiopia's productive safety net program. Technical report, Universidade Nova de Lisboa, Faculdade de Economia, NOVAFRICA.

Harris, J. R. and M. P. Todaro (1970). Migration, unemployment and development: A two-sector analysis. *The American Economic Review 60*(1), pp. 126–142.

Heblich, S., S. J. Redding, and D. M. Sturm (2020, 05). The Making of the Modern Metropolis: Evidence from London*. *The Quarterly Journal of Economics*. qjaa014.

Imbert, C. and J. Papp (2015). Labor market effects of social programs: Evidence from india's employment guarantee. *American Economic Journal: Applied Economics 7*(2), 233–63.

Imbert, C. and J. Papp (2020, 4). Short-term Migration, Rural Public Works, and Urban Labor Markets: Evidence from India. *Journal of the European Economic Association 18*(2), 927–963.

Imbert, C., M. Seror, Y. Zhang, and Y. Zylberberg (2020). Migrants and Firms : Evidence from China. Technical report.

Kline, P. M. and E. Moretti. Local Economic Development, Agglomeration Economies, and the Big Push: 100 Years of Evidence from the Tennessee Valley Authority. *129*(1), 275–331.

Lewis, W. A. (1954). Economic development with unlimited supplies of labour. *22*(2), 139–191.

Manning, A. and B. Petrongolo. How Local Are Labor Markets? Evidence from a Spatial Job Search Model. *107*(10), 2877–2907.

Monras, J. (2020). Immigration and wage dynamics: Evidence from the mexican peso crisis. *128*(8), 3017–3089.

Monte, F., S. J. Redding, and E. Rossi-Hansberg (2018, December). Commuting, migration, and local employment elasticities. *American Economic Review 108*(12), 3855–90.

Moretti, E. *Local Labor Markets*, Volume 4 of *Handbook of Labor Economics*, Chapter 14, pp. 1237–1313. Elsevier.

Muralidharan, K. and P. Niehaus (2017). Experimentation at scale. *Journal of Economic Perspectives 31*(4), 103–24.

Muralidharan, K., P. Niehaus, and S. Sukhtankar (2017, September). General Equilibrium Effects of (Improving) Public Employment Programs: Experimental Evidence from India. NBER Working Papers 23838, National Bureau of Economic Research, Inc.

Ravallion, M. (1987). Market responses to anti-hunger policies. *1987*(78).

Redding, S. J. and E. Rossi-Hansberg (2017). Quantitative spatial economics. *Annual Review of Economics 9*, 21–58.

Sukhtankar, S. (2016). India's national rural employment guarantee scheme: What do we really know about the world's largest workfare program? In *India Policy Forum*, Volume 13, pp. 2009–10.

Tsivanidis, N. (2018). The aggregate and distributional effects of urban transit infrastructure: Evidence from bogotá's transmilenio. *Unpublished manuscript*.

APPENDIX

A Additional tables and figures

Table A1: Determinants of endline attrition

	Household responded to endline			
	(1)	(2)	(3)	(4)
	Coeff	SE	Coeff	SE
Woreda Selected Year 1	0.008	0.007	0.009	0.007
Household head is female			-0.003	0.006
Age of household head			0.000	0.000
Any member of the household has a disability			0.005	0.005
Household head employed at baseline			0.002	0.004
Head education: primary school			-0.001	0.008
Head education: high school			-0.016	0.010
Max years of education in household			0.000	0.001
Head education: any higher ed			-0.004	0.011
Household rents from kebele			0.019	0.009**
Household has a hard floor			-0.001	0.005
Household has an improved toilet			0.007	0.005
Household size			0.007	0.001***
Household weekly food expenditure			0.000	0.000
P-value of F-test	0.2687		0.0008	
N	6,093		6,093	

Note: The unit of observation is a household. The table presents the results of two regressions in which the dependent variable is a dummy equal to one if the household surveyed at baseline was also surveyed at endline. Column 1 and 3 presents coefficients and Column 2 and 4 present standard errors.

Table A1: Balance at baseline

Outcome		All households		Eligible Only		Ineligible Only	
	N	CM	TE	CM	TE	CM	TE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Female HH head	5,911	0.605	0.021	0.598	0.044	0.793	0.016
			(0.024)		(0.027)		(0.037)
Age HH head	5,911	56.444	0.312	52.645	0.082	65.048	0.479
			(0.751)		(0.903)		(0.994)
Children under 5	5,911	0.350	-0.030	0.417	-0.027	0.192	-0.043
			(0.023)		(0.033)		(0.032)
Children 5 to 13	5,911	0.659	-0.050	0.780	-0.054	0.371	-0.053
			(0.045)		(0.055)		(0.062)
Children 13 to 18	5,911	0.759	0.006	0.840	0.020	0.490	0.024
			(0.042)		(0.045)		(0.066)
Household size	5,911	5.211	-0.108	5.381	-0.084	3.983	-0.057
			(0.140)		(0.150)		(0.180)
Disabled member of the household	5,911	0.171	0.000	0.164	0.005	0.266	-0.005
			(0.016)		(0.020)		(0.018)
HH head primary school	5,911	0.095	0.004	0.105	0.005	0.040	0.010
			(0.008)		(0.015)		(0.013)
HH head secondary school	5,911	0.052	-0.000	0.051	0.002	0.023	-0.002
			(0.006)		(0.009)		(0.009)
Maximum years school in hh	5,911	10.044	-0.159	9.766	-0.057	9.027	-0.090
			(0.180)		(0.226)		(0.234)
Rented from kebele	5,911	0.748	0.016	0.755	0.012	0.825	0.008
			(0.052)		(0.052)		(0.071)
Solid floor	5,911	0.461	-0.013	0.412	-0.023	0.468	0.026
			(0.040)		(0.046)		(0.045)
Improved toilet	5,911	0.204	0.005	0.221	-0.032	0.226	0.035
			(0.030)		(0.036)		(0.032)
Number of rooms	5,911	1.252	-0.013	1.143	-0.041	1.112	0.025
			(0.058)		(0.063)		(0.073)
Owns refrigerator	5,911	0.176	-0.022	0.142	-0.020	0.141	-0.021
			(0.017)		(0.021)		(0.024)
Owns a tv	5,911	0.765	0.018	0.744	0.027	0.690	0.025
			(0.022)		(0.030)		(0.025)
Owns a mobile phone	5,911	0.926	-0.013	0.941	-0.002	0.831	-0.021**
			(0.010)		(0.014)		(0.010)
Owns at satellite	5,911	0.540	0.002	0.530	0.000	0.445	0.009
			(0.029)		(0.036)		(0.037)
Owns a sofa	5,911	0.467	0.022	0.411	0.021	0.449	0.046
			(0.029)		(0.036)		(0.036)
Weekly food expenditure	5,911	348.919	-7.988	348.568	-13.873	273.433	-2.270
			(13.171)		(15.667)		(16.443)

Note: The unit of observation is a household. Each row presents the results from regressing a given outcome variable at baseline on a dummy for treated neighborhoods for three different samples: the whole sample (Columns 2 and 3), the sample of eligible households only (Columns 4 and 5) and the sample of ineligible households (Columns 6 and 7). Column 1 gives the number of observations in the whole sample. Column 2, 4, and 5 present the control mean. Column 3, 5 and 7 present the estimated treatment effect.

Table A1: Effect of the Program on Rents and Residential Mobility

	Log Rent	Emigration
	(1)	(2)
Treatment	0.035	−0.004
	(0.058)	(0.004)
Control Mean		0.021
Observations	1,021	5,813

Note: The unit of observation is a household. Each column presents the results of a separate regression. In column 1 the dependent variable is log of rents actually paid by households at endline. It is missing for 82% of households who do not pay rent. In Column 2 the dependent variable is a dummy equal to one if the households has changed location between baseline and endline. Standard errors are clustered at the neighborhood level.

Table A1: Reduced form impact on the program on households

	(1)	(2)	(3)	(4)	(5)
	Income	Pub. wages	Priv. income	Expenditure	Savings
Panel A: Intention to treat					
Treatment (T)	306.403	432.565	−105.596	−54.347	750.930
	(103.970)	(11.531)	(98.650)	(87.463)	(167.680)
Control Mean	2360.549	2	1962.6	3303.4	1879.4
Observations	5,911	5,911	5,911	5,911	5,911
Panel B: Treatment by eligibility					
T×Eligible	566.115	1,032.636	−357.530	37.736	1,763.144
	(113.928)	(7.493)	(116.968)	(102.757)	(180.535)
T×Ineligible	189.516	75.815	132.982	−80.934	78.090
	(182.640)	(8.060)	(173.784)	(112.719)	(259.511)
Con. Mean Eligible	2141.143	3.339	1829.78	3167.317	1516.006
CM Non-ligible	2805.144	0	2356.722	3720.162	2397.991
Observations	5,911	5,911	5,911	5,911	5,911

Note: All specifications include controls

Note: The unit of observation is a household. Each column presents the results of a separate regression. The dependent variable is household income in Column 1, income from public works in Column 2, private sector employment, including wage work and self-employment in Column 3, household expenditures in Column 4, and household savings in Column 5. Standard errors are clustered at the neighborhood level.

Table A1: Effects on Private Employment by Gender and Skill Level

	Private Employment			
	Female	Male	Low Skill	High Skill
	(1)	(2)	(3)	(4)
Panel A: Intention to treat				
Treatment (T)	−0.054	−0.034	−0.055	−0.044
	(0.011)	(0.016)	(0.012)	(0.011)
Control Mean	0.323	0.436	0.348	0.372
Observations	9,330	7,735	12,119	16,425
Panel B: Treatment by eligibility				
T x Eligible	−0.099	−0.041	−0.086	−0.071
	(0.013)	(0.020)	(0.015)	(0.014)
T x Ineligible	−0.006	−0.023	−0.015	−0.012
	(0.014)	(0.020)	(0.014)	(0.012)
Control Mean Eligible	0.32	0.408	0.341	0.356
Control Mean Ineligible	0.325	0.462	0.356	0.387
Observations	9,330	7,735	12,119	16,425

Note: The unit of observation is an individual survey respondent. The sample is composed of all female adults in Column 1, of all male adults in Column 2, of all adults who did not complete high school in Column 3, and of adults who completed high school in Column 4. "Private employment" denotes hours worked on private sector wage work or self-employment divided by 48 hours per week. "Neighborhood Amenities" is a standardized index of answers to five questions about neighborhood quality describe in Appendix Table A1. "Treatment" is a dummy equal to one for households in treated neighborhoods. "Eligible" is a dummy equal to one for households who are eligible to participate in the public works program, and "Ineligible" a dummy equal to one for non-eligible households. All specifications include controls. Standard error are clustered at the woreda level.

Table A1: Labor Market Spillovers by Gender and Skill Level

	Log wages at destination			
	Female	Male	Low Skill	High Skill
	(1)	(2)	(3)	(4)
Exposure	0.057	0.225	0.249	0.027
	(0.079)	(0.097)	(0.078)	(0.109)
RI p-values	0.476	0.0385	0.002	0.7925
Observations	90	90	90	85

Note: The unit of observation is a neighborhood. The dependent variable is log wages at endline and the specification controls for log wages at baseline. Exposure of a neighborhood j is defined as the sum of the treatment status of each neighborhood i weighted by the fraction of residents from i who work in neighborhood j. The sum includes neighborhood j itself. Actual exposure is recentered following Borusyak and Hull (2020) using average exposure across 2000 simulated treatment assignments.

Table A1: Commuting Elasticity with Respect to Commuting Cost

	Commuting Probability			
	Poisson	*Poisson-IV*	*Poisson*	*Poisson-IV*
	(1)	(2)	(3)	(4)
Log Commuting cost	−0.913	−4.711		
	(0.032)	(0.071)		
Log Commuting time			−1.426	−5.307
			(0.027)	(0.074)
Observations	840	840	898	898

Note: The unit of observation is a neighborhood origin×destination pair. The dependent variable is the commuting probability. Log Commuting Cost is the log of the average cost paid by commuters according to the survey. Log Commuting Time is the log of the average time spent by commuters according to the survey. Columns 1 and 3 are estimated with OLS. In Columns 2 the Log commuting cost is instrumented by Log Walking time according to Google API. In Column 4 the Log Commuting time is instrumented by Log walking time according to Google API. The number of observations is lower than in Table 5 because some commuters did not report their expenses (Columns 1 and 2) or their commuting time (Columns 3 and 4). All specifications include origin and destination fixed effects.

Table A1: Summary statistics of components of the neighbourhood amenities index

	Obs	Mean	SD
Drainage and sewerage (satisfied-yes/no)	5,710	0.554	0.497
Cleanliness of streets (satisfied-yes/no)	5,710	0.581	0.493
Public toilets (quality 1-4)	5,710	3.377	0.969
Smell of trash (how often do you notice) (-)	5,710	3.063	1.103
Smell of drains (how often do you notice) (-)	5,710	2.661	1.191

B Mathematical Appendix

B.1 Proof of Equation 3

The expected utility for worker living in i follows a Frechet distribution with cumulative distribution function:

$$G_i(u) = e^{-\Phi_i u^{-\theta}} \quad where \quad \Phi_i = \sum_j (B_i \tau_{ij} w_j)^\theta$$

The density function $g(U)$ is hence:

$$g_i(U) = \theta \Phi_i U^{-\theta-1} e^{-\Phi_i U^{-\theta}}$$

We write the expectation:

$$E[U_i] = \int_0^\infty U g(U) dU = \int_0^\infty U \theta \Phi_i U^{-\theta-1} e^{-\Phi_i U^{-\theta}} dU$$

We change variables to $V = \Phi_i U^{-\theta}$, we have $U = \Phi_i^{\frac{1}{\theta}} V^{-\frac{1}{\theta}}$ and $dV = -\theta \Phi_i U^{-\theta-1} dU$

$$E[U_{ij}] = \int_0^\infty \Phi_i^{\frac{1}{\theta}} V^{-\frac{1}{\theta}} e^{-V} dV$$

We then use the gamma distribution function: $\Gamma(\alpha) = \int_0^\infty x^{1-\alpha} e^{-x} dx$

$$E[U_{ij}] = \Phi_i^{\frac{1}{\theta}} \int_0^\infty V^{(1-\frac{1}{\theta})-1} e^{-V} dV = \Phi_i^{\frac{1}{\theta}} \Gamma\left(\frac{\theta - 1}{\theta}\right)$$

Going back to the definition of Φ_i yields the expected utility for a worker living in i:

$$E[U_i] = \Gamma\left(\frac{\theta-1}{\theta}\right) \left[\sum_j (B_i \tau_{ij} w_j)^\theta\right]^{\frac{1}{\theta}}$$

which completes the proof.

B.2 Proof of Equation 8

We obtain the change in expected utility $\widehat{U}_i$ by dividing the expression of utility in 7 with the expression in 3, and using the result in equation 4 to substitute π_{ii} for $\frac{(B_i w_i)^\theta}{\sum_j (B_i \tau_{ij} w_j)^\theta}$:

$$\begin{aligned}
\widehat{U}_i = & \frac{\left[p_i\left((1+g)\widehat{B_i}\right)^\theta (B_i w_i)^\theta + (1-p_i)\sum_j (\widehat{B_i}\widehat{\tau_{ij}}\widehat{w_j})^\theta (B_i \tau_{ij} w_j)^\theta\right]^{\frac{1}{\theta}}}{\left[\sum_j (B_i \tau_{ij} w_j)^\theta\right]^{\frac{1}{\theta}}} \\
= & \left[p_i (1+g)^\theta \pi_{ii} \widehat{B_i}^\theta + (1-p_i)\sum_j \pi_{ij} (\widehat{B_i}\widehat{w_j})^\theta\right]^{\frac{1}{\theta}} \\
= & (1+\beta_i)\left[p_i \pi_{ii} (1+g)^\theta + (1-p_i)\sum_j \pi_{ij} (\widehat{w_j})^\theta\right]^{\frac{1}{\theta}}
\end{aligned}$$

B.3 Proof of Equation 9

We consider the welfare effect of a cash transfer that has the same size as the wages earned on the public works, i.e. $pT_i(1+g)w_i$.

Expected utility for a worker living in i is:

$$\widehat{U_i^{cash}} U_i = \gamma \left[\sum_j (B_i \tau_{ij} w_j)^\theta + (B_i p T_i (1+g) w_i)^\theta\right]^{\frac{1}{\theta}}$$

We obtain the change in expected utility $\widehat{U_i^{cash}}$ by dividing this expression with the expression in 3:

$$\begin{aligned}
\widehat{U_i^{cash}} = & \frac{\gamma \left[\sum_j (B_i \tau_{ij} w_j)^\theta + (pT_i(1+g))^\theta (B_i w_i)^\theta\right]^{\frac{1}{\theta}}}{\gamma \left[\sum_j (B_i \tau_{ij} w_j)^\theta\right]^{\frac{1}{\theta}}} \\
= & \left[1 + (pT_i(1+g))^\theta \pi_{ii}\right]^{\frac{1}{\theta}}
\end{aligned}$$

C Price effects

C.1 Effects on local prices

As discussed in section 4, we do not find evidence that the program increases household expenditures (Appendix Table A1), hence it is unlikely that the program would increase demand and hence prices. Goods and services markets are also likely to be well integrated within the city, so that any local demand effect would be transmitted through the whole city and would remain small overall. In this section, we implement an empirical test for the price effects of the program.

We use the official micro data used for the Consumer Price Index, which is collected for 615 commodities from 12 markets throughout the city. We aggregate the price information into 12 expenditure classes using the official weights. We combine this data with expenditure shares from the household survey for each of the 12 expenditures classes. We exclude two expenditure classes: "Alcohol beverages and tobacco" has close to zero reported expenditures in the survey, and "Miscellaneous" could not be matched with the survey. We focus on the ten most important expenditure classes: Food, Clothing,Household items, Housing, Health,Transport,Communication, Recreation, Education, and Restaurants.

Our empirical specification consists in a market-level regression of log market prices on program exposure, where exposure is defined as a sum of treatment status in each neighborhood weighted by its eligible population and the inverse of the distance to the market. Formally, let m denote a market, p_m the price of a given class or the price index, and $Exposure_m$ denotes its exposure to treatment, we estimate with OLS the following equation:

$$\ln p_m = \alpha + \beta Exposure_m + \varepsilon_m \tag{C1}$$

Exposure of the market m is defined as:

$$Exposure_m = \left[\sum_i \frac{N_i}{d_{im}} T_i - \frac{1}{R} \sum_{0 \leq r \leq R} \sum_i \frac{N_i}{d_{im}} \tilde{T}_i^r \right]$$

where N_i is the population in each neighborhood i that is eligible to the program, d_{im} is the euclidean distance between each neighborhood and the market, and T_i is the treatment status of neighborhood i. Exposure is re-centered following Borusyak and Hull (2020) using average exposure across 2000 simulated treatment assignment $\tilde{T}_i^r$. Given the small number of observations, usual inference can be problematic: p-values are obtained via randomization inference.

The results are presented in Table C1 below. The effect overall and on the most important expenditure classes is close to zero (Columns 1 to 4). There are a few significant negative effects for Housing, Health, Recreation and Restaurant, rare expenditures for our sample who does not pay rent and does not often go out. These results do not provide any evidence that prices rise in markets and products most exposed to a potential rise in demand from eligible households.

Table C1: Impact of treatment exposure on product prices from CPI data

	All items	Food	Clothing	Household
	(1)	(2)	(3)	(4)
Exposure	−0.324	0.108	−0.338	0.341
	(1.081)	(0.419)	(0.413)	(0.626)
RI p-values	0.276	0.8605	0.371	0.5845
Observations	120	12	12	12
	Housing	Health	Transport	Communication
	(5)	(6)	(7)	(8)
Exposure	−1.421	−1.474	−0.690	0.286
	(0.687)	(0.775)	(0.592)	(0.876)
RI p-values	0.0315	0.0145	0.283	0.8055
Observations	12	12	12	12
	Recreation	Education	Restaurant	
	(9)	(10)	(11)	
Exposure	−5.565	1.223	−0.897	
	(3.139)	(1.146)	(0.288)	
RI p-values	0.051	0.5565	0.0465	
Observations	12	12	12	

Note: Each column presents the result of a separate regression. In column 1 the unit of observation is a market×expenditures class, and each observation is weighted by the expenditure share of the class in the household survey. In column 2 to 11 the unit of observation is a market. The dependent variable is log price. Exposure is the sum of treatment status in each neighborhood weighted by the population eligible to the program and the inverse of the distance from the centroid of the neighborhood to the market where the price is measured. Following Borusyak and Hull (2020) exposure is re-centered using average exposure across 2000 simulated treatment assignments. RI p-values are p-values obtained through randomization inference, with 2000 simulated treatment assignments.

D Income effects

In this section, we develop an alternative evaluation of the public works program which focuses on income gains. The advantage of this approach is that it does not require any assumption on the utility function. Its shortcoming is that it ignores the utility gains from improved amenities but instead focus on the benefits from program participation and from rising private sector wages.

Income without the program is:

$$v_0 = \sum_j \pi_{ij} w_j$$

Income with the program is:

$$v_1 = pT_i(1+g)w_i + (1-pT_i)\sum_j \pi_{ij}\widehat{w_j}w_j$$

The proportional change in income due to the program is:

$$\widehat{v_i} = \frac{pT_i(1+g)w_i + (1-pT_i)\sum_j \pi_{ij}\widehat{w_j}w_j}{\sum_j \pi_{ij}w_j}$$

Using the expression of the direct income gains from the program (equation 5 in the model), we decompose the proportional change in income due to the program in two components:

$$\widehat{v_i} = \underbrace{pT_i\frac{(1+g)w_i - \sum_j \pi_{ij}w_j}{\sum_j \pi_{ij}w_j}}_{Direct\ Effect} + \underbrace{(1-pT_i)\frac{\sum_j \pi_{ij}w_j\widehat{w_j} - \sum_j \pi_{ij}w_j}{\sum_j \pi_{ij}w_j}}_{Wage\ Effect}$$

where the direct effect is the net income gain from public sector wages minus forgone private sector wages, and the wage effect is the net increase in income from the private sector due to rising wages.

We compare the income gains from the program to those from a cash transfer that would provide the same income as public works wages but without any work requirement, i.e. without forgone income from the private sector and without any increase in private sector wages.

$$\widehat{v_i^{cash}} = \frac{pT_i(1+g)w_i + \sum_j \pi_{ij}w_j}{\sum_j \pi_{ij}w_j} \tag{D1}$$

The results are presented in Appendix table D1 below

Table D1: Income gains from public works compared to a cash transfer

Roll-out	Partial		Complete
	Control	Treatment	All
	(1)	(2)	(3)
Treatment	0.000	1.000	1.000
Exposure	0.160	0.752	1.000
Income Gain (Direct)	0.000	0.074	0.071
Income Gain (Spillovers)	0.049	0.108	0.180
Income Gain (Total)	0.049	0.182	0.251
Income Gain (Cash Transfer)	0.000	0.192	0.189

Note: Column 1 and 2 present income effects in treated and control neighborhoods when the program is only implemented in treated neighborhoods. Column 3 presents income effects when the program is implemented in all neighborhoods. The direct effect is the net income gain from public sector wages minus forgone private sector wages, and the wage effect is the net increase in income from the private sector due to rising wages. The cash transfer provides the same income as public sector wages but without work requirement, i.e. without forgone private sector income or wage effects.