

Urban Public Works in Spatial Equilibrium: Experimental Evidence from Ethiopia

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Abstract

This paper evaluates Ethiopia’s Urban Productive Safety Net Program, which provides employment on local public works to the urban poor, and was rolled out randomly across neighborhoods of Addis Ababa. We find that the program increased public employment and reduced private labor supply among beneficiaries. We also show that it improved local amenities in treated locations, for both beneficiary and non-beneficiaries. We then develop a spatial equilibrium model and leverage unique data on commuting flows to quantify the effect of exposure to changes in labor supply from treated locations on labor markets across the city. Our estimates imply that once fully rolled out the program increased private wages by 18.6%. Finally, we use the model to compute the welfare gains to the poor: when we include the indirect effects on private wages and local amenities the welfare gains are four times larger than the direct benefits from public employment alone.

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1 Introduction

In addition to their direct effects on beneficiaries, social programs can have indirect effects that 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). 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), and none in urban areas.

Estimating indirect effects of social programs is challenging for at least five reasons. First, it requires exogenous variation in the implementation of a program on a large scale, which is rare. Second, researchers need information on outcomes of beneficiaries and non-beneficiaries in treated and untreated locations. Third, when program effects spill over across space the simple comparison between treated and untreated locations is likely to yield biased estimates and miss benefits to untreated locations. These spatial spillovers are likely to be important when markets are strongly interconnected, for example in urban areas, but evidence on these connections is limited.¹ Fourth, the effects of a program once it is fully rolled out may differ from the estimated effects under partial roll-out. Finally, a comprehensive program evaluation needs to put together direct and indirect effects in a single metric, i.e. income or welfare.

This paper develops a new approach to estimate the direct and indirect effects of Ethiopia’s Urban Productive Safety Net Program (UPSNP), a large urban public works program. 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). Specifically, we exploit the gradual roll-out of UPSNP across randomly chosen neighborhoods of Addis Ababa to estimate its effect on earnings and employment, local amenities and private sector wages. A spatial equilibrium model guides our analysis: we estimate labor market spillovers across locations by regressing wages on *exposure* to the program through the commuting network. We also use the model to compute the welfare gains to the poor once the pro-

¹A typical solution to this problem is to compare among untreated units those that are within or beyond a certain radius from treated units. This parametric approach may not fully capture spatial spillovers if economic interactions are not only based on distance, e.g. if they follow a gravity model, and is ill-suited to dense urban settings.

gram was fully rolled out, including direct income gains from participation, and indirect gains from improvement in amenities and rising wages.

We proceed in six steps. In the first step, we exploit the randomized roll-out of the program across neighborhoods (woredas) of the city of Addis Ababa in its first year of implementation. We collected precisely geo-referenced panel data on beneficiary and non-beneficiary households across the city. We start by comparing households who live in woredas with and without the program.² The results suggest that the program generated public employment but reduced the labor supply to the private sector by about 12.8%, so that the net effect of the program on employment is close to zero and insignificant.³ This reduction in labor supply is likely to affect private sector wages. Since 45% of workers commute to another woreda, the wage effects of the program are likely to spill over beyond treated woredas, so that comparing wages in treated and control areas would yield unreliable estimates.⁴

To guide our evaluation of the direct and spillover effects of the UPSNP, in the second step we develop a spatial model which borrows from the urban economics literature (Heblich et al., 2020; Balboni et al., 2020). Our model is a simplified version of the canonical models, as it only includes commuting and no migration or trade.⁵ We leverage 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, and (iii) to provide counterfactual comparisons of the program under full roll-out and a cash transfer.

In the third step we estimate labor market spillovers. The model provides an expression of equilibrium wage changes in each local labor market as a function of exposure to changes in labor supply from commuters who live in treated neighborhoods. We use rich commuting data to measure wages in each labor market, i.e. in the woreda where workers earn, rather than where they live.⁶ We measure exposure of a given labor market (woreda) as the weighted sum of treatment status in all woredas, weighted by the share of commuters to that

²Treatment woredas were chosen randomly through a public lottery.

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

⁴We prespecified the experimental design, intention-to-treat specifications, and the labor supply and amenities outcomes in a pre-analysis plan at [AEARCTR-0003387](#).

⁵We do not find evidence that the program affects residential mobility or rents, consumption expenditures or local prices.

⁶Like Monte et al. (2018), we do not take a stand on the spatial extent of labor markets. Instead we define local labor markets as the most fine-grained possible geography (ie. the woreda) and the explicitly model the linkages between labor markets. Throughout the paper, “labor market” refers simply to the woreda in which people work.

labor market that come from these treated woredas in the baseline commuting data. To account for the fact that even if treatment is randomized, exposure to the treatment is not randomly assigned, we follow [Borusyak and Hull \(2020\)](#) and re-center our measure of exposure using potential exposure to 2,000 re-randomizations of the treatment assignment. Our main estimate implies that private sector wages increased by 14% in treated and 3% in untreated labor markets under partial roll-out,⁷ and by 18.6% everywhere under full roll-out. By comparison, the ITT estimate which compares wages in treated and control locations and ignores spillovers through commuting is a much smaller 9.3%.

In the fourth step, we estimate the effect of the program on local amenities. Using an index which aggregates five subjective indicators of amenities, we show that neighbourhood quality increased by 0.6 SDs in treated neighborhoods relative to untreated ones. The improvement in amenities was felt by beneficiary and non-beneficiary households. Because UPNSP projects were carried out on a small scale within treated neighborhoods, we do not expect spillover effect on amenities in control neighborhoods. To quantify the value of improvements in public goods, we correlate these measures of local amenities with private market rents. Overall, we estimate an effect on amenities equivalent to 2.5% of total local amenity value.⁸

In the fifth step 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, and therefore the welfare gains each urban resident from access to higher wages through their commuting network. 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 programs. This methods yields an estimate of 2.08. We also estimate the Frechet parameter as the elasticity of commuting with respect to commuting costs, instrumented by walking distance and find larger estimates (4.3 to 4.5).⁹ The difference may be due to the fact that spatial wage differentials have existed only since the program was implemented, whereas the commuting network has been in place for years. We check the robustness of our conclusions to using different estimates.

Finally we use the structure of the model to compute the welfare gains to the poor from the program, combining the direct income effects on participating

⁷The gains to *residents* of untreated woredas are larger than the wage increases in their home labor market, because they commute into treated labor markets.

⁸We 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.

⁹These estimates are similar to papers that apply the same method to historical European cities ([Ahlfeldt et al., 2015](#); [Heblich et al., 2020](#)).

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 implemented in all woredas. We show that under partial roll-out, residents of treated woredas were the ones who gained the most from the program, but residents of control woredas experienced substantial benefits through rising private wages: control woredas benefited 44% as much from equilibrium effects as those in treated woredas. Under complete roll-out, the welfare gains extended to all woredas and became larger, due to equilibrium effects. Welfare increased by 22.5%, including a 3.7% direct gain from participation, a 3% gain from improved amenities and a 15.9% gain from rising private sector wages across the city.¹⁰ 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.¹¹

Our paper is the first to combine a randomized control trial of a social program at scale and a spatial equilibrium model to identify and quantify its direct and indirect effects in the presence of spatial spillovers. As such, it contributes to three 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). 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. Our model-based approach allows us to estimate spatial spillovers in a network of locations linked by commuting flows under partial and full roll-out. In doing so, our paper provides a new answer to the long-standing question of how to use randomized control trials to quantify the effect of policies at scale (Deaton, 2010; Muralidharan and Niehaus, 2017; Bergquist et al., 2019).

Second, we provide a new application of spatial equilibrium models to the empirical analysis of urban change. Most papers study variations in com-

¹⁰The evaluation does not include changes in goods prices; we find no short-term impact of the program on household consumption or local prices.

¹¹In Appendix D, we show that public works still do better than cash in terms of income gains, i.e. if we do not use the structure of the model, ignore amenities and focus on participation and private wages. We also show that ignoring spillovers across neighborhoods leads to the opposite conclusion, i.e. to prefer cash over public works.

muting 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, 2019; Balboni et al., 2020). Instead, in our application to urban public works programs, we estimate the effects of changes in labor supply through the existing transport network. 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. Like Balboni et al. (2020), we overcome the challenge of data scarcity that has so far limited the application of these models to cities in developing countries (Bryan et al., 2020) by measuring amenities and wages, as well as commuting flows, costs, and times at the individual level in original survey data. We also improve on identification by exploiting random variation in the placement of the program across neighborhoods. This enables us to estimate the Frechet parameter as the elasticity of commuting with respect to exogenous changes in destination wages driven by exposure to the program. Our estimate of 2.08 is comparable to recent estimates for developing countries: Tsivanidis (2019) for Bogotá, and Kreindler and Miyauchi (2021) for Dhaka and Colombo.

Third, by quantifying equilibrium changes in wages across all locations in the urban network, we relate 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 identification and detailed information on commuting networks. In doing so, we provide some of the first evidence on the spatial extent of labor markets within developing-country cities. Cities in Africa, in particular, have been characterised as having highly fragmented labor markets, based largely on the observation that most workers walk to work (Lall et al., 2017). Evidence suggests that spatial frictions may be important in these contexts (Franklin, 2018; Abebe et al., 2021). We find that despite these frictions there is a substantial amount of commuting between neighbourhoods, so that a place-based policy that is ear-marked for local

residents still has large spillover effects to untreated neighbourhoods.

Our paper is also the first to evaluate the welfare effects of a public works program on the urban poor by estimating experimental improvements in local amenities, equilibrium wage effects and direct benefits to participants. A large literature has estimated the effects of public works programs on program beneficiaries (Berhane et al., 2014; Bertrand et al., 2017; Beegle et al., 2017; Alik-Lagrange et al., 2017).¹² The study of indirect effects via labor markets and public good provision has proved more challenging. In particular, there is very little evidence on the effect of public works programs on local amenities.¹³ 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.¹⁴ As compared to these papers, ours combines the advantage of random program placement, detailed information on commuting networks at baseline, and a spatial equilibrium model to estimate labor market spillovers. We make progress towards a comprehensive evaluation of public works programs by constructing a model-based measure of welfare effects, which allows us to put together our estimates of the effects on beneficiaries, local amenities and equilibrium wages, under partial and complete roll-out.¹⁵¹⁶

The paper proceeds as follows. Section 2 presents the program, the evaluation data and the experimental design, and briefly describes the economic lives of the beneficiaries of the program. Section 3 provides Intention-to-Treat estimates 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.

¹²For a comprehensive review of the literature on the effects of India’s employment guarantee on economic and social outcomes see Sukhtankar (2016).

¹³Gazeaud et al. (2020) use a difference-in-difference strategy and find no effect of the rural PSNP on vegetation cover in Ethiopia.

¹⁴A common rationale for public work 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 (1990).

¹⁵Our paper considers only the contemporaneous effects of the program. Alik-Lagrange 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.

¹⁶We focus our welfare calculations on poor households who are the target of the program. Richer households do not participate to the program, but may be affected by its indirect effects, e.g. they may benefit from improved amenities or suffer from having to pay higher wages as employers (Imbert and Papp, 2015; Muralidharan et al., 2017).

2 Program and data

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 chartered city), and provides guaranteed public work to targeted households. The number of beneficiary households per city varies depending on the city size and poverty rates. In the capital city, Addis Ababa, 18% of households in the city were enrolled in the program at full-scale. Due to the large size of the city 70% of all beneficiaries in the country were 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.¹⁷ 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 are 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.

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 targetting 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 activities are: cleaning streets, maintaining drains and ditches, garbage disposal, and greening of public spaces (planting of trees

¹⁷The 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.

and gardening). In a few rare cases the works included the construction of small cobbled streets in slum areas. Most beneficiaries involved in the program report doing multiple or all of these activities.

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. This transfer is considerably smaller than the wages from public works.¹⁸ 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).¹⁹ 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 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.

Take-up: Take up of the program at the household level is almost universal: fewer than 3% of the households in our evaluation sample report being offered the program and declining to be involved. Within households, public works is mostly done by women and, in particular, older women. We also find that those who participate in the public works have lower levels of education relative to the rest of their household: participation is highest for those with no formal

¹⁸The DS provides ETB 170 per person per month; the average household enrolled into DS receives 350 Birr per month, a third of average monthly public works wages.

¹⁹Note 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.

education or only primary school.²⁰

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 1 shows a map of the randomization outcomes at the *woreda* level.

Figure 1 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 the same sample of 6,096 households one year later. We identify within our sample eligible and non-eligible households (throughout the paper, we use *eligibility* 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

²⁰Figure A1 shows the propensity to engage in the public works by age and gender in our evaluation data.

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.²¹ Roughly 40% of households in our sample are beneficiaries of the public works program, across treatment control woredas alike.

Balance and attrition: Tables A2 and A3 in the Appendix shows no sign of imbalance between treated (year 1) and control (year 2) woredas at baseline for households and individuals, respectively, consistent with the randomization of the program at the woreda level and with identical sampling procedures across treatment and control woredas.

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 response rates; households living in kebele housing (publicly owned and subsidized housing) are slightly more likely to respond, perhaps because these households are less mobile.

2.3 Sample characteristics

We designed our geo-referenced household survey to measure key urban outcomes that are rarely available in developing-country cities. In particular, we are able to measure labour market outcomes and commuting flows at the individual level, housing and rents, and local urban amenities.

Employment and earnings: In our sample of working-age adults in control areas, 42% are employed at endline. Throughout the paper we will refer to all work that is not part of the UPSNP as private sector work, including wage and self employment, formal and informal work. Wage employment is the predominant form of employment in Addis Ababa: 70% of private sector workers are wage employed. Earnings in wage employment are roughly 40% higher than earnings from self employment at baseline.

The UPSNP 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.

²¹We fail to reject a joint significance test of woreda fixed-effects on beneficiary observables, which suggests that the targeting was done in a similar way across woredas in the city.

The daily wage in public works is roughly similar to daily wages in the private sector, but roughly 60% higher 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 A2 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. These attractive wages drive the almost universal take up of the program.

Commuting: Our survey data captures individual’s commuting destinations, by asking for the woreda of their place of work. This allows us to do two things in our main estimation: first, it allows us to estimate wages in each destination labor market rather than in each place of residence. Second, we compute commuting flows at the woreda-pair level for baseline and endline, which is essential to estimate how equilibrium effects spill over across woredas. We also ask about commute times, costs, and modes of transport.

We find that roughly 45% of workers commute to work by walking (this is consistent with other estimates for African cities in Lall et al. (2017) and Kumar and Barrett (2008)). However, we also find evidence of long commutes, even among those that walk. Among people who walk to work in our data, 25% commute more than 1.5 hours per day. Across all modes of transport, the average commuting time is 50 minutes and the average commuting distance is 5 kilometers (both directions). We find that 58% of all workers commute outside of their woreda for work.²² Furthermore, 34% of workers work outside of their *subcity*— the largest administrative unit in the city, of which there are 10, and which have average area of 50 square kilometers and average population of nearly half a million. By comparison, there are 32 boroughs in London, with similar area to Addis Ababa’s subcities, but smaller average population (roughly 280,000); and 62% of workers commute outside of their borough, in a city with one of the most developed transport system in the world.²³ These substantial commuting flows motivate our approach to study spillovers between woredas.

We use our commuting data to construct a bilateral commuting matrix at baseline and endline. Some commuters work outside of the city in small towns or villages, or in wealthy woredas that were not eligible for the program in the first year, and therefore do not work within our sample frame. Others commute out of their home woreda or subcity, but do not have a fixed destination of work (for example, taxi drivers), or do not know their precise destination.

²²Woredas are geographic with populations of over 35,000 on average.

²³We computed this from the 2011 census, table *wu03ew*.

These households are dropped from our main estimation, though our results are robust to imputing their destinations from their neighbors commuting destinations. In our bilateral matrix of commuting flows within the city that we use for our main estimation, we have 45% of workers that commute out of their home woreda. Commuting patterns differ across sectors: 70% of wage employed workers commute out of their woreda, compared to only 37% of self employed workers. Figures 2a and 2b show out- and in-commuting flows at the woreda level. 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.

Figure 2 here.

Housing and rents: In our sample, 75% of households live in “kebele” housing: this is government-owned housing 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, in particular, 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.

Amenities: We collected data on neighborhood amenities, by asking households to rate the quality of a different aspects of their local area. For our main analysis we use 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. This index was prespecified in a preanalysis plan and was designed to capture improvements to neighborhoods that were likely to result from the activities conducted under the public works. See Table A4 for summary statistics of these components for woredas that did not receive the program. Satisfaction with these amenities is low. For example, less than 40% of respondents are satisfied with drainage and sewerage systems in their neighbourhood. 62% of respondents say that they notice the smash of trash in their neighbourhood ‘sometimes’ or ‘very often’.

3 Intention-to-Treat estimates

3.1 Estimation

In this section, we estimate the effect of living in a treated woreda T_i on outcome $Y_{\omega hi}$ for worker ω living in household h in woreda i using the following equation:

$$Y_{\omega hi} = \alpha + \beta T_i + \gamma \mathbf{X}_{\omega hi} + \varepsilon_{\omega hi}. \quad (1)$$

The vector $\mathbf{X}_{\omega hi}$ 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_{hi} . This effect is an intention-to-treat (ITT) estimate, since some households are eligible for the program and some are not. We observe household eligibility for both treated and untreated woreda: in untreated woredas these are the households that we observe enrolled in the program in it's second year, when we conducted a second endline survey. We will estimate Equation 1 separately for eligible and ineligible households. Similarly, we will report results for male and female workers separately, as well as workers who completed or did not complete high school.

3.2 Results

Table 2 summarizes the main ITT 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 Panels B and C present separate estimates of Equation 1 for eligible and ineligible households, respectively.

Table 2 here.

The results in columns 1, 2 and 3 suggest that the program generated substantial employment on public works (4.6pp. or 12.5% of employment in the control), but also decreased labor supply to the private sector by 12.8% (4.7pp. decrease as compared the control mean of 36.6%), so that in net it did not significantly change total employment (the coefficient in column 1 is a precisely estimated zero). Panels B and C present the results separately for ineligible and eligible households: as expected, the effects are concentrated among eligible households (households who are participating in public works in treated areas) only. Eligible households experience a 22% decline in labor supply (8pp.

as compared to the control mean of 36%). Notice that roughly 44% of our total sample lives in an eligible household.

We also show the effects of the program on our index of households' self-reported neighborhood amenities. 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 the program did small scale neighborhood improvements in beneficiaries' home woredas, these amenity effects are unlikely to spill-over to neighboring woredas.²⁴

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 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.²⁵

Heterogeneity: We provide further evidence on effect heterogeneity by type of work and by worker characteristics in Appendix Table A7. Columns 3 and 4 of Table A7 decompose the effect of private employment between self-employment and wage work: both types of work are negatively affected. The reduction in wage work is larger in absolute term (-3.2pp as compared to -1.5pp for self employment), but because most people do wage work in the sample, the reduction in hours spent self-employed is larger in relative terms (-18%, as compared to -11% for wage work). Finally, Columns 5 to 8 of Appendix Table A7 presents the effects on private employment by gender and skill level. We find that the program reduces private employment for male and female

²⁴In 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 A5 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).

²⁵Appendix Table A6 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.

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.

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 $i = 1, \dots, n$ 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 location i and working in 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:

$$C_i = \overline{v}_i$$

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 with cumulative distribution function:

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

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 :

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

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):

$$\forall i \quad U_i = \gamma \left[\sum_{j=1}^n (B_i \tau_{ij} w_j)^\theta \right]^{\frac{1}{\theta}} \quad \text{where} \quad \gamma = \Gamma \left(\frac{\theta - 1}{\theta} \right) \quad (2)$$

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

$$\pi_{ij} = \frac{(B_i \tau_{ij} w_j)^\theta}{\sum_k (B_i \tau_{ik} w_k)^\theta} = \frac{\Phi_{ij}}{\Phi_i} \quad (3)$$

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 location 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 the 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, multiplied by the share of labor supply dedicated to public works in treated locations:

$$Direct\ Income\ Gain = pT_i \left[(1 + g)w_i - \sum_j \pi_{ij}w_j \right] \quad (4)$$

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:

$$\ln \widehat{w}_j = -\frac{1}{\alpha} \ln \left(\frac{\sum_i \pi_{ij}(1 - pT_i)R_i}{\sum_i \pi_{ij}R_i} \right) > 0 \quad (5)$$

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:

$$\widehat{U}_i U_i = \gamma \left[pT_i \left((1 + g)\widehat{B}_i \right)^\theta (B_i w_i)^\theta + (1 - pT_i) \sum_j (\widehat{B}_i \widehat{w}_j)^\theta (B_i \tau_{ij} w_j)^\theta \right]^{\frac{1}{\theta}} \quad (6)$$

4.7 Welfare Effects

Based on the two equations 2 and 6, we can derive the welfare gains from the public works program (see proof in appendix B):

$$\widehat{U}_i = \underbrace{(1 + \beta T_i)}_{Amenity\ Effect} \left[1 + \underbrace{pT_i (\pi_{ii}(1 + g)^\theta - 1)}_{Direct\ Effect} + \underbrace{(1 - pT_i) \left(\sum_j \pi_{ij} \widehat{w}_j^\theta - 1 \right)}_{Wage\ Effect} \right]^{\frac{1}{\theta}} \quad (7)$$

which includes the effect of improved amenities, the direct gains from participation in the program and the gains from rising private sector wages. It 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):

$$\widehat{U}_i^{cash} = [\pi_{ii}(pT_i(1 + g))^\theta + 1]^\frac{1}{\theta} \quad (8)$$

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 A5). 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 A6). 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 (georeferenced to the precise market within the city where price data was collected) and do not find evidence of price effects (see Appendix C and Table C1).

5 Quantitative Analysis

5.1 Labor market spillovers

An estimation of the wage effects of the program that does not consider spatial spillovers would follow the Intention-to-Treat approach from section 3 and compare wages earned by residents of treated woredas with wages earned by residents of control woredas. In this section, we use woredas as the unit of

analysis to correspond with locations i and j in the model. We will refer to the woreda in which someone lives as their *neighborhood*, and the woreda in which they work as their *labor market*. Following the model notation, let us denote with T_i the treatment dummy for neighbourhood i , and w_i the average wage earned by workers who live in i . The ITT specification is:

$$\ln w_i = \alpha + \beta T_i + \gamma \mathbf{X}_i + \varepsilon_i \quad (9)$$

where \mathbf{X}_i includes baseline average workers 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 woredas, this assumption is unlikely to hold. In particular, Equation 5 in the model makes it clear that the wage effects of the program are better captured by wages at 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 5 to the data, we consider as an outcome private sector wages earned by workers who work in a labor market j , and regress it on exposure to the program in that labor market:

$$\ln w_j = \alpha + \beta Exposure_j + \gamma \mathbf{X}_j + \varepsilon_j \quad (10)$$

where \mathbf{X}_j includes baseline wages in labor market j and baseline characteristics of the workers who work in j as controls. Importantly, our commuting data at the individual level is what allows us to observe w_j directly. 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 approach is similar to a shift-share instrument as in the migration literature (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 potential exposure from 2000 simulated independent treatment assignments \tilde{T}_i^r that follow the same (stratified) random allocation.

To give a graphical illustration of our argument, [Figure 3](#) plots log wages in each labor market as a function of (non-centered) exposure, for treated and control labor markets separately. Because 55% of workers live and work in the same neighborhood, treated labor markets are the most exposed to the change in labor supply due to the program. However, control labor markets are also exposed to some extent, and there is heterogeneity in exposure within the two groups. The figure makes it clear that wages are actually increasing as a function of exposure to the program, with approximately the same slope among treated and untreated labor markets.

[Figure 3](#) here.

[Table 3](#) presents our main results: estimates of β based on the two specifications [9](#) and [10](#). In Column 1, the comparison between control and treated neighborhoods from Equation [9](#) suggests that wages earned by workers living in treated neighborhoods increased by 10.2%. Column 2 presents the same results controlling for worker characteristics, and the coefficient drops slightly to 9.3%. In Column 3, the model-based estimate suggests that a labor market that would draw all its labor supply from treated areas would see its wages increase by 21.4%. Once we control for worker characteristics, the coefficient drops slightly to 18.6%, our preferred estimate.²⁶ The reduction in the size of the coefficients when we include controls suggests that changes in composition are affecting wages both in the residence-based and model-based estimates. As discussed in [Section 2.1](#), women and less educated workers, who earn lower wages, are more likely to participate in public works. However this composition effect is rather small, and we find evidence of a large increase in equilibrium wages. If we divide the 12.8% decline in labor supply we documented in [Section 3](#) by the 18.6% increase in wages, we obtain a labor demand elasticity of -0.7 .

[Table 3](#) here.

²⁶In [Appendix Table A9](#) we check the robustness of our findings to two alternative specification of the commuting matrix. In the first we use a poisson model to predict commuting probabilities following [Dingel and Tintelnot \(2020\)](#), in the second we infer the commuting probabilities of respondents who did not report where they worked based on the commuting probabilities of respondents who did. The effect of exposure in both cases is similar to our main estimate.

On average, treated woredas (labor markets) receive 77% of their labor supply from treated woredas, against 16% for control woredas. Our preferred estimate of 18.6% implies that in the partial roll-out of the program wages have increased by 14% in treated labor markets, and 3% in control labor markets.²⁷ In contrast, ITT estimates based on Equation 9 would have implied a 9.3% rise for residents of treated neighborhoods and no effect in control. Hence, the estimates from Equation 9 that ignore labor market spillovers and the failure of SUTVA miss a sizeable rise in wages in control neighborhoods, and severely underestimate the rise in wages in treated neighborhoods. What is more, our preferred estimate implies that once fully rolled out the program will increase wages by 18.6% and not 9.3% as the ITT estimates would suggest.

Heterogeneity: Appendix Table A8 provides heterogeneity analysis. We estimate the labor market spillovers separately for eligible and ineligible households, and show that the wage effects are also felt by ineligible households who do not reduce their labor supply, which is further evidence that composition effects are not driving the wage effects. We also check that labor market spillovers are present in both self-employment and wage work, with stronger effects for self-employment which experienced a larger relative reduction in labor supply (see Table A7 for the corresponding reduced-form heterogeneity of labor supply responses). We find that the effects are concentrated among low skilled workers, with a more muted and statistically insignificant effect for skilled workers. Finally, although female labor supply declined more than male labor supply, the wage effects seem stronger for men than women.

5.2 Effect on local amenities

The ITT results in Section 3 suggest that the public works program improved local amenities in the neighborhoods where it is implemented. Specifically, we measure amenities through a standardized index of qualitative assessments on different dimensions of neighborhood quality and show that the index increases by 0.574 in neighborhoods with the program. To estimate the welfare gains from better amenities, we need to 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

²⁷Residents of control neighborhoods i may also benefit from increased wages in treated labor markets j if they commute to those places. In Section D of the Appendix we confirm this: we estimate that wages increase by 4% for residents of untreated neighborhoods and 15% for residents treated neighborhoods.

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: $0.574 * 0.043 = 0.025$.²⁸

Table 4 here.

5.3 Commuting elasticities

To estimate the key model parameter θ , we derive a gravity equation from the expression of the commuting probabilities (equation 3):

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

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 π_{ij} is the share of residents from i commuting to a destination j at end-line, $\ln w_j$ 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 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 5. We use exposure to the program as instrument for the wage w_j . Table 5 presents the results. Column 1 presents the OLS estimate for the

²⁸If housing markets were fully functional, one would expect this increase in amenities to be reflected in increase in rents paid by households. Appendix Table A5 shows that the program has an insignificant positive effect on rents paid, but the point estimate is 0.035, which is close to 0.025, our estimate of the monetary value of improved amenities.

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 $\theta = 2.07$. Ours is comparable to recent estimates of the Frechet parameter in other developing country cities: [Tsivanidis \(2019\)](#) for Bogotá, and [Kreindler and Miyauchi \(2021\)](#) for Dhaka and Colombo. The first stage presented in Column 3 is positive, confirming that the destinations 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 local amenities 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.²⁹ The results are presented in Appendix Table A10. The two IV estimates are very close to each other and imply estimates of θ (4.33 and 4.55) 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 2.07 as our estimate of θ to quantify the welfare effects in the next section but present results with $\theta = 4.55$ as a robustness check.

5.4 Welfare effects

Finally, we combine reduced form and structural estimates to compute the welfare effects of the program for the representative resident of neighbourhood

²⁹This 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.

i in our sample of poor households, based on Equation 7 from the model:

$$\widehat{U}_i = (1 + \beta T_i) \left[1 + p T_i (\pi_{ii}(1 + g)^\theta - 1) + (1 - p T_i) \left(\sum_j \pi_{ij} (\widehat{w}_j)^\theta - 1 \right) \right]^{\frac{1}{\theta}} \quad (11)$$

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 $p = 4.6/36.6 = 12.6pp$. The equation includes improvement in amenities by the program, which we have valued at $\beta = 2.47\%$. It includes the changes in wages due to the program, which at the beginning of this section we have estimated to be $\widehat{w}_j = 0.186 \sum_i \lambda_{ij} T_i$ at destination labor markets j . Residents of neighbourhoods i then benefit from these wage changes across all labor markets j through the commuting network π_{ij} . These gains are mediated by the Frechet paramater θ , which we have estimated to be $\theta = 2.07$. 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%.

Figure 4 here.

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 4 and Table 6 present the results. In the partial roll-out, treated neighborhoods experience large welfare gains (16.4%), 3.4% from direct gains from participation, 9.7% from rising private sector wages, and 2.9% from improved residential amenities. By contrast, control neighborhoods experience a 4.4% increase in welfare, which is entirely due to substantial labor market spillovers: the welfare gains from wage increases in untreated areas are 41% those of treated areas.

We next estimate welfare gains to the poor in the complete roll-out scenario. The welfare gains are larger (22.5%) overall, an increase that is driven by stronger labor market spillover effects (15.9%), while the direct benefits (5.6%) 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.³⁰ At the same time, because labor supply is unaffected, there are no equilibrium wage effects. As the results in Figure 4 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. In Appendix Table A11, we present the results with $\theta = 4.55$, i.e. the estimate of the Frechet parameter as an elasticity with respect to commuting costs as in (Heblich et al., 2020). The conclusions are similar.

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. We also compute income gains we would have predicted if we had ignored wage spillovers across neighborhoods and estimated wage effects by comparing treated and untreated areas as in Table 3, Column 2. We find that we would have underestimated the income gains under full program roll-out by about 40% and would have incorrectly concluded that the cash transfer delivered higher income gains.

It is important to note that we quantify the income and welfare effects for the urban poor, who are the main target of the policy and for which our sample is representative. There may be indirect effects of the program on richer households who do not appear in our sample. On the one hand, richer households who live in the same neighborhoods as poor households may gain from improved amenities, and those who work in the same labor market as poor workers will benefit from rising wages. On the other hand, some of the richer households will be employers, and will suffer welfare losses if they have to pay higher wages (virtually no urban poor with a business employs any worker). These distributional effects through the labor market have been highlighted by the literature on rural public works (Imbert and Papp, 2020; Muralidharan et al., 2017).

³⁰The literature on cash transfers in developing countries suggests that their effects on poor households' labor supply are negligible (Banerjee et al., 2017)

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 compare treated and untreated neighborhood to present evidence that the program improves local amenities, increases public employment, and crowds out private sector employment. 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, when partially rolled out, it increased wages earned in treated labor markets by 14.2% and wages earned in control labor markets by 3%. An estimation strategy that ignores spillovers between neighbourhoods would underestimate the equilibrium effects of the program both by underestimating the gains to program neighbourhoods and missing entirely the gains to untreated neighbourhoods. What is more, it would predict that once rolled-out across the city the program would increase by only 9.3%, while our model-based estimates suggest that wages increased by 18.6%. We then rely the structure of the model to compute the welfare effects of the program on the urban poor once completely rolled-out across the city. We show that the welfare gains to the poor are four times larger than the direct gains alone once indirect gains from higher private wages and improved amenities are taken into account. 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 spatial modelling.

Tables and Figures

Figure 1: Randomization outcome of the program across eligible woredas

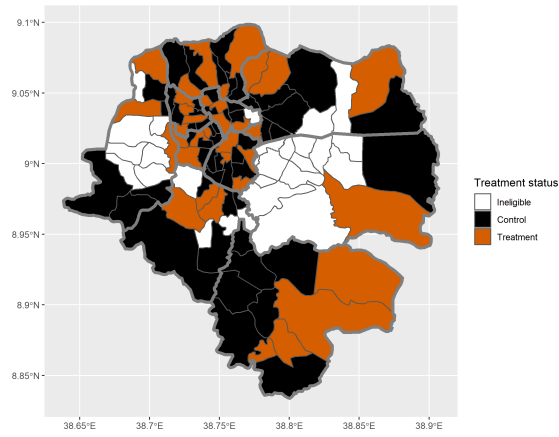


Figure 2: Commuting rates as a percentage of workers by woreda

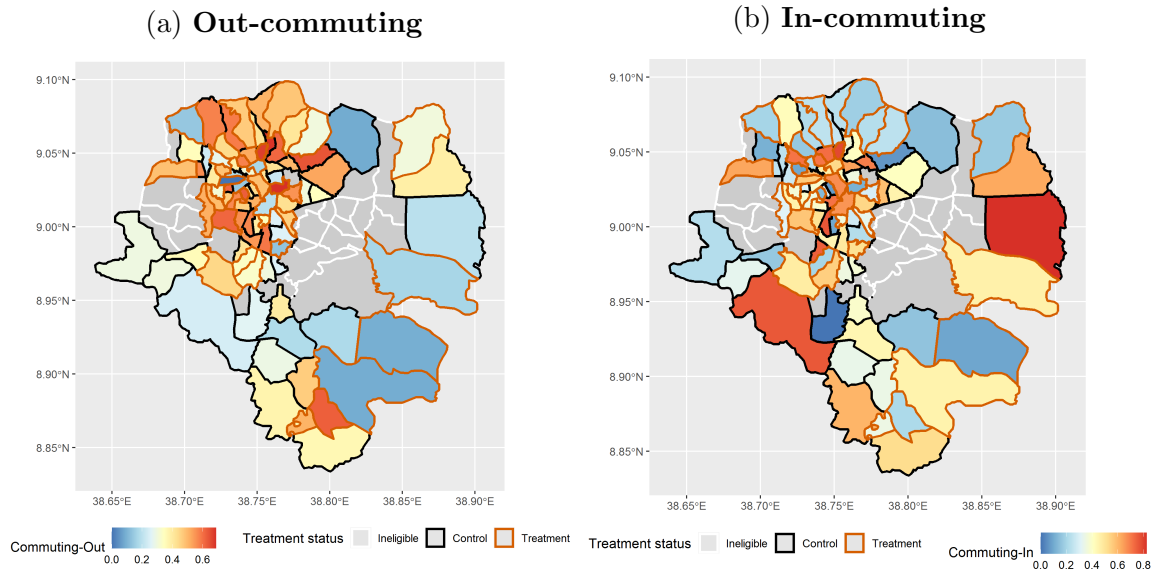


Figure 3: Wages as a function of program exposure in treated and control neighborhoods

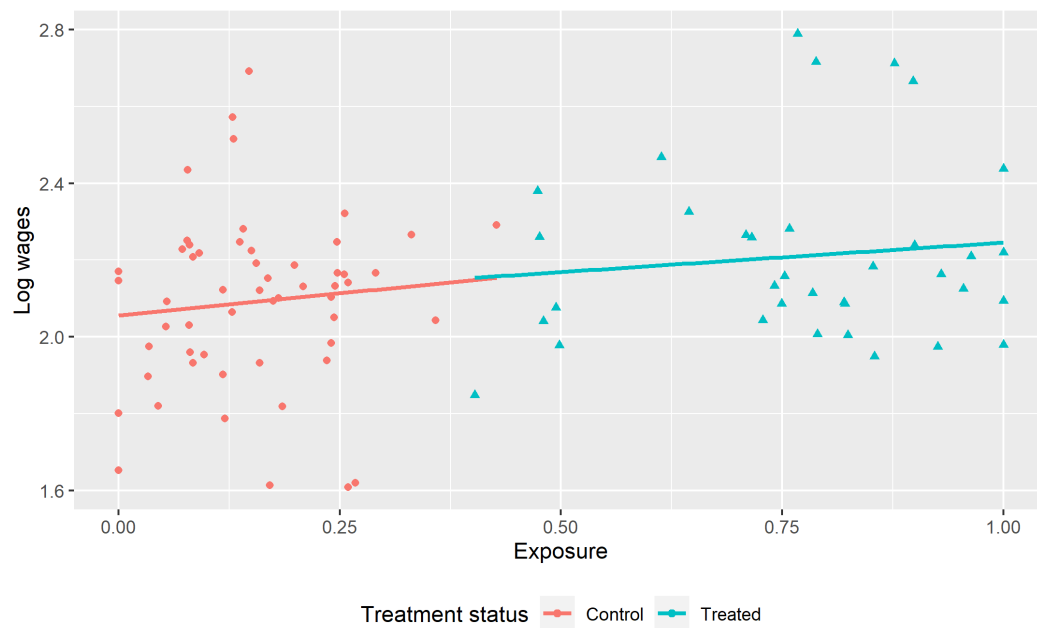


Figure 4: Welfare effects of the program under partial and full roll-out compared to a cash transfer

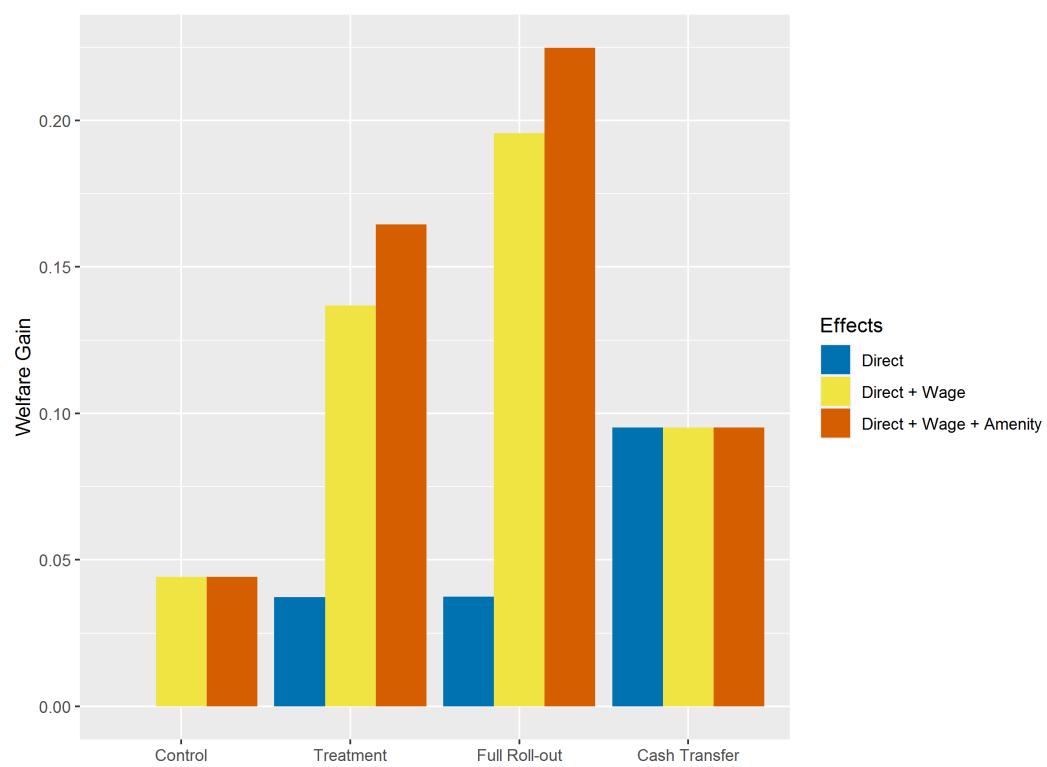


Table 1: Timeline of program roll out and data collection

| 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) |
| <i>Panel A: Intention to treat</i> | | | | |
| Treatment | -0.001 (0.012) | 0.046 (0.002) | -0.047 (0.012) | 0.574 (0.078) |
| Control Mean | 0.366 | 0 | 0.366 | 0 |
| Observations | 19,442 | 19,442 | 19,442 | 5,710 |
| <i>Panel B: Treatment for eligible households</i> | | | | |
| Treatment | 0.021 (0.015) | 0.101 (0.003) | -0.080 (0.014) | 0.620 (0.089) |
| Control Mean | 0.36 | 0 | 0.359 | 0.002 |
| Observations | 8,679 | 8,679 | 8,679 | 2,186 |
| <i>Panel C: Treatment for ineligible households</i> | | | | |
| Treatment | -0.020 (0.013) | 0.001 (0.0002) | -0.021 (0.014) | 0.541 (0.086) |
| Control Mean | 0.378 | 0 | 0.378 | -0.001 |
| Observations | 10,763 | 10,763 | 10,763 | 3,524 |

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 A4. “Treatment” is a dummy equal to one for households in treated neighborhoods. All specifications include subcity fixed effects, individual and household controls. Standard error are clustered at the neighborhood level.

Table 3: Labor Market Spillovers from the Public Works Program

| | Log wages at origin | | Log wages at destination | |
|-------------------------|---------------------|------------------|--------------------------|------------------|
| | (1) | (2) | (3) | (4) |
| Treatment at Origin | 0.102 (0.037) | 0.093 (0.040) | | |
| Exposure of Destination | | | 0.214 (0.074) | 0.186 (0.074) |
| RI p-values | 0.0235 | 0.007 | 0.0005 | 0.013 |
| Worker Controls | No | Yes | No | Yes |
| Observations | 90 | 90 | 90 | 90 |

Note: The unit of observation is a neighborhood. In Columns 1 and 2 the dependent variable is log wages earned by workers who live in that neighborhood. In Column 1 the specification includes only subcity fixed effects. In Column 2 the specification also includes worker controls. In Columns 3 and 4 the dependent variable is log wages earned by workers who work in that neighborhood. In Column 3 the specification does not include any control. In Column 4 the specification controls for the characteristics of workers who work in the 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. RI p-values are p-values obtained through randomization inference, with 2000 simulated treatment assignments.

Table 4: Correlation between Neighborhood Quality and Hypothetical Rents

| | Log Hypothetical Rent | |
|----------------------------|-----------------------|------------------|
| | (1) | (2) |
| Neighborhood Quality Index | 0.046 (0.010) | 0.043 (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 A4. In column 2 the specification includes household and housing controls selected by double lasso. Standard error are clustered at the neighborhood level.

Table 5: Commuting Elasticity with Respect to Wages

| | Communing Probability | | Log Destination Wage |
|---------------------------------|-----------------------|-------------------|-------------------------|
| | <i>Poisson</i> | <i>Poisson-IV</i> | First Stage <i>OLS</i> |
| | (1) | (2) | (3) |
| Log Destination Wage | 0.499 (0.279) | 2.077 (1.141) | |
| Destination Exposure to Program | | | 0.188 (0.0002) |
| Log walking time | -2.106 (0.082) | -2.127 (0.083) | 0.014 (0.005) |
| Observations | 7,744 | 7,744 | 7,744 |

Note: The unit of observation is a neighborhood origin \times 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 (1) | Treatment (2) | All (3) |
| Treatment | 0.000 | 1.000 | 1.000 |
| Exposure | 0.161 | 0.765 | 1.000 |
| Direct Effect | 0.000 | 0.037 | 0.037 |
| Direct + Wage Effect | 0.044 | 0.137 | 0.196 |
| Direct + Wage + Amenity Effect | 0.044 | 0.164 | 0.225 |
| Cash Transfer | 0.000 | 0.095 | 0.095 |

Note: Column 1 reports welfare gains to the poor from the public works program in untreated areas under partial-roll out. Column 2 reports welfare gains in treated areas under partial roll-out. Column 3 reports welfare gains when the program is implemented everywhere. “Exposure” for a given labor market j is equal to the sum of treatment status of all neighborhoods i weighted by the commuting probability from i to j . Rows 3 to 6 show welfare effects for the representative resident of neighborhood i . “Direct Effect” is the welfare benefits from participating into the program, i.e. earning higher wages on local public works rather than work in the private sector. “Direct + Wage Effect” is the sum of the direct effect and the effect of rising private sector wages due to labor market spillovers. “Direct + Wage + Amenity Effect” is the sum of the direct, the wage effect and the welfare gains from improved amenities. “Cash Transfer” is the welfare gain from a cash transfer program that would give the same utility as the participation in the public works without any crowd-out of private sector employment.

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APPENDIX

A Additional tables and figures

Figure A1: Individual participation within treated households by men and women

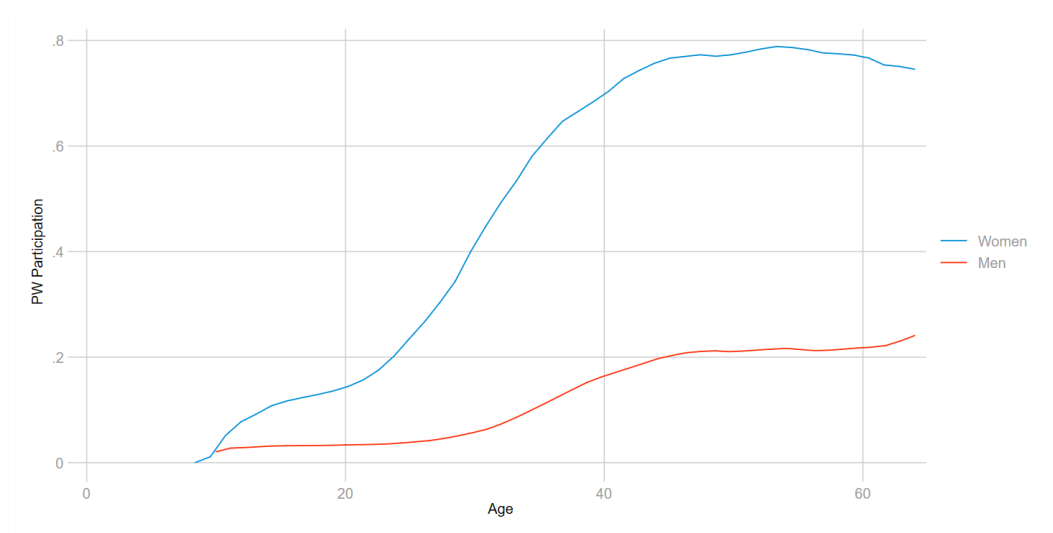


Figure A2: **Distribution of wages in public and private works at the time of the first endline survey**

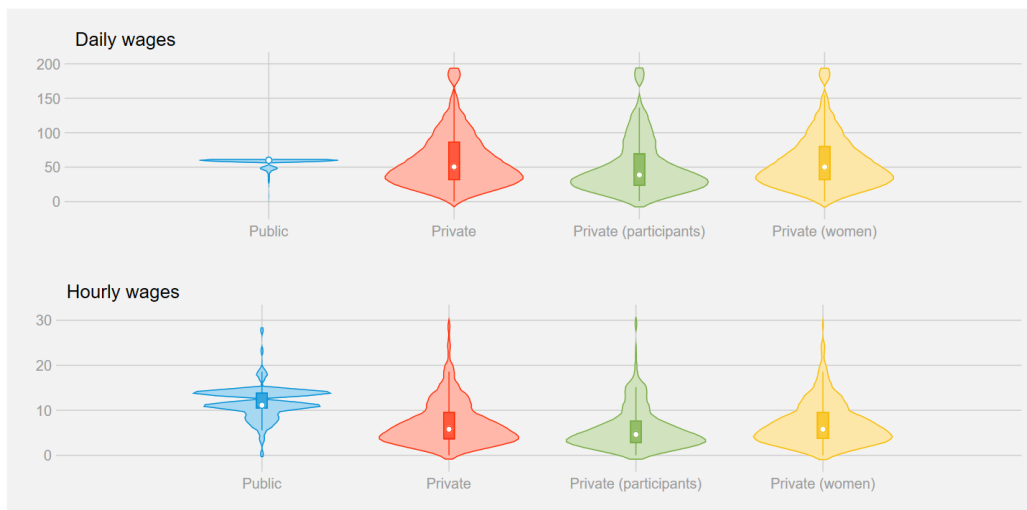


Table A1: Determinants of endline attrition

| | Household responded to endline | | | |
|--|--------------------------------|-----------|--------------|-----------|
| | (1) Coeff | (2) SE | (3) Coeff | (4) 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 A2: Balance at baseline (household level)

| Outcome | N (1) | All households | | Eligible Only | | Ineligible Only | |
|--------------------------|----------|----------------|--------------------|---------------|---------------------|-----------------|--------------------|
| | | CM (2) | TE (3) | CM (4) | TE (5) | CM (6) | TE (7) |
| Female HH head | 5,911 | 0.605 | 0.021 (0.024) | 0.598 | 0.044 (0.027) | 0.793 | 0.016 (0.037) |
| Age HH head | 5,911 | 56.444 | 0.312 (0.751) | 52.645 | 0.082 (0.903) | 65.048 | 0.479 (0.994) |
| Household size | 5,911 | 5.211 | -0.108 (0.140) | 5.381 | -0.084 (0.150) | 3.983 | -0.057 (0.180) |
| Children under 5 | 5,911 | 0.350 | -0.030 (0.023) | 0.417 | -0.027 (0.033) | 0.192 | -0.043 (0.032) |
| Disabled member | 5,911 | 0.171 | 0.000 (0.016) | 0.164 | 0.005 (0.020) | 0.266 | -0.005 (0.018) |
| HH head primary school | 5,911 | 0.095 | 0.004 (0.008) | 0.105 | 0.005 (0.015) | 0.040 | 0.010 (0.013) |
| HH head secondary school | 5,911 | 0.052 | -0.000 (0.006) | 0.051 | 0.002 (0.009) | 0.023 | -0.002 (0.009) |
| Maximum years school | 5,911 | 10.044 | -0.159 (0.180) | 9.766 | -0.057 (0.226) | 9.027 | -0.090 (0.234) |
| Rented from kebele | 5,911 | 0.748 | 0.016 (0.052) | 0.755 | 0.012 (0.052) | 0.825 | 0.008 (0.071) |
| Solid floor | 5,911 | 0.461 | -0.013 (0.040) | 0.412 | -0.023 (0.046) | 0.468 | 0.026 (0.045) |
| Improved toilet | 5,911 | 0.204 | 0.005 (0.030) | 0.221 | -0.032 (0.036) | 0.226 | 0.035 (0.032) |
| Number of rooms | 5,911 | 1.252 | -0.013 (0.058) | 1.143 | -0.041 (0.063) | 1.112 | 0.025 (0.073) |
| Owens a tv | 5,911 | 0.765 | 0.018 (0.022) | 0.744 | 0.027 (0.030) | 0.690 | 0.025 (0.025) |
| Owens at satellite | 5,911 | 0.540 | 0.002 (0.029) | 0.530 | 0.000 (0.036) | 0.445 | 0.009 (0.037) |
| Owens a sofa | 5,911 | 0.467 | 0.022 (0.029) | 0.411 | 0.021 (0.036) | 0.449 | 0.046 (0.036) |
| Weekly food expenditure | 5,911 | 348.919 | -7.988 (13.171) | 348.568 | -13.873 (15.667) | 273.433 | -2.270 (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 A3: Balance at baseline (individual level)

| Outcome | N (1) | All individuals | | Eligible only | | Ineligible only | |
|--------------------------|----------|-----------------|--------------------|---------------|---------------------|-----------------|--------------------|
| | | CM (2) | TE (3) | CM (4) | TE (5) | CM (6) | TE (7) |
| Female | 26,774 | 0.530 | -0.007 (0.005) | 0.523 | 0.006 (0.008) | 0.576 | -0.013 (0.009) |
| Age | 26,766 | 28.413 | 0.227 (0.493) | 27.050 | 0.138 (0.521) | 33.012 | 0.248 (0.630) |
| High School | 26,774 | 0.203 | -0.007 (0.011) | 0.166 | -0.006 (0.012) | 0.217 | -0.005 (0.016) |
| University | 26,774 | 0.044 | 0.001 (0.004) | 0.028 | -0.002 (0.004) | 0.038 | 0.009 (0.006) |
| Vocational qualification | 26,774 | 0.034 | 0.003 (0.004) | 0.028 | 0.002 (0.004) | 0.035 | 0.005 (0.005) |
| No formal education | 26,774 | 0.193 | -0.000 (0.009) | 0.207 | -0.004 (0.012) | 0.249 | -0.002 (0.011) |
| In labor force | 26,774 | 0.485 | 0.001 (0.014) | 0.476 | 0.004 (0.014) | 0.465 | 0.005 (0.019) |
| Employed | 26,774 | 0.344 | -0.011 (0.012) | 0.340 | -0.022* (0.013) | 0.331 | 0.003 (0.017) |
| Wage employed | 26,774 | 0.276 | -0.009 (0.011) | 0.272 | -0.017 (0.012) | 0.281 | 0.003 (0.015) |
| Self employed | 26,774 | 0.057 | -0.003 (0.005) | 0.058 | -0.005 (0.006) | 0.045 | -0.001 (0.007) |
| Hours work | 26,774 | 65.665 | -2.202 (2.522) | 64.017 | -5.067* (2.646) | 63.740 | 1.785 (3.556) |
| Earnings per month (ETB) | 26,774 | 436.190 | -4.264 (24.091) | 388.704 | -38.541 (25.090) | 393.355 | 48.053 (33.782) |
| Earnings per hour (ETB) | 26,774 | 2.608 | -0.006 (0.165) | 2.272 | -0.079 (0.187) | 2.451 | 0.108 (0.217) |

Note: The unit of observation is the individual. 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 A4: Summary statistics of components of the neighbourhood amenities index (untreated woredas only)

| | Obs | Mean | SD |
|---|-------|-------|-------|
| Drainage and sewerage (satisfied-yes/no) | 2,959 | 0.393 | 0.488 |
| Cleanliness of streets (satisfied-yes/no) | 2,959 | 0.406 | 0.491 |
| Public toilets (quality 1-4) | 2,959 | 3.427 | 0.940 |
| Smell of trash (how often do you notice) (-) | 2,959 | 2.953 | 1.138 |
| Smell of drains (how often do you notice) (-) | 2,959 | 2.552 | 1.202 |

Note: The unit of observation is a household. The table presents the mean and standard deviation of the five components of the neighborhood amenity index.

Table A5: Effect of the Program on Rents and Residential Mobility

| | Log Rent | Emigration |
|--------------|------------------|-------------------|
| | (1) | (2) |
| Treatment | 0.035 (0.058) | -0.004 (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 A6: Reduced form impact on the program on households

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

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 A7: Effects on Private Employment: Heterogeneity Analysis

| | Eligible | Ineligible | Self-Employment | Wage Employment |
|---------------|-------------------|-------------------|-------------------|-------------------|
| | (1) | (2) | (3) | (4) |
| Treatment (T) | −0.080 (0.014) | −0.021 (0.013) | −0.015 (0.005) | −0.032 (0.012) |
| Control Mean | 0.359 | 0.378 | 0.083 | 0.283 |
| Observations | 8,679 | 10,763 | 19,442 | 19,442 |
| | Female | Male | Low Skill | High Skill |
| | (5) | (6) | (7) | (8) |
| Treatment (T) | −0.058 (0.011) | −0.037 (0.017) | −0.049 (0.013) | −0.042 (0.015) |
| Control Mean | 0.321 | 0.43 | 0.332 | 0.431 |
| Observations | 10,700 | 8,742 | 12,120 | 7,322 |

Note: The unit of observation is an individual survey respondent. In Column 1 the sample is composed of respondents in eligible households, in Column 2 of respondents in ineligible households. In Column 3 and 4 we consider all respondents but use different dependent variables: self-employment (Column 3) and wage employment (Column 4). The sample is composed of all female adults in Column 5, of all male adults in Column 6, of all adults who did not complete high school in Column 7, and of adults who completed high school in Column 8. In all columns except 3 and 4 the dependent variable is “Private employment”, i.e. hours worked on private sector wage work or self-employment divided by 48 hours per week. “Treatment” is a dummy equal to one for households in treated neighborhoods. All specifications include controls. Standard error are clustered at the woreda level.

Table A8: Labor Market Spillovers: Heterogeneity

| | Eligible (1) | Ineligible (2) | Self-Employment (3) | Wage Work (4) |
|-------------------------|------------------|-------------------|------------------------|-------------------|
| Exposure of Destination | 0.123 (0.116) | 0.209 (0.094) | 0.360 (0.169) | 0.147 (0.077) |
| RI p-values | 0.359 | 0.0305 | 0.0195 | 0.0705 |
| Observations | 89 | 89 | 90 | 90 |
| | Female (5) | Male (6) | Low Skill (7) | High Skill (8) |
| Exposure of Destination | 0.126 (0.079) | 0.207 (0.104) | 0.229 (0.091) | 0.079 (0.080) |
| RI p-values | 0.1435 | 0.057 | 0.0175 | 0.3195 |
| 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. We successively consider wages earned by workers coming from eligible households (Column 1) and ineligible households (Column 2), hourly earnings from self-employment (Column 3) and from wage work (Column 4), wages of female workers (Column 5) and male workers (Column 6), workers who did not complete high school (Column 7) and workers who completed high school (Column 8). 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. RI p-values are p-values obtained through randomization inference, with 2000 simulated treatment assignments.

Table A9: Labor Market Spillovers: Robustness checks

| | Log wages at destination | |
|-------------------------|--------------------------|------------------|
| | Predicted | Imputed |
| | (1) | (2) |
| Exposure of Destination | 0.152 (0.073) | 0.152 (0.061) |
| RI p-values | 0.0475 | 0.009 |
| Observations | 90 | 90 |

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. RI p-values are p-values obtained through randomization inference, with 2000 simulated treatment assignments. In Column 1 the exposure measure is based on commuting flows predicted by a poisson model fitted on observed commuting probabilities. In Column 2 the wage and the exposure measures are computed assuming that commuters who do not know their place of work have the same probabilities of working in the different labor markets as those who do report their place of work.

Table A10: Commuting Elasticity with Respect to Commuting Cost

| | Commuting Probability | | | |
|--------------------|-----------------------|-------------------|-------------------|-------------------|
| | <i>Poisson</i> | <i>Poisson-IV</i> | <i>Poisson</i> | <i>Poisson-IV</i> |
| | (1) | (2) | (3) | (4) |
| Log Commuting cost | −1.239 (0.015) | −4.332 (0.029) | | |
| Log Commuting time | | | −1.639 (0.012) | −4.548 (0.027) |
| Observations | 838 | 838 | 911 | 911 |

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 Column 2 Log commuting cost is instrumented by Log Walking time according to Google API. In Column 4 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 A11: Welfare Effects of the Public Works Program based on a Frechet parameter estimated as elasticity of commuting w.r.t. commuting time

| Roll-out | Partial | | Complete |
|--------------------------------|----------------|------------------|------------|
| | Control (1) | Treatment (2) | All (3) |
| Treatment | 0.000 | 1.000 | 1.000 |
| Exposure | 0.161 | 0.765 | 1.000 |
| Direct Effect | 0.000 | 0.099 | 0.099 |
| Direct + Wage Effect | 0.046 | 0.180 | 0.229 |
| Direct + Wage + Amenity Effect | 0.046 | 0.208 | 0.259 |
| Cash Transfer | 1.000 | 1.118 | 1.118 |

Note: Column 1 reports welfare gains to the poor from the public works program in untreated areas under partial-roll out. Column 2 reports welfare gains in treated areas under partial roll-out. Column 3 reports welfare gains when the program is implemented everywhere. “Exposure” for a given neighborhood j is equal to the sum of treatment status of all neighborhoods i weighted by the commuting probability from i to j . “Direct Effect” is the welfare benefits from participating into the program, i.e. earning higher wages on local public works rather than work in the private sector. “Direct + Wage Effect” is the sum of the direct effect and the effect of rising private sector wages due to labor market spillovers. “Direct + Wage + Amenity Effect” is the sum of the direct, the wage effect and the welfare gains from improved amenities. “Cash Transfer” is the welfare gain from a cash transfer program which would give the same utility as the participation in the public works without any decrease in private sector employment.

B Mathematical Appendix

B.1 Proof of Equation 2

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

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

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 7

We obtain the change in expected utility \widehat{U}_i by dividing the expression of utility in 6 with the expression in 2, and using the result in equation 3 to substitute π_{ii} for $\frac{(B_i w_i)^\theta}{\sum_j (B_i \tau_{ij} w_j)^\theta}$:

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

This can be rearranged into the form of Equation 7 that provides a decomposition of direct and wage effects that classifies the reduction in labor supply to the private markets at baseline wages as part of the direct effect.

B.3 Proof of Equation 8

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

Expected utility for a worker living in i is:

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

We obtain the change in expected utility \widehat{U}_i^{cash} by dividing this expression with

the expression in 2:

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

C Price effects

C.1 Effects on local prices

As discussed in section 4, we do not find evidence that the program increased household expenditures (Appendix Table A6), hence it is unlikely that the program increased the demand for goods and services. 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 local 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:

$$\ln p_m = \alpha + \beta Exposure_m + \varepsilon_m \quad (C1)$$

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 rose 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 (1.081) | 0.108 (0.419) | −0.338 (0.413) | 0.341 (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 (0.687) | −1.474 (0.775) | −0.690 (0.592) | 0.286 (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 (3.139) | 1.223 (1.146) | −0.897 (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 4 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.

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

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 (1) | Treatment (2) | All (3) |
| Treatment | 0.000 | 1.000 | 1.000 |
| Exposure | 0.161 | 0.765 | 1.000 |
| Income Gain (Direct) | 0.000 | 0.078 | 0.078 |
| Income Gain (Spillovers) | 0.045 | 0.102 | 0.162 |
| Income Gain (Total) | 0.045 | 0.180 | 0.240 |
| Income Gain (Cash Transfer) | 0.000 | 0.210 | 0.207 |
| Income Gain (Total, No commuting) | 0.000 | 0.159 | 0.159 |

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. “Exposure” for a given labor market j is equal to the sum of treatment status of all neighborhoods i weighted by the commuting probability from i to j . Rows 3 to 6 show welfare effects for the representative resident of neighborhood i . 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. The “Total, No commuting” shows estimates for the total effects of the program including the direct and spillover effects, but where we use ITT results that do not consider commuting (ie. use estimates from Column 2 of Table 3.)