Urban Public Works in Spatial Equilibrium: Experimental Evidence from Ethiopia

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Abstract

This paper evaluates a large urban public works program randomly rolled out across neighborhoods of Addis Ababa, Ethiopia. We find that the program increased public employment and reduced private labor supply among beneficiaries and improved local amenities in treated locations. We then combine a spatial equilibrium model and unique commuting data to estimate the spillover effects of the program on private sector wages across neighborhoods: under full program roll-out, wages increased by 18.6%. Using our model, we show that welfare gains to the poor are six times larger when we include the indirect effects on private wages and local amenities.

JEL Codes: I38, J61, O18, R23.

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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, among the most popular forms of anti-poverty policy in developing countries, can improve local amenities for beneficiaries and non-beneficiaries and affect the labor market equilibrium locally and in other locations (Imbert and Papp, 2015, 2020).[1] Despite the large literature on social programs, there have been few attempts to fully quantify their effect beyond their direct effects on beneficiaries in targeted locations (Egger et al., 2022; Muralidharan et al., 2017), and none in urban areas, where spatial spillover effects are likely to be larger.

Estimating the 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.[2] 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, e.g. income or welfare.

This paper estimates the direct and indirect effects of Ethiopia's Urban Productive Safety Net Program (UPSNP). The UPSNP is a large urban public works program that offers employment at high wages on small-scale neighborhood projects to poor households for a maximum of three years.[3] Approximately 18% of households in the city were eventually enrolled in the program. We exploit the gradual roll-out of UPSNP across randomly chosen neighborhoods of Addis Ababa to estimate its short-run effect on earnings and employment, local amenities and private sector wages among the urban poor. A spatial equilibrium model guides our empirical analysis. We estimate the aggregate wage effects of the program –including spillovers between locations – by characteriz-

[1] A World Bank (2015) report found public works programs in 94 countries, 39 in Sub-Saharan Africa, including large programs in Malawi, Ethiopia, South Africa and Tanzania.

[2] A typical solution to this problem in rural settings compares among untreated villages those that are within or beyond a certain radius from treated villages (Egger et al., 2022; Muralidharan et al., 2017). This approach is ill-suited to urban settings where economic interactions between neighborhoods are strong and not only based on geographic proximity.

[3] The temporary nature of the UPSNP is typical of workfare programs in Sub-Saharan Africa (Beegle et al., 2017; Alik-Lagrange et al., 2020; Bertrand et al., 2021).

ing exposure of each location to the program through the commuting network using an expression derived from the model. We also use the model to compute the welfare gains to the poor once the program was fully rolled out, including direct income gains from participation, and indirect gains from improvement in amenities and rising wages. 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).

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.[4] We collected precisely geo-referenced panel data on poor households – both beneficiaries and non-beneficiaries– across the city. We start by comparing households who live in woredas with and without the program.[5] The results suggest that the program generated a large amount of employment on public works, the equivalent of 12.6% of hours worked in the control. However, participating households reduced their labor supply to the private sector by about 12.8% so that the net effect of the program on total hours worked is close to zero and insignificant.[6] This reduction in labor supply is likely to affect private sector wages, but since 55% of workers commute to another woreda, the wage effects of the program are likely to spill over beyond treated woredas, so comparing wages in treated and control areas would yield unreliable estimates.

To guide our evaluation of the direct and spillover effects of the UPSNP, in the second step we develop a spatial equilibrium model which borrows from the urban economics literature (Heblich et al., 2020; Balboni et al., 2021).[7] 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 analysis of the program under full roll-out and compare it with a cash transfer.

In the third step, we estimate labor market spillovers. The model provides an expression for the equilibrium wage effect 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

[4]Treatment woredas were chosen randomly through a public lottery.

[5]We prespecified the experimental design, the labor supply and amenities outcomes, and the treatment vs control specifications in a pre-analysis plan at AEARCTR-0003387.

[6]Households in the program still experience sizeable increases in income relative to control households because the program pays wages well above the level in the private sector.

[7]Our model is a simplified version of theirs as it includes no housing markets or trade. We estimate small and insignificant effects of the program on residential mobility, rents, consumption expenditures and local prices.

live.[8] We then regress wages on exposure to the program for each labor market, which we construct as the sum of treatment status in each place of residence weighted by the share of workers who commute from there. Exposure has a shift-share interpretation, where the shift is the randomly assigned program implementation, and the shares are commuting shares at baseline.[9] 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 estimates imply that under partial roll-out private sector wages increased by 14% in treated and 3% in untreated labor markets, and that under full roll-out they increased by 18.6% everywhere. By contrast, a simple comparison of wages earned by residents of treated and control woredas that ignores spatial spillovers would imply a much smaller 9.3% wage increase.[10]

In the fourth step, we estimate the effect of the program on local amenities. We use an index that aggregates five subjective indicators of amenities that might plausibly have been affected by the program. We estimate an improvement in neighborhood quality by 0.6 SDs in treated neighborhoods, perceived by both beneficiary and non-beneficiary households.[11] 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.

In the fifth step we use 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 determines how much urban residents' welfare improves when wages rise in the labor markets they commute to. We estimate the parameter as the elasticity of commuting with respect to wages at destination, which we instrument by the destination's exposure to the program. Our estimate of 2.08 is comparable to estimates by Tsivanidis (2019)

[8]Like 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 explicitly model linkages between labor markets. Throughout the paper, "labor market" refers simply to the woreda in which people work.

[9]Our use of a model-based exposure in a reduced-form estimation is reminiscent of the "market access" approach by Donaldson and Hornbeck (2016). Alternatively, we instrument labor supply to each labor market by exposure– which includes endogeneous endline commuting probabilities– and obtain consistent estimates.

[10]In Appendix E we show that alternative approaches to estimating spillovers based on Euclidean distance to other woredas or eligible households, for example, do not provide reliable estimates. We also check that the wage spillovers do not bias the ITT estimates on employment in Appendix F.

[11]Because UPNSP projects were carried out on a small scale within treated neighborhoods, we do not expect spillover effects on amenities in control neighborhoods.

for Bogotà, and Kreindler and Miyauchi (2021) for Dhaka and Colombo.[12]

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 households, equilibrium wage effects, and improvements in local amenities in treated woredas.[13] 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. Under complete roll-out, the welfare gains extended to all woredas and became larger, due to equilibrium effects. The welfare of the urban poor increased by 22.4%, including a 3.7% direct gain from participation to public works, and a 16.2% gain from rising private sector wages.[14] As a benchmark, we compare these welfare gains to the gains from a cash transfer that pays public works' wages without affecting labor supply. The cash transfer does better when one considers only the direct benefits from participation, but public works dominate once effects on amenities and wages are taken into account.[15]

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., 2022; 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. In fact,

[12]Alternatively, in Appendix G we estimate the Frechet parameter as the elasticity of commuting with respect to commuting costs instrumented by walking distance as in Heblich et al. (2020), and find a higher estimate, similar to theirs. We provide welfare calculations based on this alternative estimate and check that the conclusions are unchanged.

[13]We focus our welfare calculations on poor households who are the target of the program. Richer households do not participate in the program, but pay taxes to fund it, and may benefit from improved amenities or suffer from having to pay higher wages as employers (Imbert and Papp, 2015; Muralidharan et al., 2017).

[14]The welfare analysis does not include changes in prices or rents; we find no short-term impact of the program on household consumption or local prices, and most of the urban poor live in government housing and do not pay rent.

[15]In Appendix H, 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 gains from direct participation to public works and rising private wages.

the "donut" or "circle" approaches used in other papers provide insignificant and unstable estimates in our setting. 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 commuting costs due to changes in the transportation network in historical cities (Heblich et al., 2020; Ahlfeldt et al., 2015) and cities in developing countries today (Tsivanidis, 2019; Balboni et al., 2021). 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., 2021) to model commuting decisions, the spatial labor market equilibrium and the welfare effects of changes in wages and amenities. Like Balboni et al. (2021), 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 is similar to non-experimental estimates by Tsivanidis (2019) and Kreindler and Miyauchi (2021).

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 characterized 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 are substantial commuting flows between neighborhoods, so that a placed-based policy that is earmarked for local residents has strong local labor market effects as well as large spillover effects to untreated neighborhoods.[16]

Our paper is also the first to evaluate the welfare effects of a public works program on the urban poor by estimating experimentally 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).[17] 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.[18] Closely related to this paper, Imbert and Papp (2015) and Muralidharan et al. (2017) estimate positive equilibrium effects of India's rural public works program on rural wages and Imbert and Papp (2020) estimate spillovers on urban areas due to changes in seasonal migration flows. As compared to these papers, ours combines the advantage of random program placement, measures of local amenities, a mapping of spatial interactions, and a spatial equilibrium model to estimate labor market spillovers under partial and complete roll-out. We make progress towards a comprehensive evaluation of public works programs by including direct and indirect effects in a model-based welfare analysis.[19]

The paper proceeds as follows. Section 2 describes the program, the evaluation design, and the economic lives of program beneficiaries. Section 3 estimates the effect of the program on employment and amenities, which motivate the model we develop in Section 4. Section 5 uses the model to quantify the effect of the program on labor markets and welfare. Section 6 concludes.

[16] An additional contribution of our paper is to test and reject the "surplus labor" hypothesis in an urban setting. In a world of "surplus labor", hiring workers should have no effect on private sector employment or wages (Lewis, 1954; Harris and Todaro, 1970). Breza et al. (2021) provide experimental evidence of this in the lean season in rural India. By contrast, in our setting public works increase private wages and crowd out private sector one-for-one.

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

[18] Gazeaud et al. (2020) find no effect of the rural PSNP on vegetation cover in Ethiopia.

[19] Our 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.

2 Program and data

2.1 Program

The Urban PSNP takes its name from the PSNP (Productive Safety Nets Program) which 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. We evaluate the program in the capital city of Addis Ababa, a city of an estimated 5.2 million people. At full scale, 18% of households in the city were enrolled in the program, which translates into 70% of all UPSNP beneficiaries in the country. In what follows, we describe the roll-out and beneficiaries for Addis Ababa only. The program is implemented by local government administrative units or *woredas* within cities.

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 (a maximum of 240 days of work a year). Households are enrolled into the program for three years in total.[20] This relatively short-lived availability of the program is a common feature of workfare programs. For example, Bertrand et al. (2017) study a program that lasts for only six months. They point out that these programs are often used in response to transient negative shocks. 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 targeting. Work activities take place for between four and five hours per day, compared to an average of nine hours in the private sector. Conditional on completing the work, households are paid 60 Birr (around $2) per day, or 12 Birr per hour. This compares to 46.5 Birr per day and 8.4 Birr per hour in the private sector among eligible households in our baseline data.[21] The average beneficiary household earns roughly 1000 Birr ($33) per month, or 30% of average household consumption for eligible households in our data.

All work is done in local communities within the woreda. 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 bank accounts set up in the name of the head of the household. The work consists of small-scale activities aimed at neighborhood improvement. The most common activities are: cleaning

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

[21]We estimate an average hourly wage premium from public works of 1.67. 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.

streets, maintaining drains and ditches, garbage disposal, and greening of public spaces (planting of trees and gardening). In a few rare cases the works included construction of small cobbled streets in slum areas. Most program beneficiaries report participating to multiple or all of these activities.

Cash arm: In addition to the public works, the program includes an unconditional cash transfer arm, known as the "direct support" (DS), aimed at poor households unable to participate in public works due to chronic illness, age or disability. This transfer is considerably smaller than the wages from public works.[22] Although our study is designed to separately identify the effects of the DS, we do not focus on those results in this paper. We estimate negligible impacts of the DS 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 community committees within woredas. A strict residential requirement was enforced: only households that were resident in the woreda for at least 6 months could be selected for the program. Committees aimed to select households in their communities based on their level of poverty. We compare the characteristics of a representative sample of targeted beneficiary households against a representative household survey from the same year as our program baseline (2016). We find that targeted households look poorer (in terms of asset ownership and housing quality) than representative households below the poverty line (set at the 30th percentile of consumption for the city).

Take-up: Take-up of the program is almost universal: fewer than 3% of the households in our sample report being offered the program and declining. Within households, public works is mostly done by women and, in particular, older women. 68% of program participants are over the age of 35. We also find that individual participation is higher for those with no formal education or only primary school.[23] In most households, one person did the work for all 240 days (the average household has 3.5 working-age adults). At full-scale, the UPSNP employed nearly 4% of all adults in the city.

2.2 Evaluation and data

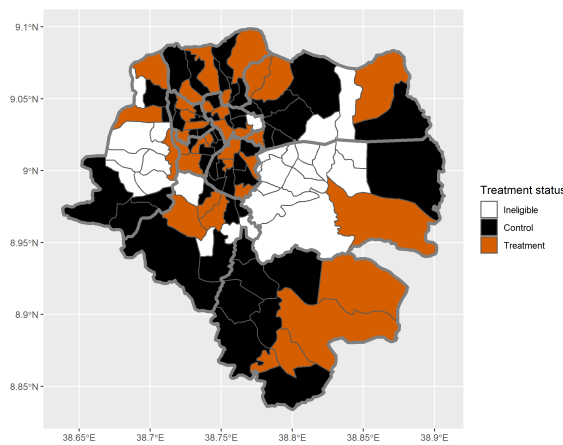
The program was randomized at the woreda level in Addis Ababa. In year 1 of the program, only households residing in woredas with poverty rates above

[22]The 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.

[23]Figure A1 shows the propensity to engage in the public works by age and gender in our evaluation data. Table A5 compares the labor market states of participants compared to the full sample at baseline.

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 other 55 woredas to receive the program in year 2 (control woredas).[24] Figure 1 shows a map of the randomization outcomes at the woreda level. Because of lower poverty rates in the 26 ineligible woredas, 86% of the poor population lived in eligible woredas. In year 2, *all* woredas were treated, including the 26 woredas that were not eligible in year 1. These 26 woredas are not included in the evaluation, because they were not eligible for randomization in year 1.

Figure 1: **Randomization of the program across eligible woredas**



We surveyed households included in our evaluation immediately after the randomization of woredas into the program but before targeting and roll-out of the program occurred.[25] Our aim was to select a representative sample of poor households, among whom we expected to see eligible households well represented in the targeting. First, we conducted a short screening survey of nearly 30,000 households drawn from a random sample of all households in the city. We used random walk sampling starting from randomly selected points within each of the 90 eligible woredas. We used this data to derive a predicted poverty score using

[24]For the Addis Ababa City Administration, public randomization offered a solution to the questions of fairness and reduced speculation about corruption. The World Bank also saw the advantage of this transparent approach, and favored randomization for the purposes of a robust impact evaluation.

[25]We describe the timeline of the roll out in more detail in the Appendix, Table A1.

a proxy means test (PMT) for consumption poverty using housing composition, assets, housing and education. Next, we selected the poorest 28% of households in the distribution of PMT scores and conducted a detailed baseline survey. This constitutes our evaluation sample of 6,096 households. All household members are included in our individual-level dataset. Our analysis focuses on working-age adults (aged 16 to 65).

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).[26] This allows us to estimate the effect of the program on both eligible and ineligible households using year 1 endline data.[27] Roughly 45% of households in our sample are beneficiaries of the public works program, across treatment and control woredas alike.[28]

Tables A3 and A4 in the Appendix show 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. Balance also holds for eligible and non-eligible separately.

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 Addis Ababa: Table 1 compares the labour market outcomes of our sample with outcomes for the city as a whole from a national labour force survey for the year of our baseline survey (2018). We look at four mutually exclusive and exhaustive categories: employment, available (not working in the last seven days, but available for work or engaged in irregular work), in education, and inactive (not in employment or education,

[26]For year 2 (control) woredas, we measured this with additional endline survey conducted a few months after the main endline when the program had been rolled out in those woredas.

[27]We 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.

[28]Attrition in our endline survey is very low, at 2.94% of households from the baseline. Table A2 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.

and unavailable). In our sample of working-age adults in control areas, 48% were employed at baseline. The employment rate in the population as a whole is higher, at 56%. The relatively low employment rate in our sample is driven in part by high rates of educational enrolment (16.3%), and high levels of inactivity (15.0%), which includes disability, early retirement, and unpaid work in the home, especially among married women. Unemployment – using the definition of having taken an active step to search for work in the last month – is only 4.7% in our sample. It is 6.0% in the city as a whole, and this concentrated among the youth. In our sample, among adults over the age of 35, only 1.7% are without working and actively seeking work.

Table 1: Description of evaluation sample at baseline compared to contemporaneous LFS (2018) in Addis Ababa (working age individuals only)

	(1) Evaluation sample		(2) LFS 2018	
	Mean	SD	Mean	SD
Age	32.31	(13.00)	31.71	(11.71)
Share female	0.524	(0.499)	0.555	(0.497)
Share in any employment	0.479	(0.500)	0.561	(0.496)
Available	0.211	(0.408)		
Inactive	0.150	(0.357)		
Studying	0.163	(0.369)	0.142	(0.349)
Unemployed (active search)	0.047	(0.211)	0.060	(0.238)
Share in self employment	0.110	(0.312)	0.191	(0.393)
Share in wage work	0.380	(0.485)	0.393	(0.488)
Share with permanent wage contract	0.082	(0.275)	0.171	(0.376)
Average working hours a week	21.19	(25.54)	28.20	(28.37)
Average share of 48 weekly hours worked	0.441	(0.532)	0.587	(0.591)
Average monthly wage (wage work only)	1544.9	(1272.2)	3353.8	(3286.5)
Average hourly wage (wage work only)	9.694	(12.76)	18.15	(20.82)
Commutes out of own Woreda (share of workers)	0.546	(0.498)		
Commuting cost (share of monthly earnings)	0.057	(0.225)		
Commuting time (mins per day)	48.54	(38.80)		
Observations	20119		8577	

Note: Working-age adults are those aged 16 to 65. The labor force survey data we use is the Urban Employment and Unemployment Survey (UEUS) conducted by the Ethiopia Statistical Agency in 2018 (for details see http://www.csa.gov.et/). We define Inactive as all workers who are not working and also not available to work. This is largely those who do unpaid work in the home (mostly women) as well as the disabled, retired, or unwilling to work for other reasons. Those "available" to work are those who did not work in the last 7 days but said they would work if offered. Roughly a quarter of this group said that they have irregular work or have some attachment to a job that they did not do in the last 7 days. We cannot compute the share of non-workers who are either "available" or "inactive" in the LFS because the survey does not ask about availability. We can compute the share who are unemployed from the module on job search. We drop households that were eligible for the "direct support" (cash transfer) arm of the UPSNP from the Evaluation sample.

Wage employment is the predominant form of employment relative to self-employment: 70% of private sector workers in the city are wage-employed. Self-employment is even less common in our evaluation sample.[29] We do not observe self-employment earnings in the LFS but a comparison of wage earnings shows that workers in our sample earn roughly half of the representative worker. Wage-employment is generally precarious and informal: less than half of wage workers have a permanent contract with job security, and this share is only 21% in our evaluation sample.[30] Due to part-time work employment expressed as a share of 48 weekly hours, our preferred outcome measure of employment, is slightly lower than employment measured at the extensive margin.

Commuting: Our survey data captures individuals' commuting destinations, by asking in which woreda they work. Such data is relatively rare in applications of spatial equilibrium models, which usually combine separate wage and commuting data at the neighborhood level. 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. Table 1 provides basic commuting statistics, and online Appendix C describes the commuting data in more detail. 55% of workers commute outside of their neighborhood: on average they commute over 5km, for 50 minutes, at a cost of nearly 6% of their earnings. The importance of commuting across neighborhoods motivate our analysis of the spillovers of the UPSNP to labor markets across the city.

We use our commuting data to construct a bilateral commuting matrix at baseline and endline. 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.

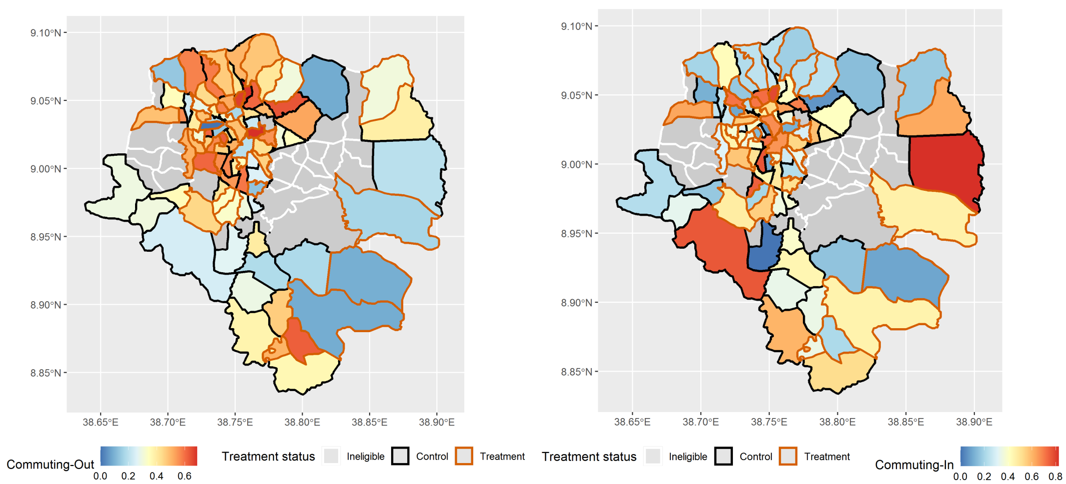
Housing and rents: In our sample, 75% of households live in "kebele" housing: this is government-owned housing where households generally live for free or for a nominal fee paid to local government officials. This housing is usually of very low quality; fewer than 10% of kebele houses have walls made of formal materials. The average rent for households who do pay rent in this

[29] Among the self-employed, retail (at stalls, markets, and kiosks) makes up roughly 25% of self-employment activities in our sample. Other highly-represented self-employment activities include: tailoring, beauty services, baking, and handicrafts.

[30] We find reasonbably high levels of churn: of wage- and self-employed workers at baseline, respectively, 19% and 24% were either unemployed or inactive one year later. Donovan et al. (2020) report global averages of 5.5% and 6.9% at the *quarterly* interval.

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

(a) **Out-commuting** (b) **In-commuting**



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 originally in kebele housing.

Amenities: We collected data on neighborhood amenities, by asking households to rate the quality of 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. Table A6 summarises the components for woredas that did not receive the program. Satisfaction with amenities is low. Less than 40% of respondents are satisfied with drainage and sewerage systems in their neighborhood. 62% of respondents say that they notice the smell of trash in their neighborhood ‘sometimes’ or ‘very often’.

3 Effects on Employment and Amenities

3.1 Estimation

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

$$Y_{\omega hi} = \alpha + \beta T_i + \gamma Y^0_{\omega hi} + \delta \mathbf{X}_{\omega \mathbf{hi}} + \varepsilon_{\omega hi}. \tag{1}$$

$Y^0_{\omega hi}$ is the outcome at baseline. The vector $\mathbf{X}_{\omega \mathbf{hi}}$ includes baseline individual and household level controls, and subcity fixed effects.[31] 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 its second year when we conducted an additional endline survey. We also estimate Equation 1 separately for eligible and ineligible households.[32] All outcomes and specifications used in this section were pre-specified.

3.2 Results

Table 2 presents the program effects on employment (hours worked) and local amenities. Panel A shows the effect of being in a treated woreda where the program is implemented (Equation 1), while Panels B and C present separate estimates of Equation 1 for eligible and ineligible households, respectively.

The results in columns 1, 2 and 3 suggest that the program generated substantial employment on public works (4.6pp. or 12.6% of hours worked in the control), but also decreased hours worked in the private sector by 12.8% (4.7pp. decrease as compared the control mean of 36.6%). In net there is no change in total hours worked: the coefficient in column 1 is a precisely estimated zero.

[31] Individual controls are: *gender*, dummies for *age* in bins of 10 years, and dummies for *educational* outcomes. Household controls are: *gender of household head*, *age of head*, *maximum years of school in the household*, dummies for *household head's educational attainment*, and dummies equal to one if the household: *has a disabled member, head was employed at baseline, rented housing from the government, has a formal floor, has an improved toilet.*

[32] The estimates of Equation 1 could be biased if the effects on labor supply spillover across woredas, which would violate SUTVA. We address this concern in Section 5.2 after introducing our spatial equilibrium framework, and find no evidence of such spillovers.

Table 2: Effects on Employment and Amenities

	Share of Hours Spent on			Neighborhood
	Employment	Public Employment	Private Employment	Amenities
	(1)	(2)	(3)	(4)
Panel A: Whole Sample				
Treatment	−0.001	0.046	−0.047	0.574
	(0.012)	(0.002)	(0.012)	(0.078)
Control Mean	0.366	0	0.366	0
Observations	19,442	19,442	19,442	5,710
Panel B: Eligible Households only				
Treatment	0.021	0.101	−0.080	0.620
	(0.015)	(0.003)	(0.014)	(0.089)
Control Mean	0.36	0	0.359	0.002
Observations	8,679	8,679	8,679	2,186
Panel C: Ineligible Households only				
Treatment	−0.020	0.001	−0.021	0.541
	(0.013)	(0.0002)	(0.014)	(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 respondent per household (the household head or their spouse). "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 described in Appendix Table A6. "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.

Panels B and C present the results separately for ineligible and eligible households (eligible households are 44% of the sample). As expected, the employment effects are concentrated among eligible households, for whom private sector work declined by 22% (8pp. as compared to the control mean of 36%).

We also show the effects of the program on our index of households' self-reported neighborhood amenities: the program improved the index by about 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.

Extensive margin of employment: It is surprising that a policy which generates the equivalent of 27% of private sector hours worked by eligible households at baseline does not increase employment. In Appendix Table A9, we show that on the extensive margin the program does increase the employment rate of members of eligible households from 43 to 53% (there is no effect among ineligible households).[33] But private sector work participation also declines from 42 to 34% in eligible households. Since public work hours are half of those in the private sector, total hours worked do not change.

Heterogeneity: We provide evidence on effect heterogeneity by type of work and by worker characteristics in Appendix Table A12. Columns 3 and 4 of Table A12 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 terms (-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 A12 presents the effects on private employment by gender and skill level. We find that the program reduces private employment for male and female workers, for workers with and without a high school diploma. Consistent with the information on program take-up discussed in Section 2, the effects are larger for female and low-skilled workers (especially in proportional terms), but the differences are not statistically significant.

Household-level outcomes: Table A8 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 el-

[33]In Appendix Table A10, we show that two thirds of this effect come from a decline in inactivity (domestic work and retirement), and one third from a reduction in the fraction of adults available for work but not working.

igible households double their savings. We test whether the improvement in amenities led to an increase in rents in treated woredas or a decrease in the fraction of households moving out of treated neighborhoods. The results in Table A7 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, with little scope for residential mobility (see Section 2).

To conclude, the comparison of household outcomes in treated and control neighborhoods suggests that the program improved local amenities, but that the employment it generated was entirely offset by a fall in private sector work. Since 18% of households in treated areas are in the program, this represents a large negative labor supply shock to the private sector. Also, since about half of the workers in the sample work outside of their neighborhood, this shock likely had spillover effects on labor markets across the city.[34] In the next sections, we use a spatial equilibrium model to quantify the labor market spillovers of the program and combine the direct and indirect effects of the program into a unified welfare analysis.

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), Heblich et al. (2020), and Balboni et al. (2021). We consider a city comprising of $i = 1, ..., n$ locations. In each location i live $\overline{R_i}$ residents, each of whom supplies 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

Utility for a worker ω residing in location i and working in j is given by:

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

[34] Table A11 shows that the probability of commuting out decreased by 2.6pp. (13%) among eligible households in treated as compared to control neighborhoods. This estimate may be biased: since commuters from control neighborhoods work in the same labor markets as those from treated ones, their commuting behavior may also be affected, and SUTVA may not hold.

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 $\epsilon_{ij}(\omega)$ is an idiosyncratic amenity shock drawn from a Frechet distribution with dispersion parameter θ:

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

4.2 Consumption

Workers consume 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 which uses only labor as production factor:

$$Y_j = A_j F(L_j) \quad \text{where } F'(.) > 0 \quad \text{and } F''(.) < 0$$

The productivity term A_j is fixed. All firms produce the same product whose price is one. We denote the labor demand elasticity with ε_D and show in appendix B.1 that it is negative:

$$\frac{\partial \ln L_j}{\partial \ln w_j} = \varepsilon_D < 0$$

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:

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

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. We show in Appendix B.3 that the expected utility of a location of residence i is:

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

We show in Appendix B.2 that the probability that a worker who lives in i will work in j is:

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

4.5 General Equilibrium

Given the endowments A_i, B_i, and R_i, the commuting costs τ_{ij}, the parameter θ, and $F(.)$, an equilibrium is a vector of wages w_j in each location which ensures that the labour markets clear:

$$\forall j \quad L_j = \sum_i \pi_{ij} R_i \tag{4}$$

Monte et al. (2018) show that this equilibrium exists and is unique. We will use the "exact hat" algebra, popular in trade (e.g. Arkolakis et al. (2012)) to study the effect of the program on this equilibrium. For every variable X, X denotes the equilibrium value without the program, X' the equilibrium value with the program and $\widehat{X} = X'/X$ the effect of the program on X.

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. Each worker ω receives a Frechet-distributed idiosyncratic utility shock ϵ_g from working in the program. We assume that there is full take-up of the program, so that in treated areas, labor supply to the private sector is $1-p$. The commuting probabilities π_{ij} are now defined conditional on doing private sector work.

The program has three effects. First, it brings 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 p in treated locations:

$$Direct \;\; Income \;\; Gain = pT_i \left[w_g - \sum_j \pi_{ij} w_j \right] \tag{5}$$

Second, the program changes the labor market equilibrium: workers who participate in public works in treated location reduce their labor supply to the private sector in each commuting destination. Given the expression of the labor demand elasticity, we show in Appendix B.4 that effect of the program on the wage in each location j can be written as:

$$\ln \widehat{w_j} = \frac{1}{\varepsilon_D} \ln \left[\sum_i \lambda_{ij} \widehat{\pi_{ij}} (1 - pT_i) \right] \tag{6}$$

where λ_{ij} is the fraction of people who work in j that come from i in the no-program equilibrium, which determines the exposure of labor market j to labor supply shocks from i. Wages will rise in each labor market proportionally to the fraction of the workforce that comes from treated locations. Since labor markets are integrated across the city, this fraction will be higher in the treated locations themselves, but will not be zero in places that do not get the program. The expression includes the immediate reduction in labor supply to the private sector in treated areas $(1-pT_i)$, but also an endogenous response of commuting probabilities for private sector workers $\widehat{\pi_{ij}}$ which will dampen the effects of the negative labor supply shock on wages.

Third, the program improves local amenities for all residents of treated locations. Let $\widehat{B_i}$ denote the relative change in amenities:

$$\widehat{B_i} = (1 + bT_i)$$

We show in Appendix B.5 that the expected utility for a worker living in i when the program is implemented writes:

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

4.7 Welfare Effects

Based on the two Equations 2 and 7, we can derive the welfare gains from the public works program (see proof in Appendix B.6). We have rearranged terms to provide the following decomposition:

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

which includes the effect of improved amenities, the direct gains from participation in the program (including the reduction in earnings due to reduced labor supply to private sector holding wages at their non-program level) 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 program and taken away from private sector work), $(1 + g_i) = \frac{w_g}{w_i}$ the woreda-specific wage premium on public works, $\widehat{w_j}$ the effect of the program on private sector wages, π_{ij} the commuting probabilities without the program, θ the Frechet parameter and $(1 + b)$ 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 has no labor market effect (see Appendix B.7 for more details and Section 5.5 for further discussion):

$$\widehat{U_i^{cash}} = 1 + pT_i(1 + g_i)\pi_{ii}^{\frac{1}{\theta}} \tag{9}$$

4.8 Discussion

The model abstracts from a few dimensions that may be important in other contexts: housing, trade, migration, capital, labor supply adjustments and taxes.

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. 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. These assumptions are also supported by our reduced form estimates: as discussed in Section 3, we find small and insignificant effects on rents and residential mobility (see Appendix Table A7) and no evidence that the program increased household expenditures (Appendix Table A8).[35] Our setting in this regard differs both from studies of urban transportation programs, who document gentrification and rising rents (Tsivanidis, 2019; Balboni et al., 2021), and from studies of rural social protection programs, which document large increases in consumption, and rising prices in more remote areas (Cunha et al., 2019; Egger et al., 2022).

We also abstract from three mechanisms that could dampen the program effect on wages. First, we allow changes in commuting to shift labor supply to different labor markets, but assume that total labor supply from each location is fixed. This assumption is justified by the fact that total hours worked are

[35] We use official georeferenced micro data from the Consumer Price Index to test empirically whether the program had any effect on local prices, and do not find evidence of price effects (see Appendix D and Table D2).

not affected by the program, with a one-for-one crowd out of private sector work by public works (Table 2). Second, we ignore migration into the city, like most papers in the literature on urban spatial equilibrium do (Tsivanidis, 2019; Balboni et al., 2021). Residential eligibility criteria make it impossible for migrant households to move to the city in order to enroll in the program, but they may move in order to benefit from higher private sector wages. This migration response could in the longer-run dampen the program effect on wages and reduce the welfare gains to residents but also, presumably, increase the welfare of migrants. A third mechanism that would mitigate the effect of the program on wages would be a downward adjustment in labor demand, e.g. by substitution of capital for labor. These forces are more likely to play in the longer-run, which makes them less relevant for our study, since the UPSNP is implemented for only a few years.

Finally, the funding of the program is outside of the model: the UPSNP is funded by a World Bank loan, to be repaid by the federal government, whose main sources of revenue are corporation tax, income tax, trade tax and VAT, which have low incidence on poor households (Harris and Seid, 2021).

5 Quantitative Analysis

5.1 Effect on Private Sector Wages

An estimation of the wage effects of the program that does not consider commuting and spatial spillovers would follow the 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 neighborhood i, and w_i the average wage earned by workers who live in i. The specification from Section 3 is:

$$\ln w_i = \alpha + \beta T_i + \gamma \ln w_i^0 + \delta \mathbf{X_i} + \varepsilon_i \tag{10}$$

where $\ln w_i^0$ are baseline wages and $\mathbf{X_i}$ a vector of average workers characteristics (gender, age and education dummies), 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, SUTVA is unlikely to hold. In particular, Equation 6 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.

Taking Equation 6 to the data is challenging, because the labor supply change includes an exogenous reduction due to the program and an endogenous change in commuting patterns in response to wage changes. There are two possible approaches to this issue: a reduced form approach which regresses wages on the exogenous component only, and a more structural approach which regresses wages on labor supply instrumented by the exogenous component.[36] We first adopt the reduced-form approach, and discuss the instrumental variable findings below. We set $\widehat{\pi_{ij}} = 1$, i.e. we ignore the endogenous change in commuting probabilities from i to j due to wage changes in j. With a linear approximation of the log function, we obtain (see Appendix B.4 for more details):

$$\ln \widehat{w_j} = \frac{1}{\varepsilon_D} \ln \left[\sum_i \lambda_{ij} \widehat{\pi_{ij}} (1 - pT_i) \right] \approx -\frac{p}{\varepsilon_D} \sum_i \lambda_{ij} T_i \tag{11}$$

We can then take Equation 11 to the data, and consider as an outcome private sector wages earned by workers who work in a labor market j, which we can construct thanks to the commuting data at the individual level. We regress wages on exposure to the program in that labor market:

$$\ln w_j = \alpha + \beta Exposure_j + \gamma \ln w_j^0 + \delta \mathbf{X_j} + \varepsilon_j \tag{12}$$

where $\ln w_j^0$ are baseline wages in labor market j and $\mathbf{X_j}$ is a vector of characteristics of workers who work in j (age, gender and education dummies).[37] 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 at baseline that a worker who works

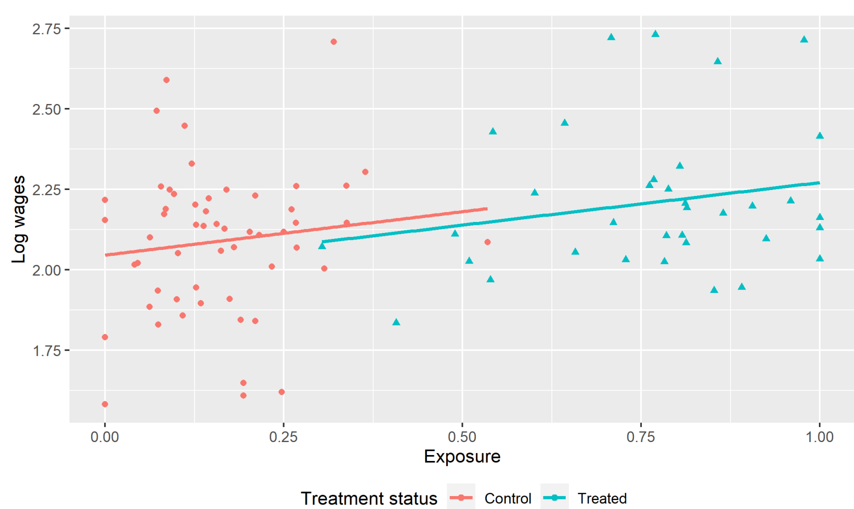
[36] In a similar vein, (Donaldson and Hornbeck, 2016) adopt a reduced form approach to estimate the effect of market access.

[37] Notice that $\ln \widehat{w_j}$ in the model indicates differences in wages between equilibria induced by the program, not differences between time periods at the woreda level. The intercept α in 12 now identifies the counterfactual wage in a woreda with zero exposure. We control for $\ln w_j^0$ for increased efficiency; this is also efficient relative to use of changes between time periods as the outcome variable (McKenzie, 2012). Our results are similar without this control.

in neighborhood j lives in neighborhood i. Our approach is similar to a shift-share instrument as in the migration literature (Imbert et al., 2022): β causally identifies the effect of exposure to the program. 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 setting is similar to the one of Borusyak and Hull (2020): 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 log wages are linearly increasing as a function of exposure to the program, with approximately the same slope among treated and untreated labor markets.

Figure 3: **Private Sector Wages as a Function of Exposure to the Public Works Program in Treated and Control Neighborhoods**



Note: Estimated slope coefficients are, for "Control" 0.3489 (0.3026), and for "Treated" 0.2606 (0.1940).

Table 3 presents the estimates of β based on the two specifications 10 and 12.

In Column 1, the comparison between control and treated neighborhoods from Equation 10 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. The decline in coefficients after including controls suggests positive selection of private sector workers: as Section 2.1 shows, women and less educated workers, who earn lower wages, are more likely to participate in public works, and hence more likely to exit the private sector. But these composition effects are small, and our estimate points to large equilibrium effects.

Table 3: Effect of the Public Works Program on Private Sector Wages

	Log wages at origin		Log wages at destination	
	(1)	(2)	(3)	(4)
Treatment at Origin	0.102	0.093		
	(0.037)	(0.040)		
Exposure of Destination			0.214	0.186
			(0.074)	(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 hourly earnings from private sector work (excluding public works) 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 hourly earnings from private sector work (excluding public works) earned by workers who work in that neighborhood. In Column 3 the specification does not include any controls. 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.

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. By contrast, estimates based on Equation 10 would have implied a 9.3% rise for residents of treated neighborhoods and no effect in control. Hence, the estimates from Equation 10 that ignore commuting and labor market spillovers 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% in all woredas and not 9.3% as the simple treatment vs control comparison would suggest.[38]

Robustness: In Columns 1 and 2 of Table A14, we address the concern that our survey-based measure of commuting probabilities λ_{ij} between 90 origins and 90 destinations may be too sparse (Dingel and Tintelnot, 2020). For this, we perform two robustness checks: (i) we predict commuting probabilities based on a poisson model, or alternatively (ii) we infer the commuting probabilities of respondents who did not report where they worked based on the commuting probabilities of respondents who did. The estimates based on these alternative measures of exposure are similar to our main estimate.[39]

Labor Demand Elasticity: Equation 11 identifies the effect of the exogeneous component of a labor supply induced by the program and therefore causally identifies the effect of exposure to the program. However, it does not identify the labor demand elasticity, since we obtained Equation 11 by (i) shutting down endogenous changes in commuting probabilities ($\widehat{\pi_{ij}} = 1$) (ii) approximating $\ln\left[1 - \sum_i p\lambda_{ij}T_i\right]$ with $-p\sum_i \lambda_{ij}T_i = -pExposure_j$.[40] We undo the approximation first: Column 3 of Table A14 regresses log wages on the log form of exposure $\ln\left[1 - \sum_i p\lambda_{ij}T_i\right]$ (setting p to 0.128 based on the ITT estimates). The implied labor demand elasticity is the inverse of the coefficient $\varepsilon_D = -0.74$. This estimate of labor demand elasticity may still be biased in the presence of endogenous changes to commuting probabilities $\widehat{\pi_{ij}}$. In Column 4 we estimate the structural equation: we regress log wages at destination on log labor supply to that destination (including changes in commuting probabilities) instrumented by the log form of exposure. The implied labor demand elasticity is the inverse of the coefficient $\varepsilon_D = -0.68$.

Heterogeneity: Table A13 in the appendix provides heterogeneity analysis. We estimate the labor market spillovers separately for eligible and ineligible

[38] Under full roll-out $T_i = 1$ everywhere, hence $Exposure_j = 1$ everywhere.

[39] As an additional robustness check, Table A16 presents the results on daily wages rather than hourly wages, and find similar effects.

[40] See Appendix B.4.

households, and show that the wage effects are also felt by ineligible households (Columns 1 and 2). Since ineligible households do not reduce their labor supply to the private sector (Panel C in Table 2), this result provides further evidence that composition effects are not driving the increase in wages. We also check that labor market spillovers are present in both self-employment and wage work (Columns 3 and 4). The effects are stronger (but not significantly so) for the 25% of workers who are self-employed, and who also experienced a larger reduction in labor supply (see Table A12). These results suggest that our modelling choice to assimilate self-employed to wage workers, with only one firm per location, is a reasonable approximation: the main difference is that in reality the labor supply of self-employed workers shifts along their own labor demand curve, rather than their employers'. In columns 5 and 6, we estimate separately the effect of the program on wages of male and female workers: although female labor supply declined more than male labor supply, the wage effects are similar for men and women, and if anything higher for men, but the coefficients are not statistically different. Finally, we also find that the effects are concentrated among low skilled workers, with a more muted and statistically insignificant effect for skilled workers (Columns 7 and 8), but the difference between the two coefficients is once again not significant.

Alternative approaches: Our approach relies on a model-based measure of exposure through the commuting network, but we also test whether more commonly used methods based on distance to treated units reach similar conclusions. In Appendix E, we consider two methods: the "donut" approach which drops from the analysis untreated units that are close to treated units, and the "radius" approach which compares untreated units within a certain radius of treated units with those further away. The estimates range from large negative to large positive values, they are very sensitive to the choice of the radius over which exposure is defined, and the standard errors are very large throughout, so that no estimate is significant. Hence the methods commonly used in the literature do not adequately capture spillovers in our setting.

5.2 Employment spillovers

A natural question, is whether wage spillovers induced employment spillovers, i.e. whether vacancies left by workers who participated to the program were filled by other workers. In Section 3 we found no evidence of within-woreda spillovers: ineligible households in treatment woredas did not work more than ineligible households in control woredas (Table 2 Panel C). There could still however be spillovers across woredas, e.g. if workers in control woredas increased their labor supply to fill in for workers in treated woredas. Importantly, these spillovers would violate SUTVA, and bias our estimates of the employment effects of the program which we obtained by simply comparing employment outcomes between residents of treatment and control woredas (Table 2).

To test this, we construct *Exposure Squared*, which captures second order effects of the program. *Exposure Squared* measures the extent to which residents of a neighborhood i may be affected by changes in wages in each labor market j due to its exposure to changes in labor supply induced by program implementation in each neighborhoods k. Formally,

$$ExposureSquared_i = \sum_j \pi_{ij} Exposure_j = \sum_j \pi_{ij} \sum_k \lambda_{jk} T_k$$

where π_{ij} is the baseline probability of commuting from i to j and λ_{jk} is the share of workers in k who come from j. We regress employment outcomes of a worker who lives in i on whether i is treated and on i's *Exposure Squared*. Table F1 present the results: we find similar employment effects from living in a treated woreda and no evidence of employment spillovers. We refer the interested reader to Appendix F for more detail.

The large wage effects and the absence of employment spillovers from the UPSNP are the mirror image of (Breza et al., 2021)'s experimental evidence of surplus labor in rural India. They hire part of the workforce in randomly chosen villages in the lean season and find no effect on wages or on employment, because other workers pick up the slack. Our results point to the opposite conclusion to theirs, i.e. that urban Ethiopia is not characterized by surplus labor.

5.3 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 by residents on different dimensions of neighborhood quality and show that the index increases by 0.574 in neighborhoods with the program.[41] To estimate the welfare gains from better amenities, we need to convert the increase in index quality into a monetary equivalent. If housing markets were fully functional, one would expect this increase in amenities to be reflected in increase in rents paid by households. However, as we discussed in Section 3, only 18% of poor households in this context pay rents, so that the program effect on rents paid is a positive

[41] Since the public works make small-scale improvements to local amenities, we assume that they do not spillover to non-treated neighborhoods. We use our exposure-based measure to test for spillovers via the commuting network, and find no evidence of spillovers for amenities (results not reported here).

but insignificant 0.035 (Table A7). To provide a more precise valuation of the improvement of amenities, we combine information on rents paid by households who do pay rent and information on hypothetical rents for those who do not, i.e. on the value that households think they could expect to pay if they were renting the place they live in, and we compute the correlation between these rents and the neighborhood 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). We combine this coefficient and the increase in the index to compute the improvement in amenities due to the public works in monetary terms: $0.574 * 0.043 = 0.025$, which is in the same ballpark as the imprecisely estimated 3.5% increase in rents.

Table 4: Correlation between Neighborhood Quality and Hypothetical Rents

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

Note: The unit of observation is a household. The dependent variable is the log of rents that each household pays for its housing or how much it would pay if it were to rent it (for households who own their housing or do not pay rents). The neighborhood quality index is a standardized index of answers to five questions about neighborhood quality describe in Appendix Table A6. In column 2 the specification includes household and housing controls selected by double lasso. Standard error are clustered at the neighborhood level.

5.4 Commuting elasticites

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 substitute iceberg commuting cost $\tau_{ij} < 1$ with commuting costs $c_{ij} = \frac{1}{\tau_{ij}} > 1$ to obtain:

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

We estimate θ as the elasticity of commuting with respect to wages with the following Poisson specification:

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

where π_{ij} is the share of private-sector workers who reside in i who commute to a destination j at endline, $\ln w_j$ is the log of the wage at destination, $\ln c_{ij}$ is our survey measure of the monetary 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 6. We use exposure to the program as instrument for the wage w_j.[42]

Table 5 presents the results. Column 1 presents the OLS estimate for the correlation between the wage at destination and commuting proability. The correlation is positive, which is expected given that commuters are more likely to go to destination with higher wages. This estimate is however likely to be downward biased, because more commuting will decrease wages at destination. The IV estimate presented in Column 2 is much larger in magnitude and significant, and implies that the Frechet parameter $\theta = 2.08$. Ours is comparable to recent estimates of the Frechet parameter in other developing country cities: Tsivanidis (2019) for Bogotà, and Kreindler and Miyauchi (2021) for Dhaka and Colombo. The first stage presented in Column 3 is almost identical to the estimation of Equation 12, confirming the positive relationship between exposure and destination wages. In Appendix G, we propose an alternative strategy inspired by Heblich et al. (2020), who estimate θ as the elasticity of commuting to commuting costs. We find a higher estimate $\theta = 4.55$ which is consistent with Heblich et al. (2020)'s own findings. We use $\theta = 2.08$ to quantify the welfare effects in the next section but present results with $\theta = 4.55$ in Appendix G.

[42]There is no mechanical effect of local public works participation on commuting probabilities in treated areas because these probabilities are conditional on private sector work.

Table 5: Commuting Elasticity with Respect to Wages

	Commuting Probability		Log Destination Wage
	Poisson	*Poisson-IV*	First Stage *OLS*
	(1)	(2)	(3)
Log Destination Wage	0.499	2.077	
	(0.299)	(1.180)	
Destination Exposure to Program			0.188
			(0.074)
Log walking time	−2.106	−2.127	0.014
	(0.103)	(0.103)	(0.031)
Observations	7,744	7,744	7,744

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

5.5 Welfare effects for the urban poor

Finally, we combine reduced form and structural estimates to compute the welfare effects of the program for the representative poor resident of neighborhood i, based on Equation 13 from the model:

$$\widehat{U}_i = \underbrace{(1+bT_i)}_{Amenity\ Effect} \left[1 + pT_i\left((1+g_i)\pi_{ii}^{\frac{1}{\theta}} - 1\right) + (1-pT_i)\left(\left(\sum_j \pi_{ij}\widehat{w_j}^{\theta}\right)^{\frac{1}{\theta}} - 1\right)\right] \tag{13}$$

where π_{ij} are commuting probabilities which vary across neighborhoods. Based on Table 2, the fraction of the labor supply devoted to public works is almost equal to the fraction taken away from the private sector $p = 4.7/36.6 = 12.8pp$. The equation includes improvement in amenities by the program, which we have valued at $b = 2.47\%$. It also includes the wage premium g_i, which is the ratio between average hourly earnings on public works and in the private sector for each neighborhood (the average wage premium across all woredas is 1.67). Finally, 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 neighborhoods 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 $\theta = 2.07$.

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 and, therefore, $\widehat{w_j} = 0.186$ everywhere. We also sequentially add the different parts of the welfare effects to show their contribution: first the direct gains from participation in public works, then wage spillover effects, then the improvement in amenity. Table 6 present the results. In the partial roll-out, treated neighborhoods experience large welfare gains (16.2%), including 3.2% from direct gains from participation, and 10.2% from rising private sector wages. 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 44% those of treated areas. We next estimate welfare gains to the poor in the complete roll-out scenario. The welfare gains are larger (22.4%) overall, an increase that is driven by stronger labor market spillover effects (16.2%), while the direct benefits and the amenity effects are basically unchanged. These results make it clear that labor market spillovers are an important part of the welfare effects of the program.

As a benchmark, we estimate the welfare gains from a counterfactual policy, a

Table 6: Welfare Effects of the Public Works Program for the Urban Poor

Roll-out	Partial		Complete
	Control	Treatment	All
	(1)	(2)	(3)
Treatment	0.000	1.000	1.000
Exposure	0.161	0.765	1.000
Direct Effect	0.000	0.032	0.033
Direct + Wage Effects	0.044	0.134	0.195
Direct + Wage + Amenity	0.044	0.162	0.224
Cash Transfer	0.000	0.160	0.162

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. "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 participation in the public works without crowd-out of private sector employment.

cash transfer which would provide to households the utility equivalent of wages received on public works. As compared to public works, this hypothetical cash transfer has the advantage of not imposing any work requirements, so that labor supply to the private sector is unchanged.[43] At the same time, because labor supply is unaffected, there are no equilibrium wage effects. To the extent that such a program may have stimulus effects through the goods market as in Egger et al. (2022), the stimulus effect of workfare wages should be similar. As it happens, we find no evidence for such effects, which may be due to our urban setting where households have better access to savings, and prices are more likely to determined outside of the local economy. As the results in Table 6 show, the cash transfer does better than 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 G we show that these conclusions are robust to using an alternative estimate of $\theta = 4.55$.

In Appendix H, 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 a third and incorrectly concluded that the cash transfer delivered higher income gains.

5.6 Discussion

Our welfare analysis quantifies the effects of the program for the urban poor in neighborhoods that were eligible in the first year of the program, for which our sample is representative, and not for the entire population of Addis Ababa. In particular, it does not speak to the effect of the program on about 14% of the poor in Addis Ababa who do not live in year-1-eligible neighborhoods. They may gain indirectly from the rise in wages we document to the extent that they work in the same labor markets as poor people in eligible neighborhoods. These neighborhoods did receive the program in year 2, and might plausibly

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

have experienced direct benefits like the other neighborhoods. There may also be indirect effects of the program on households who are too rich to be targeted by the policy, in both eligible and ineligible neighborhoods. 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. 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). Richer households may also pay higher taxes when the federal government repays the World Bank loan which funded the UPSNP.

Another important question is what the effects of the program would be if it were continued beyond its planned lifespan of three years and became a permanent program similar to the Indian rural employment guarantee, MG-NREGS. As discussed in Section 4, if more people move into treated areas in the longer run, poor households who depend on the private housing market may see rents increase, and the welfare gains from better amenities may be partly captured by richer households and landlords (Balboni et al., 2021). Similarly, part of the wage gains may dissipate if labor supply increases as more poor people move into the city or if labor demand decreases with employers substituting capital for labor (Imbert et al., 2022). Finally, if the program offers not a temporary, but a permanent positive income shock, beneficiary households may spend rather than save, which would have positive multiplier effects on local firms and households as in Egger et al. (2022). Rising demand may also raise prices, which would reduce the welfare gains from the program. However, two studies of cash transfers Cunha et al. (2019) in Mexico and Egger et al. (2022) in Kenya only find sizeable price effects in the most remote villages, which are presumably less well integrated to international markets than Addis Ababa.

Our labor market results with partial rollout show that wages rose 14% in treated markets and 3% in control markets. These results speak to the spatial concentration of the labor market effects of place-based policies in the short-run. In the long-run, commuting decisions may be more elastic as workers change their commuting patterns to take advantage of higher wages. If this is the case, we would expect the spatial spillovers to be more dispersed if a similar program was only partially rolled-out over the long term.

6 Conclusion

Our paper provides a comprehensive evaluation of the UPSNP, Ethiopia's urban public works program, for it's intended beneficiaries, the urban poor. 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 improved local amenities, increased public employment, and crowded 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 neighborhoods would underestimate the equilibrium effects of the program both by underestimating the gains to program neighborhoods and missing entirely the gains to untreated neighborhoods. What is more, it would predict that once rolled-out across the city the program would increase wages by only 9.3%, while our model-based estimates suggest that wages increased by 18.6%. We then rely on 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 welfare gains are six 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. One limitation of our experimental design is that we compare areas that receive the program one year to areas that will receive it the next, which makes it vulnerable to possible anticipation effects in control areas, and prevents us from estimating longer-run effects. Our study also suggests that once spillover effects are taken into account public works deliver higher welfare gains to the urban poor than a cash transfer of similar size. This helps rationalize why many developing countries today implement urban public works, and provides evidence in support for extending rural public works programs such as India's rural employment guarantee to urban areas (Dreze, 2020).

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Appendix: For Online Publication

A Additional tables and figures

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

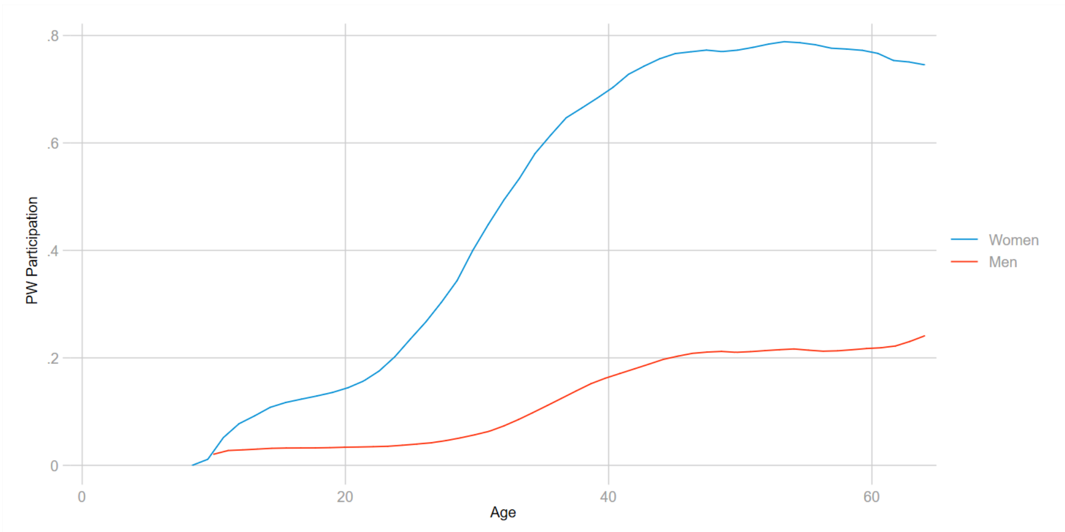

Table A1: Timeline of program roll out and data collection

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.

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

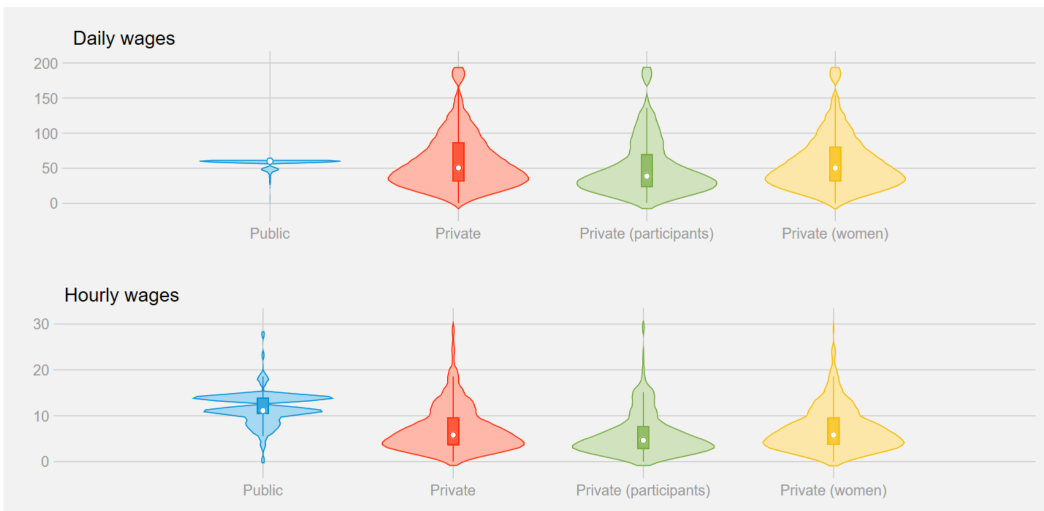

Table A2: Determinants of endline attrition

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

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

Table A3: Balance at baseline (household level)

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

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

Table A4: Balance at baseline (individual level)

Outcome		All individuals		Eligible only		Ineligible only	
	N	CM	TE	CM	TE	CM	TE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Female	26,774	0.530	-0.007	0.523	0.006	0.576	-0.013
			(0.005)		(0.008)		(0.009)
Age	26,766	28.413	0.227	27.050	0.138	33.012	0.248
			(0.493)		(0.521)		(0.630)
High School	26,774	0.203	-0.007	0.166	-0.006	0.217	-0.005
			(0.011)		(0.012)		(0.016)
University	26,774	0.044	0.001	0.028	-0.002	0.038	0.009
			(0.004)		(0.004)		(0.006)
Vocational qualification	26,774	0.034	0.003	0.028	0.002	0.035	0.005
			(0.004)		(0.004)		(0.005)
No formal education	26,774	0.193	-0.000	0.207	-0.004	0.249	-0.002
			(0.009)		(0.012)		(0.011)
In labor force	26,774	0.485	0.001	0.476	0.004	0.465	0.005
			(0.014)		(0.014)		(0.019)
Employed	26,774	0.344	-0.011	0.340	-0.022*	0.331	0.003
			(0.012)		(0.013)		(0.017)
Wage-employed	26,774	0.276	-0.009	0.272	-0.017	0.281	0.003
			(0.011)		(0.012)		(0.015)
Self-employed	26,774	0.057	-0.003	0.058	-0.005	0.045	-0.001
			(0.005)		(0.006)		(0.007)
Hours work	26,774	65.665	-2.202	64.017	-5.067*	63.740	1.785
			(2.522)		(2.646)		(3.556)
Earnings per month (ETB)	26,774	436.190	-4.264	388.704	-38.541	393.355	48.053
			(24.091)		(25.090)		(33.782)
Earnings per hour (ETB)	26,774	2.608	-0.006	2.272	-0.079	2.451	0.108
			(0.165)		(0.187)		(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 A5: Individual baseline occupation categories, all versus public works participants

	(1) All adults (age 16-65)	(2) Participants
Employed	0.477	0.431
Available	0.210	0.220
Inactive	0.153	0.356
In education	0.161	0.026
Self-employed	0.106	0.179
Wage-employed	0.381	0.262
Homemaker	0.089	0.265

Note: The unit of observation is a working age adult at baseline. Column one shows shares for all adults in the sample, column 2 only those who took up the public works. We define Inactive as all workers who are not working and also not available to work. This is largely those who do unpaid work in the home (mostly women) as well as the disabled, retired, or unwilling to work for other reasons. Those “available work” to work are those who did not work in the last 7 days but they saw they would work if offered. Roughly a quarter of this group say that they have irregular work or have some attachment to a job that they did not do in the last 7 days.

Table A6: Summary statistics of components of the neighborhood 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 A7: Effect of the Program on Rents and Residential Mobility

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

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

Table A8: Reduced form impact on the program on households

	(1) Income	(2) Pub. wages	(3) Priv. income	(4) Expenditure	(5) Savings
Panel A: Whole Sample					
Treatment (T)	306.403	432.565	−105.596	−54.347	750.930
	(103.970)	(11.531)	(98.650)	(87.463)	(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
Panel B: Treatment by eligibility					
T×Eligible	566.115	1,032.636	−357.530	37.736	1,763.144
	(113.928)	(7.493)	(116.968)	(102.757)	(180.535)
T×Ineligible	189.516	75.815	132.982	−80.934	78.090
	(182.640)	(8.060)	(173.784)	(112.719)	(259.511)
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 A9: ITT Effects on the Extensive Margin of Employment

	Any	Public	Private
	(1)	(2)	(3)
Panel A: Whole Sample			
Treatment (T)	0.039	0.109	−0.044
	(0.012)	(0.005)	(0.011)
Control Mean	0.415	0	0.41
Observations	19,442	19,442	19,442
Panel B: Eligible Households only			
Treatment	0.101	0.237	−0.081
	(0.016)	(0.007)	(0.013)
Control Mean	0.428	0.001	0.42
Observations	8,679	8,679	8,679
Panel C: Ineligible Households only			
Treatment	0.013	0.0002	0.013
	(0.013)	(0.001)	(0.013)
Control Mean	0.421	0	0.416
Observations	10,763	10,763	10,763

Note: The unit of observation is a working age adult. 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 a binary outcome for being in any kind of employment (wage, self or public works). Public employment any employment on public works. “Private employment” denotes hany work in the private sector (wage work or self-employment). “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 A10: ITT Effects on Labor Force Participation

	Dependent variable:			
	Employed	Available	Inactive	In education
	(1)	(2)	(3)	(4)
Panel A: Whole Sample				
Treatment at Origin	0.039	−0.016	−0.030	0.007
	(0.007)	(0.006)	(0.005)	(0.006)
Control mean	0.415	0.224	0.127	0.196
Observations	19,442	19,442	19,442	19,442
Panel B: Eligible Households only				
Treatment at Origin	0.109	−0.044	−0.066	−0.001
	(0.010)	(0.009)	(0.006)	(0.006)
Control mean	0.428	0.221	0.106	0.188
Observations	8,679	8,679	8,679	8,679
Panel C: Ineligible Households only				
Treatment at Origin	−0.015	0.001	0.002	0.001
	(0.009)	(0.009)	(0.006)	(0.005)
Control mean	0.421	0.232	0.099	0.190
Observations	10,763	10,763	10,763	10,763

Note: The unit of observation is an individual survey respondent and the sample is composed of all adult household members. “Employed” is indicator that the respondent worked (wage or self-employment) in the last seven days. “Available” indicates that the respondent did not work in the last seven days but does sometimes work casually and/or is available for work. “Inactive” indicates that the respondent is not available for work either because he or she works in the home, does not work to work, has a disability, or is retired (under 65). “In education” indicates that the respondent is in the full-time education. “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 A11: ITT Effects on Commuting

	Dependent variable:			
	Commute out	Hours out	Distance (km)	Time
	(1)	(2)	(3)	(4)
Panel A: Whole Sample				
Treatment at Origin	−0.016	−0.017	−0.048	−1.077
	(0.006)	(0.006)	(0.023)	(0.247)
Control mean	0.219	0.216	0.452	8.925
Observations	19,442	19,442	19,442	19,442
Panel B: Eligible Households only				
Treatment at Origin	−0.026	−0.021	−0.090	−1.320
	(0.008)	(0.008)	(0.028)	(0.340)
Control mean	0.201	0.193	0.384	8.128
Observations	8,679	8,679	8,679	8,679
Panel C: Ineligible Households only				
Treatment at Origin	−0.009	−0.014	−0.012	−0.888
	(0.008)	(0.008)	(0.035)	(0.353)
Control mean	0.234	0.234	0.504	9.543
Observations	10,763	10,763	10,763	10,763

Note: The unit of observation is an individual working age adult survey respondent (regardless of employment status). In columns 1 to 3 the sample is composed of all adult household members. “Commute out” is a dummy equal to one if the adult works outside of their woreda and zero if the adult does not work or works in their own woreda. “Hours out” measures the share of total available hours that the individual spends working out of their woreda, and is equal to zero if they do not work. “Distance” measures the distance between the household’s exact location and the centroid of the woreda in which the individual works, and is equal to zero if they do not work. “Time” measures the self-reported time that it takes for the respondent to commute to their place on an average day (one direction), and is equal to zero if they do not work. “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 A12: Effects on Private Employment: Heterogeneity Analysis

	Eligible	Ineligible	Self-Employment	Wage-Employment
	(1)	(2)	(3)	(4)
Treatment (T)	−0.080	−0.021	−0.015	−0.032
	(0.014)	(0.013)	(0.005)	(0.012)
P-value of Difference		0.002		0.218
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	−0.037	−0.049	−0.042
	(0.011)	(0.017)	(0.013)	(0.015)
P-value of Difference		0.305		0.704
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 a working age adult. 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 household and individual controls. Standard error are clustered at the woreda level.

Table A13: Effects on Private Sector Wages: Heterogeneity

	Eligible	Ineligible	Self-Employment	Wage Work
	(1)	(2)	(3)	(4)
Exposure of Destination	0.123	0.209	0.360	0.147
	(0.116)	(0.094)	(0.169)	(0.077)
RI p-values	0.359	0.0305	0.0195	0.0705
P-value of Difference		0.567		0.253
Observations	89	89	90	90
	Female	Male	Low Skill	High Skill
	(5)	(6)	(7)	(8)
Exposure of Destination	0.126	0.207	0.229	0.079
	(0.079)	(0.104)	(0.091)	(0.080)
RI p-values	0.1435	0.057	0.0175	0.3195
P-value of Difference		0.507		0.169
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 A14: Effects on Private Sector Wages: Robustness and IV estimates

	Log Wages at Destination			
	Predicted	Imputed	Log	IV
	(1)	(2)	(3)	(4)
Exposure of Destination	0.152	0.231		
	(0.073)	(0.075)		
Log(1-p*Exposure of Destination)			−1.358	
			(0.539)	
Log Change in Labor Supply				−1.469
				(0.675)
RI p-values	0.0475	0.003	0.013	
Observations	90	90	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 A15: Effects on Private Sector Wages: Estimation in Differences

	Log wages at origin		Log wages at destination	
	(1)	(2)	(3)	(4)
Treatment at Origin	0.080	0.068		
	(0.043)	(0.047)		
Exposure of Destination			0.093	0.092
			(0.101)	(0.102)
RI p-values	0.075	0.167	1	1
Observations	90	90	90	90

Note: The unit of observation is a neighborhood. In Columns 1 and 2 the dependent variable is the change between baseline and endline in log hourly earnings from private sector work (excluding public works) 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 the change between baseline and endline in log hourly earnings from private sector work (excluding public works) 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 A16: Effects on Private Sector Wages: Daily Wages

	Log wages at origin		Log wages at destination	
	(1)	(2)	(3)	(4)
Treatment at Origin	0.096	0.093		
	(0.035)	(0.038)		
Exposure of Destination			0.196	0.142
			(0.067)	(0.059)
RI p-values	0.0175	0.0075	0.0005	0.0115
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 daily 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.

B Mathematical Appendix

B.1 Proof of Equation 4.3

The representative firm in location j uses labor L_j to produce output Y_j with the following production function:

$$Y_j = a_j F(L_j) \quad \text{where } F'(.) > 0 \quad \text{and } F''(.) < 0$$

productivity a_j is fixed. All firms produce the same product whose price is one. Profit maximization implies that:

$$w_j = a_j F'(L_j)$$

Optimal labour demand is:

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

Differencing and multiplying by w_j/L_j yields the labour demand elasticity:

$$\varepsilon_D = \frac{w_j}{L_j} \times \frac{\partial L_j}{\partial w_j} = \frac{w_j}{L_j} \times \frac{1}{F''(F'^{-1}(w_j))} < 0$$

It is negative because $F'' < 0$.

B.2 Proof of Equation 3

A worker from i will work in j if the utility U_{ij} it derives from working in j is greater than the utility it derives from working in all other locations.

$$\pi_{ij} = \Pr(U_{ij} > max_{k \neq i} U_{ik})$$

Since the utility shocks draws are independent across destinations, for a given x:

$$\Pr(x > max_{k \neq i} U_{ik}) = \prod_{k \neq i} \Pr(x > U_{ik})$$

Recall that the cumulative distribution of U_{ik} is denoted with $G_k(U)$ and the density of U_{ij} denoted with $g_j(U)$. We can write the probability π_{ij} as:

$$\pi_{ij} = \int_0^\infty \left(\prod_{k \neq i} G_k(U) \right) g_j(U) dU$$

Replacing the cumulative distribution and the density by their values yields:

$$\pi_{ij} = \int_0^\infty \left(\prod_{k \neq i} e^{-\Phi_{ik} U^{-\theta}} \right) \left(\theta \Phi_{ij} U^{-\theta-1} e^{-\Phi_{ij} U^{-\theta}} \right) dU$$

Rearranging:

$$\pi_{ij} = \int_0^\infty (\theta \Phi_{ij} U^{-\theta-1} e^{-\sum_k \Phi_{ik} U^{-\theta}}) dU$$

Integrating over U:

$$\pi_{ij} = \frac{\Phi_{ij}}{\sum_k \Phi_{ik}} \left[e^{-\sum_k \Phi_{ik} U^{-\theta}} \right]_0^\infty = \frac{\Phi_{ij}}{\sum_k \Phi_{ik}}$$

Replacing Φ_{ij} and Φ_{ik} by their values completes the proof

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

B.3 Proof of Equation 2

The utility is a monotonic function of ϵ which follows a Frechet distribution, hence it also follows a Frechet distribution with cumulative distribution function:

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

Workers in a given location of residence i choose among the locations of work j the one that gives them the highest utility. Let $G_i(u)$ denote the cumulative distribution function of the maximum utility attained by workers from i, which also follows a Frechet distribution:

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

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

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

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

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

We write the expectation:

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

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

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

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

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

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.4 Proof of Equations 6 and 11

We derive here the expression for the change in wages in location j as a function of changes in labor supply coming from all origins i (including j). We use the expression of the labor demand elasticity in Equation 4.3:

$$\ln \widehat{w_j} = \frac{1}{\varepsilon_D} \ln \left[\widehat{L_j} \right] \tag{B1}$$

From equation 4 we know that the labor market equilibrium without the program is such that:

$$L_j = \sum_i \pi_{ij} R_i$$

This relies on the fact that without the program, the labor supply of each resident from i is one. With the program, labor supply of resident R_i goes from 1 to $(1-p)$ if i is treated, and remains the same otherwise. At the same time the probability of commuting from i to j may change due to equilibrium effects on wages. The labor supply to j with the program can hence be written as:

$$L'_j = \sum_i \pi'_{ij}(1 - pT_i) R_i$$

We use hat notations to recover the change in L_j between the equilibrium with and without the program:

$$\begin{aligned}
\widehat{L_j} = \frac{L'_j}{L_j} &= \frac{\sum_i \pi'_{ij}(1-pT_i)R_i}{\sum_i \pi_{ij} R_i} \\
&= \frac{\sum_i \pi_{ij}\widehat{\pi_{ij}}(1-pT_i)R_i}{\sum_i \pi_{ij} R_i} \\
&= \sum_i \frac{\pi_{ij} R_i}{\sum_k \pi_{kj} R_k} \widehat{\pi_{ij}}(1-pT_i) \\
&= \sum_i \lambda_{ij} \widehat{\pi_{ij}}(1-pT_i)
\end{aligned} \tag{B2}$$

where λ_{ij} is the fraction of people who work in j that come from i at baseline.

We can use equation B1 and B3 to obtain equation 6:

$$\ln \widehat{w_j} = \frac{1}{\varepsilon_D} \ln \left[\sum_i \lambda_{ij} \widehat{\pi_{ij}}(1-pT_i) \right]$$

To capture only the exogenous changes in labor supply due to the program we shut down endogeneous changes in commuting flows and assume that $\widehat{\pi_{ij}} = 1$:

$$\begin{aligned}\ln \widehat{w_j} &= \frac{1}{\varepsilon_D} \ln \left[\sum_i \lambda_{ij}(1 - pT_i) \right] \\ &= \frac{1}{\varepsilon_D} \ln \left[1 - \sum_i pT_i \lambda_{ij} \right] \end{aligned} \tag{B3}$$

Since $\sum_i pT_i\lambda_{ij} < p = 0.128$ is small we can use the Taylor series approximation that $\ln(1-x) \approx -x$ and complete the proof of equation 11:

$$\ln \widehat{w_j} \approx -\frac{p}{\varepsilon_D} \sum_i T_i \lambda_{ij} \tag{B4}$$

B.5 Proof of Equation 7

We compute here U', the expected utility of workers when the program is implemented. Let us consider a worker ω from neighborhood i who works for the private sector in neighborhood j. If the program is implemented in i, (i.e. if $T_i = 1$), then the worker will spend p of their labor supply on public works and $(1-p)$ part of their labor supply on private sector work:

$$U'_{ij}(\omega) = pT_i B'_i w_g \epsilon_g + (1 - pT_i) B'_i \tau_i j w'_j \epsilon_{ij}$$

The idiosyncratic terms ϵ_g and ϵ_{ij} follow a Frechet distribution. We can use the same proof as for Equation 2 for the two terms separately, and obtain U'_i:

$$U'_i = pT_i \Gamma\left(\frac{\theta-1}{\theta}\right) \left[(B'_i w_g)^\theta \right]^{\frac{1}{\theta}} + (1-pT_i)\Gamma\left(\frac{\theta-1}{\theta}\right) \left[\sum_j (B'_i \tau_{ij} w'_j)^\theta \right]^{\frac{1}{\theta}}$$

which simplifies to:

$$U'_i = \gamma \left[pT_i \left(B'_i w_g\right) + (1-pT_i) \left[\sum_j (B'_i \tau_{ij} w'_j)^\theta \right]^{\frac{1}{\theta}} \right]$$

with $\gamma = \Gamma\left(\frac{\theta-1}{\theta}\right)$ Since for every X, $\widehat{X} = X'/X$ we replace X' by $\widehat{X}X$ to obtain:

$$U'_i = \widehat{U}_i U_i = \gamma \left[pT_i \left(\widehat{B_i} B_i w_g\right) + (1-pT_i) \left[\sum_j (\widehat{B_i} B_i \tau_{ij} \widehat{w_j} w_j)^\theta \right]^{\frac{1}{\theta}} \right]$$

Replacing $\widehat{B_i}$ by $(1+bT_i)$ completes the proof:

$$U_i' = \widehat{U}_i U_i = \gamma(1+b_iT_i)\left[pT_i\left(B_iw_g\right) + (1-pT_i)\left[\sum_j \widehat{w_j}^{\theta}(B_i\tau_{ij}w_j)^{\theta}\right]^{\frac{1}{\theta}}\right]$$

B.6 Proof of Equation 13

We obtain the change in expected utility $\widehat{U}_i$ by dividing the expression of utility in 7 with the expression in 2

$$\widehat{U}_i = \frac{(1+b_iT_i)\left[p_iT_iB_iw_g + (1-p_iT_i)\left[\sum_j \widehat{w_j}^{\theta}(B_i\tau_{ij}w_j)^{\theta}\right]^{\frac{1}{\theta}}\right]}{\left[\sum_j (B_i\tau_{ij}w_j)^{\theta}\right]^{\frac{1}{\theta}}}$$

We replace w_g by $(1+g_i)w_i$ where $1+g_i$ is the public wage premium:

$$\widehat{U}_i = \frac{(1+b_iT_i)\left[p_iT_i(1+g_i)(B_iw_i) + (1-p_iT_i)\left[\sum_j \widehat{w_j}^{\theta}(B_i\tau_{ij}w_j)^{\theta}\right]^{\frac{1}{\theta}}\right]}{\left[\sum_j (B_i\tau_{ij}w_j)^{\theta}\right]^{\frac{1}{\theta}}}$$

We use equation 3 to substitute $\pi_{ii}^{\frac{1}{\theta}}$ for $\frac{(B_iw_i)}{\left[\sum_j (B_i\tau_{ij}w_j)^{\theta}\right]^{\frac{1}{\theta}}}$ and π_{ij} for $\frac{(B_i\tau_{ij}w_j)^{\theta}}{\sum_j (B_i\tau_{ij}w_j)^{\theta}}$

$$\widehat{U}_i = (1+b_iT_i)\left[p_iT_i(1+g_i)\pi_{ii}^{\frac{1}{\theta}} + (1-p_iT_i)\left[\sum_j \pi_{ij}(\widehat{w_j})^{\theta}\right]^{\frac{1}{\theta}}\right]$$

This expression can now be rearranged, by adding $1+pT_i+(1-pT_i)$ inside the square brackets to obtain:

$$\widehat{U}_i = \underbrace{(1+bT_i)}_{Amenity\ Effect}\left[1+\underbrace{pT_i\left((1+g_i)\pi_{ii}^{\frac{1}{\theta}}-1\right)}_{Direct\ Effect}+\underbrace{(1-pT_i)\left(\left(\sum\nolimits_j \pi_{ij}\widehat{w_j}^{\theta}\right)^{\frac{1}{\theta}}-1\right)}_{Wage\ Effect}\right]$$

This provides a decomposition of the effects of the program by expressing each component as the percentage increase in utility relative to the non-program equilibrium, rather than the ratio of utilities. The *Direct Effect* is the increase in utility from working in the program relative to the utility from working in the labor force *at non-program-equilibirum wages* for pT_i hours. The *Wage Effect* is the increase in utility from the increase in wages across the city due to the program for the $(1-pT_i)$.

B.7 Proof of Equation 9

We consider the welfare effect of a cash transfer which provides the same utility as the wages earned on the public works, i.e. $pT_i(1+g_i)w_i\epsilon_g$. The expected utility for a worker living in i when the cash transfer is implemented is the sum of the utility without the cash transfer (from equation 2) plus the transfer:

$$U_i' = \gamma \left[\left[\sum_j (B_i \tau_{ij} w_j)^{\theta} \right]^{\frac{1}{\theta}} + pT_i B_i (1+g_i) w_i \right] \quad with \quad \gamma = \Gamma\left(\frac{\theta-1}{\theta}\right)$$

We obtain the change in expected utility $\widehat{U_i^{cash}}$ by dividing this expression with utility without the transfer (equation 2):

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

C Overview of commuting data

We find that roughly 45% of workers commute to work by walking (this is consitent 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 49 minutes and the average commuting distance is 5 kilometers (both directions). We find that 53.4% of all workers commute outside of their woreda for work. Woredas are geographic with populations of over 35,000 on average. 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.[44]

Note that 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. These households are dropped from our main estimation. This is why the share of out-commuters is largerin the full sample (58%) , than in the sample that we use to the construct bilateral commuting matrix (45%): we know that all of these dropped workers work outside of their woreda of residence, we just do not observe precisely where. Our results are robust to imputing their destinations from their neighbors commuting destinations.

D Effects on local prices

As discussed in sections 3 and 4, we do not find evidence that the program increased household expenditures (Appendix Table A8), 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.

[44] We computed this from the 2011 UK census, table *wu03ew*.

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. Formally, let m denote a market, p_m the price of a given class or the price index, and $Exposure_m$ denotes its exposure to treatment, we estimate with OLS the following equation:

$$\ln p_m = \alpha + \beta Exposure_m + \varepsilon_m \tag{D1}$$

To measure exposure at market level, we take two different approaches. First, we use the measure of exposure used in our main specification for the labor market spillovers. In other words, we use the definition of exposure in Equation 12, but where we simply match each market to the woreda in which it is located. This approach assumes price effects will spillover across woredas in a similar way to the wage effects. This may be the case if most shopping is done by commuters around their work place.

Second, given that we think that shopping behaviour is likely more local, and based on short walking trips within the neigborhood, we take an alternative approach based on Euclidean distance between neighborhoods. Specifically, exposure is defined as a sum of treatment status in each neighborhood, weighted by its eligible population and the inverse of the Euclidean distance to the market:

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

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 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 D2 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 D1: Impact of treatment exposure via commuting network on product prices from CPI data

	All items	Food	Clothing	Household
	(1)	(2)	(3)	(4)
Exposure	0.010	0.014	−0.028	−0.151
	(0.308)	(0.120)	(0.121)	(0.175)
RI p-values	0.886	0.906	0.8425	0.451
Observations	120	12	12	12
	Housing	Health	Transport	Communication
	(5)	(6)	(7)	(8)
Exposure	0.250	−0.257	0.119	0.260
	(0.220)	(0.245)	(0.176)	(0.237)
RI p-values	0.3735	0.3865	0.52	0.2135
Observations	12	12	12	12
	Recreation	Education	Restaurant	
	(9)	(10)	(11)	
Exposure	−0.148	−0.331	−0.027	
	(1.026)	(0.329)	(0.115)	
RI p-values	0.9045	0.3555	0.8275	
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.

Table D2: Impact of treatment exposure using Euclidean distance to treated neighborhoods on product prices from CPI data

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

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

E Alternative specifications for spillovers

In Table 3 we show that if estimate Equation 10 and compare wages in treated and control areas, the estimated effect of the program on wages is about 9%. By contrast, if we use the model-based measure of exposure of each labor market to changes in commuting flows due to the program we find that once rolled-out everywhere the program would increase wages by 19%. In this section, we use alternative approaches to recover spatial spillovers and compare them to our main results. We consider two strategies which are common in the literature on spillover effects. First, we compare treated areas to plausibly "unaffected" woredas – that is, woredas that are not geographic proximate to any treated woredas. This is akin to the so-called "donut" approach (CITE). Second, we measure exposure of each woreda to spatial spillovers as the share of woredas within a certain radius which are treated, as in Egger et al. (2022) and Muralidharan et al. (2017). We call this the "radius" approach. As compared to our method, these two approaches are less demanding (i) they do not require any direct measurement of the spatial relationship between treatment units (i.e. the commuting flows) (ii) they do not rely on any modelling of this relationship (i.e. the spatial equilibrium model of commuting). It is hence important to test whether they can recover estimates of spatial spillovers that are similar to ours.

E.1 The donut approach

To implement the "donut" approach, we compare wages earned by workers from treated and control woredas, but restrict the control group to only those that are far away from all treated woredas. The logic of this approach is as follows: if the labor markets in these woredas are isolated enough from treated woredas, they are plausibly unaffected by spillovers. Therefore a comparison of these woredas to treated woredas may recover the full effect of the program on treated woredas. A back-of-envelope calculation would allow us to recover the magnitude of spillover effects to control neighbors of treated areas, by subtracting from this estimate the total treatment-control difference estimated in Equation 10.

The Map in Figure 1 immediately illustrates the limitation of this approach in our context. There are relatively few woredas that are not neighbors with at least one treated woreda, in both the treatment and control groups. We believe this is going to be the case for many geographies where treatment is rolled out at scale. The donut approach is more likely to be suitable in settings where the density of treatment is relatively sparse: that is where the percentage of a treated areas is small relative to the number of control (or spillover) areas. In our case, roughly 40% of locations are treated. This limits the size of "pure" control group in our setting. Also, importantly, these woredas are also significantly more likely to be geographically isolated, far from the city centre, and therefore may differ in many other ways from the average treated woreda, apart from the fact that they were not treated.

Table E1: Spillovers using a donut approach

	Log wages at origin				
	(1)	(2)	(3)	(4)	(5)
	Max share of neighbors treated for control woredas				
	0%	20%	30%	50%	60%
Treatment at Origin	−0.014	0.099	0.075	0.074	0.085
	(0.164)	(0.082)	(0.057)	(0.044)	(0.039)
Observations	41	49	60	73	83

Note: The unit of observation is a neighborhood. In all columns the dependent variable is daily wages earned by workers who live in that neighborhood. In all specifications worker controls and subcity fixed effects are included. "Treatment" is a dummy equal to one if the neighborhood is treated. In each column we drop from the pool of control woredas all woredas where X% or more of their neighbors (definied as sharing a border) were treated. Columns 1 to 5 gradually increase X from 0 to 60 percent.

The columns in Table E1 show the results where we drop control woredas that are "close" to treated woredas. In column 1 we drop all control woredas that share a border with a treated woreda. The control group here contains only 6 untreated woredas (there are 35 treated woredas). If we expand the radius over which we drop controls with treated neighbors within a distance larger than zero, we quickly have no control group at all, so we do not pursue this approach. Instead, in columns 2 to 5 we drop control woredas with more than 20%, 30%, 50%, and 60% of their neighbors treated, respectively. As expected, the sample size increases as we include woredas with higher shares of their neighbors treated, and our estimates start to converge to our main estimates that use all control areas. Crucially, we do not find any estimate that is larger than the one from the regression which ignores spillovers and regresses wages on treatment in the neighborhood (Column 1 and 2 of Table 3). As a result, this simpler approach does not detect the presence of any meaningful spillover of the size implied by our preferred estimates using the exposure measure (Column 3 and 4 of Table 3).

E.2 The "radius" approach

Another common approach in the literature measures exposure to spatial spillovers as the share of treated units given a certain radius. Specifically, the "radius" approach implemented in our context consists in estimating the following specification:

$$\ln w_i = \alpha + \beta T_i + \beta N_i^R + \gamma \ln w_i^0 + \delta \mathbf{X_i} + \varepsilon_i \tag{E1}$$

where N_i^R is the share of neighboring woredas within radius R of woreda i that were treated (using distances between woreda boundaries). To account for the fact that woredas have different population sizes, we also use data on the true program-eligible population in each woreda to reweight our measure of N_i^R such that it represents the share of the population in neighboring areas that are treated.

Table E2 presents the results. We vary R from 500m to 5kms, and provide the average share of all woredas or share of all total population containted within R in the bottom rows of each panel, to give a sense of the variation in R. We find wide variation in the coefficients across specifications. Standard errors get bigger as we increase the radius R, since this implicitly reduces between-woreda variation in N_i^R. Consistent with our main findings, we find the correlation bewteeen neighborhood exposure and wages is generally positive, but the standard errors are larger than the estimates, and the estimates turn negative with larger radii. This contrasts with the findings from Egger et al. (2022) and Muralidharan et al. (2017) who apply the "radius" method to rural social program, and generally find that the results are stable to changes in the radius. The difference is likely due to the fact that we study an urban context, where population and economic activity are less uniformly distributed, and where Euclidean distance is less good of a proxy for connectivity.

E.3 Exposure to treated households by Euclidean distance

The approaches above exploit data that most cluster-level randomized trials would have access to: a measure of neighboring locations, their treatment outcomes, and their populations. We are able to go further than that, without using the commuting data, by exploiting a unique georeferenced sample of all program participants, taken after the targeting was done in each woreda. We use precise GPS coordinates for each household. We now use that to reestimate equation E1, now using N_i^R as the share of eligible *households* in neighboring woredas that were treated in the first year of the program. That is...

Table E2: Spillovers using share of neighboring woredas treated

	Log wages at origin			
	(1)	(2)	(3)	(4)
Radius:	0.5km	1km	2km	5km
Panel A: Not weighted by population				
Treatment at Origin	0.103	0.102	0.101	0.098
	(0.040)	(0.038)	(0.037)	(0.038)
Neighbors treated	0.030	0.052	−0.076	−0.293
	(0.164)	(0.177)	(0.228)	(0.517)
Observations	90	90	90	90
Av. share of all woredas in R	9.1%	13.7%	25.7%	64.5%
Panel B: Weighted by population				
Treatment at Origin	0.105	0.103	0.100	0.097
	(0.040)	(0.037)	(0.037)	(0.038)
Neighbours treated	0.051	0.134	0.100	−0.198
	(0.140)	(0.156)	(0.198)	(0.470)
Observations	90	90	90	90
Av. share of all popn R	8.9%	13.6%	25.7%	62.2%

Note: The unit of observation is a neighborhood. In all columns the dependent variable is daily wages earned by workers who live in that neighborhood. In all specifications worker controls and subcity fixed effects are included. “Treatment at Origin” is a dummy equal to one if the neighborhood is treated. “Neighbors treated” is a measure of the share of neighboring woredas that are treated. Neighboring woredas are defined as all woredas within Rkm of one another at the shortest point between woreda boundaries. Columns 1 to 4 gradually increase R from 0.5 to 5 kilometers.

$N_i^R = \sum_{j \neq i} \sum_{h \in H_j} [T_{jh} * \mathbf{1}(D_h < R)]$ where T_{jh} is the treatment status of household h in the set of eligible households H_j living in woreda j and D_h is the Euclidean distance between household h and the centroid of woreda i. By nature of the randomization, we find no significant different in N_i^R between treated and untreated woredas, for example, within a radius of $R = 2$ average exposure is 0.36 in both treated and untreated woredas.

Table E3: Spillovers using share of non-resident eligible households treated within Euclidean distance bands

	Dependent variable:				
	Log wages at origin				
	Share of eligible households treated within				
	0.5km	1km	2km	5km	8km
	(1)	(2)	(3)	(4)	(5)
Treatment at Origin	0.111	0.106	0.101	0.103	0.085
	(0.038)	(0.038)	(0.037)	(0.038)	(0.043)
Share of eligible households treated	0.131	0.086	0.028	0.114	−0.611
	(0.101)	(0.136)	(0.181)	(0.338)	(0.871)
Observations	90	90	90	90	90
Resident Worker Controls	Yes	Yes	Yes	Yes	Yes
Av. share of eligible pop in R	3.9%	7.5%	17.6%	55.7%	85.0%

Note: The unit of observation is a neighborhood. In all columns the dependent variable is daily wages earned by workers who live in that neighborhood. In all specifications worker controls and subcity fixed effects are included. "Treatment at Origin" is a dummy equal to one if the neighborhood is treated. "Share of eligible households treated" is the share of households in our data that were eligible for the program and within a distance radius R from the centroid of the origin woreda. Columns 1 to 5 gradually increase R from 0.5 to 8 kilometers.

Table E3 presents the results. Here we find a generally positive relationship between exposure of surrounding non-residents to the program, *in addition* to the effect of exposure to resident's own woreda. But the estimates are very sensitive to the choice of the distance band, they are very close to zero at 2km, and negative at 8km. As with the results in Table E2, the standard errors on these estimates are large, so that none of them is close to significance.

F Robustness: labor supply estimates

In this section, we return to our main estimates of the reduced form effects of the policy. In Table 2 we showed how the program provided public employment equivalent to 4.6% of available adult working hours, and leads to a reduction in labor supply to the private sector of almost exactly the same amount of time. We used this estimate to calibrate p in our main welfare estimates, and to derive the labor demand elasticity (since p characterises the magnitude of the labor supply shock).A concern is that our estimates of p are based on a misspecification in equation 1. We interpret p as the exogeneous reduction in labor supply due household members doing the program and therefore reducing their labor supply to the private sector. We did not consider that the estimates based on equation 1 may include endogeneous changes in labor supply, for example, due to increasing wages across the city. As our own approach shows, the correct specification for estimating labor market effects should be as a function of exposure to the program, and not simply the woreda-of-residence treatment status.

This presents two challenges. First, since we want to separately identify the *direct* effect of participation in program on labor supply p from other other (endogeneous) changes in labor supply, we need to regress the labor supply of individual i on the $i's$ woreda treatment status as well as a measure of exposure to the program. Second, we need a measure of exposure for someone living in woreda i, rather than a measure of exposure for someone working in woreda j as we did for wages in equation 12. For this we calculate

$$ExposureSquared_i = \sum_j \pi_{ij} Exposure_j = \sum_j \pi_{ij} \sum_k \lambda_{jk} T_k$$

where p_ij is the baseline probability of commuting from i to j and λ_{jk} is the share of workers in k who come from j. In other words, we estimate exposure of residence i *to exposure* of all labor markets j and run the following equation at the individual level:

$$Y_{\omega hi} = \alpha + \beta_1 T_i + \beta_2 ExposureSquared_i + \gamma Y^0_{\omega hi} + \delta \mathbf{X}_{\omega \mathbf{hi}} + \varepsilon_{\omega hi}. \tag{F1}$$

The results using share of total hours are in Table F1. We find that this approach recovers our original estimate of the effect of the program on labor supply. On the other hand, we find no effect of exposure to the program on individual labor supply, which is in line with our interpretation of p. As before, the picture is slightly different when we look at the extensive margin in F2. The program increases labor supply at the extensive margin, as in Table A9. Once again, we find no evidence that labor supply adjusted as the extensive margin due to exposure to the program.

Table F1: ITT Results with Exposure Squared (Hours)

	Share of Hours Spent on		
	Employment	Public Employment	Private Employment
	(1)	(2)	(3)
Panel A: Whole Sample			
Treatment	0.001	0.049	−0.048
	(0.028)	(0.005)	(0.027)
Exposure Squared	−0.003	−0.006	0.003
	(0.061)	(0.011)	(0.059)
Control Mean	0.366	0	0.366
Observations	19,442	19,442	19,442
Panel B: Eligible Households only			
Treatment	0.002	0.095	−0.092
	(0.036)	(0.006)	(0.035)
Exposure Squared	0.050	0.017	0.033
	(0.086)	(0.015)	(0.084)
Control Mean	0.36	0	0.359
Observations	8,679	8,679	8,679
Panel C: Ineligible Households only			
Treatment	−0.008	0.0002	−0.008
	(0.031)	(0.0004)	(0.035)
Exposure Squared	−0.031	0.001	−0.032
	(0.063)	(0.001)	(0.084)
Control Mean Ineligible	0.378	0	0.378
Observations	10,763	10,763	10,763

Note: The unit of observation is an individual survey respondent. Origin exposure is the predicted exposure of resident in the origin woreda i to all of the wage increases across the city due to the program: $\sum_j \pi_{ij} Exposure_j = \sum_j \pi_{ij} \sum_k \lambda_{jk} T_k$. 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. "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 F2: ITT Effects with Exposure Squared (Extensive margin)

	Employment rate		
	Any	Public Employment	Private Employment
	(1)	(2)	(3)
Treatment	0.035	0.112	−0.043
	(0.024)	(0.010)	(0.026)
Exposure Squared	0.010	−0.009	0.0001
	(0.055)	(0.024)	(0.058)
Control Mean	0.415	0.415	0.415
Observations	19,442	19,442	19,442
Panel B: Eligible Households only			
Treatment	0.074	−0.094	−0.094
	(0.030)	(0.033)	(0.033)
Exposure Squared	0.069	0.043	0.043
	(0.072)	(0.081)	(0.081)
Control Mean	0.428	0.001	0.389
Observations	8,679	8,679	8,679
Panel C: Ineligible Households only			
Treatment	−0.012	0.001	0.001
	(0.028)	(0.029)	(0.033)
Exposure Squared	−0.008	−0.045	−0.045
	(0.059)	(0.059)	(0.081)
Control Mean Ineligible	0.421	0	0.391
Observations	10,763	10,763	10,763

Note: The unit of observation is an individual survey respondent. Origin exposure is the predicted exposure of resident in the origin woreda i to all of the wage increases across the city due to the program: $\sum_j \pi_{ij} Exposure_j = \sum_j \pi_{ij} \sum_k \lambda_{jk} T_k$. 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. "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.

G Alternative estimation of the parameter θ

In this section, we use an alternative strategy inspired by Heblich et al. (2020), and estimate θ as the elasticity of commuting to commuting costs c_{ij} in the equation:

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

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 c_{ij}, the commuting cost and commuting time reported by the survey respondents. Transportation networks and hence travel costs may be endogenous, which is why Heblich et al. (2020) instrument c_{ij} by walking distance.[45] The results are presented in Appendix Table G1. 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 the literature (e.g. Heblich et al. (2020) find $\theta = 5.25$ for 19th century London). There are at least two reasons for the difference between the two sets of estimates. On the one hand, the lower estimate is identified through random variation in the wage, while the higher estimate may suffer from omitted variable bias, e.g. if parts of the city that are closer geographically offer better job matches. On the other hand, the lower estimate reflects the response of commuting to a short-term differential in wages, which will disappear one year later once the program is implemented everywhere, while the higher estimates correspond to long-term adjustments to the commuting network.

We next present the welfare effects of the program implied by the alternative estimate of $\theta = 4.55$. The results presented in Appendix Table G2, are similar, although the welfare gains from the direct effects are nearly twice as large. This is because a higher θ implies lower dispersion of idiosyncratic utility across locations and therefore higher expected relative utility from working at home on the public works. Our main conclusions remain however unaffected: wage effects dominate direct effects and give public works an edge over an equivalent cash transfer.

[45] Heblich et al. (2020) do not actually 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.

Table G1: Commuting Elasticity with Respect to Commuting Cost

	Commuting Probability			
	Poisson	*Poisson-IV*	*Poisson*	*Poisson-IV*
	(1)	(2)	(3)	(4)
Log Commuting cost	-1.239	-4.332		
	(0.015)	(0.029)		
Log Commuting time			-1.639	-4.548
			(0.012)	(0.027)
Observations	838	838	911	911

Note: The unit of observation is a neighborhood origin$\times$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 G2: 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	Treatment	All
	(1)	(2)	(3)
Treatment	0.000	1.000	1.000
Exposure	0.161	0.765	1.000
Direct Effect	0.000	0.054	0.056
Direct + Wage Effects	0.046	0.158	0.218
Direct+Wage+Amenity	0.046	0.186	0.247
Cash Transfer	0.000	0.182	0.184

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.

H Income gains

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:

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

The proportional change in income due to the program is:

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

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

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

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.

$$\widehat{v_i^{cash}} = \frac{pT_i(1+g_i)w_i + \sum_j \pi_{ij}w_j}{\sum_j \pi_{ij}w_j} \tag{H1}$$

The results are presented in Table H1 below.

Table H1: Income gains from public works compared to a cash transfer

Roll-out	Partial		Complete
	Control	Treatment	All
	(1)	(2)	(3)
Treatment	0.000	1.000	1.000
Exposure	0.161	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	0.241
Income Gain (Cash Transfer)	0.000	0.208	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.)