# Online Appendix.

**"Urban Public Works in Spatial Equilibrium: Experimental Evidence from Ethiopia" - Simon Franklin, Clément Imbert, Girum Abebe, and Carolina Mejia-Mantilla**

## A Additional tables and figures

Figure A1: Public works participation by men and women (among all working age members of treated households)

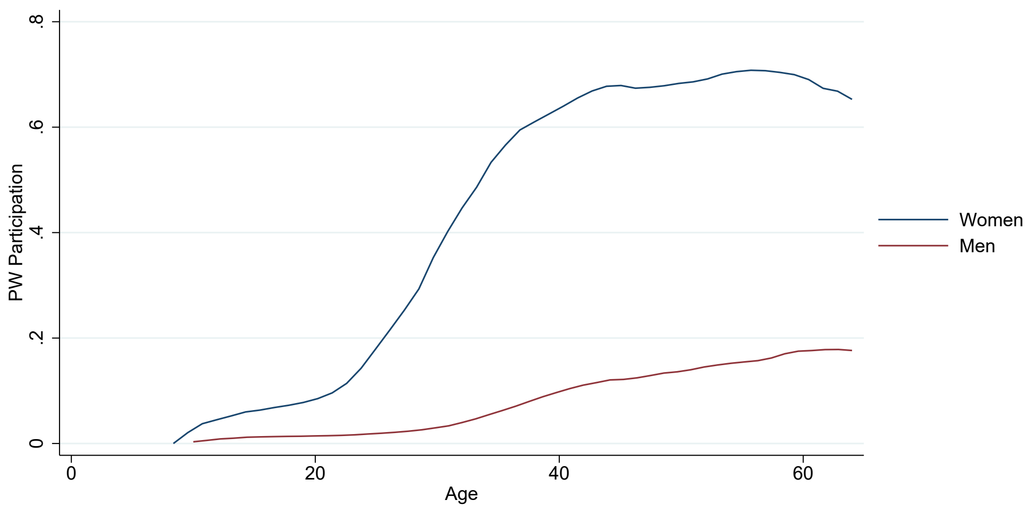

Figure A2: Public and private sector wages in the first endline survey

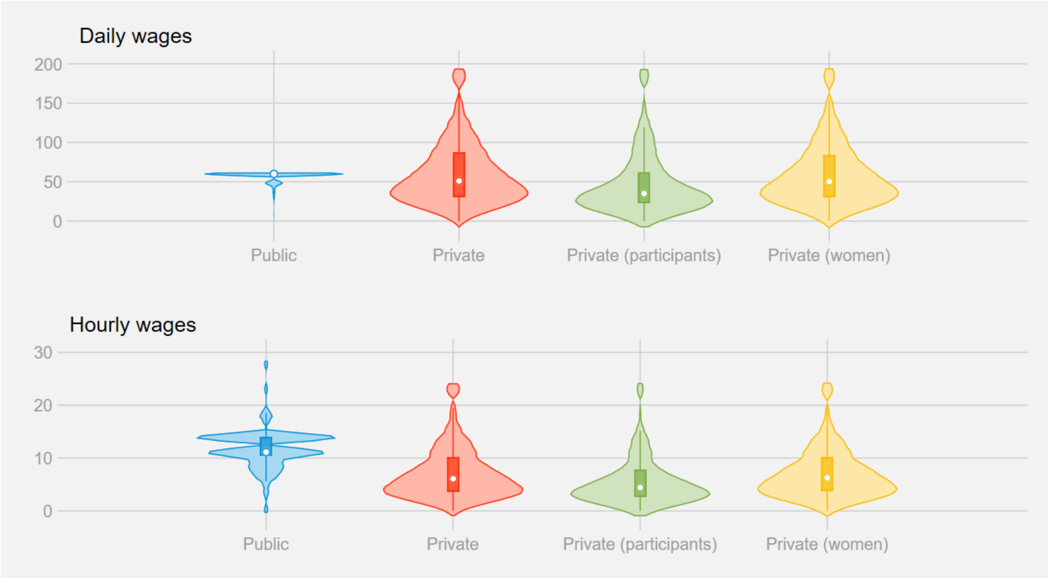

Table A1: Timeline of program roll out and data collection

| 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. |

Table A2: Determinants of endline attrition

| | Only Treatment | | | |
|---|---|---|---|---|
| | Coeff | | Coeff | |
| Dependent Variable: Household responded to endline | (1) | (2) | (3) | (4) |
| Woreda Selected Year 1 | 0.008 | 0.007 | 0.008 | 0.007 |
| Household head is female | | | -0.002 | 0.006 |
| Age of household head | | | 0.000 | 0.000 |
| Any member of the household has a disability | | | 0.004 | 0.005 |
| Household head employed at baseline | | | 0.002 | 0.004 |
| Head education: primary school | | | -0.002 | 0.008 |
| Head education: high school | | | -0.017 | 0.010* |
| Max years of education in household | | | 0.000 | 0.001 |
| Head education: any higher ed | | | -0.002 | 0.010 |
| Household rents from kebele | | | 0.019 | 0.009** |
| Household has a hard floor | | | 0.001 | 0.005 |
| Household has an improved toilet | | | 0.006 | 0.005 |
| Household size | | | 0.007 | 0.001*** |
| Household weekly food expenditure | | | 0.000 | 0.000 |
| P-value of F-test | 0.3073 | | 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<br>(1) | CM<br>(2) | TE<br>(3) | CM<br>(4) | TE<br>(5) | CM<br>(6) | TE<br>(7) |
| Age HH head | 5,917 | 56.445 | 0.404<br>(0.746) | 52.889 | 0.276<br>(0.913) | 65.490 | 0.585<br>(0.986) |
| Household size | 5,917 | 5.210 | -0.111<br>(0.143) | 5.393 | -0.134<br>(0.147) | 4.089 | 0.028<br>(0.189) |
| Female HH head | 5,917 | 0.605 | 0.022<br>(0.024) | 0.593 | 0.053*<br>(0.028) | 0.793 | 0.005<br>(0.039) |
| Disabled member | 5,917 | 0.171 | 0.003<br>(0.015) | 0.164 | 0.006<br>(0.020) | 0.264 | -0.003<br>(0.016) |
| HH head primary school | 5,917 | 0.095 | 0.004<br>(0.008) | 0.106 | 0.003<br>(0.014) | 0.045 | 0.013<br>(0.012) |
| HH head secondary school | 5,917 | 0.052 | -0.000<br>(0.006) | 0.050 | 0.002<br>(0.009) | 0.016 | -0.005<br>(0.010) |
| Maximum years school | 5,917 | 10.044 | -0.158<br>(0.181) | 9.840 | -0.164<br>(0.219) | 9.256 | 0.099<br>(0.239) |
| Rented from kebele | 5,917 | 0.748 | 0.018<br>(0.052) | 0.756 | 0.017<br>(0.052) | 0.824 | 0.008<br>(0.073) |
| Solid floor | 5,917 | 0.462 | -0.017<br>(0.040) | 0.413 | -0.022<br>(0.047) | 0.502 | 0.035<br>(0.044) |
| Improved toilet | 5,917 | 0.204 | 0.008<br>(0.030) | 0.216 | -0.026<br>(0.034) | 0.215 | 0.033<br>(0.034) |
| Number of rooms | 5,917 | 1.253 | -0.013<br>(0.059) | 1.153 | -0.055<br>(0.065) | 1.132 | 0.052<br>(0.077) |
| Owns a tv | 5,917 | 0.765 | 0.016<br>(0.022) | 0.746 | 0.023<br>(0.030) | 0.705 | 0.027<br>(0.024) |
| Owns at satellite | 5,917 | 0.541 | 0.000<br>(0.029) | 0.529 | 0.003<br>(0.037) | 0.455 | 0.006<br>(0.037) |
| Owns a sofa | 5,917 | 0.467 | 0.020<br>(0.029) | 0.416 | 0.024<br>(0.036) | 0.453 | 0.043<br>(0.035) |
| Weekly food expenditure | 5,917 | 349.011 | -8.768<br>(13.316) | 349.285 | -15.787<br>(15.103) | 283.680 | 2.544<br>(16.767) |

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

Note: The unit of observation is the individual, including children as well as adults. 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)<br>All adults (age 16-65) | (2)<br>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) | 2,960 | 0.393 | 0.488 |
| Cleanliness of streets (satisfied-yes/no) | 2,960 | 0.406 | 0.491 |
| Public toilets (quality 1-4) | 2,960 | 3.428 | 0.940 |
| Smell of trash (how often do you notice) (-) | 2,961 | 2.954 | 1.138 |
| Smell of drains (how often do you notice) (-) | 2,961 | 2.551 | 1.203 |

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 | 0.033 | $-0.004$ |
| | (0.058) | (0.003) |
| Control Mean | | 0.021 |
| Observations | 1,022 | 5,820 |

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) | 301.565 | 439.107 | −117.413 | −60.649 | 750.485 |
| | (102.919) | (12.107) | (98.029) | (87.181) | (162.484) |
| Control Mean | 2358.953 | 2 | 1961.3 | 3297.3 | 1842.7 |
| Observations | 5,917 | 5,917 | 5,917 | 5,917 | 5,917 |
| *Panel B: Treatment by eligibility* | | | | | |
| T×Eligible | 526.949 | 1,024.021 | −399.823 | 7.567 | 1,677.261 |
| | (109.007) | (8.291) | (111.557) | (99.796) | (164.173) |
| T×Ineligible | 205.948 | 3.243 | 207.557 | −52.807 | 86.601 |
| | (195.883) | (2.348) | (186.846) | (115.287) | (265.263) |
| CMn Eligible | 2145.006 | 3.954 | 1843.063 | 3174.119 | 1516.017 |
| CM Ineligible | 2836.154 | 0 | 2384.06 | 3722.46 | 2343.345 |
| Observations | 5,917 | 5,917 | 5,917 | 5,917 | 5,917 |

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 | −0.011 | −0.030 | 0.007 |
| | (0.007) | (0.006) | (0.005) | (0.006) |
| Control Mean | 0.444 | 0.213 | 0.112 | 0.192 |
| Observations | 19,442 | 19,442 | 19,442 | 19,442 |
| *Panel B: Eligible Households only* | | | | |
| Treatment at Origin | 0.109 | −0.045 | −0.066 | −0.001 |
| | (0.010) | (0.009) | (0.006) | (0.006) |
| Control Mean | 0.444 | 0.213 | 0.112 | 0.192 |
| Observations | 8,679 | 8,679 | 8,679 | 8,679 |
| *Panel C: Ineligible Households only* | | | | |
| Treatment at Origin | −0.015 | 0.010 | 0.002 | 0.001 |
| | (0.009) | (0.008) | (0.006) | (0.005) |
| Control Mean | 0.444 | 0.213 | 0.112 | 0.192 |
| 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 | 0.211 | 0.207 | 0.437 | 8.397 |
| 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 | 0.186 | 0.181 | 0.352 | 7.402 |
| 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 | 0.231 | 0.227 | 0.506 | 9.199 |
| 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 Emp. | Wage Work |
|---|---|---|---|---|
| | (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 Emp. | 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 | 0.135 | 0.226 | 0.231 | 0.058 |
| | (0.084) | (0.108) | (0.092) | (0.086) |
| RI p-values | 0.1165 | 0.045 | 0.015 | 0.4945 |
| P-value of Difference | | 0.507 | | 0.169 |
| Observations | 90 | 90 | 90 | 87 |

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: 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.

Table A16: Bootstrapped Confidence Intervals for the Welfare Calculations

| | 5th Percentile | 95th Percentile |
|---|---|---|
| **Partial Roll-out, Control** | | |
| Direct Effect | 0.0000 | 0.0000 |
| Direct + Wage Effects | 0.0000 | 0.0000 |
| Direct + Wage + Amenity | 0.0164 | 0.0753 |
| Cash Transfer | 0.0000 | 0.0000 |
| Public Works - Cash | 0.0164 | 0.0753 |
| **Partial Roll-out, Treatment** | | |
| Direct Effect | -0.0337 | 0.0602 |
| Direct + Wage Effects | -0.0109 | 0.0860 |
| Direct + Wage + Amenity | 0.0677 | 0.2648 |
| Cash Transfer | 0.0906 | 0.2051 |
| Public Works - Cash | -0.0670 | 0.0866 |
| **Full Roll-out** | | |
| Direct Effect | -0.0337 | 0.0624 |
| Direct + Wage Effects | -0.0108 | 0.0882 |
| Direct + Wage + Amenity | 0.1533 | 0.2524 |
| Cash Transfer | 0.0904 | 0.2071 |
| Public Works - Cash | 0.0406 | 0.0892 |

Note: This table presents the 5th and 95th percentiles of the welfare estimates based on 1000 bootstraps of the four key parameters: p (reduction in labor supply to the private sector), $\theta$ (elasticity of commuting with respect to wages), $b$ (effect of the program on amenities), and $\widehat{w_j}$ (effects of the program on private sector wages). For each parameter we draw from a normal distribution with mean equal to our estimate and a standard deviation equal to its standard error. “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. “Public Works - Cash” is the difference between the sum of the direct, the wage effect and the welfare gains from improved amenities and 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.

# B Mathematical Appendix

## B.1 Proof of Equation 2

The utility is a monotonic function of $\epsilon$ 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 $\Phi_i$ yields the expected utility for a worker living in $i$:

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

which completes the proof.

## B.2 Proof of Equation 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.3 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 $\pi_{ij}$ 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 $\Phi_{ij}$ and $\Phi_{ik}$ 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.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 $\lambda_{ij}$ 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 $\widehat{\pi_{ij}} = 1$:

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

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 $\omega$ 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 $\epsilon_g$ and $\epsilon_{ij}$ 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:

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

## 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

$$\widehat{U_i} = \frac{(1 + b_i T_i)\left[p_i T_i B_i w_g + (1 - p_i T_i)\left[\sum_j \widehat{w_j}^{\theta}(B_i \tau_{ij} w_j)^{\theta}\right]^{\frac{1}{\theta}}\right]}{\left[\sum_j (B_i \tau_{ij} w_j)^{\theta}\right]^{\frac{1}{\theta}}}$$

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

$$\widehat{U_i} = \frac{(1 + b_i T_i)\left[p_i T_i (1 + g_i)(B_i w_i) + (1 - p_i T_i)\left[\sum_j \widehat{w_j}^{\theta}(B_i \tau_{ij} w_j)^{\theta}\right]^{\frac{1}{\theta}}\right]}{\left[\sum_j (B_i \tau_{ij} w_j)^{\theta}\right]^{\frac{1}{\theta}}}$$

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

$$\widehat{U_i} = (1 + b_i T_i)\left[p_i T_i (1 + g_i)\pi_{ii}^{\frac{1}{\theta}} + (1 - p_i T_i)\left[\sum_j \pi_{ij}(\widehat{w_j})^{\theta}\right]^{\frac{1}{\theta}}\right]$$

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

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

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.[43]

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.

[43]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 (Ethiopian Statistical Service, 2019). 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. (2023). 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. (2023) 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.

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 $R$ | 9.2% | 13.7% | 25.7% | 64.6% |
| *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% | 25.8% | 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 $R$km of one another at the shortest point between woreda boundaries. Columns 1 to 4 gradually increase $R$ from 0.5 to 5 kilometers.

# 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

$$ExposureSquared_i = \sum_j \pi_{ij} Exposure_j = \sum_j \pi_{ij} \left[ \sum_k \lambda_{jk} T_k - \frac{1}{R} \sum_{0 \leq r \leq R} \sum_k \lambda_{jk} \tilde{T}_k^r \right]$$

where $p_i j$ is the baseline probability of commuting from $i$ to $j$ and $\lambda_{jk}$ 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:

$$Y_{\omega hi} = \alpha + \beta_1 T_i + \beta_2 ExposureSquared_i + \gamma Y^0_{\omega hi} + \delta \mathbf{X_{\omega hi}} + \varepsilon_{\omega hi}. \quad \text{(F1)}$$

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

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.

again, we find no evidence that labor supply adjusted as the extensive margin due to exposure to the program.

Table F2: ITT Effects with Exposure Squared (Extensive margin)

| | Employment rate | | |
|---|---|---|---|
| | Any | Public Employment | Private Employment |
| | (1) | (2) | (3) |
| Treatment | 0.035 | 0.112 | −0.051 |
| | (0.024) | (0.010) | (0.024) |
| Exposure Squared | 0.010 | −0.009 | 0.019 |
| | (0.055) | (0.024) | (0.056) |
| Control Mean | 0.415 | 0.415 | 0.415 |
| Observations | 19,442 | 19,442 | 19,442 |
| *Panel B: Eligible Households only* | | | |
| Treatment | 0.074 | 0.216 | −0.099 |
| | (0.030) | (0.014) | (0.030) |
| Exposure Squared | 0.069 | 0.055 | 0.046 |
| | (0.072) | (0.032) | (0.076) |
| 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 | −0.009 |
| | (0.028) | (0.001) | (0.030) |
| Exposure Squared | −0.008 | 0.001 | −0.014 |
| | (0.059) | (0.002) | (0.076) |
| 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 $\theta$

In this section, we use an alternative strategy inspired by Heblich et al. (2020), and estimate $\theta$ 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 $\nu_i$ are residence fixed effects which capture expected utility from $i$ and local amenities $B_i$, and $\mu_j$ 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.[44] The results are presented in Appendix Table G1. The two IV estimates are very close to each other and imply estimates of $\theta$ (4.05 and 4.37) 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 $\theta = 4.05$. 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 $\theta$ 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.

[44] 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 $\kappa$ and $\theta$ from the gravity equation, but calibrate $\theta$ 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 | −1.196 | −4.048 | | |
| | (0.034) | (0.068) | | |
| Log Commuting time | | | −1.750 | −4.367 |
| | | | (0.030) | (0.065) |
| Observations | 838 | 838 | 911 | 911 |

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

Table 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 | 0.161 |

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.)