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Household-level impacts of urban expansion planning: Evidence from Ethiopia's urban expansion initiative

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Household-Level Impacts of Urban Expansion Planning: Evidence from Ethiopia's Urban Expansion Initiative

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Abstract

Rapid urbanisation in Sub-Saharan Africa often outpaces infrastructure development, leading to the proliferation of unserviced settlements on urban peripheries. This study evaluates the household-level impacts of Ethiopia's Urban Expansion Initiative, a proactive planning approach that deploys arterial road grids to guide orderly city growth in peri-urban areas. Using a cross-sectional dataset of 4,000 households across eight cities, combined with retrospective information, the analysis employs Propensity Score Matching (PSM) and Difference-in-Differences (DiD) estimation strategies to isolate the causal effect of the intervention. Households in impact areas experienced a robust increase in annual income and reported improved subjective economic well-being compared to households in control areas. The intervention also successfully integrated peripheral neighbourhoods into the urban fabric, significantly reducing travel times to city centres, clinics, and markets while increasing access to piped water and sanitation. However, these physical improvements did not translate into structural employment shifts, as households did not experience a statistically significant increase in formal employment outcomes. Furthermore, while baseline homeownership is lower, relative gains in tenure accrue primarily to long-term residents rather than through the selective in-migration of advantaged households. Taken together, these results indicate that Ethiopia's Urban Expansion Initiative has generated meaningful economic and spatial benefits - most notably through higher household incomes, improved access to services, and enhanced urban connectivity. The fact that these gains emerged despite limited policy coordination and investment planning underscores the substantial unmet demand for connectivity and accessible urban land in rapidly urbanising cities. While proactive infrastructure investment can be a powerful driver of economic gains and spatial integration, achieving sustained and inclusive welfare improvements will require complementing such investments with policies that strengthen land tenure security and promote formal job creation.

Keywords: Urbanisation, Infrastructure Investment, Household Welfare, Impact Evaluation, Ethiopia

1 Introduction

The global movement of people to cities has long been associated with dramatic increases in incomes and living standards. The concentration of people, businesses, and economic activity in urban areas make cities engines of economic growth. Cities enable individuals to acquire new skills and knowledge through learning and peer effects. They also allow the cost of basic infrastructure and essential inputs to be shared across many users, reducing per-capita costs, and facilitate matching between workers and employers and between customers and service providers. The combined effect of these learning, sharing, and matching benefits explains why productivity is significantly higher in cities than in rural areas. In Sub-Saharan Africa, rapid urbanisation offers hundreds of millions of people access to these advantages if urban growth is managed effectively. However, the realization of this urbanisation dividend depends critically on how governments respond to the influx of new urban dwellers. As cities grow rapidly, a portion of the added population settles in the existing city, through densification, but the majority (77%) settle in peri-urban green field areas. This pattern is driven mainly by economic pressures including costs of housing, and municipalities often fail to anticipate and plan for peripheral growth. Consequently, 66% to 90% of the growth is informal or unplanned, and extending infrastructure and services into these new areas can be 3x – 7x more costly than installing infrastructure in planned urban areas.

Given these challenges, simplified and scalable approaches to urban infrastructure planning have the potential to significantly improve access to livelihoods and basic services. One such approach is the early deployment of arterial road grids to guide urban expansion. By securing land for roads and establishing skeleton road infrastructure in advance, municipalities can influence development patterns, reduce unplanned sprawl, guide growth away from environmentally sensitive and high-risk areas, and improve long-term connectivity. At the household level, such planned road grids are expected to lower transport and commuting costs, improve access to jobs, markets, and urban services, facilitate earlier provision of utilities and social services, and increase land security and property values - thereby shaping both economic opportunities and living conditions in newly urbanising areas.

In Ethiopia, between 2013 and 2017, the government, in collaboration with New York University, explored the implementation of arterial road grids in the peri-urban areas of 18 cities[1] as a proactive planning strategy called Urban Expansion Planning. Based on experiences from other regions, such investments are expected to affect household incomes, access to infrastructure, and the availability of essential services.

In light of this, this study evaluates the household-level impacts of urban expansion planning in Ethiopia, which applies an arterial road grid to guide orderly growth in peri-urban areas. The research addresses three main questions. First, it examines whether municipal governments in Ethiopia were successful in designing and implementing large-scale urban growth plans in anticipation of future expansion. Second, it uses descriptive analysis to assess whether households in impact areas had relatively higher socioeconomic status than households in control areas where the grid was not implemented. Finally, it applies econometric analysis to determine whether the installation of arterial roads can explain part of the observed differences in socioeconomic outcomes across treatment and control group households.

Our study contributes to the evidence base by analyzing a rigorous household-level evaluation of a large-scale, government-led urban expansion initiative in Sub-Saharan Africa. Unlike much of the existing literature,

[1] The cities covered by the urban expansion initiative are presented in Appendix Table A1.

which focuses primarily on service upgrades within established informal settlements, this analysis examines planned peri-urban expansion driven by arterial road infrastructure. By combining descriptive and econometric approaches, including mean difference tests, propensity score matching, and Difference-in-Difference estimations, the study assesses the extent to which planned infrastructure investment influences household welfare, employment, mobility, service access, and housing tenure. In addition, by explicitly comparing impact and control areas, the analysis isolates the effects of the urban expansion intervention from pre-existing differences. Overall, the study fills an important gap in the literature by providing empirical insights into how proactive urban expansion strategies shape socioeconomic outcomes in rapidly urbanising contexts.

2 Context

Ethiopia is a rapidly urbanising low-income country. The share of the population living in cities has increased from an estimated 7.1% in 1994 to 24.2% by 2025, and is estimated to rise to 39.1% by 2050. The increase in the urban share implies 44 million new urban residents in the next 25 years, more than doubling the current urban population (UN, 2014). The government of Ethiopia has also embraced urbanisation as a strategy for achieving national economic development goals, including the attainment of middle-income status.

New urban residents require land for homes and businesses, services, roads, and open spaces. Much of the new settlement occurs in peripheral areas, where land is more affordable and more easily obtained. Empirical research from the region indicates that, on average, a doubling of the population of an urban area results in a tripling of the urban area (Angel, 2023). Spatial planning of new urban areas, is, therefore, a critical element of accommodating urban population growth. Urban expansion planning was originally developed as an intervention to address the land supply bottlenecks that inhibit the production of housing in Sub-Saharan Africa. It later became clear that the proper implementation of an urban expansion plan should have considerable benefits in terms of infrastructure provision (by creating efficient routes for trunk infrastructure and providing rights of ways for pipes, wires, buses, and so on), and for economic growth. Economically, the productivity benefits of urban areas are tied to the size and connectivity of the metropolitan labour market in the city. Urban expansion planning addresses both points, integrating a large area and offering better connectivity than business-as-usual development patterns.

Despite the clear economic, social, and administrative imperatives to prepare land for future urban growth, research indicates that most cities in Sub-Saharan Africa are failing to do so. Although specific data is not available for Ethiopia, evidence gathered from 1990 to 2014 indicated that no more than 22% of new residential areas in cities were planned before settlement (Angel et al., 2016). This is at least partly due to insufficient planning instruments (Watson, 2002), which often underestimate growth or fail to offer implementable or scalable solutions (OECD, 2025).

In this context, New York University introduced Urban Expansion Planning to the Government of Ethiopia, the Ministry of Urban Development and Construction (now MUI), as a proactive strategy to prepare land for urban growth for the next three decades. The basic methodology introduced in Ethiopia consisted of a simple 4-point program:

1. Preparation of realistic 30-year growth maps,
2. Expansion of city boundaries,

3. Securing land for a 1km x 1km arterial road grid (30m wide), and
4. Protection of public open spaces.

The Ministry adopted the methodology and identified a set of four pilot cities, all regional capitals with populations of 100,000 or more and a growth rate of at least 3% per year. Those cities formed urban expansion teams under strong municipal leadership. The teams were trained by New York University technical advisors and supported by MUDC. Following a launching workshop in 2013, they proceeded to prepare urban expansion plans for their next 30 years of growth (Lamson-Hall and Martin, 2022).

Figure 1: Eighteen cities participating in the Ethiopia Urban Expansion Initiative (Phase I cities with triangles, Phase II cities with circles).

Source: Lamson-Hall et al. (2019)

City urban expansion plans were approved in 2014 and implementation on the ground started, with budget allocations from own-source funds and from the regions. Based on strong early successes, the program was extended by NYU in 2015 (with the support of the Cities Alliance) to 14 additional cities (Lamson-Hall et al., 2019). Those cities completed plans in that year. A map of participating cities is show in Figure 1.

A preliminary evaluation of this initiative in 2017 measured outputs of the program, specifically the kilometers of implemented arterial roads. It found implementation of 570 kilometers of road in the four pilot cities, but did not look at plan approval or implementation in the 14 additional cities (FDRE, 2017). Interest in urban expansion planning continued to grow and efforts were made by the Cities Alliance to extend the approach to additional cities in Ethiopia, as well as cities in Uganda and Somaliland. However, it was not until 2022 that a formal evaluation of the household-level impacts of the program was undertaken. The evaluation was supported by Open Philanthropy and conducted by New York University in collaboration with the Government of Ethiopia. Actual data collection was conducted by a neutral third-party firm in 2022 – 2023. Qualitative and quantitative approaches were used to assess outputs and outcomes in the 18 cities that received technical assistance from NYU from 2013 to 2017. Satellite imagery analysis was used to compare the approved urban expansion plans in the cities to actual construction on the ground, finding that 11 of the 18 cities succeeded in implementing their urban expansion plans to some degree. Eight cities implemented enough arterial roads to permit an evaluation of household-level impacts.

A geographic sampling frame was applied in those eight cities to identify households in impact areas (defined in the original study as households within 500 meters of an arterial road) and households in control areas (households falling at least 800 meters beyond an arterial road), and filtered to only include urban areas that were built after 2013, when the program began. Randomly generated coordinates were used to sample 500 households in each city, creating a sample of 4,000 households, evenly divided between impact and control. More details on the survey methodology can be found in the working paper prepared by Downs et al. (2024). Those households were visited by local survey teams employed by the neutral third-party firm and a survey was administered that collected data on basic socioeconomic characteristics of the households, and a number of outcome variables of interest including level of education, income and income growth over time, poverty likelihood, housing conditions, household assets, travel time to urban services, travel mode, livelihoods, economic well-being, crime and safety, food security, public health outcomes, and other health outcomes. The selection of topics corresponded to several hypotheses regarding likely benefits or impacts of urban expansion planning, including improved access to livelihood opportunities (leading to higher incomes, reduced poverty incidence, and better material conditions), improved connectivity (reducing travel time and improving vehicular access), and better access to infrastructure (facilitating basic services and public health outcomes). The cross-sectional data gathered in that survey is publicly available from New York University and was used as the key input in this paper, which analyses the data to assess the impact of the urban expansion planning interventions on key variables of interest.

3 Empirical Strategy

This study employs a quasi-experimental research design to assess the household-level impacts of the Ethiopia Urban Expansion Initiative. The analysis draws on pre-existing cross-sectional survey data collected in 2022 from 4,000 households in eight Ethiopian cities participating in the Urban Expansion Planning Initiative that implemented enough arterial roads. The primary identification challenge stems from potential self-selection bias: households may sort into areas near arterial roads based on characteristics that also affect their outcomes. Observable factors such as income, education, and social networks may influence both where households choose to settle and their subsequent well-being, while unobservable characteristics like preferences and entrepreneurial ability may similarly affect both location decisions and outcomes. Thus, simply comparing households near arterial roads to those farther away would confound the causal effect of infrastructure with pre-existing differences between groups in both observed and unobserved characteristics.

To address these identification challenges, we employ two complementary estimation strategies: Propensity Score Matching (PSM) and Difference-in-Differences (DiD). PSM constructs comparable treatment and control groups by matching households on observable characteristics that influence both residential location and outcomes, thereby mitigating selection bias from observed heterogeneity. On the other hand, DiD controls for both observed and unobserved time-invariant heterogeneity by comparing changes in outcomes over time between treatment and control groups, differencing away all fixed characteristics that might confound cross-sectional comparisons. Importantly, we estimate DiD on both the full sample and on the propensity-score-matched sample. This matched DiD approach combines the strengths of both methods. While matching ensures comparability on observables, differencing eliminates time-invariant factors, and thereby providing a doubly-robust estimation strategy.

The empirical strategy detailed below allows for the credible estimation of the initiative's causal impacts on household wellbeing, including potential differential impacts across socio-economic groups.

3.1 Propensity Score Matching (PSM)

In the potential outcomes framework of (Rubin, 1974; Imbens, 2004), our impact evaluation involves households ($i = 1, \ldots, N$), Treatment group households ($T = 1$), Control group households ($T = 0$), and potential outcomes $Y_i(1)$ and $Y_i(0)$ denote potential outcomes under treatment and control. The treatment effect for household i is:

$$\tau_i = Y_i(1) - Y_i(0) \tag{1}$$

However, the household-level treatment effect of arterial roads (τ_i) could not be identified since only one outcome is observed, leaving the counterfactual outcome unobserved.[2] Simply comparing treated households to control households yields biased estimates if the groups differ in characteristics affecting both their location choice and outcomes. Hence, the PSM method builds counterfactual outcomes for the treated and focuses on average treatment effects.

The Average Treatment effect on the Treated (ATT) is the average effect of arterial roads on impact group households, and is defined as:

$$ATT = E[Y(1)|T = 1] - E[Y(0)|T = 1] \tag{2}$$

The estimation of ATT involves a missing data problem as the counterfactual mean for impact group households ($E[Y(0)|T = 1]$) is not observed. Simply using the mean outcome of the control group households (E[Y (0)—T = 0]) might not be a good idea because heterogeneities, which determine households' choice of residence, could also determine the outcome variables of interest. Thus, the outcome variables of impact and control group households would differ even in the absence of arterial roads, leading to a self-selection bias. The ATT parameter is unbiased only in the absence of self-selection. Another parameter of interest is the Average Treatment Effect (ATE) at the population level, which is defined as:

$$ATE = E[Y(1)] - E[Y(0)] \tag{3}$$

The ATE represents the average effect if all households were exposed to the treatment - arterial roads. Since we use observational (quasi-experimental) data, ATT and ATE may not coincide because impact group households may, on average, differ systematically from the overall population of households.

[2] For the control group households, we cannot observe what would have happened to their outcomes if they had resided within 500 meters of the new arterial roads. For the impact group households, we cannot observe what would have happened to their outcomes if they had not resided within 500 meters of the new arterial roads.

PSM addresses the selection problem and compute ATT and ATE under two identifying assumptions (Rosenbaum and Rubin, 1983). The first condition is unconfoundedness or the conditional independence assumption (CIA), which imposes that participation in the impact group is independent of the potential outcomes after conditioning on the observable covariates. The implication is that selection is solely based on observable characteristics (x) and all variables that simultaneously influence participation in the impact group and households' outcomes must be observed and included in the analysis. The second condition is the common support or overlap condition ($0 < P(T = 1|x) < 1$), which rules out the possibility that households have a zero or one probability to participate in the impact group. This ensures comparable households exist in both groups.

Under these assumptions, the PSM identifies control group households that are very similar to treatment group households based on a single propensity score. As such, we compare impact group households with observably similar control group households and estimate the effect of arterial roads on outcome variables of interest as an average difference across the two groups.

Balancing strategy: Using the availability of retrospective baseline information on some of the variables, we adopt a covariate balancing strategy that prioritizes pre-treatment characteristics. In particular, treatment and comparison units are balanced on baseline (recalled) values of key outcome variables and other covariates that plausibly predate the intervention, as these variables are strong predictors of subsequent outcomes and help mitigate selection on observables. To assess robustness, we also implement alternative balancing specifications, including matching based on baseline outcomes only and specifications that additionally include time-invariant or clearly pre-treatment covariates. We estimate propensity scores with a logit model and use nearest-neighbor matching (one, three, and five nearest neighbors) with common-support restrictions. Outcome and covariate balance is assessed using standardized mean differences, t-tests, variance ratios, and percentage bias reduction, with results reported in the main text and the Appendix.

It should be noted that PSM is seriously challenged as a robust causal effect identification strategy due to the untestable CIA assumption and omitted variable bias as all critical confounders may not be observed. We address this concern by complementing PSM with Difference-in-Difference (DiD) estimation, as described in the next subsection, and conducting sensitivity analyses to assess robustness to potential unobserved confounding.

3.2 Difference-in-Difference (DiD)

For selected outcome variables, including housing conditions, assets, and other economic indicators, the survey includes retrospective questions asking households to recall conditions approximately 10 years prior. This pseudo-baseline data enables the use of DiD estimation technique, which provides an important complement to PSM by addressing different sources of bias. While PSM accounts for selection on observable characteristics, DiD eliminates time-invariant unobserved heterogeneity by comparing changes in outcomes over time. Importantly, DiD differences away all time-invariant factors, both observed and unobserved, that might confound cross-sectional comparisons. However, the method requires the parallel trends assumption and, in our context, is vulnerable to recall bias. The complementary nature of PSM and DiD allows us to triangulate findings: PSM avoids recall bias but is limited to observable confounders, while DiD addresses unobserved confounders but relies on retrospective data.

The DiD estimation is widely used to deal with the potential selection bias associated with nonrandom assignment when the evaluation entails the collection of data before (in retrospective in our context) and after the intervention for both treated and untreated groups. In our case, the DiD approach compares changes in outcomes for impact group households to changes for those outside the impact area (control group), relative to their recalled baseline values a decade earlier.

According to Athey and Imbens (2006), the standard regression-based DiD model estimates the Average Treatment Effect on the Treated (ATT) as:

$$Y_{it} = \alpha + \beta_1 Impact_i + \beta_2 Post_t + \delta(Impact_i \times Post_t) + \epsilon_{it} \tag{4}$$

Where:

- $Impact_i = 1$ if household is within 500m of arterial road
- $Post_t = 1$ after intervention
- δ = ATT (DiD estimator)

The DiD estimator cancels out bias from unobserved heterogeneity through a double differencing process. β_1 (Group Effect) captures the baseline differences between impact and control groups before the construction of the new arterial roads. β_2 (Time Effect) captures factors that would have caused changes in outcome variables of interest over time, even without the urban expansion intervention (such as broad economic, environmental, or policy changes). δ (the ATT) isolates the specific impact of the urban expansion initiative by "subtracting" the background trends from the impact group's progress. By adding household fixed effects to Equation 4, we can essentially "lock in" the unique characteristics of each specific household, which is vital for urban expansion studies. This ensures δ is estimated based only on the within-household change in the outcome variables (deviation from the household's own baseline) once the arterial road appears.

Estimation on Matched and Full samples: We estimate DiD on both the full sample and on the propensity score-matched sample. This matched DiD approach combines the strengths of both methods. While matching ensures comparability on observables, differencing eliminates time-invariant unobservables, providing a doubly-robust estimation strategy that addresses both observed and unobserved sources of selection bias.

Matching serves two important roles in strengthening the DiD approach: One, it improves the plausibility of the parallel trends assumption by ensuring treatment and control groups are similar on observable characteristics that predict outcome trends; and two, it enforces the common support by restricting the sample to comparable treatment and control group households. If matching successfully balances covariates and DiD estimates are similar on matched versus unmatched samples, this strengthens confidence in the parallel trends assumption. Conversely, if estimates differ substantially between matched and unmatched samples, this signals that selection on observables is important and that the unmatched treatment and control groups are not well-balanced. This finding underscores the value of matching in creating comparable groups and strengthens the case for relying on the matched DiD estimator, which addresses both observed differences (through matching) and unobserved time-invariant differences (through differencing).

4 Data and Descriptive Statistics

4.1 Data

The survey is specifically collected to evaluate the effects of the initiative and covers eight cities, namely Bahir Dar, Mekele, Adama, Dilla, Hawassa, Dessie, Hosanna, and Shashemene. The survey instrument captures a comprehensive list of household-level information across multiple dimensions of well-being, including economic and poverty status, health conditions, access to services and urban facilities, health outcomes, housing ownership and conditions, as well as crime and safety.

4.1.1 Variables of Interest

Outcome variables: Our analysis examines impacts across multiple dimensions of household wellbeing, grouped into five broad categories:

Economic Outcomes: household income, Poverty Probability Index, asset ownership, perceived household economic situation, and operating a business;

Housing condition and ownership: availability of toilet facilities, water source, lighting source, flooring type, and ownership status;

Access to services: travel modes and trip duration to employment centres, health facilities, market, and schools; and

Crime and Safety: perceived neighbourhood safety relative to ten years.

Treatment variable: Whether or not the household is in an arterial road impact area - within 500 meters of the new arterial roads.

Control variables: Characteristics of households such as age, gender, and highest school grade completed by the household head, family size, and years of residence (see Appendix Table A2 for summary statistics).

4.1.2 Treatment and Comparison Groups

The core intervention consists of the arterial road network constructed under the Urban Expansion Initiative. In each city, this involves a grid of roads spaced approximately 1 km apart, designed to open peripheral land for urban expansion and improve accessibility. This planned layout creates clearly defined impact zones, allowing us to adopt a comparative approach. We define treatment status based on households' proximity to these newly constructed arterial roads, comparing those located within the arterial road impact area (treatment group) to those outside it (control group). Specifically:

Treatment group (impact area): Households located within 500 meters of a newly constructed arterial road under the Urban Expansion Initiative (T = 1). This threshold is chosen to capture households most directly affected by improved accessibility while maintaining sufficient distance from the control group to minimize spillover effects.

Control group: Households residing at least 500 meters beyond the treatment threshold (i.e., more than 500 meters from new arterial roads) and at least 300 meters away from the impact area boundary (T = 0). The additional 300-meter buffer between treatment and control zones is meant to further reduce contamination from spillover effects.

This spatial definition exploits the planned nature of the road network as a source of variation in infrastructure access. The arterial roads were typically constructed on the immediate edge of existing cities or in lightly settled agricultural areas, with the grid designed based on projected urban expansion patterns rather than existing development characteristics (Lamson-Hall et al., 2019). While the planned placement reduces concerns about purely endogenous setting driven by pre-existing neighbourhood conditions, households may still self-select into areas near planned or constructed arterial roads, which we address through our matching and DiD strategies.

4.2 Descriptive Statistics and Mean-Difference Test Results

Appendix Table A2 presents descriptive statistics for the key variables used in the analysis, while Table 1 reports mean-difference comparisons between households in urban planning impact areas and control areas. The sample includes 4,000 households, evenly split between treatment and comparison groups, providing a strong basis for the matching and difference-in-differences (DiD) analysis. The descriptive statistics highlight substantial heterogeneity in household living conditions across the sample. Only 28 percent of households report living in formally developed urban areas, and fewer than half have access to all-weather vehicle roads. Slightly over half of households in expansion areas report being served by arterial roads, indicating partial exposure to planned urban infrastructure. Residential mobility is high: nearly half of households report having moved into their neighbourhood within the past five years, while about one-third have lived in the same neighbourhood for more than ten years or have always resided there. This combination of recent in-migration and long-term residence characterizes rapidly expanding urban environments.

Economic well-being indicators point to widespread vulnerability. Average poverty likelihood is close to 29 percent and remains largely unchanged compared to reported levels ten years ago. Mean household income has increased over the past decade, but the large standard deviation suggests uneven income gains across households. Food insecurity remains prevalent, with about 35 percent of households reporting difficulty obtaining sufficient food. Labor market indicators further reflect informality: although just over half of households report being employed, fewer than half have a regular place of work, and around one-quarter operate businesses from home.

Housing and service access indicators show mixed progress. Homeownership is relatively high and has increased from 62 percent ten years ago to 77 percent at present, consistent with self-built housing and gradual formalization in expansion areas. However, access to basic services remains incomplete: only 56 percent of households report access to piped water and 67 percent to improved sanitation, with little improvement over the past decade. Finally, we can see that access to transport infrastructure and services is mixed. Just under half of households have access to all-weather roads, while proximity to arterial roads suggests partial integration into urban transport networks. Spatial accessibility and liveability indicators highlight key challenges. Average travel times to the city centre and essential services range from 25 to 30 minutes, with particularly long travel times to secondary schools. Perceptions of safety are low, with only about one-third of households reporting feeling safe. Together, these patterns motivate an analysis of whether urban planning expansion improves access, service provision, and overall living conditions, beyond simply expanding the urban footprint.

Against this backdrop, the mean-difference results reported in Table 1 reveal systematic and statistically significant differences between households in impact and control areas across a range of socioeconomic, infrastructural, and service-access indicators. By examining differences in conditions both currently and retrospectively (10 years ago), the analysis provides insight into how households in the impact area differ from their counterparts and where notable advantages or disparities exist. The interpretations that follow synthesize these results into major thematic areas, highlighting meaningful patterns related to infrastructure development, economic well-being, housing tenure, service access, employment, and spatial connectivity.

Urban Development and Infrastructure Access: Across multiple indicators, households in the impact area show substantially better access to urban development and physical infrastructure than those in the control area. A significantly higher proportion of impact-area households reside in formally developed urban neighbourhoods (45 percent versus 12 percent in control areas), have access to all-weather vehicle roads, and live in expansion areas connected to arterial roads (62 percent versus 31 percent). Similarly, 66 percent of households in impact areas are located in expansion zones served by arterial roads, compared to 35 percent in control areas. These consistent and sizable positive mean differences (around +30 percentage points) indicate that the impact areas are better planned and more integrated into the urban fabric. This improved accessibility likely facilitates mobility, service delivery, and economic participation.

Poverty, Income, and Material Wellbeing: The mean-difference analysis results show meaningful and statistically significant differences in economic wellbeing. Poverty likelihood both currently (27.8 versus 29.7 percent) and ten years ago (27.2 versus 30.6 percent) is lower in the impact areas, suggesting either pre-existing advantages or improvements associated with development interventions. Consistent with this, the income midpoint is significantly higher in the impact area, both today and a decade earlier. Although the magnitude of income differences has widened over time, this pattern suggests sustained economic advantage or better livelihood opportunities in impact neighbourhoods. No significant difference is observed in reported difficulty obtaining sufficient food, implying that food insecurity affects, on average, similar proportion of households across both areas.

Access to Basic Services: Impact-area households demonstrate notably better access to critical services. Access to piped water, both currently and historically, is significantly higher in the impact area. The same trend is observed for improved toilets, indicating better sanitation infrastructure. These differences highlight more effective service provision and infrastructure investment in the impact locations relative to controls. The magnitude of historical differences also suggests that these disparities have roots going back several years, not only current improvements.

Housing Tenure and Residential Stability: Housing tenure indicators show that control-area households are more likely to own their homes, both currently and 10 years ago, although the gap has narrowed over time. Ten years ago, homeownership was nearly 9 percentage points lower in impact areas, compared to a difference of 4 percentage points today. This convergence may reflect the nature of newly developing or upgraded neighbourhoods, where land markets and tenure arrangements are still evolving. At the same time, residential mobility is higher in impact areas: a larger share of households report having moved into their current neighbourhood within the past five years, while long-term residence (more than ten years or always) is significantly more common in control areas. This combination of higher mobility and evolving tenure arrangements is consistent with ongoing urban expansion and redevelopment processes in impact locations.

Employment and Livelihood Activities: Employment rates are slightly but significantly higher in

Table 1: Mean difference test

	Impact Area		Control Area		
	Mean	Obs	Mean	Obs	Mean Diff.
Household in a formal urban development	0.45	2000	0.12	2000	0.33***
Access to all vehicle roads	0.62	2000	0.31	2000	0.31***
Household in expansion area served by arterial roads	0.66	2000	0.35	2000	0.31***
Access to all vehicle roads	0.62	2000	0.31	2000	0.31***
Poverty likelihood	27.84	1891	29.68	1864	-1.84***
Poverty likelihood_10 years ago	27.19	1744	30.62	1802	-3.43***
Income_mid point	5545.93	1633	4398.11	1590	1147.81***
Income_mid point_10 years ago	3709.97	1174	3242.81	1147	467.16***
Difficulty in obtaining sufficient food	0.34	1929	0.36	1890	-0.02
Tenure_Own house	0.75	1962	0.80	1951	-0.04***
Tenure_Own house_10 years ago	0.57	1750	0.66	1793	-0.09***
Access to piped water	0.61	1999	0.50	2000	0.11***
Access to piped water_10 years ago	0.65	1709	0.49	1783	0.16***
Access to improved toilet	0.74	2000	0.60	2000	0.14***
Employment	0.54	2000	0.49	2000	0.04***
Have regular place of work	0.44	1910	0.42	1908	0.02
Operate business from home	0.26	1891	0.22	1877	0.03**
Safety	0.36	1904	0.36	1913	0.01
Travel time to city centre	27.21	1953	30.80	1933	-3.60***
Travel time to grocery	26.66	815	26.23	775	0.42
Travel time to grocery	25.11	1826	26.92	1805	-1.82***
Travel time to clinic	23.12	1912	27.16	1936	-4.05***
Travel time to primary school	22.36	1929	22.89	1946	-0.53
Travel time to secondary school	29.88	1887	30.50	1908	-0.62
Over 10 years or always resident	0.24	1866	0.40	1870	-0.16***
Moved 6 to 10 years ago	0.20	1866	0.18	1870	0.02*
Moved 5 years ago or less	0.56	1866	0.42	1870	0.14***
HH Size ≤ 6	0.78	1994	0.80	1986	-0.02
HH Size > 6	0.22	1994	0.20	1986	0.02

the impact area, though the difference is modest. Similarly, home-based business operations are more common in these areas. However, no significant differences appear in having a regular workplace or general safety conditions. These findings suggest that while the impact area may offer somewhat better livelihood opportunities, they do not translate into major distinctions in employment formality or perceptions of personal safety.

Accessibility to Urban Services (Travel Times): Travel-time indicators consistently favour the impact area. Households in the impact neighbourhoods have significantly shorter travel times to the city centre, to clinics, and to grocery stores, indicating better spatial integration and proximity to essential services. While travel times to primary and secondary schools show no significant differences, the overall pattern suggests that impact-area residents enjoy better connectivity and more convenient access to urban amenities. This reduced travel burden may translate into economic and social advantages over time.

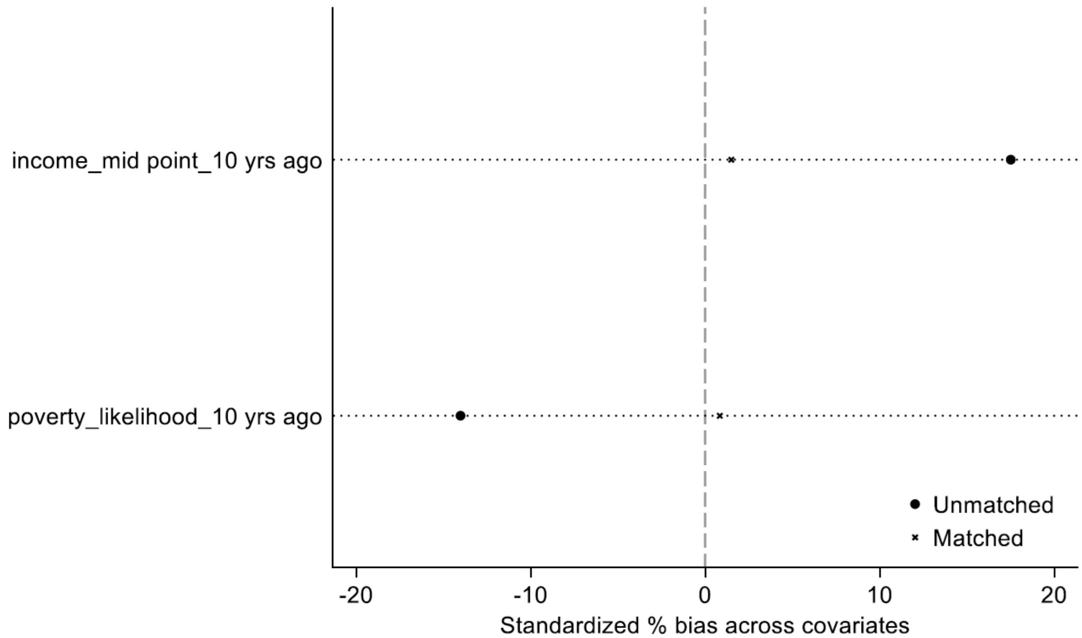
5 Results and Discussion

5.1 Propensity Score Matching (PSM) Results

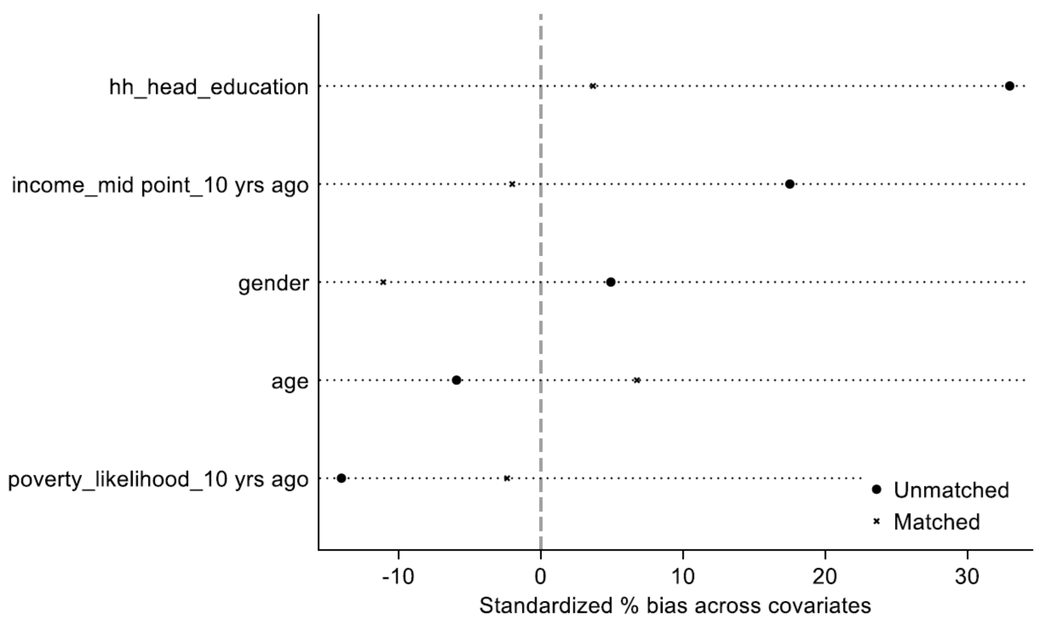
We begin by assessing covariate balance between treatment and control households following the matching procedure described above. Balance is evaluated using standardized mean differences and graphical diagnostics. Figure 2 compares balance in the full sample with that achieved after propensity score matching based primarily on baseline (retrospective) outcome variables plus select covariates. To assess robustness, we also report alternative nearest neighbor balancing strategies in the Appendix (Figure A1 and Figure A2), including matching on baseline outcomes only and on current covariates combined with baseline outcomes. Appendix Table A3-Table A5 report pre- and post-match means, standard deviations, standardized mean differences, variance ratios, and the percentage reduction in absolute bias for each covariate.

As shown in Figure 2, the PSM substantially improves balance between the treatment and control group households. While balance improvement varies across covariates, all variables fall within commonly accepted standardized mean difference thresholds after matching, except for minor worsening for gender and age. After matching, standardized differences for all the baseline outcomes fall well within commonly accepted thresholds, and the average standardized bias is reduced sharply relative to the unmatched sample. These indicate substantial improvements in comparability between households located within the arterial road impact areas and those outside them. Matching accounts for potential baseline imbalances or pre-existing advantages observed in the economic well-being of households in the impact areas. This is crucial for disentangling improvements in economic well-being that are attributable to the urban expansion initiative. In other words, the matching process effectively reduces baseline differences between the two groups and ensures that comparisons of outcomes are based on households with similar observable characteristics.

Figure 2: Standardized bias before and after matching (NN1)



a) Matching on baseline outcomes only



b) Matching on baseline outcomes and controls

We employ nearest-neighbor propensity score matching with one neighbor (NN1) as our primary specification, prioritising match quality over efficiency. Each treated household is matched to the single control household with the closest propensity score, subject to a caliper of 0.01 and common-support restrictions, thereby reducing the risk of poor-quality matches. This conservative approach maximises comparability between treatment and control groups at the cost of some efficiency. We assess robustness using alternative matching specifications, including NN3 and NN5, as well as kernel matching, which trade off some match quality for improved precision and sample retention.

Figure 3: Common support Before and After matching (NN1)

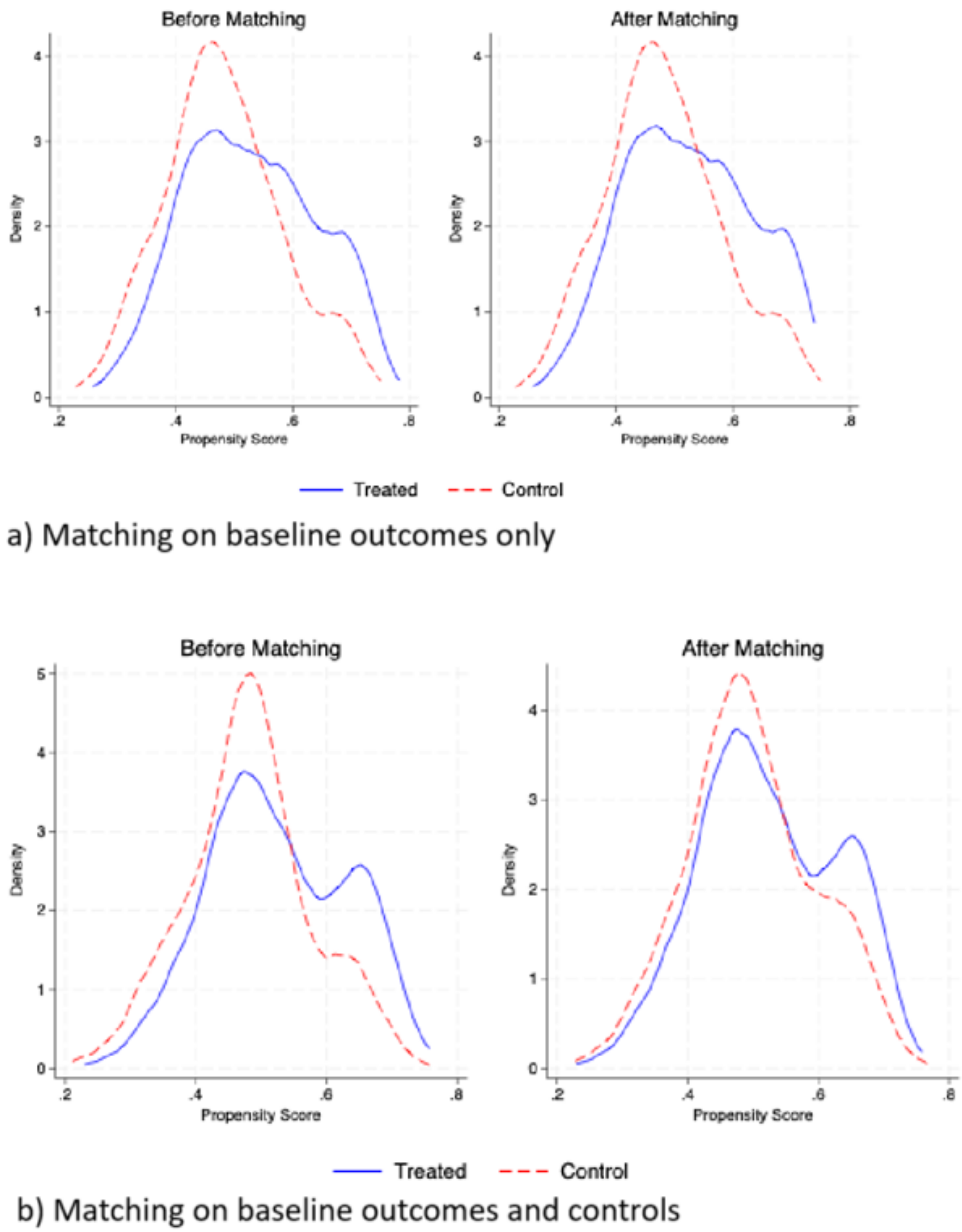


Figure 3 illustrates the common support region before and after matching, by comparing the distribution of propensity scores for both impact and control households. Accordingly, the figure shows a clear improvement in overlap between the two groups following the matching procedure, with treated and control group observations largely concentrated within a shared range of propensity scores. This indicates that the matched sample meets the common support assumption. Moreover, as shown in Appendix Table A3-Table A5, there is significant improvement in balance and bias reduction in the matched sample, particularly under the five nearest neighbor matching (NN5). The improved level of overlap, balance across outcomes and covariates and reduction in bias suggest that the matched control group households provide an appropriate counterfactual for the treated households, thereby increasing the credibility of subsequent impact estimates.

Table 2 presents the estimated impacts of the urban expansion initiative based on Propensity Score Matching (PSM) using one, three, and five nearest neighbors (nn1, nn3, nn5). The table compares outcomes for households in the impact areas with those of matched households in control areas who share similar observable characteristics. To assess robustness, we report results under alternative matching specifications, matching on baseline outcomes only and matching on baseline outcomes combined with covariates. These estimates aim to isolate the effect of the urban expansion intervention on economic well-being, employment, travel times, access to basic services, housing tenure, and perceptions of safety. The consistency of results across the different specifications strengthens confidence in the robustness of the estimated impacts.

Economic well-being: The results point to strong and consistent gains in income and perceived economic progress among households in impact areas, along with more modest effects on poverty reduction and no measurable changes in employment status. Poverty likelihood shows a small reduction across all matching specifications, although the estimates are statistically significant only under nn3 and nn5 specifications, and when matching on baseline outcomes alone. While the magnitude of the poverty effect is relatively small and sensitive to specification, the direction of the estimates consistently suggests lower poverty risk among impact-area households.

In contrast, household income shows large, positive, and highly statistically significant improvement across all matching approaches. On average, households in impact areas exhibit income levels that are in the range of 7–14 percent higher than those of matched control households. Moreover, impact-area households have about 5 percentage points higher probability of reporting an improved economic situation compared to ten years ago, with positive and statistically significant effects across all matching specifications. Together, these results provide strong evidence that the initiative is associated with improved economic well-being, even if it does not necessarily translate into broad-based poverty reduction in the short run. The employment outcome shows no statistically significant differences between the two groups, suggesting that while income increased, the dominant livelihoods or employment structures may not have shifted in a significant way due to the intervention.

Travel times: The urban expansion initiative appears to have substantially improved access to essential public and commercial services, supporting greater mobility and reduced time burdens for households. Across the majority of the specifications, households in impact areas experience statistically significant reductions in travel times to the city centre, clinics, and shopping places - by a minute or two for the city centre and groceries, and about three minutes for clinics. These consistent reductions suggest that the urban expansion initiative has improved proximity and connectivity to key urban services. In contrast, travel times to primary and secondary schools show no statistically significant differences, suggesting that educational facilities may already have comparable spatial distributions across impact and control areas, or may not be directly shaped by the intervention.

Access to basic services: There is strong and robust evidence of improved access to essential urban services for households in the impact areas. Access to piped water and improved toilets is significantly

Table 2: Estimated impacts based on PSM (Nearest-Neighbor)

	Baseline Outcomes (BOs)				BOs + Controls			
	(1) NN1	(2) NN3	(3) NN5	Obs.	(4) NN1	(5) NN3	(6) NN5	Obs.
Outcomes								
Economic Condition								
Poverty likelihood	-1.316	-1.560*	-1.640*	2062	0.328	-0.278	-0.0259	2014
	(0.892)	(0.863)	(0.848)		(1.080)	(0.952)	(0.918)	
Log Mid Income	0.138***	0.137***	0.140***	2019	0.0917***	0.0745**	0.0685**	1970
	(0.0308)	(0.0296)	(0.0293)		(0.0339)	(0.0306)	(0.0298)	
Relative Econ. Situation	0.0541**	0.0470**	0.0475**	2072	0.0553**	0.0523**	0.0464**	2022
	(0.0219)	(0.0214)	(0.0215)		(0.0261)	(0.0230)	(0.0221)	
Employment	0.0193	0.00243	0.00184	2138	-0.0162	-0.0119	-0.00861	2086
	(0.0226)	(0.0222)	(0.0222)		(0.0255)	(0.0230)	(0.0222)	
Travel Time								
To city centre	-1.901***	-1.893***	-1.853***	2089	-1.453*	-1.214*	-1.059	2037
	(0.646)	(0.632)	(0.620)		(0.760)	(0.682)	(0.662)	
To clinic	-2.853***	-2.972***	-3.034***	2087	-2.853***	-2.972***	-2.820***	2037
	(0.666)	(0.654)	(0.653)		(0.791)	(0.705)	(0.667)	
To primary school	0.424	0.441	0.469	2099	0.430	0.409	0.670	2049
	(0.608)	(0.589)	(0.583)		(0.693)	(0.645)	(0.635)	
To secondary school	-0.564	-0.344	-0.225	2071	-0.0208	-0.519	-0.741	2022
	(0.678)	(0.655)	(0.648)		(0.741)	(0.692)	(0.684)	
To grocery	-1.936***	-1.709***	-1.752***	1983	-1.295	-1.216*	-1.421**	1935
	(0.665)	(0.655)	(0.660)		(0.788)	(0.691)	(0.673)	
Basic Services								
Access to piped water	0.0838***	0.0838***	0.0742***	2138	0.0770***	0.0823***	0.0741***	2086
	(0.0224)	(0.0216)	(0.0215)		(0.0257)	(0.0227)	(0.0219)	
Access to improved toilet	0.137***	0.129***	0.131***	2138	0.121***	0.121***	0.115***	2086
	(0.0202)	(0.0200)	(0.0197)		(0.0234)	(0.0208)	(0.0202)	
Safety and Ownership								
Safety	0.0469**	0.0451**	0.0337	2091	0.00364	0.0270	0.0263	2040
	(0.0217)	(0.0214)	(0.0213)		(0.0252)	(0.0225)	(0.0217)	
Tenure_Own house	-0.0629***	-0.0566***	-0.0454**	2101	-0.0777***	-0.0573***	-0.0553***	2052
	(0.0196)	(0.0194)	(0.0195)		(0.0217)	(0.0200)	(0.0197)	

Robust standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

higher across all matching models, with meaningful impact sizes ranging from 7.4-8.4 percentage points for access to improved water and 11.5-13.7 percentage points for access to improved toilets, respectively. While earlier descriptive statistics indicate baseline differences exist in service access between impact and control areas, the persistence of these effects even after adjusting for baseline characteristics suggests that the initiative is associated with improved service provision and infrastructure development.

Safety perceptions and housing tenure: The results show improvements in safety perceptions but continued adjustment in housing tenure arrangements. Households in impact areas are less likely to own their homes than matched control households, with a negative and statistically significant association across all specifications. This pattern likely reflects higher residential mobility, greater reliance on rental housing market, and transitional land and housing markets in rapidly urbanising neighbourhoods. At the same time, the estimated impact on perceptions of improved safety from crime compared to ten years ago is positive and statistically significant for all matching specifications, except for the nn5 model when matching is based on baseline outcomes only. Overall, these results suggest that households in impact areas are more likely to feel safer today than a decade ago.

Overall, the findings are broadly consistent with evidence from other urban upgrading and expansion programs. For example, studies in Ethiopia and other African cities have shown that infrastructure improvements and proximity to urban services tend to raise household incomes and welfare (Abay et al., 2023). This is true even when employment effects remain limited and where jobs are located elsewhere or require different skills (Gobillon et al., 2007). Similarly, evaluations of urban development initiatives in Kenya and India have documented significant gains in access to water and sanitation services, though changes in housing tenure and employment structures often lag behind infrastructural improvements (Marx et al., 2013; Gulyani and Talukdar, 2010). To generate stronger employment and tenure outcomes, upgrading programs need complementary interventions. A recent study on China further suggests that off-farm employment gains from urban expansion are possible under the right conditions, underscoring the importance of economic structure and complementary policies in translating improvements in infrastructure into labour market outcomes (Sheng et al., 2022).

5.2 Difference-in-Difference Estimation Results

Table 3 and Table 4 report Difference-in-Difference (DiD) regression results estimating the causal impact of the urban expansion initiative. Table 3 presents specifications without household fixed effects, while Table 4 includes household fixed effects to control for time-invariant household-specific characteristics. The analysis focuses on five primary outcome variables - Poverty likelihood, Income, Economic situation, Tenure, and Safety -using both the full sample and a matched sample as a robustness check.

The coefficient of interest for determining the impact of the intervention is the DiD estimator, captured by the coefficient of the interaction term (Post x Impact area). This coefficient isolates the specific change that occurred in the impact (treatment) area relative to the change in the control area over the period of the urban expansion initiative. By netting out common time trends and time-invariant baseline differences between the two groups, the DiD estimator identifies the causal effect of the urban expansion initiative.

The DiD estimates show a robust, positive, and statistically significant income gain associated with the urban expansion initiative across all specifications. In the models without household fixed effects, the estimated income gains range from 10.5 percent in the matched sample to 12 percent in the full sample. These effects remain robust when household fixed effects are included, with estimated income gains of 11.3 percent in the matched sample and 14 percent in the full sample. This consistency across models of the income effects after controlling for observed and unobserved household heterogeneities suggests that the results are not driven by time-invariant household characteristics or sample selection. Coupled with our findings under the PSM, the DiD results suggest that the initiative successfully created new economic opportunities, likely through new market access, higher land or rental values, compensation/resettlement benefits, and other location-specific economic opportunities that directly translate into higher incomes for residents most exposed to the intervention.

In contrast, the DiD estimates for the likelihood of poverty are not statistically significant, although the estimated effects are consistently positive across all specifications. This pattern suggests that, although income gains are substantial, they may not be sufficiently large or evenly distributed to generate detectable reductions in poverty likelihood over the study period. It also highlights that poverty dynamics are less responsive in the short run than income measures.

Perceived economic well-being improves significantly among households in the impact area relative to control areas. The DiD estimates are positive and statistically significant across all specifications, and the coefficient becomes precisely estimated when household fixed effects are included in the matched sample. This result reinforces the objective increase in household income and suggests that the initiative not only increased household cash flow but also positively improved residents' confidence in their future economic trajectory compared to ten years ago.

The DiD estimates suggest a positive but generally statistically insignificant effect on homeownership across specifications. In the full sample without household fixed effects, the probability of owning a home increases by approximately four percentage points and is statistically significant. However, once household fixed effects are included, the positive tenure effect persists in magnitude but loses statistical significance in both the full and matched samples. Notably, these DiD results differ from the PSM estimates, which indicate lower levels of homeownership in impact areas relative to control areas. At face value, this appears contradictory, but the difference reflects the distinct dimensions captured by the two approaches. PSM highlights persistent level differences in homeownership, consistent with the structural features of expansion areas - such as higher residential mobility, greater reliance on rental housing, and transitional tenure arrangements. In contrast, the DiD estimates identify relative changes over time. Taken together, the results indicate that although households in impact areas remain less likely to own their homes in absolute terms, homeownership has increased more rapidly there than in control areas, with gains accruing to a subset of households rather than occurring uniformly across the population.

The DiD estimates for perceived safety are positive but statistically insignificant across all specifications. Although the PSM results suggested higher perceived safety in impact areas, the absence of a significant DiD effect indicates that these differences likely reflect pre-existing disparities or common time trends rather than changes causally induced by the urban expansion initiative. In other words, while impact areas may appear safer in levels, there is no clear evidence that safety perceptions improved differentially over time as a result of the intervention.

Taken together, the DiD results complement the PSM findings by clarifying which observed differences reflect causal impacts and which capture persistent structural characteristics. PSM highlights cross-sectional differences between impact and control areas after conditioning on observables, while the DiD framework -

especially with household fixed effects - isolates within-household changes over time, net of time-invariant observed and unobserved household characteristics and common trends. The convergence of evidence across methods provides strong and consistent support for income gains and improvements in perceived economic well-being attributable to the initiative. By contrast, the lack of robust DiD effects for poverty and safety, alongside the nuanced tenure dynamics, suggests that some dimensions of welfare and social outcomes adjust more slowly and may require complementary policies for income gains to translate into broader structural improvements.

Table 3: Difference-in-Difference results

	Using all observations				
	Poverty likelihood	Log Income	Econ. Situation	Tenure	Safety
Post=1	-0.821	0.262***	-0.592***	0.133***	-0.284***
	(0.731)	(0.0234)	(0.0253)	(0.0145)	(0.0259)
Impact area=1	-3.455***	0.0910***	-0.0447	-0.0854***	0.0305
	(0.785)	(0.0238)	(0.0287)	(0.0163)	(0.0277)
Post=1 × Impact area=1	1.552	0.120***	0.0937**	0.0416**	0.00601
	(1.025)	(0.0339)	(0.0364)	(0.0210)	(0.0372)
City Dummy	Yes	Yes	Yes	Yes	Yes
No. of Obs.	7301	5544	7588	7456	7634
	Using matched observations only				
	Poverty likelihood	Log Income	Econ. Situation	Tenure	Safety
Post=1	-3.071***	0.257***	-0.607***	0.155***	-0.254***
	(1.158)	(0.0322)	(0.0402)	(0.0224)	(0.0416)
Impact area=1	-4.620***	0.0843***	-0.0826*	-0.0776***	0.0174
	(1.187)	(0.0293)	(0.0450)	(0.0247)	(0.0437)
Post=1 × Impact area=1	2.317	0.105**	0.167***	0.00832	0.0345
	(1.604)	(0.0460)	(0.0576)	(0.0325)	(0.0589)
City Dummy	Yes	Yes	Yes	Yes	Yes
No. of Obs.	3085	3061	3048	3084	3092

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

5.3 Robustness Checks

We assess the robustness of our main findings through several alternative specifications, matching strategies, and checking plausible explanations to the results - the case of household mobility. These checks address concerns about sensitivity to methodological choices and alternative explanations for the results and help establish the credibility of our impact estimates.

5.3.1 Alternative Matching Algorithm: Kernel Matching

First, we examine whether our results are sensitive to the choice of matching algorithm. While nearest-neighbor matching (used in our main analysis) ensures high-quality matches by pairing each treated unit

Table 4: Difference-in-Difference results with household fixed effects

	Poverty likelihood	Log Income	Econ. Situation	Tenure	Safety
	Using all observations				
Post=1	-0.738	0.279***	-0.592***	0.136***	-0.284***
	(0.569)	(0.0209)	(0.0324)	(0.0131)	(0.0328)
Post=1 × Impact area=1	0.870	0.139***	0.0937**	0.0260	0.00601
	(0.824)	(0.0301)	(0.0465)	(0.0201)	(0.0473)
Household FE	Yes	Yes	Yes	Yes	Yes
No. of Obs.	7301	5544	7588	7456	7634
	Using matched observations only				
	Poverty likelihood	Log Income	Econ. Situation	Tenure	Safety
Post=1	-2.996***	0.249***	-0.607***	0.156***	-0.254***
	(0.883)	(0.0257)	(0.0518)	(0.0203)	(0.0529)
Post=1 × Impact area=1	2.020	0.113***	0.167**	0.0118	0.0345
	(1.240)	(0.0365)	(0.0744)	(0.0307)	(0.0753)
Household FE	Yes	Yes	Yes	Yes	Yes
No. of Obs.	3085	3061	3048	3084	3092

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

with its closest control(s), it can sacrifice precision by discarding potentially useful observations. Kernel matching offers an alternative approach that uses weighted averages of multiple control units, trading off some match quality for improved precision and sample retention.

Appendix Table A6 presents PSM results using kernel matching with an Epanechnikov kernel and bandwidth of 0.06. The results remain qualitatively similar to our nearest-neighbor estimates and, in several cases, strengthen our key findings. For economic outcomes, the kernel matching estimates show consistent impacts: slightly enhanced impacts on poverty likelihood and log mid-income compared to the nearest-neighbor matching estimates, while the estimated improvement in the relative economic situation remains quite similar. For travel time outcomes, kernel matching yields slightly larger impact estimates but with consistent patterns: travel time to city centres, clinics, and grocery stores all show significant reductions. Access to basic services (piped water and improved toilets) shows highly significant improvements that closely match the nearest-neighbor results. Notably, the kernel matching results confirm that outcomes like employment, travel time to schools, safety perceptions, and home ownership show no statistically significant treatment effects, consistent with our main findings.

The consistency of our results across matching algorithms strengthens confidence that our results reflect genuine treatment impacts rather than artifacts of the matching procedure. Besides, the precision of the estimates generally improves under kernel matching, as evidenced by smaller standard errors, reflecting the efficiency gains from using more of the available data.

5.3.2 Alternative Matching Variables: Current Covariates Combined with Baseline Outcomes

Third, we examine the sensitivity of the results to the choice of matching variables. Our main DiD specification reported in Table 4 uses observations matched on baseline outcomes only, which ensures that treatment and control groups were comparable before the intervention. However, one might be concerned that matching exclusively on pre-treatment characteristics could miss important dimensions of comparability that emerged during the intervention period.

To address this concern, Table A7 in the appendix presents DiD results using observations matched on current covariates combined with baseline outcomes. This more restrictive matching approach ensures that treated and control units are similar both before and after treatment along observed dimensions, potentially improving the plausibility of the parallel trends assumption conditional on covariates. The results demonstrate remarkable stability. Without household fixed effects (top panel of Table A7), the treatment effects remain qualitatively identical to those in Table 4, with impacts on log mid-income and economic situation slightly larger than what is reported in the main specification. When household fixed effects are included (bottom panel of Table A7), the estimates show even greater consistency: log mid-income increases by 0.15 (compared to 0.11 in Table 4) and economic situation improves by 0.14 (compared to 0.17), with point estimates differing by less than 3 percentage points.

An interesting pattern emerges regarding tenure ownership. In the main DiD specification with household fixed effects (Table 4), the tenure effect is small and statistically insignificant. However, when using the more restrictively matched sample in Table A7, tenure ownership becomes statistically significant in both specifications: 0.070 without household fixed effects and 0.072 with household fixed effects, consistent with the significant positive effect observed in the DiD specification without household fixed effects (Table 3). This suggests a 7 percentage point increase in the probability of owning (rather than renting) one's home as a result of the urban expansion initiative. The emergence of this effect when matching on both current covariates and baseline outcomes (rather than baseline outcomes alone) suggests that the additional matching requirements in Table A7 improve the comparability of treatment and control groups, thereby strengthening the evidence for a positive impact on home ownership.

The consistency of results across these two matching strategies, baseline outcomes only versus baseline plus current covariates, provides reassurance on two fronts. First, it suggests that our main results are not driven by imbalances in current covariates that might have emerged differentially between treatment and control groups. Second, it indicates that the parallel trends assumption underlying our DiD estimates is robust to different ways of constructing the comparison group.

5.3.3 Residential Mobility and Selective Sorting

Robustness checks further confirm that the estimated effects are not driven by compositional changes in impact areas. In particular, we assess whether a selective in-migration of better-off households to the impact areas could explain the observed gains by estimating the models separately by residence duration. To do so, we split the sample using alternative mobility thresholds: households that moved within the past ≤ 2 years, ≤ 5 years, and ≤ 10 years and compare them with longer-term residents in each specification.

As shown in Appendix Table A8, across these alternative cut-offs, the estimated treatment effects on income, perceived economic situation, and housing outcomes are not larger among recent movers. If anything, the magnitude of the effects is consistently stronger among households with longer residence duration, indicating that the results are not driven by the sorting of higher-income entrants into locations closer to the upgraded road network. This pattern is observed consistently across all mobility thresholds.

Taken together, these findings indicate that the observed gains reflect genuine place-based improvements associated with the urban expansion initiative. Rather than capturing short-run compositional shifts induced by higher rental potential near the roads, the results point to benefits that accrue progressively to established residents as neighbourhood conditions improve.

6 Conclusion and Policy Implications

This study evaluates the household-level impacts of Ethiopia's Urban Expansion Initiative using a quasi-experimental research design that combines Propensity Score Matching (PSM) and Difference-in-Differences (DiD) estimation techniques. Drawing on a unique survey of 4,000 households across eight cities, the analysis offers robust evidence on how arterial road-led urban expansion affects economic well-being, access to urban service, mobility, and housing outcomes at the urban periphery. The findings are reinforced through robustness checks, including alternative matching algorithms (kernel matching), different matching variable combinations, and tests for selective residential sorting. Taken together, the findings confirm that the initiative has been highly effective in fostering economic gains and spatial integration, but point to structural gaps in employment transformation.

Summary of Key Findings

The most pronounced impact of the initiative is the significant improvement in household income and spatial connectivity. Both PSM and DiD models consistently show that households located in arterial road impact areas experience substantial income gains compared to control group households, with estimated impacts ranging between 7 to 14 percent. These objective income gains are reinforced by subjective assessments – residents in impact areas are significantly more likely to report improvements in their economic situations compared to a decade ago, with estimated effects ranging from 5 percentage points under PSM to 17 percentage points under DiD specifications. Furthermore, the initiative has strengthened spatial integration by substantially reducing travel times to city centres, clinics, and commercial areas. Finally, the expansion was associated with better service delivery, as impact area households were significantly more likely to access piped water and improved toilet facilities. These improvements persist across all specifications and robustness checks, indicating genuine infrastructure and service delivery gains rather than artifacts of estimation approach. Despite these successes, the study highlights critical areas where the infrastructure intervention alone was insufficient. The observed income gains do not translate into a statistically significant reduction in the probability of poverty once time trends are accounted for under the DiD estimation. This suggests that while economic opportunities have expanded, the magnitude or distribution of income gains may be insufficient to lift households above poverty thresholds in a sustained way, or that poverty dynamics respond more slowly to income changes than other welfare measures.

Similarly, the study finds no evidence of structural shifts in employment - there are no statistically significant differences in the likelihood of having a regular workplace or formal employment between households in the impact and control areas. These results point to continued reliance on informal or spatially mobile livelihoods, even in better-connected neighbourhoods. The combination of rising incomes without formal employment gains points to economic benefits flowing through channels such as increased property values, rental income, improved market access for informal businesses, and better terms of trade, rather than through a fundamental transformation of labour market participation. Perhaps most notably, housing tenure reveals a more nuanced and dynamic adjustment process. Cross-sectional PSM estimates indicate lower levels of homeownership in impact areas, while DiD results show that homeownership has increased more rapidly over time in impact areas than in control areas. Specifically, while tenure effects are small and statistically insignificant in the main specification, they become positive and significant (approximately 7 percentage points) when using more restrictively matched samples or specifications without household fixed effects.

These seemingly contradictory findings can be reconciled by recognising what each method captures and understanding housing market dynamics along arterial corridors. PSM identifies persistent level differences in the probability of residing in owner-occupied versus rental housing, showing consistently higher rates of rental occupancy in impact areas than control areas. This pattern reflects the structural transformation of housing markets along arterial roads. Proximity to arterial roads significantly increases property and rental values, making these locations attractive for speculative investment where individuals purchase property not as primary residences but as investment assets generating rental income and benefiting from capital gains. Moreover, improved connectivity attracts mobile renters who value proximity to city centres and services but do not seek or afford permanent residence in these neighbourhoods.

The positive DiD coefficients, by contrast, capture relative changes over time rather than absolute levels. The finding that owner-occupancy increases somewhat faster in impact areas could reflect several non-mutually-exclusive dynamics. First, control areas may themselves be experiencing growing rental market development over time, which would narrow the rental gap even if impact areas maintain their rental predominance. Second, as impact areas mature and stabilize with improved accessibility and service provision, some households who previously rented out their properties may be encouraged to shift toward owner-occupancy as the neighbourhoods become more livable and convenient. The residential mobility robustness checks strongly support this interpretation by ruling out compositional changes from selective sorting and confirming that observed dynamics reflect in-place housing market adjustments. Together, these patterns indicate that arterial road expansion fundamentally reshapes local housing markets by creating strong rental incentives and speculative investment opportunities, leading to persistently higher rental occupancy in impact areas, while various countervailing forces produce modest convergence in owner-occupancy rates over time.

Concluding remarks

Overall, the evidence indicates that Ethiopia's Urban Expansion Initiative has delivered meaningful economic and spatial benefits, particularly through higher household incomes, improved service access, and better urban connectivity. The fact that these benefits accrued in the absence of coordinated policymaking or investment planning speaks to the deep demand for connectivity and accessible urban land in rapidly urbanising cities. At the same time, the results suggest that infrastructure investments alone may not be sufficient to generate immediate or universal changes in poverty status or employment structures over the study period. Importantly, the absence of statistically significant effects in some domains should not be interpreted as evidence of no impact, but rather as an indication that certain outcomes may evolve more gradually, depend on complementary policies or broader macroeconomic conditions. Based on these findings, urban expansion planning can be best understood as a necessary but not sufficient tool for maximizing long-term welfare gains from urbanisation. Although cities should not delay in taking action on the ground, coordinated interventions that link infrastructure development with labour market policies, land administration, and service planning will be needed for cities to translate the connectivity gains from urban expansion planning into more inclusive and sustained urban development.

Policy Implications

Drawing on these empirical findings, several policy implications emerge for future urban planning initiatives in Ethiopia and other developing country contexts. The finding that incomes rise without a corresponding shift in employment structure suggests that road access facilitates trade, services, informal economic activity, and casual labour but does not automatically generate formal jobs or productive economic activities characteristic of truly urban economies. This pattern reflects a broader challenge in many rapidly urbanising developing country contexts: newly developing urban centres often remain "urban" in name and density but not in economic structure. They continue to support primarily informal trade, services, and even agricultural activities rather than the manufacturing firms or modern service providers that drive structural transformation and sustained productivity growth. Urban planners should therefore pair physical infrastructure investments with zoning for commercial and industrial clusters and incentives for firm localisation along arterial routes. This would help ensure spatial proximity between workers and jobs and transition households from informal livelihood activities to regular, stable employment. Without such complementary policies, income gains may remain concentrated in property appreciation, rental income extraction, and informal trade rather than translating into structural economic transformation.

The housing tenure results reveal that urban expansion planning fundamentally reshapes local housing market structures by creating speculative investment opportunities and sustained rental demand. Impact areas function as rental hubs with persistently lower owner-occupancy rates, and this is an economically rational outcome of higher property values and improved connectivity. This market dynamic is neither inherently positive nor negative for household welfare. Lower ownership rates need not imply adverse outcomes if rental markets are functioning, affordable, and provide adequate tenure security for renters. However, allowing these market forces to operate unchecked risks concentrating benefits among property owners and investors while excluding lower-income residents from appreciation gains. The policy challenge is therefore to manage, rather than resist, housing market transitions in expansion areas through proactive land governance frameworks. Rather than assuming that expansion automatically delivers tenure security or that all households should own homes, policymakers should ensure that land administration systems, housing policies, and rental regulations evolve alongside infrastructure development.

Specifically, policymakers should: (1) strengthen land administration systems to provide clear, secure tenure rights for both owners and renters, recognising that well-regulated rental housing can offer appropriate and affordable housing security; (2) develop and enforce rental market regulations that balance landlord incentives with tenant protections, ensuring rental housing remains accessible to low- and middle-income households as neighbourhoods appreciate; and (3) facilitate diverse housing supply responses through appropriate zoning and building regulations that allow density increases and varied housing typologies, preventing artificial scarcity that concentrates gains among existing property owners. The goal should be enabling housing markets to adjust efficiently to infrastructure improvements while ensuring that these adjustments do not systematically exclude lower-income residents or concentrate benefits narrowly among property investors and speculators.

While the results indicate improved access to clinics, the analysis finds no statistically significant impact on travel times to schools. This may suggest that education facilities are already relatively evenly distributed across impact and control areas, or that school location decisions are less directly linked to road expansion than other services. Future expansion phases could nonetheless benefit from greater coordination between investments in road infrastructure and the planned placement of schools and health facilities. In contrast, the strong and consistent improvements in access to piped water and sanitation to impact areas demonstrate that arterial roads can effectively serve as corridors for utility provision. Institutionalizing the simultaneous rollout of utility lines and road expansion projects would help minimize retrofitting costs and maximize immediate public health benefits, which in turn ensures that the physical connectivity provided by arterial roads translates directly into improved human capital outcomes. Equally important, this integrated approach would avoid the substantial inefficiencies of sequential infrastructure deployment that characterize much urban development in Ethiopia, where roads are built and later excavated repeatedly to install water pipes, sewerage, and electricity in separate uncoordinated projects, multiplying costs and causing repeated disruptions to communities.

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Appendix

Appendix Table A1: Urban expansion planning implementation

High Level (arterial road grids with significant housing development)	**Bahir Dar*, Mekele***
Medium Level (multiple arterial roads, housing regularization, including in prior growth area)	Adama*, Dilla, Hawassa*
Low Level (limited extent of arterial roads)	Bishoftu, Dessie, Gondar, Hosanna, Shashemene, Wolayta Sodo
Limited or None	Adigrat, Arba Minch, Axum, Debre Birhan, Jimma, Nekemte, Shire

Notes: Eight cities included in the household survey in bold.
The four regional capitals provided with more intensive support in first phase (*)
Gondar, Nekemte, Adigrat, Axum and Shire were not surveyed due to security concerns.
Source: Downs et al. (2024). Evaluation of the Ethiopia urban expansion initiative. MUI, Ethiopia.

Appendix Table A2: Summary statistics

	Obs.	Mean	Min	Max	Std.
Impact area	4000	0.50	0.00	1.00	0.50
Household in a formal urban development	4000	0.28	0.00	1.00	0.45
Access to all vehicle roads	4000	0.47	0.00	1.00	0.50
Household in expansion area served by arterial roads	4000	0.51	0.00	1.00	0.50
Over 10 years or always resident	3736	0.32	0.00	1.00	0.47
Poverty likelihood	3755	28.75	3.78	94.99	21.09
Poverty likelihood_10 years ago	3546	28.93	3.78	94.99	24.56
Income_mid point	3223	4,979.68	1,500.00	15,000.00	3,746.75
Income_mid point_10 years ago	2321	3,479.10	1,500.00	15,000.00	2,680.09
Difficulty in obtaining sufficient food	3819	0.35	0.00	1.00	0.48
Tenure_Own house	3913	0.77	0.00	1.00	0.42
Tenure_Own house_10 years ago	3543	0.62	0.00	1.00	0.49
Access to piped water	3999	0.56	0.00	1.00	0.50
Access to piped water_10 years ago	3492	0.57	0.00	1.00	0.50
Access to improved toilet	4000	0.67	0.00	1.00	0.47
Employment	4000	0.52	0.00	1.00	0.50
Have regular place of work	3818	0.43	0.00	1.00	0.50
Operate business from home	3768	0.24	0.00	1.00	0.43
Safety	3817	0.36	0.00	1.00	0.48
Travel time to city centre	3886	29.00	3.00	60.00	14.09
Travel time to grocery	1590	26.45	1.00	60.00	13.28
Travel time to grocery	3631	26.01	1.00	90.00	14.08
Travel time to clinic	3848	25.15	1.00	65.00	14.66
Travel time to primary school	3875	22.62	1.00	60.00	13.01
Travel time to secondary school	3795	30.19	2.00	60.00	14.96
Over 10 years or always resident	3736	0.32	0.00	1.00	0.47
Moved 6 to 10 years ago	3736	0.19	0.00	1.00	0.39
Moved 5 years ago or less	3736	0.49	0.00	1.00	0.50
HH Size ≤ 6	3980	0.79	0.00	1.00	0.41
HH Size > 6	3980	0.21	0.00	1.00	0.41
Gender (Female=1)	4000	0.55	0.00	1.00	0.50
Age [18,25]	3959	0.19	0.00	1.00	0.39
Age [26,40]	3959	0.54	0.00	1.00	0.50
Age [41,60]	3959	0.21	0.00	1.00	0.41
Age > 60	3959	0.06	0.00	1.00	0.23

Appendix Table A3: Balance and bias reduction after matching (NN1)

Variable		Mean				t-test		
		Treated	Control	%bias	%reduct bias	t	p> t	V(T)/V(C)
pov_likelihood_10yrs ago	U	25.527	30.731	-21.8		-4.99	0	0.87*
	M	25.527	26.108	-2.4	88.8	-0.59	0.557	1.05
income_range_10yrs ago	U	3666.8	3291.6	14.1		3.21	0.001	1.41*
	M	3666.8	3720.5	-2	85.7	-0.43	0.67	0.99
gender	U	0.575	0.519	11.2		2.56	0.011	.
	M	0.575	0.63	-11.1	1.1	-2.59	0.01	.
age	U	2.119	2.22	-11.5		-2.63	0.009	0.69*
	M	2.119	2.056	7.1	38.3	1.74	0.081	0.89
hh_head_education	U	4.935	4.08	39.7		9.08	0	0.87*
	M	4.935	4.855	3.8	90.6	0.91	0.363	1.08
Sample	Ps R2	LR chi2	p¿chi2	Mean Bias	Median Bias	B	R	%Var
Unmatched	0.034	98.15	0	19.7	14.1	44.0*	0.95	100
Matched	0.004	10.47	0.063	5.3	3.8	14.1	1.06	0

Appendix Table A4: Balance and bias reduction after matching (NN3)

Variable		Mean				t-test		
		Treated	Control	%bias	%reduct bias	t	p¿t	V(T)/V(C)
pov_likelihood_10yrs ago	U	25.527	30.731	-21.8		-4.99	0	0.87*
	M	25.527	25.503	0.1	99.5	0.02	0.981	1.06
income_range_10yrs ago	U	3666.8	3291.6	14.1		3.21	0.001	1.41*
	M	3666.8	3652.4	0.5	96.2	0.12	0.906	1.14*
gender	U	0.575	0.519	11.2		2.56	0.011	.
	M	0.575	0.613	-7.6	32.4	-1.76	0.078	.
age	U	2.119	2.22	-11.5		-2.63	0.009	0.69*
	M	2.119	2.084	3.9	65.7	0.95	0.344	0.82*
hh_head_education	U	4.935	4.08	39.7		9.08	0	0.87*
	M	4.935	4.876	2.7	93.1	0.66	0.51	1.06
Sample	Ps R2	LR chi2	p¿chi2	Mean Bias	Median Bias	B	R	%Var
Unmatched	0.034	98.15	0	19.7	14.1	44.0*	0.95	100
Matched	0.001	4.15	0.529	3	2.7	8.9	1.06	50

Appendix Table A5: Balance and bias reduction after matching (NN5)

Variable		Mean				t-test		
		Treated	Control	%bias	%reduct bias	t	p¿t	V(T)/V(C)
pov_likelihood_10yrs ago	U	25.527	30.731	-21.8		-4.99	0	0.87*
	M	25.527	25.403	0.5	97.6	0.13	0.9	1.06
income_range_10yrs ago	U	3666.8	3291.6	14.1		3.21	0.001	1.41*
	M	3666.8	3656.8	0.4	97.3	0.08	0.934	1.17*
gender	U	0.575	0.519	11.2		2.56	0.011	.
	M	0.575	0.612	-7.4	33.8	-1.72	0.085	.
age	U	2.119	2.22	-11.5		-2.63	0.009	0.69*
	M	2.119	2.095	2.6	77	0.64	0.525	0.82*
hh_head_education	U	4.935	4.08	39.7		9.08	0	0.87*
	M	4.935	4.875	2.8	92.9	0.68	0.497	1.05
Sample	Ps R2	LR chi2	p¿chi2	Mean Bias	Median Bias	B	R	%Var
Unmatched	0.034	98.15	0	19.7	14.1	44.0*	0.95	100
Matched	0.001	3.69	0.595	2.8	2.6	8.4	1.08	50

Appendix Table A6: PSM results using Kernel matching

	Baseline Outcomes		Plus Controls	
	(1)	(2)		
	Bandwidth=0.06	Obs.	Bandwidth=0.06	Obs.
Economic Outcomes				
Poverty likelihood	-3.310***	2062	-3.174***	2014
	(0.924)		(0.937)	
Log Mid Income	0.213***	2019	0.213***	2019
	(0.0322)		(0.0325)	
Relative Econ. Situation	0.0446**	2072	0.0453**	2022
	(0.0203)		(0.0205)	
Employment	0.0184	2138	0.0219	2086
	(0.0215)		(0.0217)	
Travel time				
Travel time to city centre	-2.234***	2089	-2.130***	2037
	(0.614)		(0.623)	
Travel time to clinic	-2.962***	2087	-3.051***	2037
	(0.641)		(0.650)	
Travel time to primary school	0.518	2099	0.410	2049
	(0.562)		(0.570)	
Travel time to secondary school	-0.374	2071	-0.376	2022
	(0.642)		(0.649)	
Travel time to Grocery	1983	-1.782***	1935	
	(0.630)		(0.640)	
Basic Services				
Access to piped water	0.120***	2138	0.118***	2086
	(0.0211)		(0.0214)	
Access to improved toilet	0.167***	2138	0.166***	2086
	(0.0195)		(0.0197)	
Safety and Ownership				
Safety	0.0179	2091	0.0174	2040
	(0.0210)		(0.0213)	
Tenure_Own house	-0.0507***	2101	-0.0471**	2052
	(0.0184)		(0.0186)	

Standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table A7: Diff-in-Diff results with household fixed effects and using observations that are matched on current covariates combined with baseline outcomes

	Using matched observations only - Without HH FE				
	Poverty likelihood	Log Mid Income	Economic Situation	Tenure_Own house	Safety Situation
Post=1	-0.700	0.284***	-0.637***	0.156***	-0.320***
	(1.030)	(0.0289)	(0.0358)	(0.0201)	(0.0370)
Post=1 × Impact area=1	1.930	0.150***	0.137***	0.0699**	0.0619
	(1.434)	(0.0425)	(0.0512)	(0.0292)	(0.0530)
Household FE	No	No	No	No	No
No. of Obs.	3914	3869	3862	3909	3892
	Using matched observations only - With HH FE				
Post=1	-0.572	0.283***	-0.637***	0.156***	-0.320***
	(0.769)	(0.0225)	(0.0458)	(0.0178)	(0.0467)
Post=1 × Impact area=1	1.859*	0.153***	0.137**	0.0715***	0.0619
	(1.061)	(0.0327)	(0.0656)	(0.0271)	(0.0672)
Household FE	Yes	Yes	Yes	Yes	Yes
No. of Obs.	3914	3869	3862	3909	3892

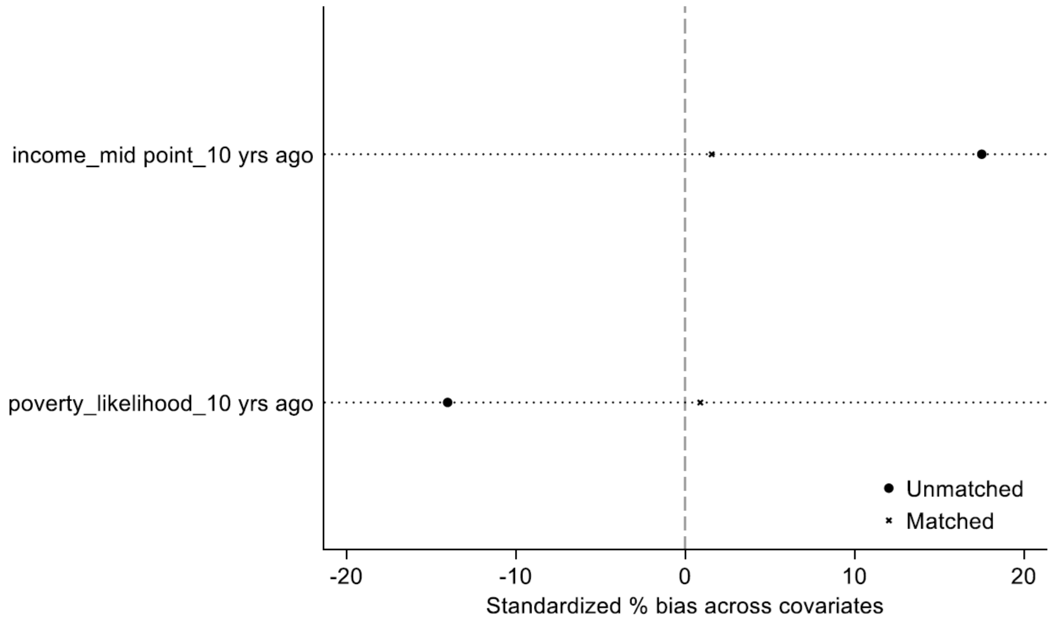
Standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table A8: Comparison between those who moved into the neighbourhood 2 and 5 years ago or less vs. those who lived in the neighbourhood for more than 2 and 5 years

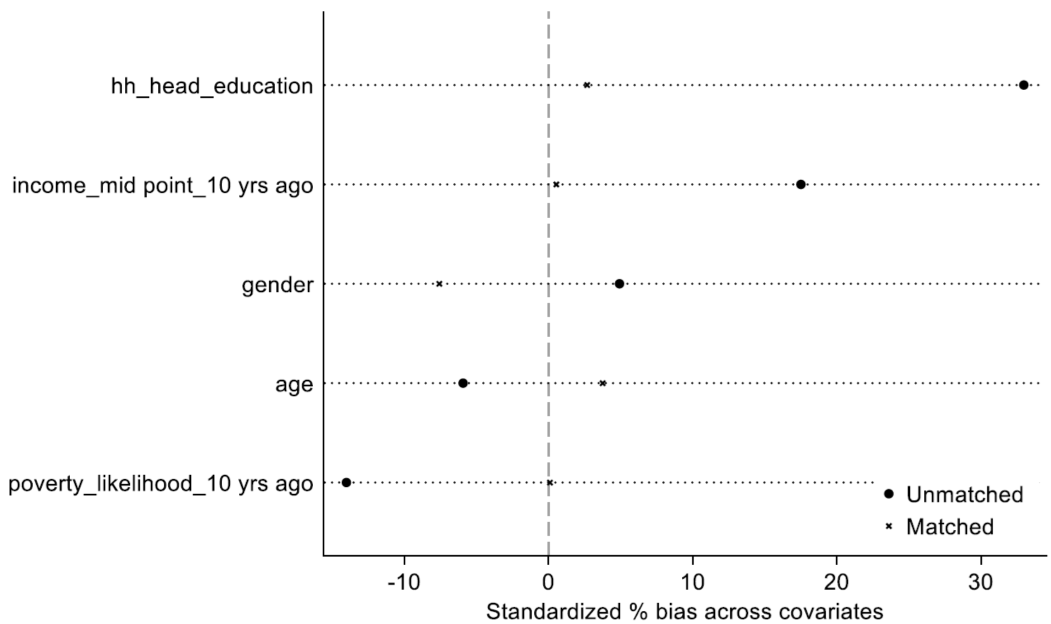
	Using matched observations only - With HH FE (More than 2 years ago)				
	Poverty likelihood	Log Mid Income	Economic Situation	Tenure_Own house	Safety Situation
Post=1	-0.378	0.286***	-0.673***	0.158***	-0.374***
	(0.895)	(0.0251)	(0.0516)	(0.0189)	(0.0524)
Post=1 × Impact area=1	1.401	0.154***	0.222***	0.103***	0.160**
	(1.276)	(0.0377)	(0.0774)	(0.0301)	(0.0785)
No. of Obs.	2861	2828	2816	2861	2848
Overall R2	.00064	.076	.11	.043	.033
	Using matched observations only - With HH FE (Less than 2 years ago)				
Post=1	-0.921	0.236***	-0.613***	0.139***	-0.193*
	(1.696)	(0.0568)	(0.105)	(0.0514)	(0.112)
Post=1 × Impact area=1	3.298	0.199***	0.00764	0.0134	-0.154
	(2.138)	(0.0746)	(0.135)	(0.0665)	(0.145)
No. of Obs.	901	890	898	908	896
Overall R2	.0013	.079	.13	.022	.027
	Using matched observations only - Without HH FE (More than 5yrs)				
Post=1	-1.014	0.283***	-0.756***	0.136***	-0.477***
	(1.032)	(0.0306)	(0.0618)	(0.0195)	(0.0629)
Post=1 × Impact area=1	0.367	0.140***	0.320***	0.0991***	0.330***
	(1.646)	(0.0485)	(0.0982)	(0.0345)	(0.0984)
No. of Obs.	1814	1784	1780	1807	1806
	Using matched observations only - With HH FE (Less than 5 years ago)				
Post=1	0.232	0.268***	-0.540***	0.178***	-0.161**
	(1.240)	(0.0348)	(0.0696)	(0.0328)	(0.0716)
Post=1 × Impact area=1	2.558*	0.180***	0.00319	0.0465	-0.162*
	(1.534)	(0.0473)	(0.0927)	(0.0438)	(0.0964)
No. of Obs.	1948	1934	1934	1962	1938

Standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix Figure A1: Standardized bias before and after matching (NN3)

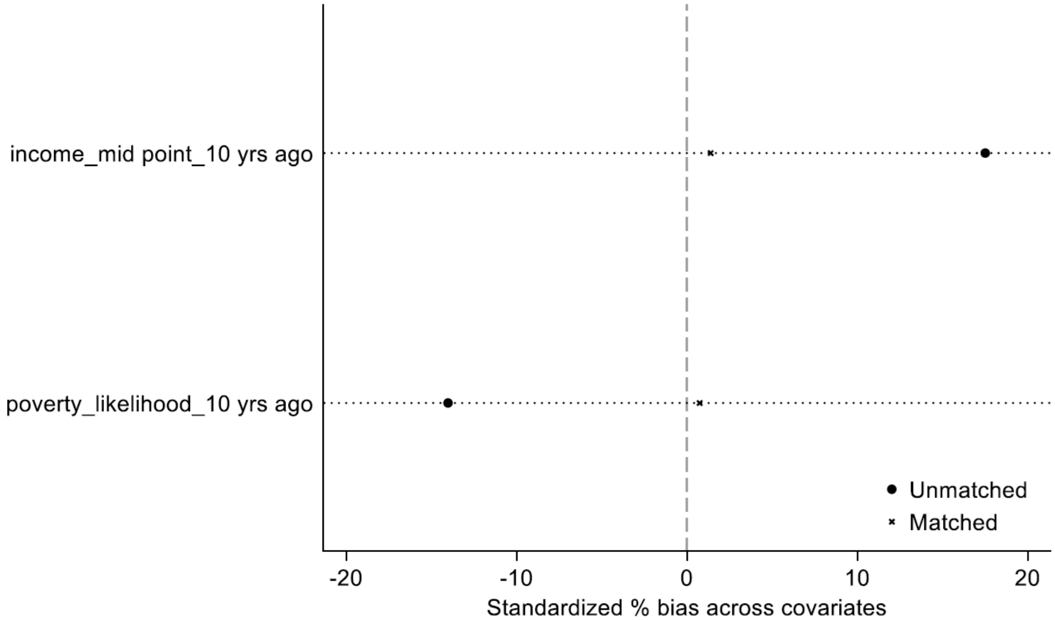


a) Matching on baseline outcomes only

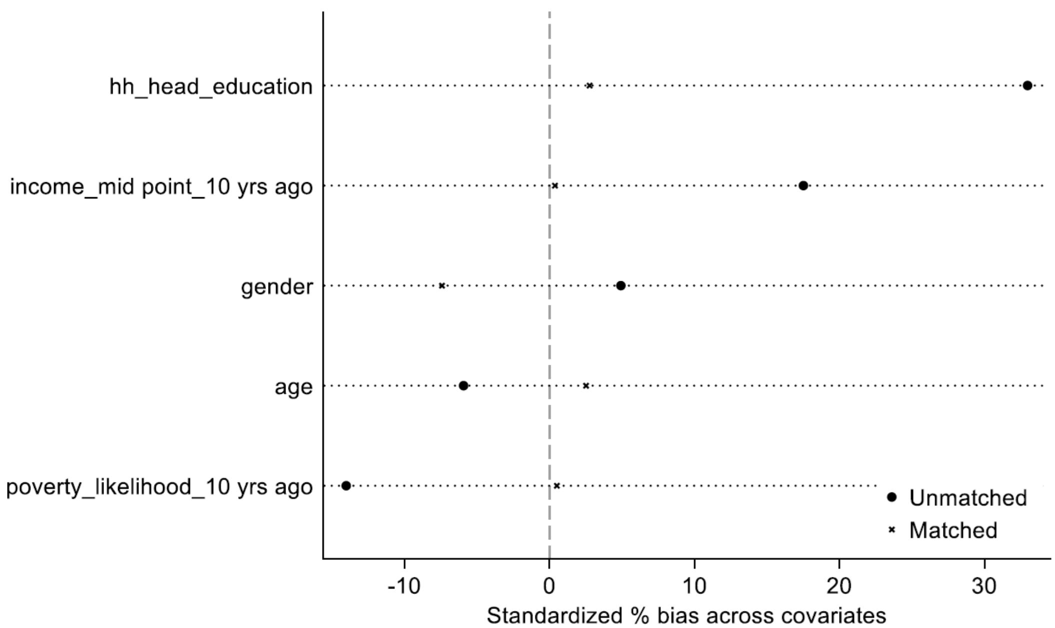


b) Matching on baseline outcomes and controls

Appendix Figure A2: Standardized bias before and after matching (NN5)



a) Matching on baseline outcomes only



b) Matching on baseline outcomes and controls theigc.org