Inclusive Infrastructure: People versus Place *

Clare Balboni †
London School of Economics

Gharad Bryan ‡
London School of Economics

Melanie Morten §
Stanford University and NBER

Caylee O'Connor ¶
Stanford University

Bilal Siddiqi ||
CEGA

July 18, 2025

Preliminary version: please check with authors for most recent draft before circulating

Abstract

Roads, rail, and other public transport in a city are inherently "place-based," in that they are built in specific neighborhoods. However, in the presence of resorting, an improvement in outcomes at the place level does not necessarily mean improvements in outcomes for the individuals who live there. We present a framework to clarify how changes in outcomes observed at the level of a *place* relate to changes in outcomes for the *people* who live there, differentiating between initial residents (those who lived there before construction) and final residents (those who live there once it is built). Using panel data tracked on locations and individuals, we provide a decomposition to separate the causal effects of transportation from the compositional effects of population resorting. We illustrate the method using data from the Bus Rapid Transit (BRT) system expansion in Dar es Salaam, Tanzania, where we find that such compositional effects can meaningfully alter assessments of the economic impacts of the new infrastructure.

Keywords: Transport, Urbanization, Gentrification, Tanzania, House Prices
JEL Classification:

*The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not necessarily represent the views of the World Bank and its affiliated organizations, or those of the Executive Directors of the World Bank or the governments they represent. We thank Jessica Mahoney, Juliana Aguilar, Justus Mugango, Rachel Steinacher, and Rachel Jones at IPA Tanzania for coordinating our data collection efforts in Tanzania. We also thank EDI for assistance with initial fieldwork. We thank Yonas Mchomvu and Sveta Milusheva at the World Bank and Ronald Lwakatare at the Dar es Salaam Rapid Transit Agency for institutional support. We also thank Nancy Lozano Garcia for sharing the World Bank LSMS data for Dar es Salaam. The research has been funded with UK aid from the UK government through the DIME ieConnect for Impact program at the World Bank. Additional funding is gratefully acknowledged from IGC, 3ie, and Stanford. Devika Lakhote, Helen Gu, and Betsenat Gebrewold provided excellent research assistance. Any errors are our own.

†Email: c.a.balboni@lse.ac.uk
‡Email: g.t.bryan@lse.ac.uk
§Email: memorten@stanford.edu
¶Email: cayleeoc@stanford.edu
||Email: bilal.siddiqi@berkeley.edu

1 Introduction

The world's cities are engines of growth and often a refuge for the poor (Glaeser, 2012). They reflect a balance between two externalities: agglomeration economies, which raise productivity and amenity and draw people into cities, and congestion externalities, which reduce health, safety, and effective density, pushing people out. A central goal of urban governance is to provide local public goods—such as parks, transportation, and police stations—that mitigate congestion and enable more people to benefit from agglomeration economies.

However, these goods are built in places, not allocated to people. As a result, who benefits from them, and how they are used, depends on an endogenous selection process. For example, public transport that a government hopes will increase access to jobs may attract those who already work but value commute convenience, such that the policy does not induce its desired outcome. Alternatively, a transit line designed to help poor local residents may lead to rent increases that price out incumbent low-income households (e.g., Balboni et al., 2021), meaning the public good ultimately accrues to wealthier newcomers and the policy misses its target. Understanding this selection process is essential if we aim to build inclusive cities and achieve desired policy outcomes from infrastructure investments.

While a growing literature studies the causal impacts of infrastructure, we know little about how endogenous selection shapes who ultimately benefits. Existing studies either evaluate average impacts on *places* (e.g., Gibbons and Machin, 2005; Donaldson and Hornbeck, 2016), or infer distributional effects via structural models with strong parametric assumptions (e.g., Tsivanidis, 2024). Two key barriers have limited more direct evidence on individual-level impacts on *people*. First, available urban data—especially in developing countries—are often repeated cross-sections, making it difficult to track individuals over time and limiting identification to place-based treatment effects. Second, we lack a clear empirical framework that relates such place-based effects to causal effects for well-defined groups of people.

This paper addresses both challenges in the context of the first phase of the Dar es

Salaam Bus Rapid Transit (BRT) system, the first of its kind in East Africa, opened in 2016. This infrastructure aims to provide rapid public transport via dedicated busways in one of Africa's fastest-growing cities. The Dar es Salaam BRT is World Bank funded, and it is hoped that programs like this will contribute to efficient, green, and inclusive mobility across African cities (World Bank, 2022). A specific goal of the project is to improve access to productive work opportunities, leading to increased employment and earnings. We concentrate on evaluating the success of the project in delivering on these labor market goals. Dar es Salaam is, however, a high mobility city—over three years, 40% of individuals in our sample (and more than 50% of renters) move house—making it unclear who will benefit from the completed project.

We collect bespoke data designed to estimate both causal and selection effects, with four key features. First, we surveyed 1,750 households living throughout the city of Dar es Salaam before the BRT began operating—one of the largest household samples of its kind in a developing city. Second, we tracked this baseline sample and conducted a follow-up survey three years later. This allows us to estimate how original residents near the BRT route responded. Third, when baseline households moved away, we surveyed the new in-movers in their original structures, collecting retrospective information on their pre-BRT outcomes. This allows us to observe the full dynamics of population turnover and estimate impacts on those who end up living near the BRT. Finally, we link the panel to detailed pre- and post-BRT travel time data, providing granular measures of actual exposure to infrastructure improvements in order to estimate causal impacts.

We develop a difference-in-differences framework that allows us to define and estimate relevant causal and selection parameters. We consider two defined and policy-relevant groups of people: initial residents (composed of households who stay and those who exit a location between baseline and endline) are those most likely to have been the policy maker's desired beneficiaries, while final residents (households who stay and those who arrive in a location between baseline and endline) are those who can ultimately access the BRT.[1] In addition to estimating treatment effects on initial and final residents

[1]Stronger assumptions would be required to estimate impacts on other subgroups, such as stayers, since the exiters and arrivers in 'treatment' and 'control' locations will be different if the BRT induces selection, and hence who is a stayer is endogenous to treatment.

we also estimate treatment effects on place. If arrivers differ to exiters, then the place-based effects will include a selection term. We define positive selection *on characteristics* as the case where arrivers into a treated location have better baseline characteristics than exiters—for example if they are more educated or are more likely to have been working at baseline. This selection term determines who is the final recipient of the infrastructure, whether the infrastructure reaches its policy goals, as well as the relationship between place-based estimates and treatment effects for well defined groups of people.

Comparing the relative causal effect of the BRT on (i) places; (ii) *initial residents*, or those who were living in locations affected by the BRT prior to construction; and (iii) *final residents* who are living in BRT-affected locations after construction yields insights into the economic interpretation of resorting. When there is no selection on characteristics, place-based estimates will be the same as final-resident estimates. However, it is possible that there is a positive place-based estimate, but no impact on final residents. This would occur, for example, if those with ex-ante high labor market engagement select into affected areas served by the BRT to benefit from convenience benefits unrelated to employment access. Considering the treatment effects on initial residents yields information about whether those who are displaced or move away from the BRT still benefit from the infrastructure. In extreme cases of gentrification, there are concerns that initial residents could end up either not benefiting as much as final residents or potentially even being harmed by place-based investment. Empirically, comparing treatment outcomes for initial residents to those of final residents measures whether those who arrive have larger gains that those who left.

Applying these methods to estimate the impacts of the Dar es Salaam BRT system illustrates how accounting for selection effects can change estimates of the causal effects of place-based investments. We show that, conditional on appropriate parallel trends assumptions, it is possible to identify the relative causal effect of the BRT on (i) places; (ii) *initial residents*, or those who were living in locations affected by the BRT prior to construction; and (iii) *final residents* who are living in BRT-affected locations after construction. In order to account for spatial endogeneity in the placement of the BRT line in our estimation, and support the parallel trends assumption underlying our approach, we demean

exposure to Phase 1 of the BRT by expected exposure to the full set of planned BRT lines (following Borusyak and Hull (2023)), and condition on a large number of potential conditioning variables following Belloni et al. (2014).

Location-based estimates reveal that there are strong positive treatment effects of the BRT on place. Areas with greater access to the BRT see increases in both labor force participation (for instance, those who are "highly treated", which we define at the eightieth percentile of the demeaned treatment variable, see on average a 4% increase in labor force participation, relative to a mean endline value of 68%) and household income (corresponding 5% increase), consistent with findings from other contexts in the existing literature that transport access can improve labor market outcomes (Duranton and Turner, 2012; Asher and Novosad, 2020; Tyndall, 2021). We then show evidence that these positive estimated impacts may be driven at least in part by selection. First, rental rates rise substantially in impacted areas. This is consistent with positive welfare impacts of the BRT, but is likely driven by those with higher willingness to pay for BRT access bidding up rents. Second, we compare baseline characteristics of final and initial residents. These results reveal that arrivers have more education, and higher individual- and household-level income at baseline, than exiters. This raises the possibility that at least some of the positive estimated place-based impacts may reflect selection of those with better labor market outcomes into areas with readier access to the new BRT line.

We use our framework to understand the importance of these selection effects and present estimates of the causal effects of the BRT system for people rather than place. This analysis reveals that treatment effects on individuals, for both labor market participation and earnings, are quantitatively smaller than treatment effects on these outcomes for place. Alongside smaller point estimates for people-based relative to place-based estimates, for some outcomes this can yield a different conclusion as to whether impacts are statistically distinguishable from zero at conventional levels: for instance, at the 5% significance level, place-based estimates would suggest statistically significant impacts on household income, while people-based estimates for both final and initial residents would not. While the BRT had strong positive impacts on labor market outcomes for place, causal impacts on labor market outcomes for people are revealed to be much

weaker. Overall, the patterns of people- and place-based estimates are consistent with positive selection on characteristics. Those who ultimately have access to the BRT are largely those who would have fared well in the labor market, even in the absence of the infrastructure.

Beyond documenting how selection shapes who gets access to infrastructure, our results also have implications for the evaluation of public infrastructure in cities. A researcher using a place-based estimate in our setting may inadvertently infer that the infrastructure has much stronger positive impacts on people than its true causal effects. It is natural to ask whether this researcher could have recovered our estimates using a repeated cross section by controlling for selection. One such strategy adopted in the existing literature is to control for changes in demographic characteristics that might predict labor market outcomes, such as educational attainment. If these demographic characteristics predict baseline outcomes well, then they can be used to control for selection on characteristics and recover final resident treatment effects.

We evaluate how successful this strategy of adding controls is in recovering causal estimates for individuals in our data. While the previous results suggest that selection on characteristics is likely to be more important than selection on treatment effects in our setting, we consider the impacts of adding controls for both demographic variables, baseline labor market variables, as well as neighborhood fixed effects to tackle selection on characteristics. We show that, using the demographic variables that we have available (including education and age), we cannot recover the final resident treatment effect. The place-based estimate with these controls remains positive and statistically significant, implying that selection on characteristics in our data is not driven by selection on demographics. Adding controls for neighborhood fixed effects and labor market variables using information available from our tracking data also fails to return individual-level estimates, suggesting that patterns of selection are complex and not fully predicted by the observable characteristics captured in our survey. As such, structural models incorporating simple uni-dimensional models of selection may fail to capture fully the complexity of selection effects that can affect estimates of the causal impacts of investments on individuals.

This paper contributes to several literatures. A fast-growing literature in urban economics uses quantitative spatial equilibrium models to study how investments in public transportation, including BRT systems, affect city structure and aggregate outcomes across a range of contexts (Allen and Arkolakis 2022; Heblich et al. 2020; Zárate 2023; Gaduh et al. 2022), as well as distributional implications for inequality (Tsivanidis 2024; Barwick et al. 2024; Couture et al. 2024; Chapelle and Ubeda 2025). Challenges in tracking individuals have meant that there is little direct evidence on whether public goods stay with those at whom they are targeted, and structural approaches to studying this typically require strong modeling assumptions. Where panel data does exist, it tends to be sourced from administrative datasets which typically contain limited covariates. For example, Warnes 2024 innovatively uses electoral registration data to measure migration responses to Buenos Aires' BRT but the electoral data only captures occupation and no work or income covariates. We complement this literature by focusing on how population resorting and accurate tracking of mobile populations can affect estimates of the implications of transport infrastructure for labor market outcomes, as well as collecting original household surveys covering income, demographic, and commuting behavior.

Several studies of broader place-based policies highlight the importance of accounting for changes in demographic composition resulting from selection when estimating causal impacts, finding muted effects on population dynamics from urban renewal policies in the United States and Spain (Collins and Shester, 2013; González-Pampillón et al., 2020), but stronger evidence in response the construction of the US interstate highway system (Weiwu, 2024). This complements a wider literature examining sorting behaviors over space, including how this can vary with skill level and over individuals' life cycles (Albouy and Faberman, 2025). We collect novel bespoke data and present a framework to be able to study the importance of such selection effects in evaluating the impacts of public transport investments which are widespread in fast-growing developing country cities.

A small but growing related literature uses a combination of repeated cross section and demographic data, together with quantitative models, to focus specifically on gentrification. Gechter and Tsivanidis (2023) provide reduced form evidence for gentrification caused by the redevelopment of old mill cities in Mumbai, and use a dynamic spatial

model to estimate welfare implications. As in the case of Weiwu (2024), gentrification effects are inferred from changes in demographics in affected areas; in contrast, we leverage panel data tracking residents as they move in order to provide direct evidence for population resorting in response to the new infrastructure. Almagro et al. (2024) and Almagro and Domínguez-Iino (2024) use structural quantitative models to examine similar impacts of Chicago's public housing demolitions and mass tourism in Amsterdam, respectively. Brummet and Reed (2021) uses longitudinal census microdata to study gentrification in the USA and finds that this is driven primarily by differential in-migration of higher educated, higher income people paying higher rents, with more modest increases in incumbent out-migration. Finally, Couture et al. (2024) uses a quantitative model to estimate the extent to which gentrification in cities accentuates rising inequality. Perhaps most closely related to this paper, Autor et al. (2025) study the causal impact of the China shock using tax data that forms a panel. While the focus of their analysis is different, they have a related emphasis on the difference between impacts on people and impacts on places.

The rest of the paper is structured as follows. Section 2 outlines the framework we use to study the causal impacts of the BRT on initial and final residents in targeted locations, and distinguish these from selection effects. Section 3 describes the empirical setting, data collection, and key motivating facts from our panel survey data. Section 4 discusses our central results on the impacts of Dar es Salaam's BRT system on place and people. Section 5.1 considers how far the selection we identify could be controlled for using information typically available in repeated cross-sectional data. Section 6 concludes.

2 Framework

This section describes our framework that links together place- and individual- treatment effects. We start with a simple example to set ideas, and then move to developing the model and modified parallel trends assumptions to identify the three sets of treatment effects: effects on place, initial, and final residents.

2.1 Intuition and notation: Place-level vs person-level changes

We begin with a simple framework to clarify how changes in outcomes observed at the level of a *place* relate to changes in outcomes for the *people* who live there. This section serves to build intuition and introduce notation; causal interpretation is discussed in the following subsections.

Let y^{BL}_{hif} denote an outcome for household h measured at baseline, when they lived in initial location i, and which later moved to final location f. Let y^{EL}_{hif} be the same outcome measured for the same household at endline. Table 1 summarizes the measured data for the case of a household h who initially lived in place p and moved to f, and a household h' who initially lived in place i and moved to place p.

Table 1: Example data: place p

	Baseline	Endline
Initial resident (exiter)	y^{BL}_{hpf}	y^{EL}_{hpf}
Final resident (arriver)	$y^{BL}_{h'ip}$	$y^{EL}_{h'ip}$
Place	y^{BL}_{hpf}	$y^{EL}_{h'ip}$

We define the following changes in outcomes observed between baseline and endline. The initial resident change $\Delta y_{hpf} = y^{EL}_{hpf} - y^{BL}_{hpf}$ describes the change in outcome for the person originally living in p. The final resident change $\Delta y_{h'ip} = y^{EL}_{h'ip} - y^{EL}_{h'ip}$ describes the change in outcome for the person living in p at endline; and the place-level change $\Delta y_{hh'p} = y^{EL}_{h'ip} - y^{BL}_{hpf}$ describes the difference in outcomes observed in location p between baseline and endline.

We now show how this place-level change can be decomposed into two intuitive components: the *person-level change* and *composition effects*.

We obtain a decomposition via final resident by adding and subtracting $y^{BL}_{h'ip}$:

$$\begin{aligned}\Delta y_{hh'p} &= y^{EL}_{h'ip} - y^{BL}_{hpf} \\ &= \left(y^{EL}_{h'ip} - y^{BL}_{h'ip}\right) + \left(y^{BL}_{h'ip} - y^{BL}_{hpf}\right) \\ &= \underbrace{\Delta y_{h'ip}}_{\text{Final Resident Change}} + \underbrace{\Sigma y^{BL}_{hh'p}}_{\text{Baseline Composition Effect}} .\end{aligned}$$

This characterizes the change in outcome at place p as the change in outcome for the *final resident*, plus the baseline difference in outcomes between initial and final residents. Note that both the final resident change and the baseline composition effects are observable if all data in Table 1 are collected.

Alternatively, adding and subtracting y^{EL}_{hpf} yields a decomposition via initial resident:

$$\begin{aligned}\Delta y_{hh'p} &= y^{EL}_{h'ip} - y^{BL}_{hpf} \\ &= \left(y^{EL}_{hpf} - y^{BL}_{hpf}\right) + \left(y^{EL}_{h'ip} - y^{EL}_{hpf}\right) \\ &= \underbrace{\Delta y_{hpf}}_{\text{Initial Resident Change}} + \underbrace{\Sigma y^{EL}_{hh'p}}_{\text{Endline Composition Effect}} .\end{aligned}$$

where the endline composition effect is the difference in outcomes between the arriver and the exiter, measured at EL. It is also directly observable if all data in Table 1 are collected.

2.2 Identifying causal effects

This section outlines how we identify the causal effect of the new transport infrastructure on people—both initial and final residents—by leveraging our novel panel data and tracking methods. We distinguish these person-level treatment effects from standard place-based estimates and show how comparisons between these estimates reveal selection on characteristics.

Our key assumption is a *conditional parallel trends* assumption which requires that, conditional on baseline characteristics, changes in accessibility are as good as randomly as-

signed with respect to (counterfactual) outcome trends for both initial and final residents. More formally, assume the city is composed of N locations and let $\Delta T = \{\Delta T_1, \ldots, \Delta T_N\}$ denote the change in travel times to the central business district caused by the new transport infrastructure. Each household h lives in an initial location i at baseline (prior to opening of the new transport infrastructure) and moves to an endogenous final location $f(\Delta T)$ at endline.[2] The potential change,

$$\Delta y_{hif(\Delta T=0)}(\Delta T = 0) = \left(y^{EL}_{hif(\Delta T=0)} - y^{BL}_{hif(\Delta T=0)}\right)(\Delta T = 0),$$

denotes the baseline to endline change in outcome y that would have occurred in the counterfactual scenario in which the new transport infrastructure had not been built (i.e., $\Delta T = 0$). Our conditional parallel trends assumption then implies that:

$$\Delta y_{hif(\Delta T=0)}(\Delta T = 0) \perp\!\!\!\perp \Delta T_i, \Delta T_{f(\Delta T)} \mid x^{BL}_{hif(\Delta T=0)}, \tag{CPT}$$

where $x^{BL}_{hif(\Delta T=0)}$ are baseline characteristics of household h.

Initial resident treatment effects

Consider the regression:

$$\Delta y_{hif} = \alpha^I + \beta^I \Delta T_i + f(x^{BL}_{hif}) + \epsilon_{hif}.$$

Assumption (CPT) implies that $\epsilon_{hif} \perp\!\!\!\perp \Delta T_i$. The term α^I captures the counterfactual common trend for all initial residents, and β^I identifies the causal effect of the *relative* change in travel times caused by the new transport infrastructure on initial residents (assuming a linear effect).

It is worth emphasizing that, without further assumptions, β^I captures relative effects. For example, suppose $\beta^I > 0$, but the new transport infrastructure caused a common decrease in outcomes for *all* locations. This common decrease would be captured in δ^I and we would only know that initial residents who saw a larger reduction in travel times

[2] It may be that $i = f(\Delta T)$. In this case h is termed a 'stayer'.

saw a relatively smaller decrease. The final part of this section discusses how relative changes are sufficient to establish our central claim that estimated effects on place and people differ in important ways.

Final resident treatment effects

Consider the related regression:

$$\Delta y_{hif} = \alpha^F + \beta^F \Delta T_f + f(x_{hif}^{BL}) + \epsilon_{hif}.$$

Again, (CPT) implies that $\epsilon_{hif} \perp\!\!\!\perp \Delta T_f$ conditional on α^F, which captures counterfactual trends that would have occurred for final residents in the absence of the new transport infrastructure. As previously, β^F captures a *relative* causal effect, this time for final residents. The conditional parallel trends assumption (CPT) implies that, conditional on covariates, final and initial residents have common counterfactual trends: $\delta^I = \delta^F$.

Place treatment effects

A place p may have different household residents in the baseline and endline data. To formalize this, let each location $p \in N$ have a potentially different resident at baseline (h) and endline (h'), and let

$$\Delta y_{hh'p}(\Delta T = 0) = \left(y_{h'ip(\Delta T=0)}^{EL} - y_{hpf(\Delta T=0)}^{BL} \right) (\Delta T = 0)$$

be the change in outcomes that would have occurred in place p if the new transport infrastructure had not been built. Authors studying the causal effect of infrastructure on place use a variety of identification assumptions. A common example (e.g., Gibbons and Machin (2005) and Faber (2014)), which we follow here, is that, conditional on the baseline covariates of *initial* residents, the new transport infrastructure was not targeted at places where outcomes would have evolved differently in the absence of the new infrastructure:

$$\Delta y_{hh'p}(\Delta T = 0) \perp\!\!\!\perp \Delta T_p \mid x_{hpf}^{BL}. \tag{1}$$

This may arise, for instance, if those planning the new transport infrastructure system may have targeted the lines based on characteristics of the population, but did not have access to information on population trends. With this assumption, the regression

$$\Delta y_{hh'p} = \alpha^P + \beta^P \Delta T_p + f(x^{BL}_{hpf}) + \epsilon_{hh'p}$$

identifies the relative causal effect of the new transport infrastructure on place.

We discuss the relationship between this parallel trends assumption and the assumption (CPT) defined above to identify causal people-based estimates. This comparison reveals that (CPT) is weaker than common parallel trends assumptions invoked to estimate place-based effects. To compare the two assumptions, note that the set of people living in location p can be decomposed into three groups: stayers $S_p = \{h : i = p, f = p\}$; exiters $E_p = \{h : i = p, f \neq p\}$; and arrivers $A_p = \{h : i \neq p, f = p\}$. Denote $\mathbb{1}_h^{S_p}$ to be an indicator that takes the value one when h is in the stayer set for place p (and similarly for exiters and arrivers). The change in outcomes in location p can then be written

$$\begin{aligned} \Delta y_{hh'p}(\Delta T = 0) &= \mathbb{1}_h^{S_p} \Delta y_{jif(\Delta T=0)}(\Delta T = 0) + \mathbb{1}_h^{E_p} \left(y^{EL}_{h'il} - y^{BL}_{hlf} \right) \\ &= \left(\mathbb{1}_h^{S_p} \Delta y_{h \in S_p} + \mathbb{1}_h^{E_p} \left(\Delta y_{h' \in A_p} + (y^{BL}_{h \in A_p} - y^{BL}_{h \in E_p}) \right) \right) (\Delta T = 0). \end{aligned}$$

Conditioning only on x^{BL}_{hif}, (CPT) implies that the first term (which applies to stayers), and $y^{BL}_{h \in E_p}$ (which applies to exiters) are independent of ΔT. The remaining terms, which apply to entrants, are not independent of Δ_p because we have not conditioned on their characteristics. This then implies that (1) requires, in addition to (CPT), that arrivers' trends are *unconditionally* independent of travel time changes and arrivers' baseline characteristics (or equivalently that the new transport infrastructure should be independent of arrivers' endline characteristics). A consequence is that under this stronger assumption, required to estimate place-based effects, all trends are common: $\delta^P = \delta^I = \delta^F$.

2.3 Identifying selection effects

As earlier, relating place-based estimates to final resident estimates yields:

$$\begin{aligned}
\Delta y_{hh'p} &= \mathbb{1}_h^{S_p}\left[y_{h'\in S_p}^{EL} - y_{h'\in S_p}^{BL}\right] + \mathbb{1}_h^{E_p}\left[y_{h'\in A_p}^{EL} - y_{h\in E}^{BL}\right] \\
&= \mathbb{1}_h^{S_p}\left[y_{h'\in S_p}^{EL} - y_{h'\in S_p}^{BL}\right] + \mathbb{1}_h^{E_p}\left[y_{h'\in A_p}^{EL} - y_{h'\in A_p}^{BL}\right] + \mathbb{1}_h^{E_p}\left[y_{h'\in A_p}^{BL} - y_{h\in E_p}^{BL}\right] \\
&= \underbrace{\Delta y_{h'p'p}}_{\text{Final Resident}} + \underbrace{\mathbb{1}_h^{E_p}\left[y_{h'\in A_p}^{BL} - y_{h\in E_p}^{BL}\right]}_{\text{Selection on characteristics (Baseline composition effect)}}.
\end{aligned}$$

Hence, the change in outcomes experienced in the place is equal to the change for final residents, plus a selection term: $y_{h'\in A}^{BL} - y_{h\in E}^{BL}$. We refer to this as *selection on characteristics* (and was termed the baseline composition effect earlier). This term implies that changes in outcomes at the level of a place will be larger than changes in *final* resident outcomes if arrivers were doing better in the baseline than exiters. Note that here characteristics refers both to the characteristics of the person, and the characteristics of the place they are living when the data is recorded; we return to this below. In our DID framework, this then implies:

$$E(\Delta y_{hh'p})(\Delta T) = \delta^P + \beta^P \Delta T = \delta^F + \beta^F \Delta T + \sigma^C \Delta T$$

where $\sigma^C = \pi_{h\in E_p}\left[y_{h'\in A_p}^{BL} - y_{h\in E_p}^{BL}\right]$. As a consequence, selection on characteristics can be identified by comparing place and final resident treatment effects: $\sigma^C = \beta^P - \beta^F$.

Using a similar approach to relate place-based to initial resident estimates:

$$\begin{aligned}
\Delta y_{hh'p} &= \mathbb{1}_p^{S_p}\left[y_{h\in S_p}^{EL} - y_{h\in S_p}^{BL}\right] + \mathbb{1}_p^{E_p}\left[y_{h'\in A_p}^{EL} - y_{h\in E_p}^{BL}\right] \\
&= \mathbb{1}_p^{S_p}\left[y_{h\in S_p}^{EL} - y_{h\in S_p}^{BL}\right] + \mathbb{1}_p^{E_p}\left[y_{h\in E_p}^{EL} - y_{h\in E_p}^{BL}\right] + \mathbb{1}_p^{E_p}\left[y_{h'\in A_p}^{EL} - y_{h\in E_p}^{EL}\right] \\
&= \underbrace{\Delta y_{hpp'}}_{\text{Initial Resident}} + \underbrace{\mathbb{1}_p^{E_p}\left[y_{h'\in A_p}^{EL} - y_{h\in E_p}^{EL}\right]}_{\text{Selection on outcomes (Endline composition effect)}}. \qquad (2)
\end{aligned}$$

The final term here, labeled "selection on outcomes", is not a standard selection term. It indicates that place effects will be larger than initial resident effects if those who arrive have better outcomes (endline composition effect from earlier) than those who exit. This combines the usual notion of selection with an endogenous causal effect.

To summarize and link our estimates to concrete policy goals, consider a policymaker who has both targeting and outcome goals. Specifically, suppose that goal of the new transport infrastructure is to (a) improve labor market outcomes for initial residents but, if that is not possible, to (b) ensure that labor market outcomes are improved for at least some residents. Suppose she observes a positive place-level treatment effect on labor market indicators. Can she conclude that either goal has been achieved? The framework above implies that this is not necessarily the case. In the worst-case scenario, the observed place-level effect is entirely driven by selection on characteristics: the new transport infrastructure causes no real improvement for anyone, but simply draws in individuals who would have performed better regardless. In this case, final residents do not benefit, nor do labor market outcomes improve because of the new transport infrastructure itself—the apparent gains reflect only compositional change. Why might this happen? Consider that individuals already in the labor force may place higher value on improved transport and relocate accordingly, raising average employment in treated areas without affecting anyone's actual employment trajectory. The question about whether welfare for initial residents is not directly observable from place-based effects. A positive place-based estimate could coexist with a negative initial resident estimate if the displacement effects were severe. Welfare may rise through better matching or convenience, but it may fail on both counts: it neither improves outcomes for initial residents, nor does it causally improve labor market outcomes for anyone.

2.4 General equilibrium, spillovers, and SUTVA

We have so far emphasized that all of our treatment effects are *relative*: they compare outcomes between more- and less-treated locations, but do not capture the *total* general equilibrium (GE) effect of the new transport infrastructure. For example, even if areas with improved new transport infrastructure access show gains, these may be offset by declines elsewhere—yielding no net impact at the city level.

To interpret relative effects as total causal effects, we must invoke additional assumptions—typically some version of the *Stable Unit Treatment Value Assumption (SUTVA)*. One

common form of SUTVA, in the context of place-level analysis, assumes that:

$$\Delta y_{hh'p}(\Delta T_1, \ldots, \Delta T_p, \ldots, \Delta T_N) = \Delta y_{hh'p}(\Delta T'_1, \ldots, \Delta T_p, \ldots, \Delta T'_N)$$

whenever ΔT_p is unchanged.[3] That is, the change in outcomes at p does not depend on travel time changes elsewhere. Combined with a conditional parallel trends assumption, this implies that estimated place-based effects are also valid estimates of the new transport infrastructure's total impact on those locations.

3 Empirical setting and data collection

This section describes the empirical setting in Dar es Salaam and our data collection efforts.

3.1 Dar es Salaam Bus Rapid Transit system

With a growth rate of 6.5%, Dar es Salaam is one of the fastest-growing cities in Africa (The World Bank, 2019). Traffic congestion is a severe problem (Mpogole and Msangi, 2016), costing the city an estimated $1.8 million each day in lost productivity (The World Bank, 2019). To help address these challenges, a six-phase BRT system of dedicated trunk lanes spanning 141km, shown in Figure 1, is being being built in the city between 2005-2035, the first of its kind in East Africa.

Operations commenced on the system's first phase, connecting the city's central business district to residential areas in the city's northwest (Kinondoni), in May 2016. Within its first year of operations, the system was carrying 165,000 passengers per day. Later phases of the BRT system are planned along other radial routes connecting the central business district to the south (Temeke district, Phase 2), southwest (Ilala district, Phase 3), and north (Phase 4), as well as orbital and connecting routes (Phases 5 and 6).

[3] This assumption is implicit in all paper that make use of DID methods to evaluate transportation infrastructure, and indeed in paper using other methods such as IV. See, for example, Gibbons and Machin (2005), Faber (2014).

3.2 Data collection

We fielded household and individual level panel surveys that allow us to track both structures and residents from before the opening of the BRT until three years afterwards. We collected a baseline survey of 1748 structures and interviewed one male and one female respondent from each. We then track along two dimensions during our endline survey three years later. We resurveyed baseline respondents, wherever they were living at endline (creating a panel of households), and we surveyed any new households residing in the original structures surveyed at baseline (creating a panel of structures). For new residents of original structures, we also collected retrospective data on their key outcomes three years prior, in order to obtain baseline observations for all individuals in the panel.[4] Appendix Figure A1 outlines the timeline of survey and operational activities.[5]

3.2.1 Baseline household survey

Our baseline household survey was conducted in January-February 2016, before the first phase of the BRT began operations in April. We used a geographical sampling strategy to ensure coverage across the entire city of Dar es Salaam by selecting 141 clusters at equal intervals along 12 arcs at radii increasing at 1.5km intervals from the central business district, as shown in Figure A2. We conducted interviews at 125 of these clusters.[6] At each cluster location, a random walk was used to select 12-14 households to interview, yielding a total of 1748 households that were available for and consented to interviews.

We conducted three interviews at each household. A household module was conducted with any knowledgeable household member found at the house, covering household demographics, dwelling information, assets, consumption, and summary education and employment information for all household members. A separate survey was then

[4]The retrospective data is collected for all households, allowing validation for the initial sample by comparing the actual responses collected at baseline to the retrospective responses collected at endline. We discuss these results Section **??**.

[5]In addition to the primary baseline and endline survey rounds, we completed a midline attrition survey as well as several rounds of phone surveys, included in Appendix Figure A1. These additional surveys were used primarily to assist with tracking households.

[6]The remaining 16 clusters were military or special residence compounds, non-residential areas, or designated hazardous areas where we were not able to conduct interviews.

administered to one male and one female respondent aged above 17 years, randomly selected from their respective qualifying group in the household. This survey included more detailed questions on employment, income, commuting and neighborhood amenities. This survey was administered to a total of 3104 individuals.

All results are presented with survey weights that re-weight our sample to match the 2012 National Census population and home ownership rates in Dar es Salaam (see Appendix A for the derivation of weights). We also examine the robustness of the central results to not applying population weights in Section 4.4.

3.2.2 Endline household survey

Our endline household survey was conducted from February-May 2019.[7] All baseline respondents and structures were physically tracked as part of this survey. Hence, if a baseline household had moved out of its baseline structure, as long as they remained living in Dar es Salaam, the household was tracked to its new structure, and interviews were conducted with both the original household in its new structure and with the new occupants of their original structure. Baseline respondents who split from their baseline household and moved elsewhere were also tracked and a household survey was conducted for their new household.

Appendix Table A1 summarizes structure- and individual-level recontact rates at endline. Approximately three years after baseline, we locate 92% of our baseline structures; 89% of our initial households; and 85% of our initial individuals. We successfully survey 76% of our baseline structures; 80% of the initial households; and 70% of the initial individual residents. Our successful survey rates between baseline and endline conditional on having successfully located at least one member of the household during the midline attrition survey[8] are 88% for the structure, 92% for the household, and 81% for the in-

[7] In addition to the endline survey, we conducted a follow-up telephone survey in September-October 2019 to refine some variables given inconsistencies in some survey responses. Refer to the Appendix Figure A1 for a full timeline of activities.

[8] During the midline attrition survey, we discovered that the initial survey firm had not accurately collected full GPS coordinates of all structures, contributing to the 8% of structures that were not located. The other reasons for not completing the survey were household refusal at either midline or endline (10% of structures) and the structure being torn down or empty (5.8% of structures).

dividual respondents. We present balance tests for attrition across the three samples in Appendix Table A2. We do not find that distance to the BRT predicts who was successfully surveyed, but, as perhaps expected in locations with a large amount of mobility, we are more likely to be able to recontact individuals who were owners rather than renters, and those who had lived in their structure for more years.

Our sample frame was to interview one randomly selected male and female respondent from structures we enrolled at baseline. We then followed the male and female respondents if they moved, and attempted to re-enroll a new male or female to replace the mover. Our analysis dataset is therefore at the structure-by-gender level. Appendix Table A3 shows the coverage of our final dataset for measuring initial resident treatment effects (requires the initial resident to be successfully tracked between baseline and endline), final resident treatment effects (requires the initial resident to be surveyed and the replacement resident to be enrolled), and structure (requires the initial resident to be surveyed and the replacement resident to be enrolled). Out of a maximum sample of 3492 structure-by-gender pairs, we have 318 cases where we did not have a baseline respondent. From this, we are able to estimate all three treatment effects (initial, final, and place) for 60% of baseline structures.[9] This will be our primary analysis sample.

3.2.3 Measuring travel speeds

Our main treatment variable is change in travel time between a structure and the central business district (CBD). We discuss the construction of this variable in the next subsection, but it has two key ingredients: baseline travel speeds, and endline travel speeds along BRT routes. To measure baseline travel speeds we conducted a travel time survey in January 2016. Enumerators traveled between six points on the periphery of Dar es Salaam and the central business district by daladala (minibus), taxi, motorbike, and bajaji (rickshaw), recording GPS locations and time stamps along each journey route. A total of 812 trips were completed, spanning different days of the week and times of the day. These data were used to calculate average baseline travel speeds by car and public trans-

[9] In 24% of cases, we cannot compute any of the initial, final, or place estimates. This is primarily due to the structure not being found or household refusing. The remained of the cases we can compute a subset of the three treatment effects.

port, where the latter averaged speeds across daladala (minivan) and bajaji (motobike) travel.

To construct endline travel speeds on BRT routes, we obtained recorded departure and arrival times at stops along the BRT route between November 2017 - April 2018 from the Dar Rapid Transit Agency (DART).

3.3 Measuring exposure to the BRT

Exposure to the BRT is measured as the decrease in log travel time from the structure to the CBD, assigned to the centroid of Gerezani ward, as a result of the construction of BRT phase 1. This is calculated using the Network Analyst tool in ArcGIS and the travel speed data described above. Predicted baseline travel times from each structure to the CBD are calculated by assigning to each stretch of the road network a travel speed equal to the average travel speed in the baseline travel time survey. Predicted endline travel time calculations isolate changes in travel times resulting from the BRT by assigning the same travel speeds to all stretches of the road network *except* for stretches along the BRT Phase 1 route, which are assigned average BRT travel speeds. The implied reduction in log travel time to the CBD using this measure is visualized in Figure 2, panel A.

A recent literature highlights potential challenges in estimating the causal effects of travel time reductions based on transportation improvements if neighborhood characteristics are correlated with exposure to the treatment (Borusyak and Hull (2023)). For example, since BRT lines are more likely to be constructed along radial routes that connect to the CBD, more treated locations may be expected to see differential travel time improvements relative to less treated locations even in the absence of BRT construction. To address this, we follow the method proposed in Borusyak and Hull (2023) by demeaning the measure shown in Figure 2, panel A – capturing exposure to the BRT Phase 1 line – by the expected reduction in travel time implied across the first four proposed lines of the BRT system, shown in Figure 2, panel B.

This procedure yields our key treatment variable, the demeaned predicted reduction in log travel time from each location to the CBD, shown in Figure 2, panel C. The distribu-

tion of the demeaned exposure variable, shown in Appendix Figure A3, reveals a median value of -0.27 and an eightieth percentile value of 0.101. When interpreting coefficient magnitudes, we consider results at this value of the exposure variable (i.e., those households with a demeaned reduction in travel time to the CBD of more than 10.1% due to the BRT) to capture estimated effects for an average respondent who was highly exposed to Phase 1 of the BRT. Regression tables display the average effect for "highly exposed", referring to this definition, to ease interpretation of magnitudes.

Table 3 shows the computation of the demeaned variable. The first row of the table shows the average reduction in log travel time to the CBD, separately for low-exposure and high-exposure households. Low-exposure households have an average reduction in travel time to the CBD of 3% due to Phase 1. However, on average, low-exposure households would have an average reduction of 5% across Phases 1-4. Therefore, on average, the demeaned exposure measure for low-exposed households is an *increase* in demeaned travel time of 2%. Highly-exposed households differ: on average, they face a reduction of 22% in log travel time to the CBD from Phase 1, but would only have an average reduction of 7% across Phases 1-4. Therefore, the demeaned reduction is 16%.

3.4 Sample summary and facts about commuting, migration and BRT usage

Table 2 presents summary statistics during both survey rounds, for both structures and initially-enrolled individual respondents.[10]

The survey includes several measures of labor force participation and earnings, the key outcomes that are the focus of our analysis. Our primary measure of employment is defined from the question: "What is your main current occupation?", where people who respond that they are unemployed, too old to work, a student, or a housewife are not coded as employed. By this measure, 68% of individual respondents are employed at

[10]The same table, unweighted, is presented in Appendix Table A4. While summary statistics are broadly aligned in the weighted and unweighted samples, because we weight to match census home-ownership rates for Dar es Salaam there are some mechanical differences. For instance, a larger share of households own their house in the unweighted sample, with a commensurately smaller share of individuals reporting that they moved house between the baseline and endline surveys.

baseline. We also ask if the respondent engaged in any kind of job or work for payment in the last seven days; employment by this measure is 43% at baseline, reflecting the large amount of labor market informality present in many low-income countries. Finally, we ask if respondents operated any self-employed business or did any self-employed activity over the last month (including agriculture); 43% of respondents report doing so.

Survey questions on income and earnings are asked in several ways. Our primary measure is earnings in the last 7 days (the answer to the question: "What were your average total weekly earnings from all jobs, after tax, in the last 7 days?"). At baseline, respondents report earning 35,100 TSH (approximately 16 USD) over the last 7 days on average. We also measure self-employed income (the response to: "What is your gross (net) income over the last 1 month (from self-employed business/activity)?"), and household income (the response to: "What is the total gross (net) income for the household over the last 1 month from all sources?"). [11]

We measure poverty by household expenditure. As expected for the capital city, poverty levels are relatively low, with 95% of respondents above Tanzania's national poverty line of 49,320 TSH per person per month.

3.4.1 Commuting

Eighty four percent of working individuals in the sample commute to work (i.e., work outside the home) at baseline. Commute times are long, averaging 47 minutes across the sample, with especially long commutes among respondents living towards the outskirts of the city. Thirty six percent walk to work, while 58% commute by public transport. The main modes of public transit available in Dar es Salaam before the introduction of the BRT were shared minibuses (daladalas) and three wheeler taxis (bajajis), with smaller shares accounted for by motorbike taxis and car taxis.

Figure 3, panel (a), displays baseline commuting flows at the level of 52 spatial units

[11] Table 2 shows a decline in household income between 2016 and 2019. GDP data for Tanzania over this time period show a relatively flat value: based on the World Bank Development Indicator's database, GDP per capita was $953 in 2016 and $1063 in 2019. All analyses will include year fixed effects and so we will focus on how relative reported income changes as a function of exposure to BRT; the level effects between years will not be important for the analysis.

across Dar es Salaam. These spatial units are based on the 77 wards (which represent third level administrative divisions in Tanzania) within the geographic coverage of our study area, aggregated as necessary to ensure non-zero observations in all units, and are also used for clustering of standard errors in subsequent regression specifications. Commuting flows are strongly concentrated in trips from outlying spatial units towards locations closer to the central business district.

3.4.2 Migration

Panel A of Table 2 describes structure-level summary statistics from the baseline and endline surveys. At baseline, 37% of surveyed respondents own their house, while 56% are renting. Dwellings comprise 3.44 households on average, with each household occupying an average of 2.45 rooms.

Rates of household churn in the data are high, suggesting that there may be significant scope for population resorting. Nineteen percent of structures surveyed had completely new households with no original members living in the household. Forty percent of structures had at least one of the individual (male or female) respondents move, with especially pronounced churn rates among renters.[12]

Appendix Table A5 shows the share of tracked individuals that moved within Dar es Salaam versus to other locations. Of these individuals, 63% are living in the same structure, 29% have moved elsewhere in Dar es Salaam, 6% have left Dar es Salaam, and 2% have died. Appendix Table A6 shows self-reported reasons for moving house. Twenty eight percent of individuals report family reasons (e.g., moving out of the family home, marriage, needing to look after a family member, or relationship breakup) as the primary reason they moved; 22% report characteristics of the neighborhood, such as community, safety, or attractiveness of the area; 19% report economic reasons, such as being closer to work, school, or public transport; and 17% report moving due to the cost of housing.

Panel (b) of Figure 3 displays migration flows in the sample between the baseline and endline surveys, at the level of the same 52 spatial units as used in the commuting

[12]To benchmark these figures, on average 6% of metropolitan respondents (and 10% of renters) in the 2016-2018 American Community Survey in the United States reported moving house within the last 12 months.

visualization in panel (a). Many of the migrants over this period can be seen to have migrated from more central areas towards suburban areas further from the city center.

3.4.3 BRT use

Table 4 reveals that a large share of individuals have ridden the BRT at the time of the endline survey. Individuals residing in locations with stronger exposure to the BRT – measured as those respondents for which the demeaned predicted decrease in log travel time from the neighborhood to the CBD as a result of the opening of BRT Phase 1 exceeds the 80th percentile value of 0.101 (hereafter 'highly exposed'), as described in Section 3.3 – are intuitively more likely to use it. For ease of interpretation, we include the magntitude for the 'highly exposed' households in all regression tables that follow.

In high exposure locations, 61% of respondents report having used the BRT in the last seven days, taking an average of 2.33 BRT trips over this period. In these locations, 37% report using the BRT to get to work, 54% report using it for family reasons, 52% for shopping, 40% for leisure and 38% for health purposes.[13] The endline survey asked respondents about their perceptions of the BRT after it had opened. Responses to these questions are summarized in Panels 2 and 3 in Table 4. Highly-exposed respondents intuitively report higher satisfaction with BRT accessibility and usefulness, while reported constraints to use include route usefulness, service reliability and station accessibility.

3.5 Analysis of pre-trends

To support our identification assumptions, we consider evidence for pre-trends in our key outcomes between 2015 and our baseline survey, using data from the World Bank's Measuring Living Standards Within Cities project, which was collected in Dar es Salaam in 2014 and 2015.[14]This dataset captures different households to those included in our survey data collection, and includes location identifiers at the level of enumeration areas.

[13]This is consistent with evidence from smartphone app location data in Tanzania that highlights the importance of consumption as a driver of high-frequency mobility Blanchard et al. (2025).

[14]To do this, we accessed anoymized restricted-access data that contained geographical information of households at the level of enumeration areas. We thank Nancy Lozano Garcia at the World Bank for facilitating data access.

Our analysis of pre-trends is therefore conducted at the level of enumeration areas.

To link the two datasets, we assign each structure in our baseline survey to the nearest enumeration area in the World Bank data. Some enumeration areas from the 2015 World Bank survey are never assigned to structures in our baseline survey due to distance, and data from these areas are excluded from the pre-trends analysis.

We focus on variables that were consistently collected across both datasets, including imputed rent per room, duration in the house, typical number of days worked per week, typical number of hours worked per day, and household asset ownership. Due to differences in income reporting between the two surveys, income is not included in our pre-trends analysis.

Using the survey data, we generate a residualized rent variable by regressing log rent per room on household and structure characteristics, including access to electricity for lighting, use of a non-latrine toilet, availability of piped water, cooking fuel (oil, gas, or electricity), and whether the structure has cement walls or a slate roof. We also construct a wealth index using Principal Component Analysis (PCA) on household asset data, with the first principal component serving as the index.

The pre-trends analysis follows the same approach as our main analysis, using the post double selection lasso method to control for initial characteristics. As in the main analysis, our dependent variables are expressed as difference variables.

The results, presented in Table A8, indicate no significant differences in pre-existing trends across treatment intensities before the BRT's implementation.

4 Results

This section presents the three sets of treatment effects: effects on place, final residents, and initial residents. We find evidence of place-based effects, but much weaker effects on final residents, suggesting the place-based effects are driven by selection. We find no evidence that initial residents are harmed by the BRT: treatment effects are positive, but noisy.

4.1 Place-based impacts

Table 5 examines the impacts of the BRT on *place*, which we obtain using a dwelling-level specification estimable using standard repeated cross-sectional data. Consistent with much of the existing literature such as Tsivanidis (2024), these estimates suggest that the BRT has strong positive impacts on place.

First, we confirm in column (1) of Table 5 that residents in areas served by the BRT self-report differential reductions in travel times to the CBD, with highly-exposed residents reporting a sizable reduction of 8 minutes relative to a mean endline value of 70 minutes. Column (2) reveals that treated locations also experience sharp increases in rental rates: the average highly-exposed structure sees a 30% increase in expected rent per room (accounting for owners' expectations of what the room would rent for). This pattern is consistent with the BRT providing a service that people are willing to pay for. Taken alone, however, this estimate does not reveal which individuals value the BRT, or what aspects of access to the BRT they are willing to pay for.

The central set of results in Table 7 turn to establishing the causal effects of the BRT on labor market outcomes for people and place, and the key methodological contribution of distinguishing causal impacts on different groups of individuals from selection effects. The first two columns of the table consider impacts on employment status (whether the respondent reports having an occupation, and whether they report having worked last week, respectively), while the subsequent two columns consider impacts on income measured at the individual and household level.

The top panel of Table 7 displays estimates of BRT impacts obtained using a structure-level specification estimable using standard repeated cross-sectional data. This reveals that there are positive estimated place-based impacts on labor market outcomes. On average, highly-exposed locations experience a 4 percentage point increase in the probability of being employed, relative to a mean endline value of 68%. Similarly, the probability of having worked in the last seven days increases by 5 percentage points, relative to a mean endline value of 39%. The corresponding estimates in columns (3) and (4) reveal that this translates into increases in earnings: point estimates at the individual level are

positive but statistically insignificant, while household income increases by 17,263 TSH on average in highly-exposed locations, a 5% increase relative to the mean endline value of 345,398 TSH.

Evidence of selection

We next turn to our unique data to understand whether selection effects may contribute to these results. To analyze selection effects, we start by comparing the baseline characteristics of the initial and final residents in treated locations in Table 6. We compare time-invariant characteristics (age and education), which are typically available in repeated cross-sectional data, as well as time-varying characteristics such as labor market participation and commuting status, which are only available if respondents are tracked over time.

We first consider suggestive evidence from information about time-invariant outcomes. As shown in column (1) of Table 6, the evidence here is consistent with positive selection into BRT-affected locations by baseline years of education, though the magnitude of this effect is quantitatively small: on average, those who moved into highly exposed locations (arrivers) have 0.2 more baseline years of education relative to those who move out (exiters). Conversely, there is no statistically signifcant evidence for selection into treated locations based on age.

Our tracking data also allows us to examine evidence for selection in terms of detailed time-varying employment outcomes, as shown in columns (3) to (7) of Table 6. This reveals that arrivers also have significantly higher baseline income levels (measured at the individual or household level) relative to exiters from locations that are highly exposed to the BRT. Positive though statistically insignificant effects are also evident for baseline employment status. These results suggest that positive selection into treated locations may account for at least some of the positive place-based impacts of the BRT described in the previous subsection. Building on this finding, the next section uses the framework described in Section 2 to consider whether these selection effects are sufficiently strong that the positive placed-based estimates may be driven entirely by selection without positive people-based estimates.

4.2 People-based impacts

Given this evidence suggesting positive selection on baseline characteristics into areas treated with the BRT, we turn next to examining the causal impacts of the BRT on *people*. As outlined in Section 2, our tracking data allows us to distinguish causal from selection effects and isolate true causal effects on final residents (stayers and arrivers) and initial residents (stayers and exiters) in treated locations.

Our primary object of interest is whether individuals who end up living near the BRT receive positive causal treatment effects from the new infrastructure, or whether positive estimated effects at the structure level on labor market outcomes are instead driven entirely by selection into treated locations of those with stronger labor market outcomes at baseline. This is crucial in determining whether the new infrastructure caused real economic changes, rather than simply leading to population resorting. The results in the second panel of Table 7 display estimated causal effects on final residents living near the BRT at endline. Across the labor market outcomes of interest, the point estimates for final resident effects are smaller than the place-based estimates. The outcomes relating to labor market participation (employment status and whether the respondent worked last week) see declines in the point estimates of 17% and 13% respectively, while individual-level and household- level incomes see much larger declines of 80% and 79%. All final resident estimates are statistically insignificant at the 5% significance level, with only the employment status outcome remaining significant at the 10% level.

The pattern of results is consistent with the positive place-based estimates in the first panel of Table 7 reflecting at least in part the fact that those who had higher education and incomes, previously living elsewhere, moved into areas more strongly treated by the BRT. This is intuitive if, for instance, those already attached to the labor market are more likely to demand convenience in transport, and so move into treated locations, displacing incumbent residents. While the results are somewhat underpowered to distinguish statistically significant differences between structure-level and final resident estimates, the differences between these two sets of estimates are sufficient to yield different statistical conclusions about whether or not the BRT had positive impacts on labor market outcomes

for those living in treated locations. At conventional levels, place-based estimates would suggest positive causal impacts, while the final resident effects would not.

As outlined in Section 2, our tracking data allows us to also isolate the causal effect of the BRT on initial residents (stayers and exiters) in treated locations. Initial residents may be of particular policy important to policymakers if these households were the target of infrastructure. The initial resident estimates are included in the third panel of Table 7. For the labor market participation outcomes in columns (1) and (2), initial resident estimates are positive, but slightly attenuated relative to place-based estimates, but point estimates are similar across the two sets of estimates and the statistical significance of results is unchanged. For the income variables in columns (3) and (4), both the point estimates and statistical significance are reduced in the initial resident relative to the place-based estimates. Taken together, we find no evidence that on average initial residents were harmed from the opening of the BRT line, despite some residents moving away.

Appendix Table A16 considers additional outcomes related to potential mechanisms that might drive the labor market results in Table 7. The results in column (1) show positive effects on the probability of respondents commuting, but these estimates are not statistically significant. The results in columns (2) and (3) show strongly statistically significant evidence for improvements in self-reported happiness with public transport and respondent perceptions that where they live is convenient for where they want to go. While these survey questions were not included among the questions asked retrospectively (such that final resident effects cannot be estimated for these outcomes), the effects remain strongly statistically significant for initial residents, suggesting that these impacts are more likely to be causal positive effects of the new infrastructure.

Taken together, the findings in Table 7 suggest that the positive structure-level estimates in the first panel are likely to be driven at least in part by selection on baseline characteristics. The comparisons between structure, initial resident and final resident estimates highlight that a conventional structure-level analysis could lead researchers interested in impacts of the BRT on individuals to erroneous conclusions about its causal impacts on individuals' labor force participation and earnings. While the BRT had statistically significant positive labor market impacts on *place*, the estimates suggest that this

reflects at least in part the compositional effects of population resorting, with individual-level estimates finding no statistically significant evidence for positive impacts on the *people* that ended up living near the new infrastructure.

4.3 Results for renters

Tables A9 and A10 consider results separately restricting attention to the 56% of respondents who were living in rented accommodation at the time of the baseline survey. This group are likely to be disproportionately mobile and, as such, it may be expected that place-based estimates in areas where rental rates are high might be especially prone to being driven by selection effects.

Table A9 demonstrates that the differences between final and initial residents in structures that were rented at baseline do indeed demonstrate stronger evidence for positive selection than in the full sample (shown in Table 6). Arrivers into treated locations where structures are rented are statistically significantly more educated, more likely to be working and to have higher earnings, relative to exiters from these locations. The estimated magnitudes of these effects in the sample of structures rented at baseline in Table A9 are also much larger (in some cases even an order of magnitude larger) than those estimated in the full sample.

Commensurate with this, Table A10 shows that the differences between place-based and people-based estimates are especially pronounced among the sample of structures rented at baseline, in terms of differences in both point estimates and the statistical significance of these two sets of estimates. This is consistent with more mobile populations being more readily able to select into treated locations in response to new infrastructure investments, resulting in a higher likelihood that positive place-based estimates may be driven by selection effects rather than positive causal impacts on individuals.

4.4 Robustness tests

In this section, we examine the robustness of the central results to alternative choices in variable construction and key estimating equations.

Our sample is constructed to be representative of the overall population of Dar es Salaam, by reweighting observations using population weights as described in Section 3.2.1. Appendix Table A11 presents our key estimation results where we do not apply population weighting. The central results are qualitatively robust without applying population weights – with reductions in both point estimates and statistical significance when moving from structure- to initial- or final-resident estimates.

Appendix Table A12 considers the robustness of the main results in Table 7 to alternative choices of controls in the central regression specifications. In particular, while our central estimates do not constrain the set of controls that may be chosen by the post double selection lasso estimation, the results in Table A12 restrict the choice of controls such that the baseline value of the change *cannot* be selected as a potential control. Results are similar in this case.

As discussed in Section 3 and Appendix B, we collect retrospective data for all households in order to validate for the initial sample by comparing actual baseline responses to retrospective endline responses. Appendix Table A13 shows the robustness of results to using this retrospective data for the full sample, rather than only when required for endline residents.

Appendix Table A14 considers impacts on other measures of income and employment that are asked independently in the survey: income from self-employment (column (1)), usual working hours at least 4 hours per day (columns (2), and whether the respondent was self-employed last month(column (3). There is no evidence for statistically significant changes in any of these outcomes at the level of either places or people.

The main results demean calculated exposure to Phase 1 of the BRT by the expected travel time reduction across the first four proposed lines of the BRT system, as described in Section 3.3. Appendix Table A15 shows that similar results obtain where exposure to the BRT Phase 1 line is instead demeaned by the expected reduction in travel time across Phases 1 to 3 only.

5 Selection and measurement

Our empirical results suggest that the true casual effect of the BRT on the individuals who end up living near it is smaller than the casual effect on the neighborhoods. This section first asks whether we could simply control for characteristics of residents to recover the final-resident treatment effect from the place-based treatment effect. Given our results, we then discuss some suggestions for data collection.

5.1 Could selection be controlled for using repeated cross-sectional data?

Finally, we ask whether an analyst with access only to repeated cross-sectional data could have recovered the true causal effects of the BRT on those who have access to it, described in the previous section. Studies in the existing literature with access to such data typically take one of two approaches to addressing selection: controlling for changes in demographic composition, or using structural models.

Figure 4 suggests that the former strategy of controlling for changes in demographic composition is not successful in recovering the causal effects of the BRT on final residents in our sample. For each of the four outcomes shown, the estimate shown at the bottom of the sub-figure (labeled 'structure') corresponds to the structure-level estimate shown in the first panel of Table 7, while the estimate shown at the top (labeled 'final resident') corresponds to the final resident estimate shown in the second panel of Table 7. The penultimate estimate in each sub-figure (labeled 'demographic') adds controls for demographic characteristics (age and education) that can typically be inferred from repeated cross-sectional data since these are largely time-invariant outcomes. The figure shows that adding such controls barely changes the point estimate relative to the structure-level estimate, and for no outcome changes our conclusion about whether there is a statistically significant effect. As such, in our sample, this strategy of including controls from data available in repeated cross-sections does not work: while those who select in to treated areas are better educated (see Table 6), education alone does not predict labor market outcomes sufficiently to serve as an effective control.

The remaining estimates in each sub-figure of Figure 4 add sequential controls for

other characteristics using data that is available in our tracking data, but which would not be available in standard cross-sectional datasets. These include labor market variables ('labor market'), controls for the neighborhood of respondents ('neighborhood'), and all of these controls simultaneously ('all'). Similarly to the estimates including only demographic characteristics, there is little change in the point estimate relative to the structure-level estimate in any of these cases, and none lead to changes in statistical significance. This suggests that patterns of selection in this setting are complex and not fully described by the set of observable characteristics observed captured in our survey. This in turn suggests that the second common strategy in the literature of using structural models may also be insufficient to recover true causal effects, since complex selection patterns may not be consistent with the simple, one-dimensional selection on taste or ability that underpins most existing models.

5.2 Discussion: data collection

In the absence of rich administrative data, researchers need to collect their own data. Our field team tracked individuals over time (leading to initial resident effects for exiters) and asked retrospective questions of arrivers (leading to the final resident effects for arrivers). Of the two, it is much simpler to ask retrospective questions than track individuals – it relies on a single cross-sectional survey rather than two waves of data with respondent tracking. It may not even be possible to know *whom* to track without an initial baseline survey.

One advantage of adding retrospective questions of key outcomes is that it immediately allows computation of the final resident effect, and so gives a diagnostic test for the presence of selection. However, a challenge with relying on retrospective data is validity of responses. In Appendix B, we assess this by identifying individuals surveyed at both baseline and endline and comparing their baseline responses to their retrospective responses at endline for key outcomes. For binary and categorical variables (e.g., employment status or commuting behavior), we report the proportion of respondents who gave consistent answers across surveys. For continuous variables, we report the share of

responses within 10% or 20% of each other, as shown in Appendix Table A19. We find that binary variables exhibit moderate consistency: 78% of individuals report the same employment status (correlation = 0.47), and 50% report consistent commuting behavior (correlation = 0.47). We do find high consistency for retrospective location (retrospective location data was only asked for baseline individuals who moved house) – 70% of movers report a previous location that is consistent (correlation = 0.67). However, we also find that continuous variables are much less consistent. The correlation for wages in the last 7 days is 0.22, and for gross monthly household income, 0.24. These results align with other studies that find limited reliability in retrospective reporting, such as Fuller et al. (2024). Despite these measurement issues, the retrospective data may still contain useful signal, and indeed, when we compute our treatment effects using retrospective data for the entire sample rather than only for those who were not enrolled at baseline, results are very similar (Appendix Table A13. Given this, we cautiously suggest researchers consider asking retrospective data questions of select key outcome variables to compute a simple diagnostic test for selection.

6 Conclusion

Urban infrastructure investments are typically place-based. However, when individuals are mobile, the effects of individuals may not equal the effects on place. This paper develops a framework to show how place, initial resident, and final resident treatment effects are linked. We show the place-based treatment effect is equivalent to the final resident treatment effect and a selection term measured at baseline; alternatively, the place-based treatment effect is equivalent to the initial resident treatment effect and a selection term on outcomes, measured at endline. We then estimate these three treatment effects using panel data that tracks both individuals and structures before and after the introduction of East Africa's first BRT system.

Our results highlight the limitations of standard cross-sectional data in assessing the effects of place-based investments. A conventional neighborhood-level analysis would suggest large gains in labor force participation and earnings. However, by comparing

place-based estimates to the estimates for the residents who end up living near the BRT (final residents), we show that these estimates are in part driven by selection, rather than reflecting only real economic improvements for residents.

More broadly, our findings illustrate how infrastructure investments can reshape the composition of affected neighborhoods. Our results highlight the need for researchers to directly track individual populations where possible, given our finding that time-invariant characteristics of individuals, such as education and age, do not accurately control for differential selection. This has important implications for both the design and evaluation of place-based policies: failing to account for population mobility either in the policy stage or the research evaluation stage can lead to misleading conclusions about who actually benefits from infrastructure investment.

References

Albouy, David and R. Jason Faberman, "Skills, Migration, and Urban Amenities over the Life Cycle," 2025.

Allen, Treb and Costas Arkolakis, "The Welfare Effects of Transportation Infrastructure Improvements," *The Review of Economic Studies*, 2022, *89* (6), 2911–2957.

Almagro, Milena and Tomás Domínguez-Iino, "Location Sorting and Endogenous Amenities: Evidence from Amsterdam," *Econometrica (forthcoming)*, 2024.

_ , Eric Chyn, and Bryan A Stuart, "Neighborhood Revitalization and Inequality: Evidence from Chicago's Public Housing Demolitions," 2024.

Asher, Sam and Paul Novosad, "Rural Roads and Local Economic Development," *American Economic Review*, 2020, *110* (3), 797–823.

Autor, David, David Dorn, Gordon Hanson, Maggie R. Jones, and Bradley Setzler, "Places versus People: The Ins and Outs of Labor Market Adjustment to Globalization," *NBER Working Paper 33424*, 2025.

Balboni, Clare, Gharad Bryan, Melanie Morten, and Bilal Siddiqi, "Could Gentrification Stop the Poor from Benefiting from Urban Improvements ?," *AEA Papers and Proceedings*, 2021, *111*, 532–537.

Barwick, Panle Jia, Shanjun Li, Andrew Waxman, Jing Wu, and Tianli Xia, "Efficiency and Equity Impacts of Urban Transportation Policies with Equilibrium Sorting," *American Economic Review*, 2024, *114* (10), 3161–3205.

Belloni, A., V. Chernozhukov, and C. Hansen, "Inference on Treatment Effects after Selection among High-Dimensional Controls," *The Review of Economic Studies*, April 2014, *81* (2), 608–650.

Blanchard, Paul, Doug Gollin, Martina Kirchberger, and Michael Peters, "High-Frequency Human Mobility in Three African Countries," Technical Report, CEPR Discussion Paper 2025.

Borusyak, Kirill and Peter Hull, "Non-Random Exposure to Natural Experiments," *Econometrica : journal of the Econometric Society*, 2023, *91* (6), 2155–2185.

Brummet, Quentin and Davin Reed, "The Effects of Gentrification on Incumbent Residents," 2021.

Chapelle, Guillaume and Morgan Ubeda, "Competing for Opportunity: Transport Infrastructures and Localized Unemployment," Working Paper, CY Cergy Paris University 2025.

Collins, William J and Katharine L Shester, "Slum Clearance and Urban Renewal in the United States," *American Economic Journal: Applied Economics*, 2013, *5* (1), 239–273.

Couture, Victor, Cecile Gaubert, Jessie Handbury, and Erik Hurst, "Income Growth and the Distributional Effects of Urban Spatial Sorting," *Review of Economic Studies*, 2024.

Donaldson, Dave and Richard Hornbeck, "Railroads and American Economic Growth: A "Market Access" Approach," *The Quarterly Journal of Economics*, 2016, *131* (2), 799–858.

Duranton, Gilles and Matthew A Turner, "Urban Growth and Transportation," *Review of Economic Studies*, 2012, *79* (4), 1407–1440.

Faber, Benjamin, "Trade Integration, Market Size, and Industrialization: Evidence from China's National Trunk Highway System," *Review of Economic Studies*, 2014, *81* (3), 1046–1070.

Fuller, Rob, Simone Lombardini, and Cecilia Poggi, "How Accurate Are Retrospective Survey Data? Evidence from Rural Ethiopia," *Journal of Development Effectiveness*, 2024, *16* (1), 142–158.

Gaduh, Arya, Tadeja Gračner, and Alexander D. Rothenberg, "Life in the Slow Lane: Unintended Consequences of Public Transit in Jakarta," *Journal of Urban Economics*, March 2022, *128*, 103411.

Gechter, Michael and Nick Tsivanidis, "Spatial Spillovers from High-Rise Developments: Evidence from the Mumbai Mills," 2023.

Gibbons, Stephen and Stephen Machin, "Valuing Rail Access Using Transport Innovations," *Journal of Urban Economics*, 2005, *57* (1), 148–169.

Glaeser, Edward, *Triumph of the City: How Our Greatest Invention Makes Us Richer, Smarter, Greener, Healthier, and Happier*, Penguin, 2012.

González-Pampillón, Nicolás, Jordi Jofre-Monseny, and Elisabet Viladecans-Marsal, "Can Urban Renewal Policies Reverse Neighborhood Ethnic Dynamics?," *Journal of Economic Geography*, 2020, *20* (2), 419–457.

Heblich, Stephan, Stephen J Redding, and Daniel M Sturm, "The Making of the Modern Metropolis: Evidence from London," *The Quarterly Journal of Economics*, November 2020, *135* (4), 2059–2133.

Mpogole, Hosea and Samira Msangi, "Traffic Congestion in Dar Es Salaam: Implications for Workers' Productivity," *Journal of Sustainable Development*, November 2016, *9* (6), 103.

The World Bank, "Untying Dar Es Salaam's Traffic Knots, One Feeder Road at a Time," 2019.

Tsivanidis, Nick, "The Aggregate and Distributional Effects of Urban Transit Infrastructure : Evidence from Bogotá's TransMilenio," *American Economic Review (forthcoming)*, 2024.

Tyndall, Justin, "The Local Labour Market Effects of Light Rail Transit," *Journal of Urban Economics*, 2021, *124*, 103350.

Warnes, Pablo Ernesto, "Transport Infrastructure Improvements and Spatial Sorting: Evidence from Buenos Aires," 2024.

Weiwu, Laura, "Opportunity in Motion: Equilibrium Effects of a Place-based Policy on Economic Mobility," 2024.

World Bank, "With Bus Rapid Transit, African Cities Are Riding toward a Better Future," 2022.

Zárate, Román D., "Spatial Misallocation, Informality, and Transit Improvements: Evidence from Mexico City," *American Economic Review (R&R)*, 2023.

Tables and figures

Table 2: Summary stats

	(1) Baseline	(2) Endline
Structure/household-level		
Electricity in house for lighting	0.71	0.77
Street has lights	0.060	0.079
Road is paved	0.15	0.25
HH uses non-latrine toilet	0.51	0.74
Number of households in dwelling	3.44	2.65
Number of members in household	4.02	3.85
Rooms household occupies in dwelling	2.45	2.91
Number of rooms per household member	0.71	0.96
Monthly rent expected per room, Tsh	36103.8	32299.8
Own house	0.37	0.45
Rented	0.56	0.47
Share of consumption on (imputed) rent	0.17	0.19
Share of consumption on food	0.45	0.50
Share of consumption on transportation	0.11	0.11
Above Tanzania national poverty line	0.96	0.90
All initial household members moved out	.	0.19
At least one initial male/female respondent moved out	.	0.40
N	1517	1800
Individuals		
Age	36.8	39.1
Years education	8.39	8.53
Worked for pay last 7 days	0.43	0.42
Employed (as per occupation variable)	0.68	0.65
Operate any self-employed business or activity last month	0.43	0.38
Typical days worked per week	5.87	5.69
Typical hours worked per day	8.56	9.03
Wages last 7 days (1000 TSH)	35.1	30.5
Gross household income (1000 TSH)	566.2	378.0
Net household income (1000 TSH)	419.9	309.9
Gross income from self-employment (1000 TSH)	133.4	103.5
Net income from self-employment (1000 TSH)	91.5	70.0
Commutes (if employed)	0.84	0.73
Commute by walking (if employed)	0.36	0.39
Commute by public transport (if employed)	0.58	0.50
Commute time (mins)	46.7	69.2
Happiness public transport (scale 1-10)	3.21	6.23
Moved house between baseline and endline	.	0.40
Moved house between baseline and endline (renting BL)	.	0.51
N	3104	3824

Notes: The first panel of the table shows summary stats for the structures. The second panel shows summary stats for the initially-enrolled male/female respondents. Employed is defined from the question: what is your main current occupation, and indicating if someone has an usual occupation other than unemployed, too old to work, student, or housewife. Worked last 7 days is defined from the question: During the last 7 days, were you engaged in any kind of job or work for payment? Self-employment is defined from the question: Did you operate any self-employed business or do any self-employed activity over the last month (including agriculture)? Wages last 7 days is the response to: what were your average total weekly earnings from all jobs, after tax, in the last 7 days? Self-employed income is the response to: what is your gross (net) income over the last 1 month (from self-employed business/activity). Household income is the response to: what is the total gross (net) income for the household over the last 1 month from all sources? All initial household members moved out means that none of the original household (including male/female respondents) remain living in the house. At least one initial male/female respondent moved out means at least one of the male/female respondents moved out, but other household members remained. Commute is a measure that the individual works outside of the home. Modes of public transport are daladala, rickshaw, motorbike, train in baseline, with BRT an additional option in endline. Statistics weighted by constructed survey weight.

Table 3: Reduction in travel time

	Low exposure	High exposure
Reduction in log travel time to CBD with Phase 1	0.03	0.22
Average reduction in log travel time to CBD with Phases 1-4	0.05	0.07
Demeaned reduction in log travel to CBD	-0.02	0.16
N	1665	425

Notes: Table shows the predicted change in log travel to the CBD with Phase 1 compared to without any BRT and with Phases 1-4 compared to without any BRT. High exposure is households above the 80th percentile of demeaned travel time reduction, as explained in the text. Demeaned reduction is the reduction in log travel time due to Phase 1 minus the reduction in log travel time with Phases 1-4. A negative value for the demeaned reduction in log travel time implies that the realized reduction from Phase 1 was less than the expected average reduction across Phases 1-4. Statistics weighted by constructed survey weight.

Table 4: BRT usage and preferences

	Low exposure	High exposure
BRT usage		
Have you ever ridden on the BRT?	0.69	0.97
BRT used in commute (if employed)	0.03	0.22
Number BRT trips last 7 days	0.76	2.33
Rode BRT last 7 days	0.25	0.61
If so, for what reasons?		
... Work	0.33	0.37
... Shopping	0.36	0.52
... Educ	0.05	0.07
... Health	0.28	0.38
... Leisure	0.28	0.40
... Family	0.52	0.54
... Other	0.05	0.02
Out of 10, how much do you agree?		
BRT station is easily accessible	4.48	6.73
BRT driver/vehicle safe	8.80	8.18
BRT is cheap	6.90	5.63
BRT goes to places useful to me	5.96	7.43
BRT makes easier daily jobs done	7.28	7.45
BRT improves public transport options	7.62	7.43
Main constraint from using BRT		
Route not useful	0.34	0.21
Service not reliable	0.38	0.14
Limited accessibility station	0.15	0.12
Vehicle or driver safety, personal security, harrassment	0.02	0.03
N	2179	501

Notes: Table shows BRT usage and preference stats for male and female respondents, measured at endline. High exposure is households above the 80th percentile of demeaned travel time reduction, as explained in the text. Number of trips in last 7 days winsorized at the 99th percentile. Reasons for riding BRT in last 7 days are conditional on making at least one trip in the last days. Respondents could respond with multiple trip reasons. Responses measured on a scale of 1-10, 10 is favorable. Respondents could only answer one response to the main constraint from using BRT. Statistics weighted by constructed survey weight.

Table 5: Structure-level results

	(1) Self-reported time to CBD (mins)	(2) Log rent per room (expected)	(3) Different household in structure at EL
Reduction in log traveltime (demeaned)	-84.751	3.012	-0.073
	28.016***	0.434***	0.096
N	1176	1113	1215
Mean EL value	70.299	9.882	0.072
Average effect highly-exposed	-8.560	0.304	-0.007

Notes: An observation is a structure that was enrolled at baseline. Demeaned variable is equal to predicted decrease in travel time to downtown along BRT Phase 1 minus average predicted decrease in travel time to downtown along planned BRT Phases 1-4. Self-reported time to CBD is the time respondents report it would take to travel to the downtown market. Log rent per room (expected) is the cost to rent the household's section of the house divided by the number of rooms the household occupies. Owners are asked about rent if they were to rent out their section of the house. Different household instructure at EL measures whether the baseline household moved between baseline and endline survey. Standard errors clustered at aggregate spatial unit. Statistics weighted by constructed survey weight.

Table 6: Differences between final and initial residents

	(1) Education	(2) Age	(3) Employed	(4) Worked last week	(5) Income	(6) Household income
Reduction in log traveltime (demeaned)	1.823	-2.205	0.084	0.059	37.451	115.314
	0.525***	1.924	0.053	0.056	16.926**	52.680**
N	1870	1883	1888	1885	1876	1776
Mean EL value	8.187	42.370	0.678	0.391	27.456	345.818
Average effect highly-exposed	0.184	-0.223	0.008	0.006	3.783	11.647

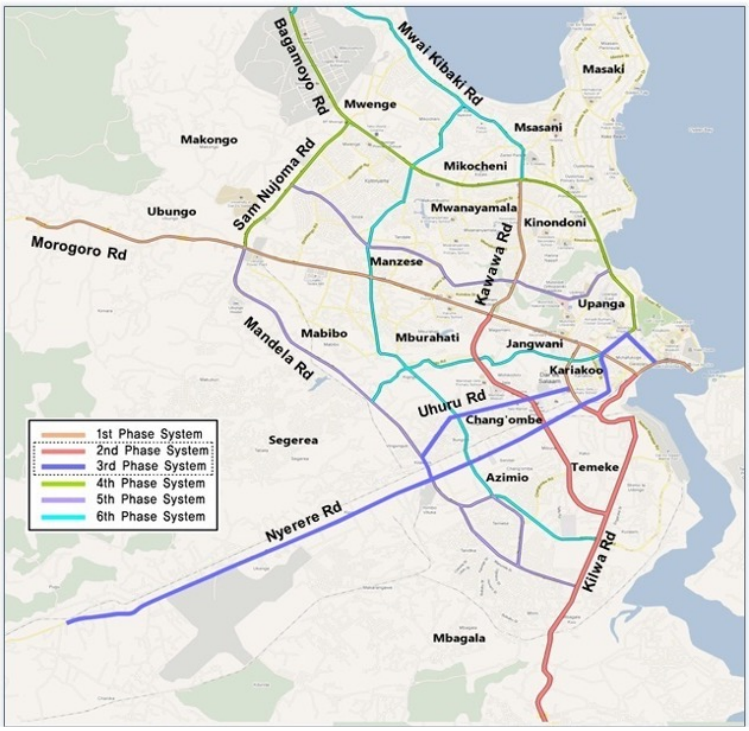
Notes: An observation is an initially-enrolled structure-by-gender pair. Dependent variable is the difference between the characteristics of the final resident and the initial resident, measured at baseline (through baseline data for a respondent who was enrolled at baseline and retrospective data for a respondent who was not enrolled at baseline) unless noted below. Demeaned variable is equal to predicted decrease in travel time to downtown along BRT Phase 1 minus average predicted decrease in travel time to downtown along planned BRT Phases 1-4. Education is measured as years of education, captured at endline for both initial and final resident. Age is age in years, measured at endline. Employment is measured by whether the household member reports an usual occupation other than unemployed, student, housewife, or too old to work. Worked last 7 days is whether the household member reports working in the last 7 days. Earnings are the earnings (in 1000 TSH) during the last seven days. Household income is the total income for all household members over the last month. Standard errors clustered at aggregate spatial unit. Statistics weighted by constructed survey weight.

Table 7: Effects of the BRT on place, initial, and final residents

	(1) Employed	(2) Worked last week	(3) Income	(4) Household income
Place-based				
Reduction in log traveltime (demeaned)	0.327	0.432	39.497	170.919
	0.144**	0.245*	22.913*	84.325**
N	1888	1888	1834	1483
Mean EL value	0.678	0.391	27.445	345.398
Average effect highly-exposed	0.033	0.044	3.989	17.263
Final residents				
Reduction in log traveltime (demeaned)	0.261	0.385	12.660	36.433
	0.165	0.240	26.358	101.239
N	1888	1885	1826	1470
Mean EL value	0.678	0.391	27.456	345.980
Average effect highly-exposed	0.026	0.039	1.279	3.680
Initial residents				
Reduction in log traveltime (demeaned)	0.307	0.400	32.919	119.296
	0.156**	0.243	22.028	82.443
N	1888	1888	1832	1502
Mean EL value	0.678	0.405	28.062	345.680
Average effect highly-exposed	0.031	0.040	3.325	12.049

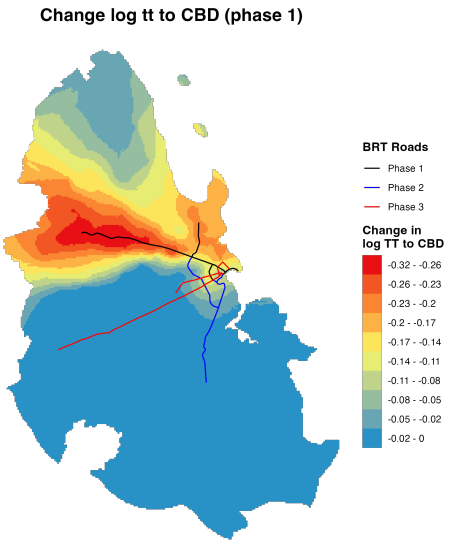
Notes: An observation is a structure by gender pair. Demeaned variable is equal to predicted decrease in travel time to downtown along BRT Phase 1 minus average predicted decrease in travel time to downtown along planned BRT Phases 1-4. Highly-exposed is the effect for households above the 80th percentile of demeaned travel time reduction, as explained in the text. Employment is measured by whether the household member reports an usual occupation other than unemployed, student, housewife, or too old to work. Worked last 7 days is whether the household member reports working in the last 7 days. Earnings are the earnings (in 1000 TSH) during the last seven days. Household income is the total income for all household members over the last month. Standard errors clustered at aggregate spatial unit. Statistics weighted by constructed survey weight.

Figure 1: BRT network in Dar es Salaam

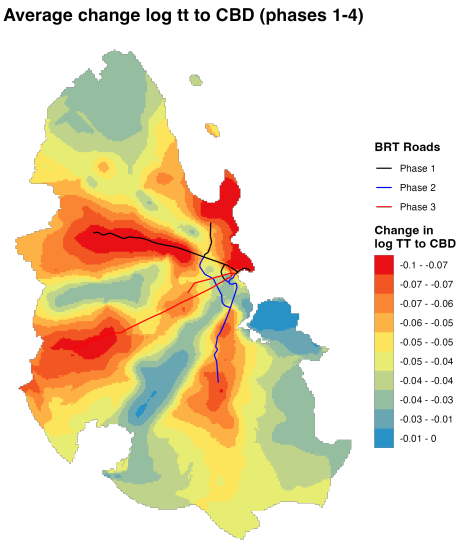


Notes: The figure shows the planned 6-phase BRT network in Dar es Salaam. We examine the impact of Phase 1 that runs northwest along Morogoro Road. Image source: Dar Rapid Transit (DART).

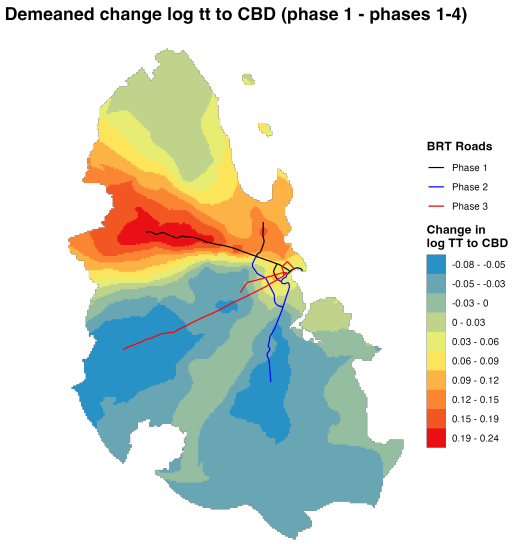
Figure 2: Reduction in log travel time to CBD due to BRT



(a) Change (Phase 1)



(b) Change (average across Phases 1-4)

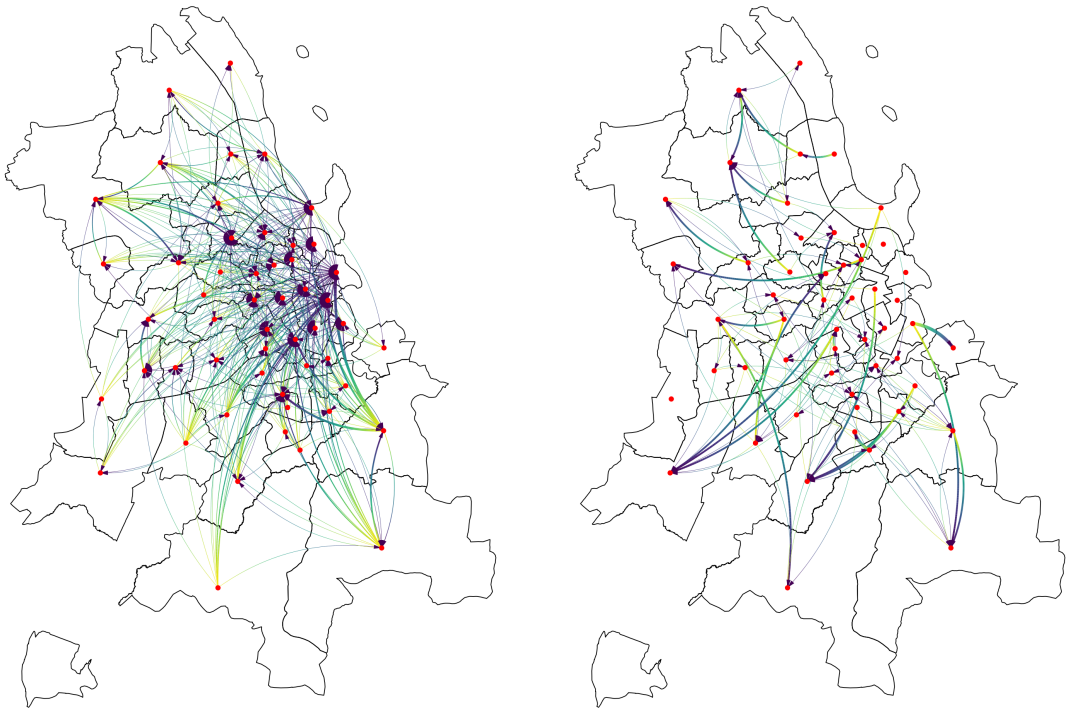


(c) Demeaned travel time

Notes: The figure shows the calculated reduction in log travel time to the CBD from phases of the BRT. Panel (a) shows the value comparing Phase 1 to no BRT. Panel (b) shows the average reduction in travel time to CBD from Phases 1-4. Panel (c) shows the value after demeaning log travel time by the average decrease for Phases 1 to 4.

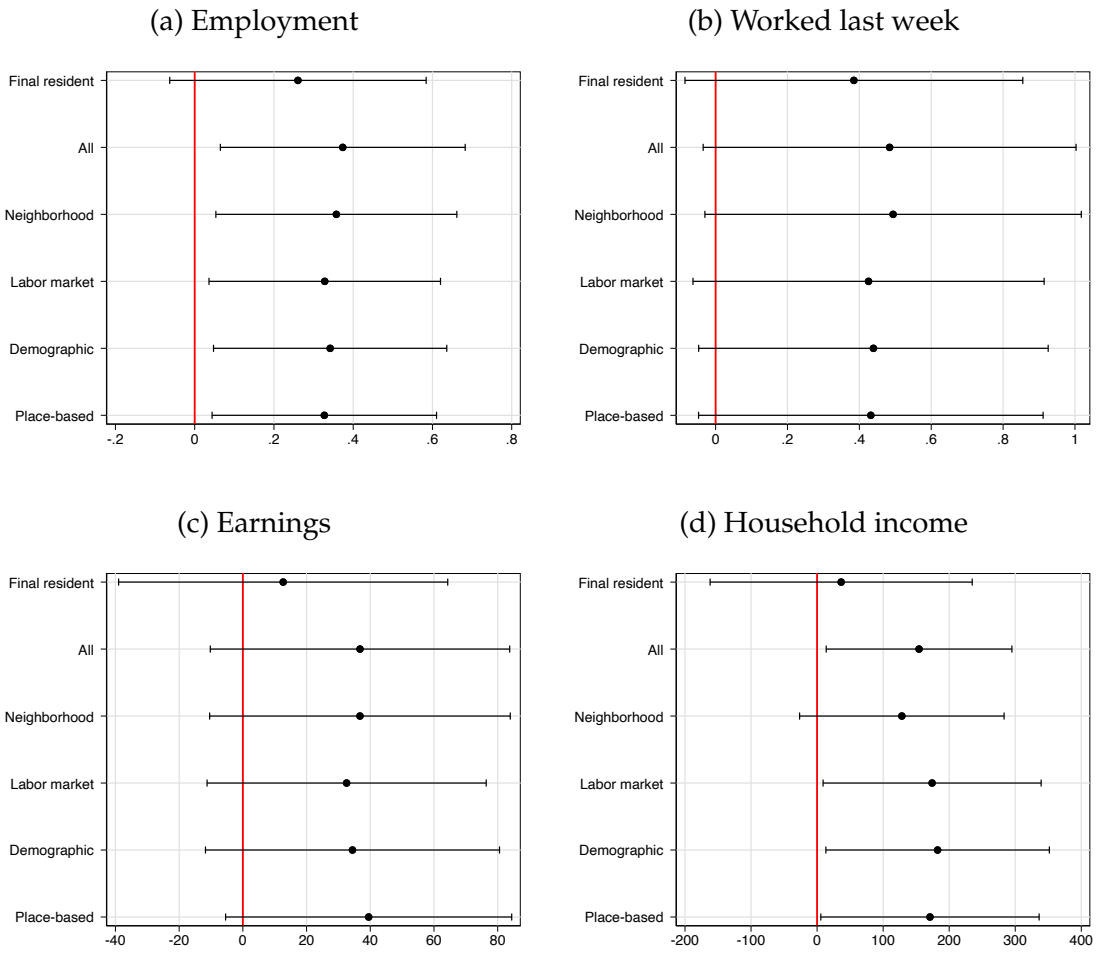
Figure 3: Commuting and migration flows

(a) Baseline commuting flows (b) Migration flows from baseline to endline



Notes: The figure shows directional baseline commuting flows (left hand panel) and migration flows of baseline respondents between baseline and endline (right hand panel). Direction of travel is shown from yellow towards purple. Arrow thickness indicates magnitude of commuting or migrant flows.

Figure 4: Decomposing the selection effect



Notes: The figure plots the point estimate and 95th percentile confidence interval of the demeaned coefficient from the place-based regression. An observation is a structure x gender pair. Place-based effect is the place-based coefficient. Selection is accounted for by allowing Post Double Selection Lasso to select the difference in endline characteristics of the baseline and endline respondents within the structure. Demographic adds the change in demographic outcomes (age and years of education) between the final and initial resident as a potential control. Labor market is the change between being employed and weekly earnings (both measured at baseline) between final and initial resident. Neighborhood is the change in location fixed effects at baseline. All includes demographic, labor market, and neighborhood controls. The final resident treatment effect is included for comparison. Standard errors clustered at aggregate spatial unit. Statistics weighted by constructed survey weight.

APPENDIX: FOR ONLINE PUBLICATION ONLY

Figure A1: Timeline of research and operational activities

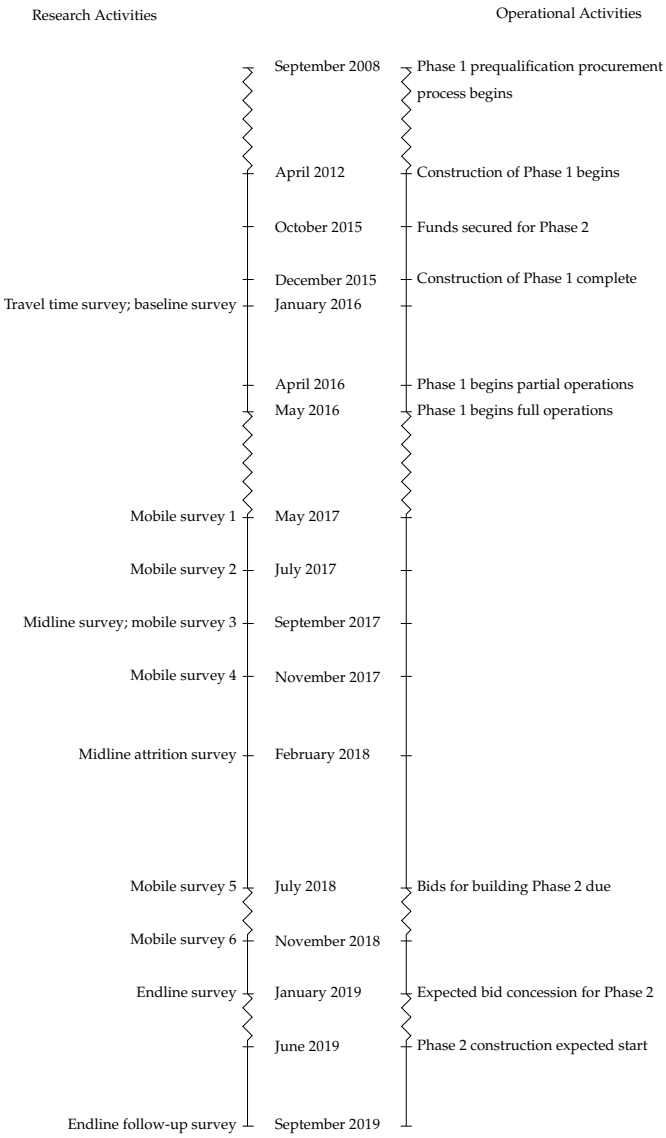
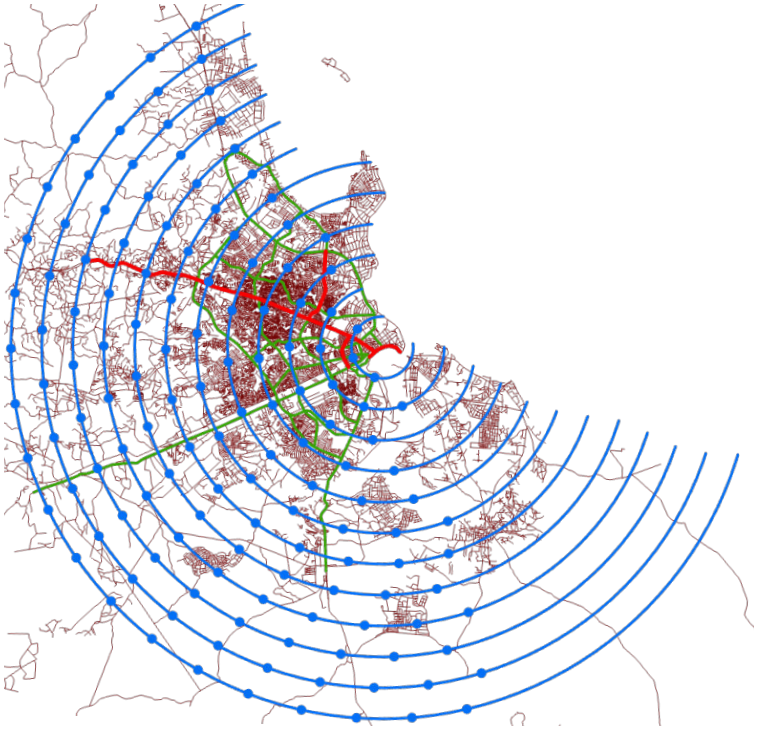
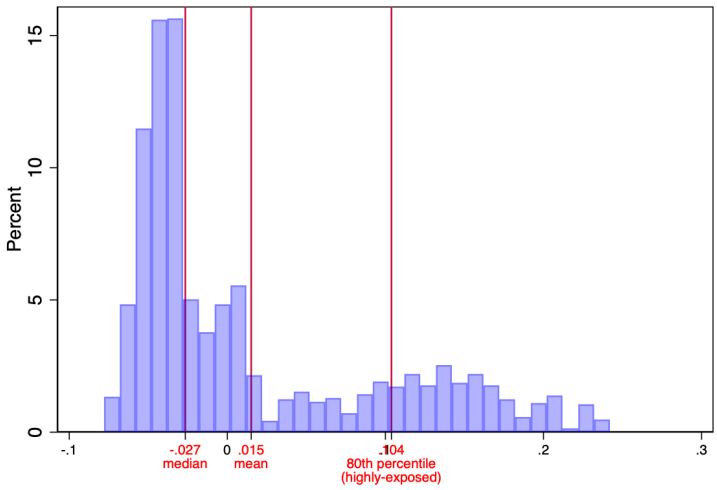


Figure A2: Spatial sampling frame



Notes: The figure shows the spatial sampling frame used to construct the sample. We constructed 12 arcs at radii increasing at 1.5km intervals from the CBD, shown in the figure as blue arcs. We enrolled households in 125 of the 141 clusters shown as blue dots along these arcs; 12-14 households were enrolled in each cluster, yielding a sample of 1748 households. The full BRT network is shown in green, with the Phase 1 route highlighted in red.

Figure A3: Distribution of demeaned log travel time



Notes: This figure shows the distribution of demeaned log travel time to the CBD. A unit of observation is a baseline structure. The 80th percentile of the demeaned distribution is shown on the figure. The 80th percentile value is used in the tables and text to indicate an "highly exposed" household.

Table A1: Contact rates of sample at endline

	(1) All	(2) Located ML
Structures		
Found and survey complete	76.0	87.5
Found and refused /incomplete	10.0	3.6
Not found	8.2	2.1
Torn down / empty	5.8	6.7
N	1748	1517
Households		
Found and survey complete	79.6	92.2
Found and refused/incomplete	9.2	2.3
Not found	11.2	5.6
N	1748	1510
Male/female respondents		
Found and survey complete	69.9	81.3
Found and refused /incomplete	13.1	6.0
Not found	15.1	10.6
Died	1.8	2.1
N	3104	2668

Notes: Table shows percent in each status. We initially enrolled 1748 structures and households. Up to two individual respondents (one male, one female) were enrolled per household. Column (2) shows contact rates conditional on locating at least one member of the household in the midline attrition survey. We stopped tracking structures after the midline attrition survey if they (i) refused, or (ii) we were not able to locate either the structure or the household after exhausting all contact information available. We did not attempt to find non-tracked sample at endline. Statistics are unweighted.

Table A2: Correlates of attrition at the individual and structure level

	(1) Structure	(2) Individual
Log BL HH monthly gross income per capita	-0.032***	-0.030**
	(0.010)	(0.012)
Log BL monthly rent per room	-0.035***	-0.020
	(0.013)	(0.013)
BL number of years lived in the structure	0.002	-0.001
	(0.001)	(0.001)
BL number of HH in the structure	0.006	0.009
	(0.006)	(0.006)
Dwelling rented by HH	-0.145**	-0.136***
	(0.055)	(0.036)
Dwelling owned by HH	0.100**	0.009
	(0.046)	(0.030)
BL distance to phase 1	0.004	0.002
	(0.003)	(0.003)
BL distance to phase 2	0.005	-0.001
	(0.004)	(0.003)
BL Age		-0.000
		(0.001)
Male dummy		-0.057***
		(0.014)
N	1748	2646
Mean dependent variable	0.760	0.699

Notes: Dependent variable is completing the endline survey. A unit of analysis is either a structure that was enrolled at baseline (Column (1)) or an individual that was enrolled at baseline (Column (2)). Standard errors, clustered by aggregate spatial units, are reported in parentheses. Missing values for the independent variables are dummied out.

Table A3: Categorization of treatment effects

	(1) Male	(2) Female	(3) All
Struc, initial, final	859	1029	1888
Struc, final only	72	104	176
Initial only	139	143	282
Final only	47	27	74
None (BL resp exists)	409	345	754
None (no BL resp)	220	98	318
N	1746	1746	3492

Notes: An observation is a structure x gender. Sample is restricted to structures present at BL.

Table A4: Summary stats (unweighted)

	(1) Baseline	(2) Endline
Structure/household-level		
Electricity in house for lighting	0.62	0.73
Street has lights	0.028	0.034
Road is paved	0.098	0.18
HH uses non-latrine toilet	0.52	0.73
Number of households in dwelling	2.01	2.05
Number of members in household	4.50	4.16
Rooms household occupies in dwelling	3.02	3.34
Number of rooms per household member	0.79	1.02
Monthly rent expected per room, Tsh	38164.6	29389.6
Own house	0.68	0.65
Rented	0.23	0.26
Share of consumption on (imputed) rent	0.19	0.18
Share of consumption on food	0.43	0.50
Share of consumption on transportation	0.11	0.11
Above Tanzania national poverty line	0.95	0.89
All initial household members moved out	.	0.11
At least one initial male/female respondent moved out	.	0.33
N	1517	1800
Individuals		
Age	37.8	40.1
Years education	8.25	8.45
Worked for pay last 7 days	0.40	0.40
Employed (as per occupation variable)	0.68	0.66
Operate any self-employed business or activity last month	0.44	0.39
Typical days worked per week	5.85	5.74
Typical hours worked per day	8.38	9.09
Wages last 7 days (1000 TSH)	34.6	29.0
Gross household income (1000 TSH)	544.7	418.2
Net household income (1000 TSH)	387.6	339.5
Gross income from self-employment (1000 TSH)	134.4	107.0
Net income from self-employment (1000 TSH)	89.8	72.9
Commutes (if employed)	0.79	0.70
Commute by walking (if employed)	0.37	0.36
Commute by public transport (if employed)	0.56	0.50
Commute time (mins)	49.2	88.0
Happiness public transport (scale 1-10)	2.85	5.85
Moved house between baseline and endline	.	0.29
Moved house between baseline and endline (renting BL)	.	0.50
N	3104	3824

Notes: The first panel of the table shows summary stats for the structures. The second panel shows summary stats for the initially-enrolled male/female respondents. Employed is defined from the question: what is your main current occupation, and indicating if someone has an usual occupation other than unemployed, too old to work, student, or housewife. Worked last 7 days is defined from the question: During the last 7 days, were you engaged in any kind of job or work for payment? Self-employment is defined from the question: Did you operate any self-employed business or do any self-employed activity over the last month (including agriculture)? Wages last 7 days is the response to: what were your average total weekly earnings from all jobs, after tax, in the last 7 days? Self-employed income is the response to: what is your gross (net) income over the last 1 month (from self-employed business/activity). Household income is the response to: what is the total gross (net) income for the household over the last 1 month from all sources? All initial household members moved out means that none of the original household (including male/female respondents) remain living in the house. At least one initial male/female respondent moved out means at least one of the male/female respondents moved out, but other household members remained. Commute is a measure that the individual works outside of the home. Modes of public transport are daladala, rickshaw, motorbike, train in baseline, with BRT an additional option in endline. Statistics unweighted.

Table A5: Location of tracked individuals between baseline and endline

	(1) Overall share	(2) Share if found
Same structure	57	76
Moved within Dar	13	18
Left Dar	3	4
Died	2	2
Unknown	25	
Total	100	100
N	3104	2331

Notes: Table shows the location of individuals enrolled at baseline. We are able to fill in some location information from household members and tracking information even if the respondent didn't complete the survey directly.

Table A6: Why did individuals move

	Share
Family reasons	31
Proximity to public transport, work, education, or social services	21
Community/safety/attractiveness of area	20
Cheaper rent	14
Other	13
Total	100
N	504

Notes: Table shows answers to the question: "What is the main reason you moved house?".

Table A7: Time use

	Low exposure	High exposure
Work	4.09	4.86
Travel to/from work	0.63	0.70
Travel to/from other activities	1.90	1.73
Education	0.16	0.42
Sleep	7.92	7.48
Rest of day	9.36	9.09
N	2021	461

Notes: Table shows time use at endline, measured in hours, across activities for individual respondents. High exposure is households above the 80th percentile of demeaned travel time reduction, as explained in the text. Rest of day is computed from 24 hours minus the sum of other activities. Statistics weighted by constructed survey weight.

Table A8: Pre-trends analysis

	(1) Log rent per room (expected)	(2) Lived in house < 3 yrs	(3) Typical number of hours worked per week	(4) Wealth index (PCA)	(5) Earnings in last 7 days (1000 TSH)
De-meaned pred. decrease TT CBD Ph1	0.584	-0.027	-2.400	0.123	61.692
	0.656	0.365	13.915	1.164	40.175
N	81	81	81	81	81
Mean baseline value	10.257	0.327	52.466	-0.114	33.491

Notes: Unit of observation is enumeration area (EA). Individual-level data is weighted using constructed survey weight when collapsed at the EA level. Standard errors are clustered at relevant aggregate spatial unit. Treatment is defined at the EA level, where pre-period structures lie within an EA and post-period structures are assigned to the nearest EA, regardless of distance; however, only post-period structures that lie within 2 km of an EA are included in the sample. Pre-period data come from WB Measuring Living Standards in Cities 2014-2015 survey, and post-period data come from Dar BRT baseline survey.

Table A9: Differences between final and initial residents (structures rented at BL)

	(1) Education	(2) Age	(3) Employed	(4) Worked last week	(5) Income	(6) Household income
Reduction in log traveltime (demeaned)	3.930	-1.407	0.426	0.315	72.124	-22.805
	1.979**	3.970	0.167**	0.206	26.384***	105.784
N	296	300	302	301	297	277
Mean EL value	8.355	36.803	0.666	0.405	31.145	301.206
Average effect highly-exposed	0.397	-0.142	0.043	0.032	7.285	-2.303

Notes: An observation is an initially-enrolled structure-by-gender pair. Dependent variable is the difference between the characteristics of the final resident and the initial resident, measured at baseline (through baseline data for a respondent who was enrolled at baseline and retrospective data for a respondent who was not enrolled at baseline) unless noted below. Demeaned variable is equal to predicted decrease in travel time to downtown along BRT Phase 1 minus average predicted decrease in travel time to downtown along planned BRT Phases 1-4. Education is measured as years of education, captured at endline for both initial and final resident. Age is age in years, measured at endline. Employment is measured by whether the household member reports an usual occupation other than unemployed, student, housewife, or too old to work. Worked last 7 days is whether the household member reports working in the last 7 days. Earnings are the earnings (in 1000 TSH) during the last seven days. Household income is the total income for all household members over the last month. Standard errors clustered at aggregate spatial unit. Statistics weighted by constructed survey weight.

Table A10: Effects of the BRT on place, initial, and final residents (structures rented at BL)

	(1) Employed	(2) Worked last week	(3) Income	(4) Household income
Place-based				
Reduction in log traveltime (demeaned)	0.735	1.047	77.198	111.621
	0.238***	0.353***	44.971*	184.781
N	302	302	295	256
Mean EL value	0.666	0.407	31.058	299.855
Average effect highly-exposed	0.074	0.106	7.797	11.274
Final residents				
Reduction in log traveltime (demeaned)	0.302	0.692	14.051	196.647
	0.246	0.355*	58.670	189.871
N	302	301	290	248
Mean EL value	0.666	0.405	31.145	301.201
Average effect highly-exposed	0.030	0.070	1.419	19.861
Initial residents				
Reduction in log traveltime (demeaned)	0.662	0.868	45.374	53.849
	0.251***	0.349**	31.319	174.118
N	302	302	294	260
Mean EL value	0.689	0.447	32.591	316.446
Average effect highly-exposed	0.067	0.088	4.583	5.439

Notes: An observation is a structure by gender pair. Demeaned variable is equal to predicted decrease in travel time to downtown along BRT Phase 1 minus average predicted decrease in travel time to downtown along planned BRT Phases 1-4. Highly-exposed is the effect for households above the 80th percentile of demeaned travel time reduction, as explained in the text. Employment is measured by whether the household member reports an usual occupation other than unemployed, student, housewife, or too old to work. Worked last 7 days is whether the household member reports working in the last 7 days. Earnings are the earnings (in 1000 TSH) during the last seven days. Household income is the total income for all household members over the last month. Standard errors clustered at aggregate spatial unit. Statistics weighted by constructed survey weight.

Table A11: Effects of the BRT on place, initial, and final residents (unweighted estimates)

	(1) Employed	(2) Worked last week	(3) Income	(4) Household income
Place-based				
Reduction in log traveltime (demeaned)	0.327	0.432	39.497	170.919
	0.144**	0.245*	22.913*	84.325**
N	1888	1888	1834	1483
Mean EL value	0.678	0.391	27.445	345.398
Average effect highly-exposed	0.033	0.044	3.989	17.263
Final residents				
Reduction in log traveltime (demeaned)	0.261	0.385	12.660	36.433
	0.165	0.240	26.358	101.239
N	1888	1885	1826	1470
Mean EL value	0.678	0.391	27.456	345.980
Average effect highly-exposed	0.026	0.039	1.279	3.680
Initial residents				
Reduction in log traveltime (demeaned)	0.307	0.400	32.919	119.296
	0.156**	0.243	22.028	82.443
N	1888	1888	1832	1502
Mean EL value	0.678	0.405	28.062	345.680
Average effect highly-exposed	0.031	0.040	3.325	12.049

Notes: An observation is a structure by gender pair. Demeaned variable is equal to predicted decrease in travel time to downtown along BRT Phase 1 minus average predicted decrease in travel time to downtown along planned BRT Phases 1-4. Highly-exposed is the effect for households above the 80th percentile of demeaned travel time reduction, as explained in the text. Employment is measured by whether the household member reports an usual occupation other than unemployed, student, housewife, or too old to work. Worked last 7 days is whether the household member reports working in the last 7 days. Earnings are the earnings (in 1000 TSH) during the last seven days. Household income is the total income for all household members over the last month. Standard errors clustered at aggregate spatial unit. Statistics weighted by constructed survey weight. Sample is unweighted.

Table A12: Effects of the BRT on place, initial, and final residents (no baseline pdslasso controls)

	(1) Employed	(2) Worked last week	(3) Income	(4) Household income
Place-based				
Reduction in log traveltime (demeaned)	0.318	0.427	43.422	345.192
	0.144**	0.273	23.044*	169.046**
N	1888	1888	1834	1483
Mean EL value	0.678	0.391	27.445	345.398
Average effect highly-exposed	0.032	0.043	4.386	34.864
Final residents				
Reduction in log traveltime (demeaned)	0.252	0.336	6.774	197.077
	0.162	0.262	24.506	159.965
N	1888	1885	1826	1470
Mean EL value	0.678	0.391	27.456	345.980
Average effect highly-exposed	0.025	0.034	0.684	19.905
Initial residents				
Reduction in log traveltime (demeaned)	0.298	0.408	39.018	263.715
	0.156*	0.257	22.523*	162.569
N	1888	1888	1832	1502
Mean EL value	0.678	0.405	28.062	345.680
Average effect highly-exposed	0.030	0.041	3.941	26.635

Notes: An observation is a structure by gender pair. Demeaned variable is equal to predicted decrease in travel time to downtown along BRT Phase 1 minus average predicted decrease in travel time to downtown along planned BRT Phases 1-4. Highly-exposed is the effect for households above the 80th percentile of demeaned travel time reduction, as explained in the text. Employment is measured by whether the household member reports an usual occupation other than unemployed, student, housewife, or too old to work. Worked last 7 days is whether the household member reports working in the last 7 days. Earnings are the earnings (in 1000 TSH) during the last seven days. Household income is the total income for all household members over the last month. Standard errors clustered at aggregate spatial unit. Statistics weighted by constructed survey weight.

Table A13: Effects of the BRT on place, initial, and final residents (using only retro responses)

	(1) Employed	(2) Worked last week	(3) Income	(4) Household income
Place-based				
Reduction in log traveltime (demeaned)	0.350	0.483	33.849	191.428
	0.149**	0.247*	23.306	83.125**
N	1888	1888	1834	1483
Mean EL value	0.678	0.391	27.445	345.398
Average effect highly-exposed	0.035	0.049	3.419	19.334
Final residents				
Reduction in log traveltime (demeaned)	0.257	0.401	22.727	107.076
	0.162	0.249	25.660	82.844
N	1888	1885	1826	1470
Mean EL value	0.678	0.391	27.456	345.980
Average effect highly-exposed	0.026	0.040	2.295	10.815
Initial residents				
Reduction in log traveltime (demeaned)	0.268	0.405	29.451	132.740
	0.163*	0.236*	22.017	84.447
N	1888	1888	1832	1502
Mean EL value	0.678	0.405	28.062	345.680
Average effect highly-exposed	0.027	0.041	2.975	13.407

Notes: An observation is a structure by gender pair. Demeaned variable is equal to predicted decrease in travel time to downtown along BRT Phase 1 minus average predicted decrease in travel time to downtown along planned BRT Phases 1-4. Highly-exposed is the effect for households above the 80th percentile of demeaned travel time reduction, as explained in the text. Employment is measured by whether the household member reports an usual occupation other than unemployed, student, housewife, or too old to work. Worked last 7 days is whether the household member reports working in the last 7 days. Earnings are the earnings (in 1000 TSH) during the last seven days. Household income is the total income for all household members over the last month. Standard errors clustered at aggregate spatial unit. Statistics weighted by constructed survey weight.

Table A14: Treatment effects: additional income and employment variables

	(1) Income from self-employment	(2) Works at least 4 hours a day	(3) Self employed last month
Place-based			
Reduction in log traveltime (demeaned)	-15.528	-0.144	-0.094
	39.254	0.154	0.167
N	1815	1882	1882
Mean EL value	74.256	0.327	0.412
Average effect highly-exposed	-1.568	-0.015	-0.010
Final residents			
Reduction in log traveltime (demeaned)	-63.404	-0.111	-0.040
	44.494	0.166	0.181
N	1811	1674	1674
Mean EL value	73.437	0.331	0.422
Average effect highly-exposed	-6.404	-0.011	-0.004
Initial residents			
Reduction in log traveltime (demeaned)	2.410	-0.021	0.042
	38.850	0.128	0.136
N	1812	1882	1882
Mean EL value	74.211	0.327	0.417
Average effect highly-exposed	0.243	-0.002	0.004

Notes: An observation is a structure by gender pair. Demeaned variable is equal to predicted decrease in travel time to downtown along BRT Phase 1 minus average predicted decrease in travel time to downtown along planned BRT Phases 1-4. Self employment is the individual net income from self employment in the last month, if the individual operates a self-employment business. It is measures in 1000 TSH. Household income is the total net household income from all sources in the last month (measured in 1000 TSH). We only provide the household average as it is a household variable. Works at least four hours a day is equal to one if the respondent reports their usual daily hours are at least four. It is set to zero if the respondent does not work. Worked last 7 days is equal to one if the respondent reports earning income in the last 7 days. It is zero otherwise. Self employed last month is equal to one if the respondent reports earning income from self employment in the last month. It is zero otherwise. Standard errors clustered at aggregate spatial unit. Statistics weighted by constructed survey weight.

Table A15: Effects of the BRT on place, initial, and final residents (using demeaned 123 instead of 1234)

	(1) Employed	(2) Worked last week	(3) Income	(4) Household income
Place-based				
Reduction in log traveltime (demeaned)	0.327	0.432	39.497	170.919
	0.144**	0.245*	22.913*	84.325**
N	1888	1888	1834	1483
Mean EL value	0.678	0.391	27.445	345.398
Average effect highly-exposed	0.033	0.044	3.989	17.263
Final residents				
Reduction in log traveltime (demeaned)	0.261	0.385	12.660	36.433
	0.165	0.240	26.358	101.239
N	1888	1885	1826	1470
Mean EL value	0.678	0.391	27.456	345.980
Average effect highly-exposed	0.026	0.039	1.279	3.680
Initial residents				
Reduction in log traveltime (demeaned)	0.307	0.400	32.919	119.296
	0.156**	0.243	22.028	82.443
N	1888	1888	1832	1502
Mean EL value	0.678	0.405	28.062	345.680
Average effect highly-exposed	0.031	0.040	3.325	12.049

Notes: An observation is a structure by gender pair. Demeaned variable is equal to predicted decrease in travel time to downtown along BRT Phase 1 minus average predicted decrease in travel time to downtown along planned BRT Phases 1-4. Highly-exposed is the effect for households above the 80th percentile of demeaned travel time reduction, as explained in the text. Employment is measured by whether the household member reports an usual occupation other than unemployed, student, housewife, or too old to work. Worked last 7 days is whether the household member reports working in the last 7 days. Earnings are the earnings (in 1000 TSH) during the last seven days. Household income is the total income for all household members over the last month. Standard errors clustered at aggregate spatial unit. Statistics weighted by constructed survey weight.

Table A16: Treatment effects: commute

	(1) Commutes	(2) Happy public transport	(3) Live convenient
Place-based			
Reduction in log traveltime (demeaned)	0.162	5.684	6.809
	0.217	1.731***	1.730***
N	1888	1882	1799
Mean EL value	0.321	5.764	6.118
Average effect highly-exposed	0.016	0.574	0.688
Final residents			
Reduction in log traveltime (demeaned)	0.073		
	0.199		
N	1888		
Mean EL value	0.321		
Average effect highly-exposed	0.007		
Initial residents			
Reduction in log traveltime (demeaned)	0.108	5.073	5.747
	0.202	1.569***	1.712***
N	1888	1882	1796
Mean EL value	0.329	5.764	6.119
Average effect highly-exposed	0.011	0.512	0.580

Notes: An observation is a structure by gender pair. Demeaned variable is equal to predicted decrease in travel time to downtown along BRT Phase 1 minus average predicted decrease in travel time to downtown along planned BRT Phases 1-4. Commute is a measure that the individual works outside of the home. Happy public transport is the response, out of 10, to the question: how happy are you with the public transport options from the house. Live convenient is the response, out of 10, to agreeing with the statement: where I live is convenient for where I want to go to? Standard errors clustered at aggregate spatial unit. Statistics weighted by constructed survey weight.

Table A17: Treatment effects: cons

	(1) Food	(2) Transport	(3) Total
Place-based			
Reduction in log traveltime (demeaned)	143.051	50.456	223.356
	42.847***	30.378*	104.997**
N	1789	1589	1888
Mean EL value	243.975	59.466	398.091
Average effect highly-exposed	14.448	5.096	22.559
Final residents			
Reduction in log traveltime (demeaned)	125.123	37.489	209.001
	49.952**	28.197	115.483*
N	1774	1586	1888
Mean EL value	244.268	59.250	398.091
Average effect highly-exposed	12.637	3.786	21.109
Initial residents			
Reduction in log traveltime (demeaned)	100.787	40.680	184.614
	45.707**	24.191*	82.274**
N	1793	1600	1888
Mean EL value	241.057	59.038	395.909
Average effect highly-exposed	10.180	4.109	18.646

Notes: An observation is a structure by gender pair. Demeaned variable is equal to predicted decrease in travel time to downtown along BRT Phase 1 minus average predicted decrease in travel time to downtown along planned BRT Phases 1-4. Food spending is spending on food. Transport spending is spending on transport. Total is total spending. Data is measured at the household level. Standard errors clustered at aggregate spatial unit. Statistics weighted by constructed survey weight.

A Construction of household weights

We construct a household weight that reweights our sample to match the 2012 Tanzanian Census on ownership population and the distribution of population across wards. This section describes the procedure.[15]

Population data for enumeration areas in Dar es Salaam were obtained from the country's most recent population census, the 2012 Population and Housing Census. This gives aggregated total population counts by sex for each enumeration area. The surveyed area covers 87% of the population of Dar es Salaam.

Define a structure s such that the total population of structures in our data at time t, $t = \{0, 1\}$ is n_t. Within each of these structures, define survey household a_s, which is indexed by two additional dimensions: $b \in \{owner, resident\}$ and $c = \{1, 2, ..., 5+\}$, where c is a categorical variable for the number of households in structure s. N_hh_{st} is the total number of households living in structure s at time t, a inclusive. Then, the total population of households at time $t = \sum_s N_hh_{st}$. Let I be the set of all households. At each time period t, we surveyed a subset $x \in I$, $x = \{a_1, a_2, ..., a_{n_t}\}$. Define our actual surveying probability as $\tilde{\pi}_{bct}$ and the population probability as $\pi_{bct} \forall b, c$. Then, the weights are given by $\Omega_{bct} = N_hh_{st}(\pi_{bct}/\tilde{\pi}_{bct})$. If a renter household lives in a structure with all renters, $\pi_{bct} = \tilde{\pi}_{bct}$ and thus $\Omega_{bct} = N_hh_{st}$.

At baseline, we did not ask renters who lived in dwellings with more than one household whether the owner was one of the other households that lived in the dwelling. As a result, we cannot determine the rate at which we over- or under-sampled owners at baseline. Thus, we ammend the previous section such that π_{bc} and $\tilde{\pi}_{bc}$ are both time-invariant, with $\pi_{bc} = \pi_{bc,t=1}$ and $\tilde{\pi}_{bc} = \tilde{\pi}_{bc,t=1}$.

We know the number of individuals living in each of our 52 aggregate spatial units from the 2012 Tanzania Census; however, we want to know how many households reside in each unit. To convert from individuals to households, we divide the total number of individuals by the average number of members in a household in Dar (3.9), per the 2011/2012 Tanza-

[15] In our survey, we group households that live in their structure for free with households that own their structure when asking questions related to the dwelling, i.e. do you sublet any part of this dwelling. To be consistent, we do the same when generating weights.

nia HIV/AIDS and Malaria Indicator Survey (AIS).[16]

Appendix Table A18 shows the distribution of structures across ownership status. Column (1) shows the raw data. Column (2) shows the data after applying the constructed weights. Column (3) shows the distribution arising from the 2012 Census. Column (4) compares the data from the World Bank MLSC data that we use for pretrends.

Table A18: Distribution of households by ownership status of dwelling units

	Endline data		2012 Census data	World Bank MLSC data
	(1) Unweighted	(2) Weighted	(3)	(4)
Owned	65.0	44.7	36.9	36.8
Lived in without paying rent	8.8	8.4	5.5	6.4
Rented	26.2	46.9	57.6	56.8
N	1795	1795	1083381	1964

Notes: Endline data sample consists of all households that answered StatDwel at endline. Constructed weight accounts for number of households in the structure, distribution of owners and renters, and population across city wards.

[16] Accessed via https://www.statcompiler.com/en/

B Validation of retrospective data

To assess the reliability of the retrospective data we collected at endline, we identify individuals who were surveyed at both baseline and endline and compare their baseline responses with their endline retrospective responses for key outcome variables of interest.

For binary or categorical outcomes, such as employment status or the aggregate spatial unit one resides in, we calculate the proportion of respondents who gave consistent answers at baseline and retrospectively at endline. For continuous variables, we report the share of responses that fall within 10% or 20% of each other across the two surveys.

Appendix Table A19 presents the results of this exercise. Binary and categorical variables show higher consistency when reported retrospectively, whereas continuous variables are less likely to be reported with similar values at baseline and endline.

Home aggregate spatial unit has a lower sample size as it is only asked retrospectively to individuals who have moved to a new structure at endline.

Table A19: Comparison between individuals' baseline and endline retrospective responses

	(1) Employed (main job)	(2) Commutes (distance to work >0)	(3) Home aggregate spatial unit	(4) Wages in last 7 days	(5) Gross monthly HH income	(6) Total monthly HH consumption
Prop. consistent	0.775	0.797	0.690	0.296	0.066	0.013
Prop. within 10%				0.306	0.097	0.096
Prop. within 20%				0.333	0.195	0.194
Correlation coef.	0.467	0.471	0.666	0.221	0.237	0.179
N	2164	862	525	2032	1556	2864

Notes: Employment status is determined by the respondent's reported main occupation, and is a binary variable. Commute status is defined as one if a respondent reports working and the distance to their work is greater than 0 km, and is defined as zero if the respondent reports not working or reports working but the distance to their work is 0 km. Prop. consistent shows the proportion of individuals whose EL retrospective response matches their BL response – this is primarily informative for the binary and categorical variables. Prop. within 10% [20%] shows the proportion of individuals whose EL retrospective response was within 10% [20%] of their BL response – this is only informative for the continuous variables.