No Place Like Home? The Causal Effect of Forced Relocation from Central Addis Ababa *

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Abstract

Do central slums provide essential economic and social benefits to the poor? We collected bespoke data for 5,000 households to study mass forced clearances in Addis Ababa. Evictees were offered alternative subsidized housing further from the center. Exploiting sharp clearance zone boundaries, regression-discontinuity estimates show negative impacts on social networks, but positive impacts on work, earnings, housing quality and environmental amenity. Relocating households close to their ex-ante neighbors eliminates social costs. Slums are not essential: relocation policies can be designed to fully compensate residents, and the sale value of cleared land more than covers the cost.

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Central slums – communities of low income people, living in poor quality housing, in central locations – are a defining feature of developing country cities. Central slums also split opinion. To some they should be prized and preserved, to others they should be condemned and cleared.

For preservers, central slums are an irreplaceable resource for the poor and represent a fragile balance between housing quality, centrality and community (Young and Wilmott, 1957; Jacobs, 1961).[1] On this account, low quality housing is a feature not a bug. Small, crowded houses with limited access to water and sanitation don't attract the wealthy. This keeps housing affordable, allowing communities of poor people to survive in central locations. The community provides essential social services and access to work, but the network is fragile and subject to collapse if disturbed (Perlman, 1976). Centrality provides essential access to jobs and other amenities that cannot be had elsewhere. Labor markets are not integrated, jobs are scarce on the periphery, and poorly function transport markets mean commuting is infeasible (Glaeser et al., 2008).

Clearers, in contrast, see central slums as a cause of urban poverty (de Siqueira Filha et al., 2022; Alam and Mahal, 2014). Low quality housing represents a failure to provide public goods, harms the health and earnings of the poor, and keeps them trapped in poverty (Marx et al. 2013; Feler and Henderson 2011).[2] Clearance presents an opportunity to improve living conditions and comes at minimal costs because social networks are robust to disruptions, and well functioning labor, housing, and transport markets mean that a central location is not essential. Clearance frees misallocated land for redevelopment, allows economic growth, and provides government with funds to compensate the displaced (Henderson et al. 2021; Lall et al. 2008). For clearers, preservation not only harms the poor, it stifles economic growth and development.

Despite the importance of this debate, and a growing literature that studies relocation in developing country cities, we lack a clear understanding of which view is correct. Important work that shows limited negative impacts of voluntary relocation cannot rule out the large negatives that preservers fear, given a reasonable assumption that volunteers will fare better (Barnhardt et al. 2017; Franklin 2025; Belchior et al. 2023). Papers that study small programs and find minimal upside cannot rule out the positives that clearers expect, given a reasonable hypothesis that small programs do not allow social networks to coordinate their movement, nor the government to realize increased land values (Barnhardt et al. 2017). Studies showing the welfare costs of punitive or low quality programs don't challenge the views of clearers who agree on the need to compensate (Carrillo et al. 2023). Finally, influential recent work that shows the value of a central location, by comparing households from central slums that received better housing on the periphery to those who received better housing centrally, does not challenge clearers views that good housing on the periphery is better than bad housing in the center (Rojas-Ampuero 2022).

[1] We use the term slum throughout as a descriptor of low quality housing. This follows the UN definition that defines a home as being a slum if it lacks: durability; sufficient living area; access to water; access to sanitation; or secure tenure.

[2] The precarity of living without secure tenure may limit labor supply (Field, 2007; Franklin, 2020).

We fill the gap by studying a large-scale forced eviction program in Addis Ababa, which relocated over 40,000 households out of central slums. The program targeted two types of households. Those who were living informally on publicly owned land ex-ante were (for the most part) provided new public housing in a less central location allowing us to understand whether clearance can help improve public service provision. Those who were private renters mostly continued as such after eviction, allowing us to understand whether private markets provide alternatives that are close substitutes to cleared central slums.

To study the program, we combined a baseline survey of over 30,000 households with detailed spatial data on eviction plans, identifying 5,000 households located on either side of sharp boundaries around planned clearance sites. Not all planned clearances were carried out: households in areas that were actually cleared form our treatment group, while those in planned but uncompleted sites form a placebo sample. We tracked these households and collected bespoke follow-up data four years after clearance. This places us in the rare position of having rich, spatially dense, panel data from a developing-country city.[3] The existence of the initial baseline means the data spans from *before* the treatment. We were able to resurvey 89% of the selected 5,000 baseline respondents in our endline, an unusually good outcome given the mobility of affected households.

Causal impacts are estimated using a regression discontinuity approach. The baseline data enable a clean test of continuity at the boundary prior to implementation, while the placebo group allows for a parallel test after clearance. Our tailored survey instruments further allow us to measure the causal impacts of relocation across multiple domains that are often difficult to capture: social networks, economic networks, access to amenities, and labor market outcomes.

We argue that our results are inconsistent with the strongest versions of the preservers' view, and offer qualified support for the clearers. In particular, we show that it is possible to design clearance programs that do no harm and that may, in some domains, improve outcomes. We also provide back of the envelope estimates that suggest that the value of land transferred to the government is more than enough to pay for a well designed compensation program.

We concentrate first on average treatment effects, and begin by documenting impacts on outcomes that follow closely from the compensation package: the impact of the program on location, geographically based social networks, and pre-clearance amenity of destination neighborhoods and housing. Affected households live 4.5km, or 14 minutes, further from the city center, and between 4 and 5km further from their original set of neighbors.[4] We also observe a 0.48 sd. ($se = 0.1$) decline in self-reported access to government provided public goods such as schools, and hospitals. The areas households move to, however, seem to have nicer ex-ante characteristics, with lower population density and large homes in the 2007 census. Relocated households also report a 0.52 sd. ($se = 0.1$) improvement in an index of housing quality. These results confirm that

[3] Franklin et al. 2024 (who use the same baseline as our paper) and Bryan et al. 2025 feature the only other such datasets that we know of.

[4] We define a neighbor as someone in our sample living within 50 or 100m at baseline.

our setting captures key areas of agreement. Clearers and preservers both accept that relocation will reduce centrality and disrupt existing social networks, but provide the opportunity to improve housing quality.

We then examine whether there are lasting social and economic consequences four years later. Consistent with preservers' concerns, we find persistent declines in reported social network quality. Displaced households score 0.34 sd. ($se = 0.01$) worse in an index of loneliness, report having 2 fewer people in their local social network ($se = 2$), and express 0.66 sd. ($se = 0.1$) lower satisfaction with their social ties. A summary index shows a 0.54 sd. ($se = 0.1$) reduction in social network quality. However, we also provide evidence that these effects can be ameliorated through program design. For those who were initially in public housing, and hence received a new public home, we have plausibly exogenous variation in how many ex-ante neighbors were rehoused nearby. We show that a one unit increase in the share of former neighbors relocated to within 1 km leads to a 1.2 sd. ($se = 0.4$) improvement in our index of network quality. Extrapolating this effect, network quality is robust so long as households end up living within 1km of 50% of their original neighbours. With thoughtful implementation, the social costs of clearance can be mitigated – a partial win for the robust networks view of clearers.

Economic networks appear more resilient. Overall, we find a small and statistically insignificant increase in an index of economic network quality. Disaggregated results show a modest statistically significant reduction in the size of financial networks, no significant impact on job network size, but an increase in access to network-based finance. This pattern may reflect the distinct nature of economic ties, which can often be maintained over distance through mobile phones and digital transfers (Caria et al. 2023) and potentially rely more on weak links (Granovetter 1973).

We then examine labor market outcomes and expenditure. Contrary to the preservers' view, we find increases in both labor force participation and earnings. The share of household members working rises by 6 percentage points ($se = 0.028$), and earnings per household member increase by 245 Birr, from a base of about 960 Birr ($se = 94$). These additional earnings are largely absorbed by higher transport and rent costs, which rise by about 25 ($se = 7$) and 200 ($se = 30$) Birr respectively. Crucially, we see no evidence of a decline in household expenditure in a other categories, including food, nor a change in prices faced. We can certainly rule out large reductions in consumption feared by preservers. These results suggest that labor markets are integrated, job networks remain intact, commuting is feasible, and improved living conditions may enhance labor supply.

Finally we turn to what we call endogeneous amenities. Preservers fear that network disruption will reduce the community's ability to provide stability, safety, and other public goods, leading to social decay. Our results suggests that social isolation does not inevitably lead to this broader collapse. As already noted, we find a statistically significant 0.52 sd. ($se = 0.1$) improvement in an index of self reported housing quality, indicating that the high quality housing provided does not decay within the 4 years we can study. We also see a 0.5 sd. ($se = 0.09$) increase in an index of environmental amenity, reflecting clean and quiet neighborhoods. In aggregate, perceptions of safety remain unchanged, although there is a small uptick in perception of serious crime which increased by 0.2 sd. ($se = 0.09$).

Overall, our results suggest that a relocation program that is careful about moving residents with their neighbors can maintain social and economic networks, while increasing neighbourhood and housing amenity, and boosting earnings. If government provided more schools and hospitals it would be all upside. There is little here to support preservers, but clearers need to understand that program design matters. We also provide some conservative back of the envelope cost calculations. These suggest that if the government sold the cleared and consolidated land it would earn 33,000 USD per evictee. In comparison the government paid about 11,500 USD per evictee in compensation. This leaves plenty available to improve public good provision and help neighbours move together.

We then examine how the average effects documented above differ across our two distinct tenure groups. The first group is composed of long-standing residents of nationalized *kebele* plots who, for decades, paid little or no rent for centrally located housing. These residents were offered heavily subsidized ownership of newly built condominiums on the urban edge or alterative kebele housing further from the center. These compensated households make up the majority of our sample, and their impacts largely mirror the aggregate results, although this group sees a small but insignificant increase in life satisfaction, which was muted in the averages. Perhaps more surprising our second group, which consists of private renters who received no compensation, also saw similar impacts. Relative to the average effects, they saw a smaller increase in housing quality, but also no significant drop in social network quality. They also continue to see a positive treatment effect on earnings, although the estimate is noisy. This pattern suggests that that private rental markets are relatively robust, as clearers would maintain.

We also estimate spillovers, and adjust direct our estimates to account for them. If clearance disturbs evictees social networks, it will also affect the networks of those left behind. Using geo-coded data, we construct for each control (unevicted) household an exposure index—the proportion of neighbours evicted—capturing the loss of local interactions in labor, social, and retail markets. To separate this effect from simple proximity to slum areas, we follow Borusyak and Hull (2023) and control for a placebo exposure measure based on planned but unrealised evictions. This delivers a difference-in-difference style estimate of spillovers, which in turn allows us to adjust our main RDD estimates to recover the direct effect of eviction, and overcome a potential SUTVA violation. Overall, we find limited evidence of spillovers. We see no reduction in social network quality, some loss of amenity (for example an increase in the smell of trash) and increase in crime for the left behind neighbors. At the point of data collection no redevelopment had taken place, so these results are perhaps unsurprising. Adjusting our estimates of direct effects does not change any or our qualitative conclusions We see the spillover evidence as in line with the clearers views: among those left behind, social networks are robust.

We make several contributions. A nascent literature uses micro-data to study developing country cities (e.g., Franklin et al. 2024; Bryan et al. 2025). To this literature we provide a large panel data set in a developing country city that captures key outcome such as social networks and job market success. As noted, several recent papers study relocation, although the majority focus on (equally important) voluntary programs (e.g., Barnhardt et al. 2017; Franklin 2025; Belchior et al. 2023). To this literature we add a focus on large scale forced relocation.

We also contribute to the broader literature that looks at the place-based consequences of urban renewal projects, including in the US (Collins and Shester, 2013; LaVoice, 2024), and developing countries (Harari and Wong, 2024; Gechter and Tsivanidis, 2020; Michaels et al., 2021). Relative to this work we focus on outcomes of directly displaced households. Most broadly our results contribute to the large literature evaluating the importance of place (Chetty and Hendren, 2018; Rojas-Ampuero, 2022), and of mobility (Nakamura et al., 2022; Deryugina et al., 2018; Bryan et al., 2014) including mobility out of low-income public housing in the US (Chyn, 2018).

1 Setting and Program

In 2017, the Addis Ababa city government approved a new comprehensive urban redevelopment plan, officially named the "Addis Ababa City Structure Plan." This ambitious plan outlined extensive land-use changes for many centrally located neighborhoods, envisioning a shift from informal (slum) housing to formally planned areas that included private housing, commercial properties, and government-built condominiums. The government's goal was eliminating all slums in Addis Ababa, a city where 80% of housing was classified as slums. Additionally, the plan included constructing significant new roads to enhance city connectivity.

Implementation occurred incrementally during 2017 and 2018, triggering large-scale evictions of slum residents, and complete demolition of their previous dwellings. We refer to these areas as "completed eviction sites." Figure 1 shows satellite photos of an eviction site before and after clearance. This approach to land use change, based on clearance and redevelopment, is common across the world. Appendix Table A1 provides a summary of recent mass evictions, highlighting the number of households evicted and the type of compensation, if any.

Critically, households received little to no prior notice of the eviction plans. Our baseline data confirm that residents' anticipation of eviction was no higher inside designated eviction boundaries than just outside. Media reports and related academic studies also corroborate that these evictions were typically sudden and unexpected by residents.

Due to the piecemeal and incomplete nature of the implementation—and its suspension in 2019—several designated eviction sites remained untouched at the time of our endline data collection. We exploit these "placebo eviction sites" in our analysis. We do not compare them to completed sites, but rather use them to test the validity of the continuity assumption required for

Figure 1: An eviction side– before and after

a regression discontinuity approach.

Using a combination of our eviction site maps and 2007 Census data, we estimate that at least 37,968 individuals were evicted during 2017 and 2018. This number likely underestimates actual evictions, considering population growth in these areas between 2007 and 2017.

1.1 Compensation policy

Compensation depended on households' initial legal status. Broadly, there were three relevant types: *kebele* residents; private renters; and owners.

The majority of our sample (and residents of Addis Ababa) lived in *kebele* housing—government-owned, low-quality dwellings rented at nominal rates close to zero. Residents of kebele housing were typically offered one of two things. Some were given chance to purchase government-built condominiums on the city's periphery, with subsidized mortgages and available financing.[5] Others were offered replacement kebele housing in a new neighborhood, usually further away but slightly bigger. Neither group had a choice of location, which was decided randomly for condominiums and bureaucratically for kebele homes.

In our survey data, 61% of kebele households reported being offered a condominium, while most of the rest were offered alternative kebele housing; about 11% reported receiving no offer. Among those given both options, take-up was split (55% kebele, 45% condominium), and overall 78% of those offered condominiums accepted. Figure 2 shows an example of kebele housing alongside a condominium housing block on the edge of the city. These condominiums are in high demand due to government subsidies (see Franklin (2025) for details) but mortgage repayments are expensive compared to the nominal rents paid in kebele housing.

Those who were private renters and informal squatters at the time of evictions were gener-

[5]Some redevelopment schemes originally planned to accommodate evicted households back on-site in newly built government housing, but by our endline data collection, this redevelopment had not occurred.

Figure 2: Old slum housing and new condominium housing. Photo credits: LSE Cities

ally not compensated. Residents owning homes in eviction sites were provided new plots of land—generally further away—and a substantial cash payment, approximately equivalent to two years of household income, to rebuild their homes.

2 Data and Measurement

This section describes our data, the regression discontinuity approach we use to establish causality, and how we measure outcomes key to clearers' and preservers' arguments.

2.1 Baseline Data

Our baseline data comes from a large household survey undertaken in early 2016. The survey was designed to provide a representative sample of poor households to allow evaluation of the Urban Productive Safety Net Programme (Franklin et al., 2024). A sample of 30,000 households was drawn from the universe of all households in the city using random walk sampling starting from randomly selected points across the city. Household locations were geo-coded.[6]

2.2 Redevelopment Site Maps

To understand spatial variation in eviction status, we collected Local Development Plans (LDPs) from The Addis Ababa City Plan office and The Land Development and Urban Renewal Agency. Each plan involved a scoping study, mapping of a particular local area, and a proposed land-use plan. Where these plans propose land use change it is implemented through eviction. From these we created a digitized lists of LDPs, and created shapefiles covering the footprints of each LDP. Together, these cover a large share of central Addis Ababa. We then identified the subset of LDPs that were given an official start year for re-development. Among these we identified those that

[6]A proxy means tests was used to identify approximately 6,000 of the poorest households to undertake a more detailed survey for the evaluation in (Franklin et al., 2024). Some 1,200 of those households also appear in our sample.

were actually executed during the period 2017 to 2018, and confirmed this by direct observation using satellite data. These sites form our "completed eviction sites", while those given a date but not implemented form our "placebo eviction sites."

We also identified new roads that would lead to evictions. Extracting the existing road use plan from the new proposed one, we built a map of new roads, and then identified cases where those roads would run through existing settlements and lead to eviction.

2.3 Endline Surveys

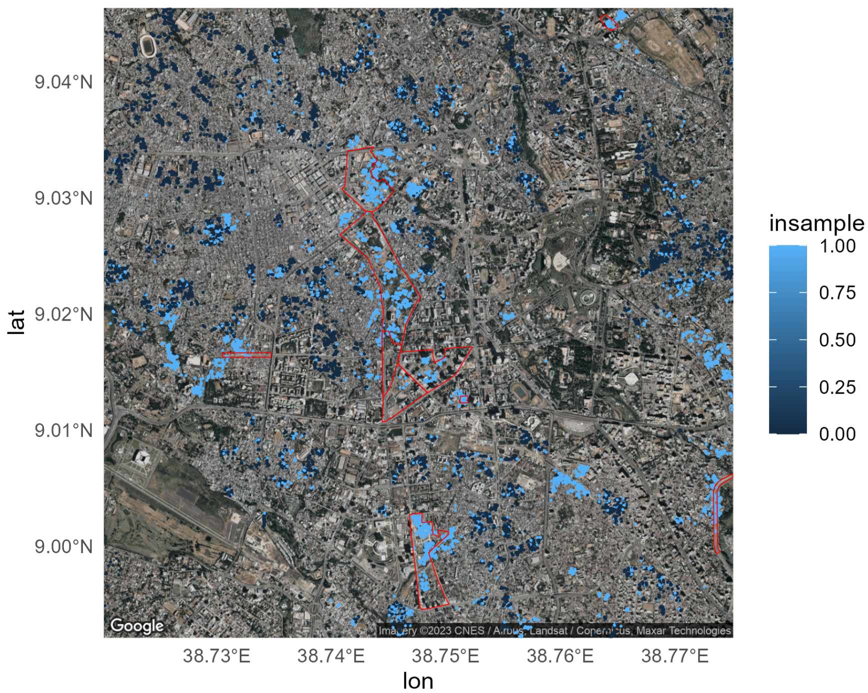


Figure 3: Endline Sampling - Oversampling Around Eviction Sites

We combined the geo-coded baseline survey with our re-development maps, and measured the distance between each household and the boundary of every re-development site. We sampled all households inside the sites (completed or not), all household within 150 meters of a site, and then randomly sampled households further away from the sites within distance bands, with decreasing probability of selection moving further away from treated sites, up to 1200m from the sites. Figure 3 shows a map of complete projects in one part of the city with sampled households in light blue and households in our baseline that were not sampled in dark blue. Endline data was collected in 2021, 3 to 4 years after evictions took place. We discuss attrition in our identification section 3.1, but we managed to interview 89% of the households we aimed to.

2.4 Measurement

Many of the concerns raised by preservers, such as negative impacts on social networks, and the gains hoped for by clearers, such as improved access to sanitation, would not be measured in a typical labor force survey or census. We designed our endline survey to capture these outcomes with emphasis on self-reported social networks, economic networks, local amenities, and food prices. We discuss how we measure these below. We add to this more standard measures on housing quality, labor force participation, earnings and consumption.

In most domains we have multiple measures. For example, we can measures social contact through self reported loneliness, or through number of local friends. In all cases, we combine multiple questions into summary indices by coding responses so that higher values represent more desirable outcomes, standardizing as z-scores, and applying inverse-covariance weighting. Where appropriate, we first form sub-indices and then aggregate them in the same way.

Social networks. We capture both structural and perceptual aspects of respondents' networks. Structural measures include reported network size, and size within a 5-minute walk. Perceptual measures cover satisfaction with one's network, feelings of loneliness, and the extent to which neighbors can be relied upon for help, typically measured on Likert scales. We summarize social networks impacts in and index that draws on nine questions, including a sub-index on non-financial support received from neighbours.

Economic networks. We distinguish economic ties from social ones, asking households to report interactions related to job search, financial transactions, and informal loans. We ask about the size of networks used for job information or finance, as well as the existence and size of any informal loans in or out.

Amenities. Amenities can be divided into those provided directly by the government, which we call exogenous, and those that are generated at least in part by the behaviors of the community, which we call endogenous. For exogenous amenities, we measure reported satisfaction with public goods such as parks, schools, hospitals, and street lighting. For endogenous amenities, we build two sub-indices: respondents experience of environmental amenities (e.g. sewage, litter, noise) and public safety (e.g. burglary, violent crime).

Food prices. Finally, moving out of central locations may come with a change in prices. To study this, we collect self-reported prices for common food items. These are weighted by reported expenditure shares and combined into a food price index using the same inverse-covariance weighting procedure.

Item non-response: For fourteen households that we could not interview in person, a short-form questionnaire was conducted over the phone, resulting in 14 fewer observations for certain detailed outcomes. For these households, aggregate indices are constructed that exclude missing items.

2.5 Sample Description

Our total endline sample consists of 5089 households. Our main analysis uses the subset of households living within 0.18km of a completed eviction boundary at baseline, whether inside or out. This sample restriction gives us a total of 2,069 households, 47 percent of which lived within the boundary of a completed eviction site at baseline. The remainder of the 5089 households are within placebo evictions sites, just outside placebo sites, or more than 0.18 km from any boundary.

Table 1 provides summary statistics for the entire sample, and the main analysis sample. Households are young with the average adult being 28 years of age at baseline. On average, the most educated household member has not completed high school (10.8 years of schooling). Only 56 percent of household heads are employed at the time of baseline, in line with very high unemployment rates in urban setting in other poor countries (e.g. Balboni et al. 2025). Over 50% of the sample rent housing from the Kebele (the most local unit of government in Ethiopia). Sizable minorities of households rent privately (17%) or own their homes (14%). Our sample lives in housing conditions consistent with international definitions of slums. About 40% of houses have a mud floor, the vast majority have a corrugated iron sheet roof, and mud or wooden walls. Households predominantly rely on shared pitted latrine toilets or public toilets. Most households have access to piped water but for nearly 50% this pipe is shared. For cooking fuel, nearly 50% of households use charcoal, highlighting imperfect access to electricity in these areas.

3 Identification

We use a spatial regression discontinuity design to compare households just inside the boundary of completed eviction sites to households just outside of those sites, controlling for Euclidean distance from eviction site boundaries Gelman and Imbens (2019). We use the following specification:

$$y_{is,t} = \alpha + \beta_1 1\{D_{is,t-1} > 0\} + \beta_2 D_{is,t-1} + \beta_3 1\{D_{is,t-1} > 0\} * D_{is,t-1} + \gamma_s X_{is,t-1} + \eta_s + \epsilon_{is,t}, \quad (1)$$

where $y_{is,t}$ is an outcome for household i, who lived closest to eviction site s, and t is the period of our endline data. $D_{is,t-1}$ is the signed distance (positive inside the boundary, negative outside) between household i and their nearest site, measured at baseline $(t-1)$. We include controls for household characteristics at baseline $X_{is,t-1}$, including distance from the city center and distance to main roads. We also include site fixed effects η_s.

Following best practice, we specify the effect of distance as linear, and then test the robustness of our results to different bandwidths. Our preferred bandwidth is 180 meters: this is the minimum bandwidth required to include all households that were evicted, since evicted households lived up to a maximum of 180m “inside” a boundary. Section 7 shows that the main results are robust to a set of alternative bandwidths, including the optimal bandwidth selection procedure of Calonico et al. (2019).

Table 1: Summary Statistics of Sample at Baseline

	Full sample		*Restricted sample*	
	Mean	SD	Mean	SD
People living in household	4.32	1.90	4.39	1.91
Average age at baseline- adults	30.81	11.40	31.02	11.43
Employed head of household	0.56	0.50	0.56	0.50
Education of most educated HH member	10.95	3.68	10.77	3.64
Owned home	0.13	0.33	0.14	0.34
Rented from Kebele	0.61	0.49	0.59	0.49
Rented privately	0.20	0.40	0.17	0.38
Mud/dung floor	0.34	0.48	0.37	0.48
Cement screed floor	0.59	0.49	0.55	0.50
Corrugated iron sheet roof	0.98	0.15	0.95	0.21
Mud/wood walls	0.89	0.31	0.88	0.32
Flush toilet	0.01	0.11	0.01	0.08
Shared pit latrine or public toilet	0.90	0.30	0.90	0.30
Piped water	0.86	0.35	0.82	0.38
Shared water source	0.46	0.50	0.47	0.50
Cook with charcoal	0.42	0.49	0.43	0.50
Cook with electricity	0.49	0.50	0.46	0.50
Observations	5089		2069	

Note: The first two columns shows baseline statistics for our full sample. The restricted sample consistent of all households who lived within 180m of treated eviction site boundaries at baseline.

β_1 identifies the causal effect of eviction under the assumption that, absent eviction, outcome y would have changed smoothly with distance to the boundary. We assess this in two ways. First, we are in the relatively rare position of being able to test for discontinuities at the boundary using baseline data. Specifically, we examine baseline characteristics of households successfully located at endline, thereby testing for balance conditional on attrition. We cannot test balance at baseline for all endline outcomes because the baseline questionnaire was relatively short.

Second, we test for continuity of outcomes at both *baseline* and *endline* across the boundaries of placebo sites. If boundaries coincide with features, such as roads, that generate discontinuities and lead to bias, we would expect to see them at both treatment and placebo sites.[7] We use the same specification to run these tests as we do for the main analysis.

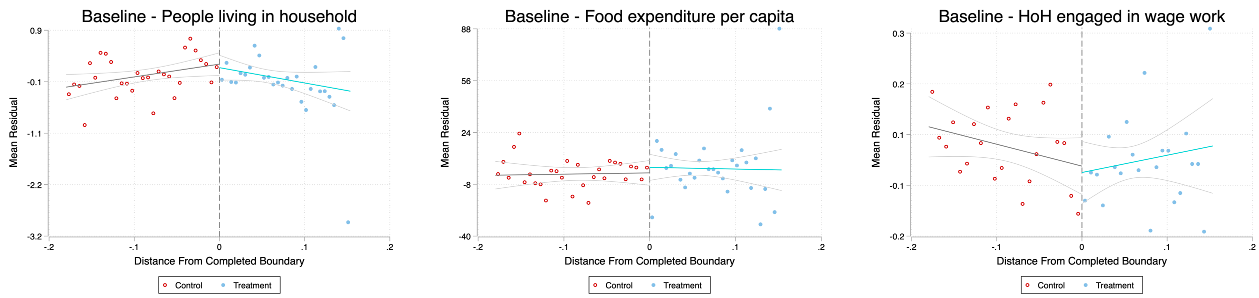
Figure 4 provides a visual summary of our continuity tests for selected outcomes. Outcomes are residualized relative to site specific fixed effects. Panel A presents discontinuities across completed-site boundaries in baseline outcomes. Panel B shows discontinuities across placebo-site boundaries in endline outcomes. We see no visual evidence of discontinuity in these graphs:

[7] Note that we do not treat placebo sites as a control group, which would require the strong assumption that completed eviction sites were selected from the pool of potential sites in a way orthogonal to household outcomes.

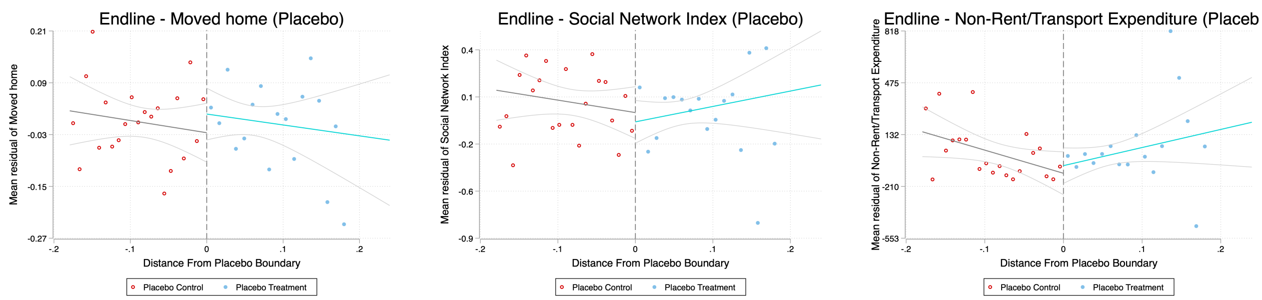
at baseline there was no discontinuity in household size, food expenditure per capita, or labor force participation. At endline, we see no discontinuity at placebo boundaries for eviction, social networks or rent and transport expenditure.

Figure 4: Illustration of Balance Checks - Baseline and Placebo RDD

Panel A: Completed Sites - Baseline



Panel B: Placebo Sites - Endline



Note: Scatterplot of binned means of outcome variable with local linear regression with baseline controls and project site fixed effects. Distance > 0 denotes inside (completed or placebo) project boundary.

We present a more complete set of outcomes in Appendix Table A2 for baseline discontinuity along actually cleared boundaries and Appendix Table A3 for baseline continuity at placebo boundaries (we report and discuss endline placebo tests while discussing our main results). Overall, we see very little evidence of discontinuity. We present 50 estimates across these two tables and observe 7 coefficients that are statistically significant at the 10% level or less, consistent with what would be expected under the null of no discontinuity. In all main regressions, we control for variables that show imbalance at baseline.

3.1 Attrition

We want to be sure that our results aren't driven by differential attrition. Achieving a high response rate is especially challenging in a setting where households were displaced from their original residences many years earlier. Attrition rates in the literature have generally been very high. Through extensive tracking—using multiple contacts, friends, and neighbors—we obtained an overall response rate of 89.3%.

We do, however, have a 3.9 (se=1.1) percentage point lower response rate for those who were located within the eviction boundary at baseline. This raises a concern that we were able to survey different types of people among the evictees. If this were the case, we would expect to observe a discontinuity in baseline characteristics at the eviction boundary among those households who were contacted for an endline. The baseline discontinuity results presented above (Appendix Table A2) are in fact restricted to the sample that was interviewed at endline, and show no sign of a troubling discontinuity.

3.2 Spillover effects

In order to interpret our RD estimates as the causal effect of eviction for evictees, our identification strategy requires a SUTVA assumption: households that fall just outside of evicted boundaries are not affected by having their neighbours evicted. Our setting is one where spillovers make SUTVA breaches possible. If social networks are important for social support and job search, then eviction will have spillover impacts on nearby neighbours. Land use change in cleared areas may also have impacts. The magnitude of these potential spillovers is also a direct object of interest.

To estimate spillovers we define for each non-evicted household i an index of realized exposure to evictions:

$$E_i^r = \frac{\sum_k T_k A_k / d_{ik}}{\sum_k A_k / d_{ik}} \tag{2}$$

where k is a census enumeration area, A_k is the population of that area, T_k is a dummy that takes on value 1 if enumeration area k was cleared, and $d_i k$ is the Euclidian distance between household i and the centroid of the enumeration area. This measure lies between 0 and 1, and takes on higher values when more people within the city are evicted, and when more of the people live near i.[8]

We also define a potential exposure measure

$$E_i^p = \frac{\sum_k (T_k + C_k) A_k / d_{ik}}{\sum_k A_k / d_{ik}}, \tag{3}$$

where C_k is a dummy that takes the value one when there was a plan to clear enumeration area k, but that plan was not followed through.

We estimate spillover effects with the regression

$$Y_{is} = \alpha + \beta E_i^r + \gamma E_i^p + \delta X_i + \eta_s + \epsilon_{is}, \tag{4}$$

where Y_{is} is an outcome for household i that lives closest to potential eviction site s, η_s are eviction site fixed effects, and X_i is a set of baseline characteristics of household i, including geographic controls for distance to the city center, latitude and longitude.

[8] Our results are robust to alternative approaches, including another common approach in the literature, which defines exposure as the share of nearby areas that were evicted within a fixed radius R.

The identification assumption is a spatial analogue of the parallel trends condition in difference-in-differences designs. Specifically, in the absence of treatment, outcomes would exhibit the same spatial gradient with respect to actual eviction sites as they do with respect to placebo sites. The specification allows for rich spatial heterogeneity because of the fixed effects, and for households close to eviction sites to be fundamentally different from those far away.

3.3 Adjusting Direct Effects for Spillovers

If the impacts of eviction spillover onto left behind households they will impact our control group, causing a SUTVA violation. Standard RD estimates would then show how eviction altered the *relative* outcomes of evicted and non-evicted households rather than the absolute effect of eviction.

We use our estimates of the spillovers from Section 3.2 to adjust our RD estimates and recover absolute impacts. We calculate

$$\hat{\beta}^{A} = \hat{\beta}^{RD} + \hat{\beta}^{S}\bar{E}^{r}$$

where β^{RD} is our regression discontinuity estimate of the direct effect (from equation 1), β^{S} is our spillover estimate (from 4), $\bar{E}^{r}$ is the average exposure to spillover for control households who live at the boundary of a clearance neighborhood, and β^{A} is our adjusted estimate of the absolute direct effect.

In practice, we normalise E^{r} such that $\bar{E}^{r} = 1$ before estimating equation 4. To do so, we take our raw measure of exposure and divide it by the inferred average at the boundary, by plotting a local polynomial of exposure by distance, reading off the intercept, which is approximately 0.18 (See Appendix Figure B1, which plots our exposure measure against distance to clearance boundaries). We present our estimates of $\hat{\beta}^{A}$ below our main RD and spillover estimates for all outcomes (in Panel D in the regression tables that follow), with standard errors calculated by assuming independence between β^{RD} and β^{S}, i.e. $\text{Var}(\hat{\beta}^{A}) = \text{Var}(\hat{\beta}^{RD}) + \text{Var}(\hat{\beta}^{S})$.

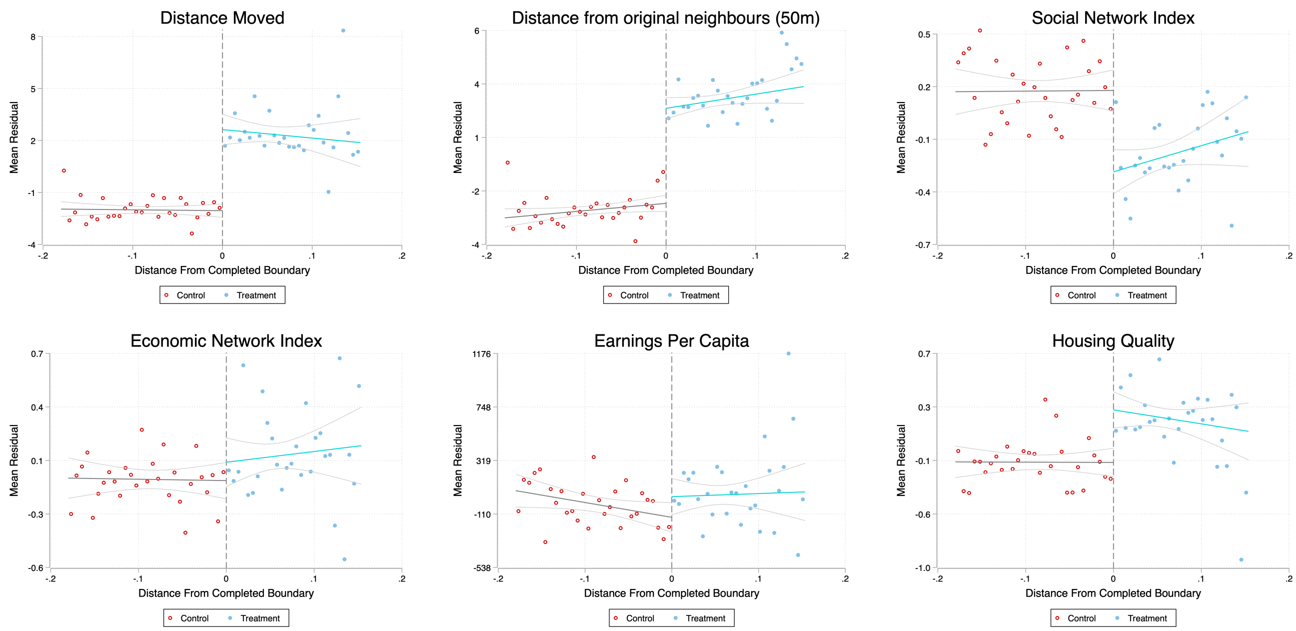
4 Results

This section reports our main results. We first look at direct, more mechanical, effects, and show that the program did lead to both physical and short run social isolation. We then turn to more endogenous outcomes asking whether the initial dislocation led to ongoing impacts on social networks, economic networks, labor market outcomes, and environmental amenities.

4.1 Graphical Summary of Average Impacts

We look at many outcomes, and so present our main results in table form. Before doing so, however, Figure 5 previews the main results visually. These figures show: eviction causes people to move away from where they were living (physical isolation); away from their ex-ante neighbours (social isolation); lowers the quality of their social networks 4 years later; has little impact on economic networks; increases per capital earnings; and allows households to live in higher quality housing. We will show below that the negative impact on social networks is ameliorated when households move with their neighbors. The strong discontinuities shown in Figure 5 are in stark contrast to the lack of discontinuity in the baseline and placebo tests in Figure 4.

Figure 5: Summary of Key Outcomes – RDD Graphs



Note: Scatterplot of binned means of outcome variable with local linear regression with baseline controls and project site fixed effects. Distance > 0 denotes inside completed project boundary (treatment).

4.2 Direct Effects of Clearance

Panel A of Table 2 reports RD estimates of the direct impact of the clearance program. These results mostly reflect choices made by the government when it designed the program and compensation. Results are consistent with the ground that is conceded by both clearers and preservers.

Clearance leads to increased spatial isolation. Column 1 shows that those affected move about 5KM from their initial location, and where they end up is about 4.5km or 14 minutes further from the center of the city. These impacts are strongly statistically significant.

This physical isolation leads to isolation from pre-existing social networks. Cleared households live between 4 and 5km further away from their initial close neighbours, and they are moved to locations that have a lower population density as measured in the 2007 Ethiopian census. Whether this last fact is a negative depends on whether lower population density allows for higher amenity. Appendix Table A4 provides further details on the pre-existing characteristics of destination neighbourhoods, measured at the time of the 2007 census. In general, these neighborhoods house younger people, with less education and more assets, in larger homes.

Physical isolation, however, allows for an improvement in the quality of people's homes. Column 8 shows that our index of housing quality improves by 0.5 standard deviations, a result that is strongly statistically significant. Appendix Table A5 breaks this index into its constituent parts, showing a general increase in home size, facilities such as private toilets and piper water, and an improvement in building materials.

Finally, there is evidence that, while the government program takes advantage of movement to improve housing quality, it could do better in the provision of public goods. Column 7 shows a statistically significant 0.48 standard deviation reduction in an index measuring access to public goods. Appendix table A6 disaggregates the index. Evicted households are less likely to report working street lights and state that they are further away from public space, schools and hospitals. These impacts are troubling, but they are clearly choices that have been made – schools and hospitals could have been built near the condos. We will argue below that the cost of the condos were low compared to earnings the government could make by selling the land. This opens significant fiscal headway to do a better job with public goods.

Panel B of Table 2 reports results from the same specification but for placebo clearance areas. The remaining tables in the paper are structured similarly, with RD estimates in Panel A and placebo estimates in Panel B. Looking across all 8 columns we see only minimal suggestion of discontinuity for placebo sites. The only statistically significant result related to movement. Those who were living inside placebo boundaries at baseline are, by the time of the endline, about 0.9km further from their neighbours. Three classes of explanation are possible. First, it may be that there was a pre-existing difference in density around boundaries, but this we do not see in the baseline data. Second, those in placebo areas may have started to move in anticipation of eviction. This is partially supported by the effect on distance moved of (a statistically significant) 0.6km, in Column 1. This would not be a problem for our design, which does not care to distinguish between those actually evicted and those who moved due to anticipation. Finally, the slight discontinuity may reflect noise in the data. Looking across all our tables, in total we present 34 coefficients for these placebo regressions and find only 4 that are statistically significant at the 10% level or better. Overall, the placebo tests seem to support our assumption of continuity at the boundary in the absence of eviction.

Table 2: First Stage Regression Discontinuity Results

	Spatial Isolation			**Social Isolation**			**Exogeneous Amenities**	
	Distance Moved	Distance to Centre (km)	Time to centre	Distance from original neighbours (50m)	Distance from original neighbours (100m)	Pop. Density	Public Goods Index	Housing Quality Index
Panel A: RDD-Estimated Treatment Effects								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD_Estimate	5.353	4.425	13.926	5.166	4.030	-5632.183	-0.481	0.524
	(0.489)***	(0.453)***	(2.637)***	(0.300)***	(0.290)***	(1767.385)***	(0.095)***	(0.102)***
Control Mean	0.65	3.86	32.55	1.38	1.63	33796.36	-0.02	-0.06
Observations	1805	1805	1800	1801	1816	1805	1805	1819
Panel B: Placebo Treatment Effects								
RD_Estimate	0.605	0.467	-0.048	0.954	0.881	-4115.985	0.088	0.055
	(0.373)	(0.345)	(2.421)	(0.357)***	(0.336)***	(2617.197)	(0.133)	(0.123)
Control Mean	0.80	2.67	25.44	1.51	1.48	31959.04	0.06	-0.18
Observations	844	844	843	819	836	844	844	844

Notes: Columns correspond to different household dependent variables regressed against the running variable: distance from completed project boundary. Positive distances imply household resides inside the project boundary (treatment). All RDD regressions use a cutoff of 0 (the project boundary) such that the right and left of the cutoff are the treatment and control respectively. RD Estimates assess the difference in the dependent variable between treatment and control at the cutoff. Observations refers to the observations in each group that fall within the selected bandwidth. Panel A corresponds to households near completed project boundaries. Panel B corresponds to households near uncompleted project boundaries, thus forming a placebo sample. All subjective variables are z-scores where higher numeric value corresponds to better outcomes. Baseline household covariates used as controls including household demographics, employment status of the household head, household assets, and household weekly expenditure. Columns (1) and (2) are kilometer-based measures of distance. Column 3 measures time to city centre in minutes. Columns (4) and (5) measure social isolation by computing the endline average distance (in km) from neighbours that were within 50 meters and 100 meters respectively of the household at baseline. Column (6) measures population density of the endline location using the latest census data from 2007. Columns (7) and (8) represent z-scored inverse-covariance weighted indices that measure the quality of public goods in the endline locations and housing quality, which is determined by the housing the government provided the majority of evicted households. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4.3 Endogenous Social Networks

The results above suggest that pre-existing social networks are disrupted by clearance, people live further away from their former neighbors. Where clearers and preservers disagree, however, is on the medium to long run implications of these initial effects. Preservers believe social networks are fragile and predict ongoing negative impacts, while clearers believe networks are robust, suggesting repair. Panel A of Table 3 shows impacts on these endogenous medium to long run social outcomes, and suggests that, consistent with preservers arguments, repair is not automatic. In this table, and all subsequent tables, all variables are coded so that a positive coefficient is socially desirable. For example, a negative treatment effect on loneliness indicates that people are more lonely.

Column 1 reports a statistically significant reduction of 0.55 standard deviations in our index of social networks.[9] This includes a 0.34 standard deviation increase in loneliness, a 0.66 sd reduction in a self-reported network satisfaction, and a 0.49 sd reduction in a neighours support index (sub-components presented in Appendix Table A7.). On average, the program has ongoing negative impacts on social networks, and increases social isolation. As before, Panel B of Table 3 reports results of our placebo tests. Again, there is little evidence of a discontinuity in the absence of eviction.

Panel C of Table 3 reports our estimates of spillover effects, using the methods discussed in section 3.2. We see little evidence of spillover effects, but what we do see makes sense. Those who are close to eviction sites are slightly more likely to report that they are lonely, a likely outcome of their neighbors being evicted. The effect, however, is small (0.025 standard deviation increase in loneliness due to having approximately 40% of neighbours evicted). Overall, while clearance surely disrupted the social networks of those left behind in nearby neighborhoods, it does not seem to have large ongoing negative impacts. Panel D shows our attempt to adjust our RD estimates for this spillover effect. This adjustment does very little to alter the treatment effects displayed in Panel A.

While worrying, it might be possible to ameliorate negative impacts on social networks through good program design. A government could try to collect neighbors together, and move them to the same destination. Our data contains some plausibly exogenous variation that allows us to test whether this approach would work. As noted already, the vast majority of those living in Kebele housing prior to clearance were offered a condo home or alternative kebele house, the geographic location of which was effectively random.[10] This creates exogenous variation in the distance between cleared households and their ex-ante neighbors.

Panel A in Table 4 reports the results of regressing our social network measures on the share of

[9] All components of this index are reported in the table.

[10] To confirm this, we regress endline pairwise distance between evicted households on their baseline pairwise distance and find no correlation, after controlling for eviction-site fixed effects. In other words, conditional on being from the same site, households were not able to colocate with their old neighbours.

Table 3: Effects on Social Network Quality

	Social Network Index	Network size	Local network size	Network satisfaction	Loneliness	Neighbours Support Index
Panel A: RDD-Estimated Treatment Effects						
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-0.546	-1.870	-1.973	-0.659	-0.344	-0.493
	(0.100)***	(2.117)	(1.889)	(0.099)***	(0.099)***	(0.091)***
Control Mean	0.04	14.98	9.91	0.08	-0.01	0.02
Observations	1819	1818	1819	1805	1805	1805
Panel B: Placebo Treatment Effects						
RD_Estimate	0.012	0.203	-0.081	0.068	-0.066	0.043
	(0.123)	(2.718)	(1.418)	(0.139)	(0.134)	(0.136)
Control Mean	0.15	17.98	9.87	0.10	0.10	0.16
Observations	844	844	844	844	844	844
Panel C: Spillover Effects						
Exposure	-0.012	0.077	0.090	-0.007	-0.026	-0.012
	(0.008)	(0.186)	(0.144)	(0.008)	(0.009)***	(0.009)
Control Mean	0.09	14.92	9.91	0.11	0.08	0.10
Observations	3529	3529	3529	3528	3528	3528
Panel D: Spillover-Corrected Treatment Effects						
RD_Estimate	-0.558	-1.793	-1.883	-0.666	-0.370	-0.506
	(0.101)***	(2.126)	(1.895)	(0.099)***	(0.099)***	(0.092)***
RDD Bandwidth	0.18	0.18	0.18	0.18	0.18	0.18
Project FEs	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Columns correspond to different household dependent variables regressed against the running variable: distance from completed project boundary. Positive distances imply household resides inside the project boundary (treatment). All RDD regressions use a cutoff of 0 (the project boundary) such that the right and left of the cutoff are the treatment and control respectively. RD Estimates assess the difference in the dependent variable between treatment and control at the cutoff. Observations refers to the observations in each group that fall within the selected bandwidth. Panel A corresponds to households near completed project boundaries. Panel B corresponds to households near uncompleted project boundaries, thus forming a placebo sample. Panel C presents spillover results for different household dependent variables regressed against the a continuous exposure variable based on the amount of surrounding physical area evicted, with an additional control for placebo treated households and for total potential exposure, which includes exposure to uncompleted eviction sites. Panel D presents Panel A results with the SUTVA-corrected estimate, which accounts for the spillovers estimated in Panel C. Baseline household covariates used as controls including household demographics, employment status of the household head, household assets, and household weekly expenditure. Column (1) is the overall z-score inverse-covariance weighted index for social network quality. Columns (2) and (3) measure the number of individuals in different networks. Column (4) is a z-scored measure of satisfaction with one's network. Column (5) is a z-scored measure of the incidence of loneliness (positively coded). Column (6) is itself a z-scored inverse-covariance weighted index of various measures to quantify the support provided by neighbours. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

co-evicted neighbors that end up within one KM at endline. The sample is restricted to those that were living in Kebele housing ex-ante, and that were evicted; the sample for which location is plausibly random. The result in column 1 shows that moving from complete isolation (no ex-ante neighbors within 1km) to no isolation (all neighbors within 1km) increases our index of social network quality by about 1.2 standard deviations. We see similar large positive effects on the sub-components, although we lack precision for some outcomes.

Panel B of the table reports RD estimates of the impact of eviction, restricting the sample to those who were living in Kebele housing ex-ante. Eyeballing the numbers, a household who is evicted and has 0.5 more than average of its ex-ante neighbours within 1km would see no negative impact on its social network index. Finally, Panel C does this adjustment more formally, showing the estimated treatment effect for a hypothetical household that was relocated with all its ex-ante neighbours. The result suggests no reduction in the quality of social networks would result from such a policy.

4.4 Endogenous Economic Networks

We have established that the clearance program, as implemented, led to some ongoing social isolation, but that the negative impacts could have been ameliorated with careful policy design. We now look at medium term impacts on economic networks. There are good reasons to believe that economic networks will be more robust than social networks. While socialization almost surely requires personal contact, economic interaction, such as getting job referrals, may rely more on weak links that do not require constant physical interaction.

Table 5 reports the results. Column 1 shows a small positive, but statistically insignificant, improvement in an index of the quality of economic networks.[11] Looking at the constituent parts of this index the message is a little mixed. Most outcomes see no statistically significant change, but we do see an insignificant reduction in financial networks size with evicted households reporting about 0.46 fewer people in their financial networks from a base of about 1.8 people. This reduction in the number of people in the network does not seem to affect the ability of evictees to access network finance, with evicted households reporting that they are about 6.6 percentage points more likely to have an informal loan (3 percentage points of which is made up of loans from residential neighbours). While some of these loans may have been required to cover relocation costs, the results do not suggest that evicted households are unable to access financial assistance when needed. Importantly, our results suggest only small impacts on job network size. Evictees report that there are on average 0.3 fewer people in their job network, from a base of 3.6 people. This effect is not statistically significant and seems unlikely to be economically significant either.

As above, Panels B to D report our placebo and spillover estimates. Consistent with the patterns throughout we see little of concern in the placebo estimates. Most estimates are small and

[11] All sub-components of this index are reported in the table.

Table 4: Kebele Sample – Effects of Co-Location on Social Networks of the Evicted

	Social Network Index	Network size	Local network size	Network satisfaction	Loneliness	Neighbours Support Index
Panel A: Effect of Co-Location						
	(1)	(2)	(3)	(4)	(5)	(6)
Share Co-evicted Within 1km	1.194	19.213	17.531	0.551	0.205	0.148
	(0.421)**	(7.478)**	(7.828)**	(0.371)	(0.288)	(0.214)
Mean	-0.52	12.41	7.39	-0.49	-0.31	-0.26
Observations	571	570	571	562	562	562
Panel B: RDD Treatment Effects - Kebele Sample						
RD_Estimate	-0.585	-2.380	-2.943	-0.655	-0.303	-0.275
	(0.128)***	(2.541)	(2.554)	(0.116)***	(0.114)***	(0.064)***
Observations	1259	1258	1259	1249	1249	1249
Panel C: Treatment Effects Adjusted by Average Co-location Share						
RD_Estimate	0.420	13.799	11.819	-0.191	-0.131	-0.151
	(0.440)	(7.898)*	(8.234)	(0.389)	(0.310)	(0.223)
RDD Bandwidth	0.18	0.18	0.18	0.18	0.18	0.18
Project FEs	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1259	1258	1259	1249	1249	1249

Notes: This table is restricted to households that were in Kebele Housing at baseline and therefore received government-administered housing following eviction. Panel A reports the coefficient from regressing the share of co-evicted neighbours (within 200 meters at baseline) that remained within 1 km of the household at endline on a set of social network outcomes. These dependent variables are the same as Table 3. We interpret these results as the effect of co-location with other evicted neighbours on social network outcomes. Panel B then reports the RDD-estimated treatment for Kebele households only. Finally, Panel C reports the effects presented in Panel B adjusted by the treatment effect of co-location scaled by (1-the average share of co-located evicted neighbours) to simulate the gain to a government program that locates all displaced households together (co-location share = 1). Standard errors are adjusted as well.

statistically insignificant. We do see a statistically significant placebo impact on financial network size, but as discussed above the number of impacts we see across our placebo results is consistent with a null of no impact. We see no evidence of important spillovers.

Overall, these results provide little support to preservers' claims of ongoing isolation from economic networks, and suggest that, inline with clearers' views, economic networks are relatively robust to eviction.

4.5 Economic Outcomes: Employment, Earnings and Expenditures

Ultimately, we care about spatial and social isolation because we worry it will lead to a worsening of economic outcomes, such as employment and earnings. We have shown that there are no strong ongoing impacts on economic networks, but evicted households are definitely less central, which may have negative impacts on labor market access.

Table 6 shows medium term impacts of eviction on economic outcomes. Column 1 shows a 6.6 percentage point increase in the share of working age household members that report being in work. Column 2 shows a 244 Birr increase in earnings per household member, from a base of around 958 Birr, implying a 25% increase in earnings per household member. Together these findings suggest both an increase in labor market participation and an increase in earnings for those who were already working. These claims seem inconsistent with preservers main economic claim. Any social or physical isolation created by eviction does not seem to translate into reduced labor market participation or earnings. It would appear that economic networks are robust, and spatial labor markets are sufficiently integrated to ensure that economic opportunity is available in more spatially isolated locations.

Column 3 shows that all of the 244 Birr increase in earnings is spent. About 200 goes to rent, consistent with higher quality housing, and 25 to transport, consistent with commuting to work and traveling further for shopping and leisure.[12] Importantly, we see no evidence that households reduce expenditures in other categories such as food. A final residual category (not shown in Table 6) of expenditure on school, durables, festivals and leisure also shows no statistically significant change.[13] Similarly, an aggregate of non-rent non-transport expenditure is not significantly affected. We also see no evidence that evictees face higher prices.

As for other outcomes, placebo estimates are uniformly small and insignificant, spillovers are marginal at best, and correcting direct effects for spillovers makes no qualitative difference.

Overall, displacement increases labor force participation and earnings, and after paying for increased rent and transport, evictees disposable income is unchanged. Again, these results are

[12]Housing costs include repayments of highly subsidized 20-year mortgages if the received condominiums as compensation.

[13]We report this coefficient in Panel of A Table 9 below.

Table 5: Effects on Economic Network Quality

	Economic Network Index	Job network size	Financial network size	Has Informal Loan	Has neighbour loan	Informal loan amount	Informal transfers in (amt)
Panel A: RDD-Estimated Treatment Effects							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RD_Estimate	0.135	-0.336	-0.462	0.066	0.034	1708.948	526.332
	(0.105)	(0.509)	(0.284)	(0.031)**	(0.018)*	(1402.162)	(796.634)
Control Mean	-0.05	3.64	1.76	0.09	0.03	752.98	3522.22
Observations	1819	1757	1757	1819	1819	1819	1819
Panel B: Placebo Treatment Effects							
RD_Estimate	0.104	0.183	1.136	-0.039	-0.031	1441.177	447.118
	(0.141)	(0.791)	(0.509)**	(0.046)	(0.029)	(1687.995)	(1033.651)
Control Mean	0.01	3.36	1.57	0.11	0.04	1404.52	4152.47
Observations	844	831	831	844	844	844	844
Panel C: Spillover Effects							
Exposure	-0.003	0.006	-0.004	-0.003	-0.001	-44.622	51.443
	(0.009)	(0.051)	(0.034)	(0.003)	(0.002)	(82.803)	(69.372)
Control Mean	-0.03	2.99	1.53	0.10	0.03	1081.09	4227.95
Observations	3529	3461	3461	3529	3529	3529	3529
Panel D: Spillover-Corrected Treatment Effects							
RD_Estimate	0.133	-0.330	-0.466	0.063	0.033	1664.326	577.774
	(0.105)	(0.511)	(0.286)	(0.031)**	(0.018)*	(1404.605)	(799.648)
RDD Bandwidth	0.18	0.18	0.18	0.18	0.18	0.18	0.18
Project FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Columns correspond to different household dependent variables regressed against the running variable: distance from completed project boundary. Positive distances imply household resides inside the project boundary (treatment). All RDD regressions use a cutoff of 0 (the project boundary) such that the right and left of the cutoff are the treatment and control respectively. RD Estimates assess the difference in the dependent variable between treatment and control at the cutoff. Observations refers to the observations in each group that fall within the selected bandwidth. Panel A corresponds to households near completed project boundaries. Panel B corresponds to households near uncompleted project boundaries, thus forming a placebo sample. Panel C presents spillover results for different household dependent variables regressed against the a continuous exposure variable based on the amount of surrounding physical area evicted, with an additional control for placebo treated households and for total potential exposure, which includes exposure to uncompleted eviction sites. Panel D presents Panel A results with the SUTVA-corrected estimate, which accounts for the spillovers estimated in Panel C. All expenditure and income variables are winsorized at the 99th percentile. Index variables are based on the inverse covariance index of a set of z-scored variables. All subjective variables are z-scores where higher numeric value corresponds to better outcome. Baseline household covariates used as controls including household demographics, employment status of the household head, household assets, and household weekly expenditure. Column (1) is the aggregate index for measuring the quality of the economic network, which is a z-scored inverse-covariance weighted index. Columns (2) and (3) measure network size in terms of individuals. Columns (4) and (5) are binary indicators for whether the household has an informal loan or a loan from a neighbour. Columns (6) and (7) are the values of loans measured in Birr. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

more inline with clearer's views, which emphasize the efficient and integrated nature of labor markets.

4.5.1 Individual Labor Market Outcomes and Gender

Tables A8 and A9 in the appendix give more information on labor market effects and job quality. While the previous tables concentrated on household level outcomes, these tables show impacts for individuals, and are broken down by gender, a key dimension on which we may be concerned about unequal effects.[14]

At the individual level, we confirm the statistically significant 5 percentage point in the probability that an evicted individual is employed. There is suggestive evidence that this impact is concentrated on women, but the difference in men's and women's effects is statistically insignificant. Overall, we see little evidence of negative effects concentrated among women. Women and men do, however, adjust differently. Men's increase in labor market participation is largely driven by increases in wage labor, while women's is driven by an increase in self-employment. While men see a 3 hour increase in hours work, women see a smaller statistically insignificant 1.5 hour increase. Finally men see larger increases in commuting and commute costs, while women see a smaller increase in commute time, and no statistically significant increase in commute cost, probably reflecting the fact that women's self employment is within the neighbourhood.

Table A9, Panel A, shows very little evidence that eviction affects job quality. There is some evidence of a decrease in manual wage work, but this is economically small and statistically weak. Men's and women's experiences, however, are a little different. Conditional on work, eviction sees men move from a 45 to a 49 hour work week, an effect that is statistically significant at the 10% level. Whether this is a positive or negative depends very much on how households weight consumption and leisure. Women on the other hand see a reduction in manual wage work, an increase in self employment in retail work and a small decrease in hours worked and earnings conditional on working (neither are significant). One interpretation would be that women move further from wage work opportunities and are limited in their commuting opportunities so move to self-employment near the home.

4.6 Endogenous Amenity

The negative social network impacts that we report above are of direct concern, but they may also be a harbinger of greater social and environment problems. If social networks are not strong, then community cohesion may suffer resulting in increased crime, violence and environmental decay. Again, this is an endogenous long run outcome that preservers fear will be the result of slum clearance.

Table 7 shows medium term impacts on our measures of location amenity and social cohesion.

[14] Commute costs in these tables are a monthly variable, while transport costs in the main tables are a weekly variable.

Table 6: Effects on Household-Level Labor Market Outcomes and Expenditures

	Labor Market		**Expenditure Per Capita**				
	Share Working	Earnings Per Capita	Total	Transport	Rent/ Mortgage	Food	Food Price Index
Panel A: RDD-Estimated Treatment Effects							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RD_Estimate	0.066	244.428	247.342	25.322	202.106	27.976	-0.021
	(0.028)**	(94.127)***	(83.008)***	(7.391)***	(30.095)***	(56.477)	(0.095)
Control Mean	0.51	958.43	1271.59	56.46	55.91	821.57	-0.05
Observations	1819	1819	1819	1819	1819	1819	1788
Panel B: Placebo Treatment Effects							
RD_Estimate	0.000	-48.255	-43.126	-19.769	-17.405	-0.801	-0.172
	(0.041)	(121.263)	(126.876)	(9.093)**	(36.036)	(89.349)	(0.119)
Control Mean	0.52	953.27	1421.66	56.60	109.92	902.90	-0.01
Observations	844	844	844	844	844	844	842
Panel C: Spillover Effects							
Exposure	-0.007	-15.052	-7.052	0.346	-1.868	-7.836	-0.002
	(0.003)**	(8.986)*	(7.618)	(0.610)	(2.001)	(5.764)	(0.009)
Control Mean	0.52	1007.24	1371.32	55.11	77.86	895.42	-0.02
Observations	3529	3529	3529	3529	3529	3529	3515
Panel D: Spillover-Corrected Treatment Effects							
RD_Estimate	0.059	229.376	240.290	25.668	200.238	20.140	-0.023
	(0.028)**	(94.555)**	(83.357)***	(7.416)***	(30.162)***	(56.770)	(0.096)
RDD Bandwidth	0.18	0.18	0.18	0.18	0.18	0.18	0.18
Project FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Columns correspond to different household dependent variables regressed against the running variable: distance from completed project boundary. Positive distances imply household resides inside the project boundary (treatment). All RDD regressions use a cutoff of 0 (the project boundary) such that the left and right of the cutoff are the treatment and control respectively. RD Estimates assess the difference in the dependent variable between treatment and control at the cutoff. Observations refers to the observations in each group that fall within the selected bandwidth. Panel A corresponds to households near completed project boundaries. Panel B corresponds to households near uncompleted project boundaries, thus forming a placebo sample. Panel C presents spillover results for different household dependent variables regressed against the a continuous exposure variable based on the amount of surrounding physical area evicted, with an additional control for placebo treated households and for total potential exposure, which includes exposure to uncompleted eviction sites. Panel D presents Panel A results with the SUTVA-corrected estimate, which accounts for the spillovers estimated in Panel C. All expenditure and income variables are winsorized at the 99th percentile. Index variables are based on the inverse covariance index of a set of z-scored variables. All subjective variables are z-scores where higher numeric value corresponds to better outcome. Baseline household covariates used as controls including household demographics, employment status of the household head, household assets, and household weekly expenditure. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Column 1 shows a 0.5 standard deviation improvement in our overall index of environmental amenity, a result that is strongly statistically significant. Disaggregating, we see this improvement comes from a reduction in the number of households reporting the smell of sewerage, and a reduction in reports of litter. Table A10 provides more details of the index, and shows further improvements in noise. Column 4 shows a statistically insignificant reduction in our safety index. Appendix Table A11 disaggregates, and shows some evidence of a concerning loss of social cohesion, with column 3 showing a 0.2 standard deviation increase in self reported "serious" (including violent) crime. Quantitatively, evicted households are 9 percentage points more likely to report having ever experienced a serious crime in their neighborhood, relative to a mean of 30 percent among un-evicted households. Only about 8 percent of respondents say that serious crime occurs at least once a month or more frequently, and there is no effect at this margin. Still, this is, perhaps, the main worrying aspect of our results, and is in line with some of the worst fears of preservers.

Finally, the last two columns of Table 7 show impacts on self reported life satisfaction, and predicted future life satisfaction. These are potential summary measures of all impacts and we see no statistically significant or economically meaningful impacts.

As has been the trend, we see no evidence of important discontinuities at placebo boundaries, although it is interesting to note that the coefficient on violent crime is very similar to that for our main results, highlighting the perils of taking one variable too seriously (Table A11). This is, however, the one time we see important spillovers, and these are uniformly negative. Those most exposed to the evictions report a lowered environmental amenity and safety. This makes sense. At the time of our endline data collection, the cleared areas had not been redeveloped in any way, but had been left to decay, with standing water and garbage clearly visible in satellite photos. Hopefully this is only a short run impact and will dissipate when re-development is complete. Correcting for these spillovers makes little difference to the direct effects, as shown in Panel D.

Once again we conclude that there is little support or the arguments of critics. While social networks are harmed, this does not lead to broad community decay in our sample. Nor does it lead to a drop in self reported life-satisfaction, a potentially summary measure of all impacts.

Table 7: Effects on Endogeneous Amenities and Life Satisfaction

	Environmental Amenities			Crime	Subjective Well-being	
	Environ-mental Amenities Index	Smell of sewerage (-)	Litter	Safety Index	Life satis-faction	Life satis-faction - predicted 1 years
Panel A: RDD-Estimated Treatment Effects						
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.506	0.539	0.128	-0.126	0.032	-0.013
	(0.094)***	(0.096)***	(0.101)	(0.092)	(0.094)	(0.037)
Control Mean	-0.20	-0.10	-0.08	-0.03	-0.07	-0.09
Observations	1805	1805	1805	1805	1819	1805
Panel B: Placebo Treatment Effects						
RD_Estimate	-0.074	-0.022	-0.058	-0.147	-0.066	0.006
	(0.140)	(0.127)	(0.150)	(0.140)	(0.140)	(0.042)
Control Mean	0.00	-0.07	-0.05	-0.00	0.05	0.05
Observations	844	844	844	844	844	844
Panel C: Spillover Effects						
Exposure	-0.027	-0.009	0.003	-0.023	0.003	0.007
	(0.009)***	(0.009)	(0.009)	(0.008)***	(0.008)	(0.010)
Control Mean	-0.05	-0.06	-0.03	0.02	0.03	-0.01
Observations	3528	3528	3528	3528	3529	3528
Panel D: Spillover-Corrected Treatment Effects						
RD_Estimate	0.479	0.530	0.131	-0.149	0.035	-0.006
	(0.095)***	(0.096)***	(0.101)	(0.093)	(0.094)	(0.038)
RDD Bandwidth	0.18	0.18	0.18	0.18	0.18	0.18
Project FEs	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Columns correspond to different household dependent variables regressed against the running variable: distance from completed project boundary. Positive distances imply household resides inside the project boundary (treatment). All RDD regressions use a cutoff of 0 (the project boundary) such that the left and right of the cutoff are the treatment and control respectively. RD Estimates assess the difference in the dependent variable between treatment and control at the cutoff. Observations refers to the observations in each group that fall within the selected bandwidth. Panel A corresponds to households near completed project boundaries. Panel B corresponds to households near uncompleted project boundaries, thus forming a placebo sample. Panel C presents spillover results for different household dependent variables regressed against the a continuous exposure variable based on the amount of surrounding physical area evicted, with an additional control for placebo treated households and for total potential exposure, which includes exposure to uncompleted eviction sites. Panel D presents Panel A results with the SUTVA-corrected estimate, which accounts for the spillovers estimated in Panel C. All expenditure and income variables are winsorized at the 99th percentile. Index variables are based on the inverse covariance index of a set of z-scored variables. All subjective variables are z-scores where higher numeric value corresponds to better outcome. Baseline household covariates used as controls including household demographics, employment status of the household head, household assets, and household weekly expenditure. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5 Heterogeneity and Compensation Type

Our sample of evictees can be broken into three groups based on their ex-ante tenure status. 68% of our sample lived in Kebele housing, 20% were private renters or illegal squatters, and 12% were private owners. These categories determine both what they lost and how they were compensated, and allow us to understand a little about the importance of compensation and the functioning of housing markets.

Those living in Kebele housing were, ex-ante, the recipients of a government subsidy. They lived nearly rent free in a central area. As we will see, the opportunity cost for the government was large. Eviction took away this implicit entitlement, and residents were compensated. They were offered the option to buy a "condominium": one of the newly built government houses constructed on the outskirts of the city. These were sold to eligible households with access to a subsidized mortgage. Households that could not make the required down payment for a condominium house, or simply did not want to buy one, were offered alternative kebele housing. This alternative kebele housing was located all over the city, and the location offered was chosen by government.

Private renters, in contrast, were not ex-ante recipients of government subsidy. They received no compensation and had to make their own arrangements. Studying their responses allows us to understand how households would have responded without compensation, and to assess whether rental markets work sufficiently well to allow evictees to find suitable alternative accommodation. In an efficient equilibrium, we would expect them to find a suitable substitute for their pre-eviction home. They might pay a disruption cost of having to move but we would expect minimal loss in the medium term. If housing markets are inefficient, however, negative impacts could be substantial.

Finally, private owners were given new land, usually toward the edge of the city, and a relatively large cash endowment (equivalent to 2 years of household income, on average) as compensation.

Table 8 reports the impacts of eviction on housing type, broken down into these three categories, as well as the average impacts. Panel B shows that compensation for kebele residents was incomplete. Compared to those who were not evicted, evictees are about 22% more likely to live in a condo, 45% less likely to live in Kebele housing, and 21% more likely to be in private rentals. This is consistent with 61% of these households being offered a condominium, and about 78% of them accepting it. Panel C shows that, for the most part, private renters continued in the private rental market, although some seemed to have been able to secure a condo. They move shorter distances (note, from the control mean, that this group is more mobile anyway) and do not move to significantly less dense areas. Panel D shows that most private owners continue as such (although some move into renting) but much further from the centre, consistent with them taking up the offer of land on periphery.

Table 9 reports regression discontinuity estimates of the impact of eviction for each ex-ante tenure

Table 8: First Stage Results by Original Tenure.

	Distance to Centre (km)	Distance Moved	Pop. Density (census)	Lives in Condo	Rents Kebele	Rents Privately	Privately Owned (non-condo)
Panel A: Average Treatment Effects							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RD_Estimate	4.425	5.353	-5632.18	0.182	-0.329	0.177	-0.030
	(0.453)***	(0.489)***	(1767.39)***	(0.029)***	(0.037)***	(0.037)***	(0.026)
Control Mean	3.86	0.65	33796.36	0.02	0.63	0.18	0.16
Observations	1805	1805	1805	1819	1819	1819	1819
Panel B: Kebele Housing at Baseline							
RD_Estimate	4.256	5.295	-5090.06	0.218	-0.441	0.207	0.016
	(0.522)***	(0.575)***	(2066.14)**	(0.038)***	(0.047)***	(0.043)***	(0.028)
Control Mean	3.56	0.57	36824.31	0.02	0.85	0.10	0.03
Observations	1258	1258	1258	1268	1268	1268	1268
Panel C: Housing Privately Rented at Baseline							
RD_Estimate	2.150	2.726	-297.00	0.085	-0.034	0.013	-0.065
	(1.264)*	(1.409)*	(4635.66)	(0.055)	(0.065)	(0.106)	(0.069)
Control Mean	4.57	1.66	29205.36	0.04	0.16	0.75	0.05
Observations	302	302	302	304	304	304	304
Panel D: Housing Privately Owned at Baseline							
RD_Estimate	5.823	6.412	-9521.78	0.027	0.027	0.097	-0.151
	(1.268)***	(1.293)***	(4315.13)**	(0.046)	(0.073)	(0.098)	(0.112)
Control Mean	4.24	0.21	24021.16	0.00	0.05	0.08	0.87
Observations	245	245	245	247	247	247	247
Bandwidth	0.18	0.18	0.18	0.18	0.18	0.18	0.18
Project Site FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Joint sig. P	0.114	0.109	0.344	0.003	0.000	0.044	0.011

Notes: Columns correspond to different household dependent variables regressed against the running variable: distance from completed project boundary. Positive distances imply household resides inside the project boundary (treatment). All RDD regressions use a cutoff of 0 (the project boundary) such that the left and right of the cutoff are the treatment and control respectively. RD Estimates assess the difference in the dependent variable between treatment and control at the cutoff. Observations refers to the observations in each group that fall within the selected bandwidth. Panel A corresponds to all households near completed project boundaries. Panels B-D represent sub-samples based on baseline housing type: households who owned their housing (Panel B), households who rented private housing (Panel C), households who lived in public/Kebele housing (Panel D). All expenditure and income variables are winsorized at the 99th percentile and logged. Index variables are based on the inverse covariance index of a set of z-scored variables. All subjective variables are z-scores where higher numeric value corresponds to better outcome. Baseline household covariates used as controls including household demographics, employment status of the household head, household assets, and household weekly expenditure. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

type. We see these as intent to treat estimates. We show results for a subset of the outcomes presented above that show strong impacts. Panel A shows the aggregate impacts for these outcomes, with the remaining panels reporting heterogeneity.

Looking across the ex-ante housing groups there are a few key differences. First, those in Kebele housing ex-ante are the majority of the sample, and unsurprisingly their results are very similar to the aggregate results discussed above.

Second, those who were not compensated, and are left to find alternative housing on their own, show a different pattern of trade-offs from those who were compensated. They choose to move less far (Column 2 - 3, Panel C of Table 8), do not choose higher quality housing (Column 4 Table 9) but also do not see as large a reduction in the quality of their social networks, although the differences are insignificant (Column 6 Table 9). We do not know whether this set of choices reflects preference for location over housing quality, because the high quality condos offered to kebele residents were, for the most part, not available to renters.

Overall, subject to the caveat that renters are small proportion of our sample and so our power is low, we see these results as consistent with the view that rental markets are well functioning and evicted renters were able to find alternative homes that are close substitutes. Even without compensation, households do not seem to suffer the large losses that concern preservers.

Third, the group in Panel D, who owned their slum homes and were compensated with peripheral land, seem to fare the worst in terms of social networks and reductions in life satisfaction. Again, acknowledging lower statistical power for this group, the pattern is consistent with their compensation package falling short of the value they lost, highlighting the importance of designing compensation schemes that fully offset the costs of displacement.

6 Cost–benefit accounting of the compensation scheme

This section provides some back of the envelope cost benefit calculations. We cannot account for all benefits, nor all costs, but the results are suggestive that the program generates positive returns.

We start by summarizing the results above as showing a zero cost to evicted households. We believe this to be broadly consistent with the results, and avoids us having to convert impacts into monetery values to compare with government costs. While social connections are weakened, this does not translate into worse economic outcomes, and could be mitigated by co-locating evicted households at minimal additional fiscal cost. This means the primary costs and benefits lie with the government which must compensate evictees, but gain access to land in return. We look only at the compensation paid to those who were living in Kebele housing, who make up the bulk or our sample. We also assume that compensation would always be provided in the form of a condominium, consistent with a large scale program that removes all kebele housing.

Table 9: Results by original tenure - main outcomes

	Earnings Per Capita	Non-Rent/Transport Expenditure	Rent	Housing Quality Index	Environmental Amenities Index	Social Network Index	Life satisfaction today
Panel A: Average Treatment Effects							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RD_Estimate	244.428	19.534	202.106	0.524	0.506	-0.546	0.032
	(94.127)***	(68.581)	(30.095)***	(0.102)***	(0.094)***	(0.100)***	(0.094)
Control Mean	958.43	1148.63	55.91	-0.06	-0.20	0.04	-0.07
Observations	1819	1819	1819	1819	1805	1819	1819
Panel B: Kebele Housing at Baseline							
RD_Estimate	177.755	-50.170	187.509	0.622	0.471	-0.472	0.115
	(108.576)	(79.556)	(33.397)***	(0.118)***	(0.112)***	(0.139)***	(0.112)
Control Mean	915.46	1131.35	13.03	-0.11	-0.22	0.05	-0.12
Observations	1268	1268	1268	1268	1258	1268	1268
Panel C: Housing Privately Rented at Baseline							
RD_Estimate	503.659	90.067	229.588	0.103	0.268	-0.309	-0.083
	(246.890)**	(169.343)	(90.552)**	(0.223)	(0.221)	(0.215)	(0.194)
Control Mean	1104.50	1160.12	312.76	-0.14	-0.07	-0.12	-0.09
Observations	304	304	304	304	302	304	304
Panel D: Housing Privately Owned at Baseline							
RD_Estimate	236.528	342.059	234.410	0.414	0.789	-0.987	-0.218
	(278.176)	(176.542)*	(88.290)***	(0.272)	(0.259)***	(0.237)***	(0.207)
Control Mean	1095.64	1203.97	25.94	0.26	-0.19	0.07	0.26
Observations	247	247	247	247	245	247	247
Bandwidth	0.18	0.18	0.18	0.18	0.18	0.18	0.18
Project Site FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Joint sig. P	0.350	0.039	0.655	0.055	0.290	0.074	0.214

Notes: Columns correspond to different household dependent variables regressed against the running variable: distance from completed project boundary. Positive distances imply household resides inside the project boundary (treatment). All RDD regressions use a cutoff of 0 (the project boundary) such that the left and right of the cutoff are the treatment and control respectively. RD Estimates assess the difference in the dependent variable between treatment and control at the cutoff. Observations refers to the observations in each group that fall within the selected bandwidth. Panel A corresponds to all households near completed project boundaries. Panels B-D represent sub-samples based on baseline housing type: households who owned their housing (Panel B), households who rented private housing (Panel C), households who lived in public/Kebele housing (Panel D). All expenditure and income variables are winsorized at the 99th percentile and logged. Index variables are based on the inverse covariance index of a set of z-scored variables. All subjective variables are z-scores where higher numeric value corresponds to better outcome. Baseline household covariates used as controls including household demographics, employment status of the household head, household assets, and household weekly expenditure. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The benefit to the government is they can now improve or sell off the central land for other purposes. First, we can roughly calculate the unimproved value of these homes. In 2017, our baseline survey records the average rent for similar-quality, centrally located slum housing that is rented on the private market at 1,276 Birr per month, or about 720USD annually. Capitalising this stream with a price-to-earnings ratio of 15 (on the low end for a developed country urban housing market) yields a valuation of around 11,000USD. We see this undeveloped value as a lower bound on the benefit to the government. Second, we can attempt to calculate the value of the land for redevelopment after the existing housing is removed. Auction prices for consolidated, centrally located plots averaged 22,000–25,000 birr per m^2 in 2013—or about 1,100USD per m^2.[15] This is land that is not yet developed but comes with development permission. With a median slum-house footprint of 30m^2, the land under each kebele home is worth roughly 33,000 USD in the auction market. These estimates may still be on the low end if redevelopment creates broader economic gains Henderson et al. (2021). In summary, a conservative estimate of the benefit to the government is between 11,000 and 33,000 USD per evicted household, with social benefits potentially larger.

Each evicted household who was living in Kebele housing was offered the opportunity to purchase a newly built 40m^2 flat. These units cost the city approximately 22,000 USD to construct, but are sold to beneficiaries for just 10,500 USD.[16] The cost to the government is thus 11,500 USD per evicted household.

In summary, the benefit to the government is between 11,000 and 33,000 USD per evicted household, and the cost of compensation is about 11,500 USD per household. If one is willing to buy our argument that there are negligible welfare costs to evictees it is hard to escape the conclusion that the program creates more benefits than costs.

7 Robustness

As is always the case with regression discontinuity estimates, different bandwidths can yield different estimates. We present robustness tests in Appendix C, reporting results for each broad outcome area. For each outcome we show naive OLS comparisons between evicted and non-evicted, our preferred 0.18 km bandwidth, narrower bandwidths (0.15 km and 0.10 km), and the optimal bandwidth from Calonico et al. (2019).[17]

Overall, bandwidth choice does not alter quantitative results in economically or statistically important ways and our qualitative results remain the same regardless of bandwidth choice.

[15] Own calculations from city land-auction data.
[16] See Franklin (2025) for detailed calculations.
[17] Recall that 0.18 km is the largest feasible bandwidth that has equal distance on treatment and control sides.

8 Conclusions

Should slums be preserved or cleared? We provide what we believe to be the first causal evidence on the impacts on evictees of a large scale slum clearance program.

Our results show that it is possible to move residents out of slums without large welfare costs. Two cautions are due. First, the program must be well designed and include compensation. We doubt we would have seen similar results if the Ethiopian government had not build new Condo houses and made them available, and we show that avoiding negative social impacts requires being careful to move people *with* their neighbours. Second, we see one concerning impact. Evicted households report more serious (including violent) crime in their neighbourhoods. This may simply reflect noise in our data, but we intend to collect longer run data to understand how worrying this impact is.

Our back of the envelope calculations suggest that required compensation can be easily paid for if the government can capture even a portion of the increased land value that comes from re-development. If there are any economic growth benefits from increasing density in central locations, then it is hard to escape the view that well designed clearance programs can be part of a developing country urban renewal and economic growth plan.

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A Additional Tables

Table A1: Evictions around the World

Date	Location	Evictions	Compensation Details	Source
2004-2010	Mumbai, India	300,000	Cash, housing, or both	Mumbai's SRA
2005-2010	Lagos, Nigeria	200,000	Limited compensation	Amnesty International
2009-2011	Rio de Janeiro, Brazil	19,000	Cash, housing, or rental aid	Amnesty International
2013-2016	Jakarta, Indonesia	40,000	Partial housing assistance	Human Rights Watch
2016-2019	Nairobi, Kenya	30,000	Rare compensation or housing	Amnesty International
2018-2021	Addis Ababa, Ethiopia	40,000	Cash or housing assistance	Al Jazeera
2009-2014	Istanbul, Turkey	7,500	Partial housing or cash	Urban Transformation
2010-2014	Cape Town, South Africa	12,000	Limited compensation/housing	GroundUp
2012-2016	Detroit, USA	8,000	Cash, housing, or both	Detroit News
2015-2018	Ho Chi Minh City, Vietnam	4,500	Partial housing or cash	ResearchGate
2011-2016	Harare, Zimbabwe	20,000	Limited compensation	Human Rights Watch
2014-2017	Manila, Philippines	14,000	Cash, housing, or both	ADB
2016-2018	Mexico City, Mexico	5,000	Cash, housing, or rental aid	ResearchGate
2010-2013	Baku, Azerbaijan	3,000	Limited compensation	Human Rights Watch
2013-2016	Quito, Ecuador	2,000	Partial housing or cash	Habitat for Humanity
2012-2015	Santiago, Chile	1,500	Cash, housing, or both	ScienceDirect
2018-2020	Tbilisi, Georgia	1,000	Limited compensation/housing	ResearchGate
2017-2020	Dhaka, Bangladesh	25,000	Limited compensation	Amnesty International
2018-2021	Karachi, Pakistan	21,000	Rare compensation or housing	Human Rights Watch
2019-2021	Phnom Penh, Cambodia	3,000	Limited compensation	Amnesty International

Table A2: RDD Balance Test for Main Baseline Variables

Baseline Variable	RD Estimate	Std. Error	Effective Obs
People living in household	-0.107	0.263	1,819
Male Head of Household	-0.031	0.066	1,819
Number of kids under 5	0.112	0.081	1,819
Number of kids between 5 and 13	-0.001	0.087	1,819
Number of kids between 13 and 18	0.032	0.096	1,819
Average age at baseline- adults	-0.912	1.197	1,819
Employed head of household	0.085	0.066	1,819
Permanent Employment	0.034	0.049	1,819
Education of most educated HH member	-0.104	0.467	1,819
Privately Owned	0.031	0.047	1,819
Privately Rented	0.087*	0.049	1,819
Mud/dung floor	0.070	0.063	1,819
Cement screed floor	-0.097	0.065	1,819
Corrugated iron sheet roof	-0.022*	0.013	1,819
Mud/wood walls	0.050	0.034	1,819
Flush toilet	-0.002	0.011	1,819
Shared pit latrine or public toilet	0.025	0.036	1,819
Piped water	-0.074	0.047	1,819
Shared water source	0.055	0.063	1,819
Cook with firewood or charcoal	0.074	0.068	1,819
Weekly food expenditure	-26.389	46.692	1,819
Asset index	-0.135**	0.066	1,819
Ethnicity - Amahara	0.086	0.064	1,819
Ethnicity - Oromo	-0.004	0.056	1,819
Ethnicity - Tigrayan	-0.024	0.036	1,819

Table A3: RDD Placebo Balance Test for Key Baseline Variables

Baseline Variable	RD Estimate	Std. Error	Effective Obs
People living in household	0.061	0.325	844
Male Head of Household	0.124	0.095	844
Number of kids under 5	0.026	0.128	844
Number of kids between 5 and 13	-0.259*	0.149	844
Number of kids between 13 and 18	0.076	0.134	844
Average age at baseline- adults	-0.170	1.651	844
Employed head of household	0.159*	0.094	844
Permanent Employment	0.058	0.077	844
Education of most educated HH member	0.374	0.642	844
Privately Owned	-0.025	0.043	844
Privately Rented	-0.023	0.066	844
Mud/dung floor	-0.013	0.096	844
Cement screed floor	-0.014	0.101	844
Corrugated iron sheet roof	-0.008	0.011	844
Mud/wood walls	-0.175**	0.075	844
Flush toilet	-0.036	0.025	844
Shared pit latrine or public toilet	0.001	0.055	844
Piped water	0.025	0.065	844
Shared water source	-0.058	0.096	844
Cook with firewood or charcoal	0.192**	0.097	844
Weekly food expenditure	28.898	45.728	844
Asset index	0.023	0.084	844
Ethnicity - Amahara	-0.113	0.104	844
Ethnicity - Oromo	0.065	0.080	844
Ethnicity - Tigrayan	-0.015	0.048	844

Table A4: First Stage - Change in Neighborhood Characteristics

	Pop. Density	Predicted Poverty Rate	Num. Rooms	Age	Years Education	Owns TV	Private Toilet	Migrant Rate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD_Estimate	-5632.183	0.021	0.253	-2.703	-0.368	0.110	-0.021	0.029
		(0.012)*	(0.076)***	(0.255)***	(0.166)**	(0.030)***	(0.024)	(0.019)
	(1767.385)***							
Control Mean	33796.36	0.40	1.80	26.94	2.72	0.44	0.12	0.17
Observations	1805	1804	1796	1805	1707	1796	1796	1805
Bandwidth	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18
Project FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Columns correspond to different household dependent variables regressed against the running variable: distance from completed project boundary. Positive distances imply household resides inside the project boundary (treatment). All RDD regressions use a cutoff of 0 (the project boundary) such that the left and right of the cutoff are the treatment and control respectively. RD Estimates assess the difference in the dependent variable between treatment and control at the cutoff. Observations refers to the observations in each group that fall within the selected bandwidth. Panel A corresponds to households near completed project boundaries. Panel B corresponds to households near uncompleted project boundaries, thus forming a placebo sample. All expenditure and income variables are winsorized at the 99th percentile. Index variables are based on the inverse covariance index of a set of z-scored variables. All subjective variables are z-scores where higher numeric value corresponds to a better outcomes. Baseline household covariates used as controls including household demographics, employment status of the household head, household assets, and household weekly expenditure. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Effects on Housing Quality – Index and Sub-components

	Housing Quality Index	Num. of Rooms	Num. of Bedrooms	Share of Days with Stable Electricity	Num. of HHs Sharing Toilet	Separate Kitchen	Cook Using Electricity	Flush Toilet	Piped Water	Non-Mud Floor
Panel A: RDD-Estimated Treatment Effects										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
RD_Estimate	0.524	0.162	0.131	0.002	3.020	0.100	0.008	0.240	0.121	0.085
	(0.102)***	(0.077)**	(0.065)**	(0.025)	(1.232)**	(0.044)**	(0.045)	(0.038)***	(0.046)***	(0.033)**
Control Mean	-0.06	1.87	0.82	0.71	-9.42	0.52	0.69	0.13	0.44	0.81
Observations	1819	1819	1805	1766	1226	1805	1819	1819	1819	1819
Panel B: Placebo Treatment Effects										
RD_Estimate	0.055	0.013	-0.079	-0.004	0.894	-0.075	-0.008	0.027	0.074	0.019
	(0.123)	(0.104)	(0.084)	(0.032)	(1.714)	(0.062)	(0.056)	(0.037)	(0.067)	(0.045)
Control Mean	-0.18	1.84	0.82	0.71	-10.48	0.45	0.71	0.08	0.46	0.83
Observations	844	844	844	842	721	844	844	844	844	844
Panel C: Spillover Effects										
Exposure	-0.007	-0.001	0.008	-0.003	-0.442	0.004	0.001	0.007	-0.003	-0.002
	(0.008)	(0.008)	(0.006)	(0.002)*	(0.088)***	(0.004)	(0.004)	(0.003)**	(0.004)	(0.003)
Control Mean	-0.07	1.88	0.83	0.71	-8.95	0.49	0.69	0.11	0.46	0.82
Observations	3529	3529	3528	3510	2970	3528	3529	3529	3529	3529
Panel D: Spillover-Corrected Treatment Effects										
RD_Estimate	0.517	0.161	0.139	-0.002	2.579	0.104	0.008	0.246	0.118	0.082
	(0.102)***	(0.077)**	(0.066)**	(0.025)	(1.235)**	(0.044)**	(0.045)	(0.038)***	(0.046)**	(0.033)**
RDD Bandwidth	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18
Project FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Columns correspond to different household dependent variables regressed against the running variable: distance from completed project boundary. Positive distances imply household resides inside the project boundary (treatment). All RDD regressions use a cutoff of 0 (the project boundary) such that the left and right of the cutoff are the treatment and control respectively. RD Estimates assess the difference in the dependent variable between treatment and control at the cutoff. Observations refers to the observations in each group that fall within the selected bandwidth. Panel A corresponds to households near completed project boundaries. Panel B corresponds to households near uncompleted project boundaries, thus forming a placebo sample. Panel C presents spillover results for different household dependent variables regressed against a continuous exposure variable based on the amount of surrounding physical area evicted, with an additional control for placebo treated households and for total potential exposure, which includes exposure to uncompleted eviction sites. Panel D presents Panel A results with the SUTVA-corrected estimate, which accounts for the spillovers estimated in Panel C. All expenditure and income variables are winsorized at the 99th percentile. Index variables are based on the inverse covariance index of a set of z-scored variables. All subjective variables are z-scores where higher numeric value corresponds to better outcome. Baseline household covariates used as controls including household demographics, employment status of the household head, household assets, and household weekly expenditure. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Effects on Quality of Public Goods – Index and Sub-components

	Public Goods Index	Street Lights	Public Space	Primary School	Hospital
Panel A: RDD-Estimated Treatment Effects					
	(1)	(2)	(3)	(4)	(5)
RD_Estimate	-0.481	-0.272	-0.401	-0.271	-0.224
	(0.095)***	(0.092)***	(0.095)***	(0.095)***	(0.098)**
Control Mean	-0.02	-0.02	-0.05	0.02	0.00
Observations	1805	1805	1805	1805	1805
Panel B: Placebo Treatment Effects					
RD_Estimate	0.088	0.155	-0.030	0.158	-0.087
	(0.133)	(0.132)	(0.152)	(0.139)	(0.118)
Control Mean	0.06	-0.01	0.05	-0.03	0.13
Observations	844	844	844	844	844
Panel C: Spillover Effects					
Exposure	-0.021	-0.020	-0.016	-0.005	-0.007
	(0.009)**	(0.009)**	(0.009)*	(0.009)	(0.009)
Control Mean	0.09	0.06	0.05	0.05	0.04
Observations	3528	3528	3528	3528	3528
Panel D: Spillover-Corrected Treatment Effects					
RD_Estimate	-0.502	-0.292	-0.417	-0.276	-0.231
	(0.096)***	(0.093)***	(0.096)***	(0.096)***	(0.098)**
RDD Bandwidth	0.18	0.18	0.18	0.18	0.18
Project FEs	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes

Notes: Columns correspond to different household dependent variables regressed against the running variable: distance from completed project boundary. Positive distances imply household resides inside the project boundary (treatment). All RDD regressions use a cutoff of 0 (the project boundary) such that the left and right of the cutoff are the treatment and control respectively. RD Estimates assess the difference in the dependent variable between treatment and control at the cutoff. Observations refers to the observations in each group that fall within the selected bandwidth. Panel A corresponds to households near completed project boundaries. Panel B corresponds to households near uncompleted project boundaries, thus forming a placebo sample. Panel C presents spillover results for different household dependent variables regressed against the a continuous exposure variable based on the amount of surrounding physical area evicted, with an additional control for placebo treated households and for total potential exposure, which includes exposure to uncompleted eviction sites. Panel D presents Panel A results with the SUTVA-corrected estimate, which accounts for the spillovers estimated in Panel C. All expenditure and income variables are winsorized at the 99th percentile. Index variables are based on the inverse covariance index of a set of z-scored variables. All subjective variables are z-scores where higher numeric value corresponds to better outcome. Baseline household covariates used as controls including household demographics, employment status of the household head, household assets, and household weekly expenditure. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Effects on Quality of Neighbours Support – Index and Sub-components

	Neighbours Support Index	Neighbours - help fix	Neighbours - caring	Neighbours - advice	Difficult situations	Without someone to talk to
Panel A: RDD-Estimated Treatment Effects						
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-0.493	-0.198	-0.303	-0.362	-0.333	-0.351
	(0.091)***	(0.097)**	(0.098)***	(0.093)***	(0.091)***	(0.101)***
Control Mean	0.02	0.01	0.07	0.11	-0.05	-0.03
Observations	1805	1805	1805	1805	1805	1805
Panel B: Placebo Treatment Effects						
RD_Estimate	0.043	0.075	-0.055	-0.016	-0.019	0.135
	(0.136)	(0.142)	(0.129)	(0.132)	(0.133)	(0.122)
Control Mean	0.16	0.11	0.01	0.05	0.13	0.13
Observations	844	844	844	844	844	844
Panel C: Spillover Effects						
Exposure	-0.012	-0.010	0.011	0.013	-0.020	-0.025
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)**	(0.008)***
Control Mean	0.10	0.05	0.05	0.05	0.07	0.08
Observations	3528	3528	3528	3528	3528	3528
Panel D: Spillover-Corrected Treatment Effects						
RD_Estimate	-0.506	-0.208	-0.292	-0.348	-0.353	-0.377
	(0.092)***	(0.098)**	(0.098)***	(0.094)***	(0.091)***	(0.101)***
RDD Bandwidth	0.18	0.18	0.18	0.18	0.18	0.18
Project FEs	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Columns correspond to different household dependent variables regressed against the running variable: distance from completed project boundary. Positive distances imply household resides inside the project boundary (treatment). All RDD regressions use a cutoff of 0 (the project boundary) such that the left and right of the cutoff are the treatment and control respectively. RD Estimates assess the difference in the dependent variable between treatment and control at the cutoff. Observations refers to the observations in each group that fall within the selected bandwidth. Panel A corresponds to households near completed project boundaries. Panel B corresponds to households near uncompleted project boundaries, thus forming a placebo sample. Panel C presents spillover results for different household dependent variables regressed against the a continuous exposure variable based on the amount of surrounding physical area evicted, with an additional control for placebo treated households and for total potential exposure, which includes exposure to uncompleted eviction sites. Panel D presents Panel A results with the SUTVA-corrected estimate, which accounts for the spillovers estimated in Panel C. All expenditure and income variables are winsorized at the 99th percentile. Index variables are based on the inverse covariance index of a set of z-scored variables. All subjective variables are z-scores where higher numeric value corresponds to better outcome. Baseline household covariates used as controls including household demographics, employment status of the household head, household assets, and household weekly expenditure. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Effects on Individual Labor Market Outcomes by Gender

	Employed	Wage work	Self employed	Hours work	Earnings	Commut-ing time	Commut-ing cost	Walk to work (cond.)	Workplace dist to center
Panel A: Average Treatment Effects									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
RD_Estimate	0.051	0.057	-0.009	2.754	195.050	6.884	64.483	-0.065	4.146
	(0.027)*	(0.027)**	(0.018)	(1.495)*	(115.487)*	(1.491)***	(16.228)***	(0.041)	(1.006)***
Control Mean	0.50	0.34	0.11	21.73	1273.89	10.86	63.44	0.35	16.28
Observations	5249	5249	5249	5249	5249	5249	5249	2341	2072
Panel B: Men									
RD_Estimate	0.037	0.086	-0.044	3.744	270.237	8.395	104.543	-0.043	4.619
	(0.040)	(0.037)**	(0.026)*	(2.152)*	(210.377)	(2.015)***	(24.231)***	(0.048)	(1.370)***
Control Mean	0.54	0.38	0.12	24.68	1718.08	12.73	82.51	0.31	16.57
Observations	2336	2336	2336	2336	2336	2336	2336	1177	959
Panel C: Women									
RD_Estimate	0.058	0.012	0.036	1.749	57.858	5.223	31.168	-0.127	4.612
	(0.035)*	(0.032)	(0.022)	(1.780)	(107.920)	(1.612)***	(22.418)	(0.051)**	(1.178)***
Control Mean	0.47	0.31	0.10	19.60	956.21	9.62	51.64	0.36	15.70
Observations	2913	2913	2913	2913	2913	2913	2913	1164	1113
Bandwidth	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18
Project Site FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Male=female p	0.683	0.135	0.018	0.475	0.369	0.219	0.026	0.228	0.997

Notes: Columns correspond to different individual-level dependent variables regressed against the running variable: distance from completed project boundary. Positive distances imply household resides inside the project boundary (treatment). All RDD regressions use a cutoff of 0 (the project boundary) such that the left and right of the cutoff are the treatment and control respectively. RD Estimates assess the difference in the dependent variable between treatment and control at the cutoff. Observations refers to the observations in each group that fall within the selected bandwidth. Panel A corresponds to average treatment effects of all working-aged adults near completed project boundaries. Panel B and C corresponds to working-aged men and women respectively. All expenditure and income variables are winsorized at the 99th percentile. Baseline household covariates used as controls including household demographics, employment status of the household head, household assets, and household weekly expenditure. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Effects on Job Quality of Individuals by Gender

	Permanent work	Casual labour work	White collar wage work	Manual wage work	Self-employed retail work	Earnings (conditional)	Hours work (cond.)	Same job for 6 years
Panel A: Average Treatment Effects								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD_Estimate	0.016	0.012	0.006	-0.022	0.011	143.921	0.988	-0.036
	(0.021)	(0.011)	(0.015)	(0.010)**	(0.012)	(197.305)	(1.774)	(0.026)
Control Mean	0.16	0.03	0.07	0.03	0.04	2539.11	43.31	0.70
Observations	5249	5249	5249	5249	5249	2634	2634	5249
Panel B: Men								
RD_Estimate	0.012	0.014	0.016	-0.011	-0.008	299.383	3.988	-0.058
	(0.028)	(0.018)	(0.020)	(0.016)	(0.014)	(292.619)	(2.239)*	(0.037)
Control Mean	0.17	0.05	0.06	0.05	0.03	3196.05	45.91	0.69
Observations	2336	2336	2336	2336	2336	1280	1280	2336
Panel C: Women								
RD_Estimate	0.010	0.003	-0.004	-0.030	0.033	-86.364	-1.539	-0.021
	(0.024)	(0.008)	(0.017)	(0.010)***	(0.017)**	(182.163)	(2.116)	(0.033)
Control Mean	0.17	0.01	0.09	0.02	0.05	2016.72	41.33	0.70
Observations	2913	2913	2913	2913	2913	1354	1354	2913
Bandwidth	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18
Project Site FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Male=female p	0.949	0.579	0.470	0.307	0.058	0.263	0.073	0.459

Notes: Columns correspond to different individual-level dependent variables regressed against the running variable: distance from completed project boundary. Positive distances imply household resides inside the project boundary (treatment). All RDD regressions use a cutoff of 0 (the project boundary) such that the left and right of the cutoff are the treatment and control respectively. RD Estimates assess the difference in the dependent variable between treatment and control at the cutoff. Observations refers to the observations in each group that fall within the selected bandwidth. Panel A corresponds to average treatment effects of all working-aged adults near completed project boundaries. Panel B and C corresponds to working-aged men and women respectively. All expenditure and income variables are winsorized at the 99th percentile. Baseline household covariates used as controls including household demographics, employment status of the household head, household assets, and household weekly expenditure. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Effects on Neighborhood Quality – Index and Sub-components

	Environmental Amenities Index	Smell of trash (-)	Smell of sewerage (-)	Litter	Noise at Night	Noise during Day
Panel A: RDD-Estimated Treatment Effects						
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.506	0.449	0.539	0.128	0.217	0.253
	(0.094)***	(0.092)***	(0.096)***	(0.101)	(0.093)**	(0.092)***
Control Mean	-0.20	-0.12	-0.10	-0.08	-0.15	-0.17
Observations	1805	1805	1805	1805	1805	1805
Panel B: Placebo Treatment Effects						
RD_Estimate	-0.074	-0.010	-0.022	-0.058	-0.023	-0.079
	(0.140)	(0.127)	(0.127)	(0.150)	(0.147)	(0.139)
Control Mean	0.00	0.04	-0.07	-0.05	0.06	0.11
Observations	844	844	844	844	844	844
Panel C: Spillover Effects						
Exposure	-0.027	-0.025	-0.009	0.003	-0.036	-0.036
	(0.009)***	(0.009)***	(0.009)	(0.009)	(0.009)***	(0.009)***
Control Mean	-0.05	-0.00	-0.06	-0.03	-0.00	-0.00
Observations	3528	3528	3528	3528	3528	3528
Panel D: Spillover-Corrected Treatment Effects						
RD_Estimate	0.479	0.424	0.530	0.131	0.181	0.217
	(0.095)***	(0.092)***	(0.096)***	(0.101)	(0.094)*	(0.092)**
RDD Bandwidth	0.18	0.18	0.18	0.18	0.18	0.18
Project FEs	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Columns correspond to different household dependent variables regressed against the running variable: distance from completed project boundary. Positive distances imply household resides inside the project boundary (treatment). All RDD regressions use a cutoff of 0 (the project boundary) such that the left and right of the cutoff are the treatment and control respectively. RD Estimates assess the difference in the dependent variable between treatment and control at the cutoff. Observations refers to the observations in each group that fall within the selected bandwidth. Panel A corresponds to households near completed project boundaries. Panel B corresponds to households near uncompleted project boundaries, thus forming a placebo sample. Panel C presents spillover results for different household dependent variables regressed against the a continuous exposure variable based on the amount of surrounding physical area evicted, with an additional control for placebo treated households and for total potential exposure, which includes exposure to uncompleted eviction sites. Panel D presents Panel A results with the SUTVA-corrected estimate, which accounts for the spillovers estimated in Panel C. All expenditure and income variables are winsorized at the 99th percentile. Index variables are based on the inverse covariance index of a set of z-scored variables. All subjective variables are z-scores where higher numeric value corresponds to better outcome. Baseline household covariates used as controls including household demographics, employment status of the household head, household assets, and household weekly expenditure. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

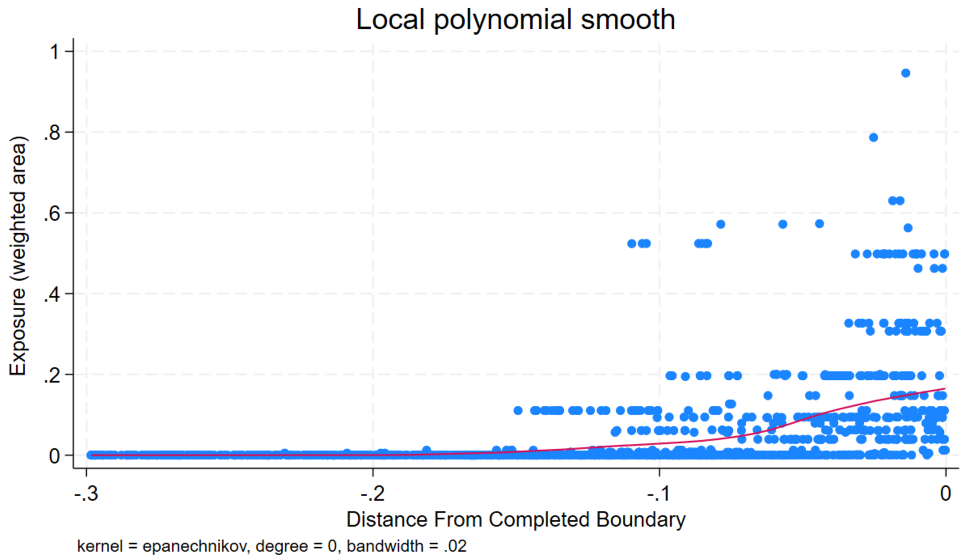
Table A11: Effects on Safety Measures – Index and Sub-components

	Safety Index	Burglary/theft	Serious Crime	Unsafe at Night
Panel A: RDD-Estimated Treatment Effects				
	(1)	(2)	(3)	(4)
RD_Estimate	-0.126	-0.033	-0.231	-0.054
	(0.092)	(0.097)	(0.083)***	(0.101)
Control Mean	-0.03	-0.01	-0.01	-0.05
Observations	1805	1805	1805	1805
Panel B: Placebo Treatment Effects				
RD_Estimate	-0.147	-0.087	-0.142	-0.121
	(0.140)	(0.139)	(0.135)	(0.147)
Control Mean	-0.00	-0.11	0.09	0.03
Observations	844	844	844	844
Panel C: Spillover Effects				
Exposure	-0.023	-0.016	-0.006	-0.032
	(0.008)***	(0.009)*	(0.008)	(0.008)***
Control Mean	0.02	-0.03	0.02	0.06
Observations	3528	3528	3528	3528
Panel D: Spillover-Corrected Treatment Effects				
RD_Estimate	-0.149	-0.049	-0.237	-0.085
	(0.093)	(0.098)	(0.083)***	(0.102)
RDD Bandwidth	0.18	0.18	0.18	0.18
Project FEs	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes

Notes: Columns correspond to different household dependent variables regressed against the running variable: distance from completed project boundary. Positive distances imply household resides inside the project boundary (treatment). All RDD regressions use a cutoff of 0 (the project boundary) such that the left and right of the cutoff are the treatment and control respectively. RD Estimates assess the difference in the dependent variable between treatment and control at the cutoff. Observations refers to the observations in each group that fall within the selected bandwidth. Panel A corresponds to households near completed project boundaries. Panel B corresponds to households near uncompleted project boundaries, thus forming a placebo sample. Panel C presents spillover results for different household dependent variables regressed against the a continuous exposure variable based on the amount of surrounding physical area evicted, with an additional control for placebo treated households and for total potential exposure, which includes exposure to uncompleted eviction sites. Panel D presents Panel A results with the SUTVA-corrected estimate, which accounts for the spillovers estimated in Panel C. All expenditure and income variables are winsorized at the 99th percentile. Index variables are based on the inverse covariance index of a set of z-scored variables. All subjective variables are z-scores where higher numeric value corresponds to better outcome. Baseline household covariates used as controls including household demographics, employment status of the household head, household assets, and household weekly expenditure. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B Additional Figures

Figure B1: Exposure to spillovers as function of distance to treated areas among untreated households



C Robustness to Bandwidth Selection

Table C12: Effect of Bandwidth Specification on RDD Results Using Distance Moved

	OLS with 0.15km	Bandwidth - 0.18km	Bandwidth - 0.15km	Bandwidth - 0.10km	Optimal Bandwidth
	(1)	(2)	(3)	(4)	(5)
Treatment Status	5.650				
	[0.492]***				
RD_Estimate		5.449	5.331	4.893	5.356
		[0.487]***	[0.498]***	[0.548]***	[0.496]***
Constant	-2.242				
	[1.364]				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	0.564	0.652	0.564	0.627	0.602
Bandwidth		0.18	0.15	0.10	0.16
Treatment Obs.		978	876	580	896
Control Obs.		827	825	730	827
Kernel		Manual	Manual	Manual	mserd
Bandwidth Method	1703	1805	1701	1310	1723

Notes: Columns correspond to the coefficient estimated by different regression specifications using the specified dependent variable. Positive distances imply household resides inside the project boundary (treatment). Column (1) reports an OLS estimate of the treatment effect of being inside vs. outside the eviction boundary for households within a bandwidth of 0.15km on either side of the boundary. Column (2-5) report estimates from running RDD regression with varying badnwidths. All RDD regressions use a cutoff of 0 (the project boundary) such that the left and right of the cutoff are the treatment and control respectively. Observations refers to the observations in each group that fall within the selected bandwidth. The selected bandwidths are as follows: column (2) uses 0.18km, column (3) uses 0.15km, column (4) uses 0.10km, and column (5) uses the optimal bandwidth estiamted using the *rdrobust* command in Stata. All expenditure and income variables are winsorized at the 99th percentile. Index variables are based on the inverse covariance index of a set of z-scored variables. All subjective variables are z-scores where higher numeric value corresponds to better outcome. Baseline household covariates used as controls including household demographics, employment status of the household head, household assets, and household weekly expenditure. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C13: Effect of Bandwidth Specification on RDD Results Using Distance from original neighbours (100m)

	OLS with 0.15km	Bandwidth - 0.18km	Bandwidth - 0.15km	Bandwidth - 0.10km	Optimal Bandwidth
	(1)	(2)	(3)	(4)	(5)
Treatment Status	4.359				
	[0.288]***				
RD_Estimate		4.057	3.857	3.370	3.679
		[0.290]***	[0.299]***	[0.341]***	[0.311]***
Constant	5.471				
	[0.840]***				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	1.582	1.629	1.582	1.948	1.251
Bandwidth		0.18	0.15	0.10	0.13
Treatment Obs.		976	874	580	774
Control Obs.		840	838	741	824
Kernel		Manual	Manual	Manual	mserd
Bandwidth Method	1714	1816	1712	1321	1598

Notes: Columns correspond to the coefficient estimated by different regression specifications using the specified dependent variable. Positive distances imply household resides inside the project boundary (treatment). Column (1) reports an OLS estimate of the treatment effect of being inside vs. outside the eviction boundary for households within a bandwidth of 0.15km on either side of the boundary. Column (2-5) report estimates from running RDD regression with varying badnwidths. All RDD regressions use a cutoff of 0 (the project boundary) such that the left and right of the cutoff are the treatment and control respectively. Observations refers to the observations in each group that fall within the selected bandwidth. The selected bandwidths are as follows: column (2) uses 0.18km, column (3) uses 0.15km, column (4) uses 0.10km, and column (5) uses the optimal bandwidth estiamted using the *rdrobust* command in Stata. All expenditure and income variables are winsorized at the 99th percentile. Index variables are based on the inverse covariance index of a set of z-scored variables. All subjective variables are z-scores where higher numeric value corresponds to better outcome. Baseline household covariates used as controls including household demographics, employment status of the household head, household assets, and household weekly expenditure. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C14: Effect of Bandwidth Specification on RDD Results Using Housing Quality Index

	OLS with 0.15km	Bandwidth - 0.18km	Bandwidth - 0.15km	Bandwidth - 0.10km	Optimal Bandwidth
	(1)	(2)	(3)	(4)	(5)
Treatment Status	0.543				
	[0.100]***				
RD_Estimate		0.527	0.528	0.519	0.525
		[0.101]***	[0.105]***	[0.124]***	[0.110]***
Constant	-1.429				
	[0.278]***				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	-0.047	-0.065	-0.047	-0.060	0.636
Bandwidth		0.18	0.15	0.10	0.13
Treatment Obs.		979	877	581	790
Control Obs.		840	838	741	826
Kernel		Manual	Manual	Manual	mserd
Bandwidth Method	1717	1819	1715	1322	1616

Notes: Columns correspond to the coefficient estimated by different regression specifications using the specified dependent variable. Positive distances imply household resides inside the project boundary (treatment). Column (1) reports an OLS estimate of the treatment effect of being inside vs. outside the eviction boundary for households within a bandwidth of 0.15km on either side of the boundary. Column (2-5) report estimates from running RDD regression with varying badnwidths. All RDD regressions use a cutoff of 0 (the project boundary) such that the left and right of the cutoff are the treatment and control respectively. Observations refers to the observations in each group that fall within the selected bandwidth. The selected bandwidths are as follows: column (2) uses 0.18km, column (3) uses 0.15km, column (4) uses 0.10km, and column (5) uses the optimal bandwidth estiamted using the *rdrobust* command in Stata. All expenditure and income variables are winsorized at the 99th percentile. Index variables are based on the inverse covariance index of a set of z-scored variables. All subjective variables are z-scores where higher numeric value corresponds to better outcome. Baseline household covariates used as controls including household demographics, employment status of the household head, household assets, and household weekly expenditure. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C15: Effect of Bandwidth Specification on RDD Results Using Public Goods Index

	OLS with 0.15km	Bandwidth - 0.18km	Bandwidth - 0.15km	Bandwidth - 0.10km	Optimal Bandwidth
	(1)	(2)	(3)	(4)	(5)
Treatment Status	-0.470				
	[0.098]***				
RD_Estimate		-0.480	-0.468	-0.465	-0.462
		[0.095]***	[0.100]***	[0.115]***	[0.104]***
Constant	-0.190				
	[0.272]				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	0.012	-0.024	0.012	0.056	0.649
Bandwidth		0.18	0.15	0.10	0.13
Treatment Obs.		978	876	580	770
Control Obs.		827	825	730	809
Kernel		Manual	Manual	Manual	mserd
Bandwidth Method	1703	1805	1701	1310	1579

Notes: Columns correspond to the coefficient estimated by different regression specifications using the specified dependent variable. Positive distances imply household resides inside the project boundary (treatment). Column (1) reports an OLS estimate of the treatment effect of being inside vs. outside the eviction boundary for households within a bandwidth of 0.15km on either side of the boundary. Column (2-5) report estimates from running RDD regression with varying badnwidths. All RDD regressions use a cutoff of 0 (the project boundary) such that the left and right of the cutoff are the treatment and control respectively. Observations refers to the observations in each group that fall within the selected bandwidth. The selected bandwidths are as follows: column (2) uses 0.18km, column (3) uses 0.15km, column (4) uses 0.10km, and column (5) uses the optimal bandwidth estiamted using the *rdrobust* command in Stata. All expenditure and income variables are winsorized at the 99th percentile. Index variables are based on the inverse covariance index of a set of z-scored variables. All subjective variables are z-scores where higher numeric value corresponds to better outcome. Baseline household covariates used as controls including household demographics, employment status of the household head, household assets, and household weekly expenditure. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C16: Effect of Bandwidth Specification on RDD Results Using Social Network Index

	OLS with 0.15km	Bandwidth - 0.18km	Bandwidth - 0.15km	Bandwidth - 0.10km	Optimal Bandwidth
	(1)	(2)	(3)	(4)	(5)
Treatment Status	-0.611				
	[0.095]***				
RD_Estimate		-0.563	-0.553	-0.486	-0.512
		[0.101]***	[0.107]***	[0.129]***	[0.120]***
Constant	-0.440				
	[0.263]*				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	0.035	0.036	0.035	0.058	0.668
Bandwidth		0.18	0.15	0.10	0.12
Treatment Obs.		979	877	581	681
Control Obs.		840	838	741	795
Kernel		Manual	Manual	Manual	mserd
Bandwidth Method	1717	1819	1715	1322	1476

Notes: Columns correspond to the coefficient estimated by different regression specifications using the specified dependent variable. Positive distances imply household resides inside the project boundary (treatment). Column (1) reports an OLS estimate of the treatment effect of being inside vs. outside the eviction boundary for households within a bandwidth of 0.15km on either side of the boundary. Column (2-5) report estimates from running RDD regression with varying badnwidths. All RDD regressions use a cutoff of 0 (the project boundary) such that the left and right of the cutoff are the treatment and control respectively. Observations refers to the observations in each group that fall within the selected bandwidth. The selected bandwidths are as follows: column (2) uses 0.18km, column (3) uses 0.15km, column (4) uses 0.10km, and column (5) uses the optimal bandwidth estiamted using the *rdrobust* command in Stata. All expenditure and income variables are winsorized at the 99th percentile. Index variables are based on the inverse covariance index of a set of z-scored variables. All subjective variables are z-scores where higher numeric value corresponds to better outcome. Baseline household covariates used as controls including household demographics, employment status of the household head, household assets, and household weekly expenditure. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C17: Effect of Bandwidth Specification on RDD Results Using Economic Network Index

	OLS with 0.15km	Bandwidth - 0.18km	Bandwidth - 0.15km	Bandwidth - 0.10km	Optimal Bandwidth
	(1)	(2)	(3)	(4)	(5)
Treatment Status	0.140				
	[0.103]				
RD_Estimate		0.132	0.137	0.131	0.138
		[0.104]	[0.109]	[0.126]	[0.110]
Constant	-0.227				
	[0.286]				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	-0.030	-0.045	-0.030	-0.012	0.818
Bandwidth		0.18	0.15	0.10	0.15
Treatment Obs.		979	877	581	862
Control Obs.		840	838	741	838
Kernel		Manual	Manual	Manual	mserd
Bandwidth Method	1717	1819	1715	1322	1700

Notes: Columns correspond to the coefficient estimated by different regression specifications using the specified dependent variable. Positive distances imply household resides inside the project boundary (treatment). Column (1) reports an OLS estimate of the treatment effect of being inside vs. outside the eviction boundary for households within a bandwidth of 0.15km on either side of the boundary. Column (2-5) report estimates from running RDD regression with varying badnwidths. All RDD regressions use a cutoff of 0 (the project boundary) such that the left and right of the cutoff are the treatment and control respectively. Observations refers to the observations in each group that fall within the selected bandwidth. The selected bandwidths are as follows: column (2) uses 0.18km, column (3) uses 0.15km, column (4) uses 0.10km, and column (5) uses the optimal bandwidth estiamted using the *rdrobust* command in Stata. All expenditure and income variables are winsorized at the 99th percentile. Index variables are based on the inverse covariance index of a set of z-scored variables. All subjective variables are z-scores where higher numeric value corresponds to better outcome. Baseline household covariates used as controls including household demographics, employment status of the household head, household assets, and household weekly expenditure. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C18: Effect of Bandwidth Specification on RDD Results Using Earnings Per Capita

	OLS with 0.15km	Bandwidth - 0.18km	Bandwidth - 0.15km	Bandwidth - 0.10km	Optimal Bandwidth
	(1)	(2)	(3)	(4)	(5)
Treatment Status	201.676				
	[102.218]**				
RD_Estimate		248.086	265.303	327.577	276.616
		[95.138]***	[98.837]***	[113.624]***	[100.633]***
Constant	797.395				
	[284.473]***				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	941.432	958.435	941.432	943.985	1010.884
Bandwidth		0.18	0.15	0.10	0.14
Treatment Obs.		979	877	581	830
Control Obs.		840	838	741	832
Kernel		Manual	Manual	Manual	mserd
Bandwidth Method	1717	1819	1715	1322	1662

Notes: Columns correspond to the coefficient estimated by different regression specifications using the specified dependent variable. Positive distances imply household resides inside the project boundary (treatment). Column (1) reports an OLS estimate of the treatment effect of being inside vs. outside the eviction boundary for households within a bandwidth of 0.15km on either side of the boundary. Column (2-5) report estimates from running RDD regression with varying badnwidths. All RDD regressions use a cutoff of 0 (the project boundary) such that the left and right of the cutoff are the treatment and control respectively. Observations refers to the observations in each group that fall within the selected bandwidth. The selected bandwidths are as follows: column (2) uses 0.18km, column (3) uses 0.15km, column (4) uses 0.10km, and column (5) uses the optimal bandwidth estiamted using the *rdrobust* command in Stata. All expenditure and income variables are winsorized at the 99th percentile. Index variables are based on the inverse covariance index of a set of z-scored variables. All subjective variables are z-scores where higher numeric value corresponds to better outcome. Baseline household covariates used as controls including household demographics, employment status of the household head, household assets, and household weekly expenditure. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C19: Effect of Bandwidth Specification on RDD Results Using Expenditure Per Capita

	OLS with 0.15km	Bandwidth - 0.18km	Bandwidth - 0.15km	Bandwidth - 0.10km	Optimal Bandwidth
	(1)	(2)	(3)	(4)	(5)
Treatment Status	229.078				
	[89.166]**				
RD_Estimate		257.176	283.818	371.559	353.409
		[84.622]***	[88.379]***	[101.365]***	[98.833]***
Constant	1553.762				
	[248.148]***				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	1285.008	1271.593	1285.008	1306.198	1384.114
Bandwidth		0.18	0.15	0.10	0.11
Treatment Obs.		979	877	581	631
Control Obs.		840	838	741	768
Kernel		Manual	Manual	Manual	mserd
Bandwidth Method	1717	1819	1715	1322	1399

Notes: Columns correspond to the coefficient estimated by different regression specifications using the specified dependent variable. Positive distances imply household resides inside the project boundary (treatment). Column (1) reports an OLS estimate of the treatment effect of being inside vs. outside the eviction boundary for households within a bandwidth of 0.15km on either side of the boundary. Column (2-5) report estimates from running RDD regression with varying badnwidths. All RDD regressions use a cutoff of 0 (the project boundary) such that the left and right of the cutoff are the treatment and control respectively. Observations refers to the observations in each group that fall within the selected bandwidth. The selected bandwidths are as follows: column (2) uses 0.18km, column (3) uses 0.15km, column (4) uses 0.10km, and column (5) uses the optimal bandwidth estiamted using the *rdrobust* command in Stata. All expenditure and income variables are winsorized at the 99th percentile. Index variables are based on the inverse covariance index of a set of z-scored variables. All subjective variables are z-scores where higher numeric value corresponds to better outcome. Baseline household covariates used as controls including household demographics, employment status of the household head, household assets, and household weekly expenditure. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C20: Effect of Bandwidth Specification on RDD Results Using Environmental Amenities Index

	OLS with 0.15km	Bandwidth - 0.18km	Bandwidth - 0.15km	Bandwidth - 0.10km	Optimal Bandwidth
	(1)	(2)	(3)	(4)	(5)
Treatment Status	0.604				
	[0.098]***				
RD_Estimate		0.536	0.505	0.377	0.426
		[0.094]***	[0.098]***	[0.114]***	[0.107]***
Constant	-0.373				
	[0.271]				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	-0.206	-0.199	-0.206	-0.298	0.659
Bandwidth		0.18	0.15	0.10	0.12
Treatment Obs.		978	876	580	688
Control Obs.		827	825	730	785
Kernel		Manual	Manual	Manual	mserd
Bandwidth Method	1703	1805	1701	1310	1473

Notes: Columns correspond to the coefficient estimated by different regression specifications using the specified dependent variable. Positive distances imply household resides inside the project boundary (treatment). Column (1) reports an OLS estimate of the treatment effect of being inside vs. outside the eviction boundary for households within a bandwidth of 0.15km on either side of the boundary. Column (2-5) report estimates from running RDD regression with varying badnwidths. All RDD regressions use a cutoff of 0 (the project boundary) such that the left and right of the cutoff are the treatment and control respectively. Observations refers to the observations in each group that fall within the selected bandwidth. The selected bandwidths are as follows: column (2) uses 0.18km, column (3) uses 0.15km, column (4) uses 0.10km, and column (5) uses the optimal bandwidth estiamted using the *rdrobust* command in Stata. All expenditure and income variables are winsorized at the 99th percentile. Index variables are based on the inverse covariance index of a set of z-scored variables. All subjective variables are z-scores where higher numeric value corresponds to better outcome. Baseline household covariates used as controls including household demographics, employment status of the household head, household assets, and household weekly expenditure. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.