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

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Abstract

Does living in well-located slums provide economic benefits essential to their residents? We analyse large-scale clearances in central Addis Ababa that displaced more than 40,000 households away from the centre, their neighbourhoods, and their social networks. Using a spatial regression-discontinuity design and rich primary survey data gathered before eviction and five years later, we estimate impacts on evicted households and on nearby non-evicted neighbours. Average effects are limited: labour-market outcomes improve modestly, while economic networks, subjective well-being and consumption remain unchanged, and spillovers are negligible. This pattern holds for both market-rent tenants, who received no compensation, and rent-free households, offered higher-quality government units of comparable value but further away. For the latter, who suffer larger losses in social ties, relocating alongside former neighbours substantially attenuates those losses. These findings indicate that the loss of free or cheap central access can be effectively compensated.

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

For the preservers, central slums are an essential and irreplaceable resource for the poor (Young and Wilmott, 1957; Jacobs, 1961).[1] Preservers see a fragile balance between community, centrality and housing quality. A community of similar people provides a unique social network that supports livelihoods, directly improves well-being, and provides basic public services (Perlman, 1976). But this network is fragile and subject to collapse if disturbed. A central location gives access to jobs and markets that are otherwise denied to the poor (Glaeser et al., 2008). Labor markets work poorly and commuting is costly, so moving to affordable high quality housing would cut the poor off from their livelihoods. Finally, low quality housing is a feature not a bug; slums don't attract the rich which keeps central locations affordable. For preservers any attempt at slum clearance of redevelopment is misguided – as Jane Jacobs wrote: "cities need old buildings."

For the clearers, central slums are inessential and may even be a trap for the poor (de Siqueira Filha et al., 2022; Alam and Mahal, 2014). Clearers see a much more robust environment. While community has value, social networks are robust and self-repairing, labor markets are spatially integrated and commuting means people need not work where they live, and far from a feature the low quality of housing represents a failure to provide public goods that harms the health and welfare of the poor. On this view, clearing is unlikely to harm and can be an opportunity to provide access to better quality housing. If unsanitary conditions in slums act to reduce health and trap residents in poverty, then clearance may even increase earnings (Marx et al. 2013).[2] A weaker version of this view is that clearance may have involve some adjustment costs but these, and other losses of value of lower-than-market rents can be fully compensated. An added bonus of clearance is that it frees misallocated land for redevelopment that could more than cover the costs of such compensation (Henderson et al. (2021); Lall et al. (2008)).

[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 labour supply (Field, 2007; Franklin, 2020).

We adjudicate between the views of preservers and clearers by studying a large-scale forced eviction program in Addis Ababa, which relocated households out of central slums. The design of our study is analytically informative: relocation was compulsory, while the comparison group remained in the slums—representing the status quo. In addition, the majority displaced households, those who lost access to free slum housing, were offered subsidized access to newly built government condominiums; and while these new homes were not centrally located, they remained within the metropolitan area and were generally considered to be of high quality. Another group of renters were uncompensated. We combine a baseline survey of over 30,000 households conducted before the evictions with detailed spatial data on eviction sites to identify households living near sharp displacement boundaries. Bespoke follow-up data collected four years after clearance enable us to implement a regression discontinuity design, addressing both identification and measurement challenges and allowing us to examine causal impacts across the multiple domains of disagreement: social networks, economic networks, 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 view of 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. But these benefits depend critically on implementation—clearance alone is not sufficient, and poorly designed programs risk substantial harm.

We begin by documenting the impact of the program on location, geographically based social networks, and pre-clearance amenity of destination neighborhoods. Affected households, on average, live 5km, or 18 minutes, further from the city center, and nearly 6km further from their original set of neighbours, confirming critics concerns about both geographic and social isolation.[3] The areas they move to, however, seem to have nicer ex-ante characteristics having lower population density and large homes in the 2007 census. We observe a 0.52 standard deviation decline in access to government provided public goods such as schools, and hospitals, driven by increased travel times to these services.

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

We then examine whether these spatial shifts had lasting social consequences four years later in our follow-up data. Consistent with critics' concerns, we find persistent declines in reported network quality: displaced households are more likely to report loneliness, report having 3 fewer people in their local social network, and express lower satisfaction with their social ties. However, these effects are not inevitable. Exploiting variation in how many neighbors were rehoused nearby, we show that households relocated within 1 km of their former neighbors experience no decline in network quality. This suggests that with thoughtful implementation, the social costs of clearance can be mitigated.

Economic networks appear more resilient. We find a small, statistically insignificant increase in an index of economic network quality. Disaggregated results show a modest reduction in the size of financial networks, but a corresponding increase in access to network-based finance. This pattern may reflect the distinct nature of economic ties, which can often be maintained through mobile phones and digital transfers and potentially rely more on weak links (Granovetter 1973). These findings highlight the differentiated impact of relocation on social versus economic connectivity.

We then examine labor market outcomes and expenditure patterns. Contrary to the critics' view, we find increases in both labor force participation and earnings. The share of household members working rises by 6 percentage points ($se = 0.062$), and earnings per household member increase by 200 Birr, from a base of about 950 Birr ($se = 94.388$). These additional earnings are largely absorbed by higher transport and rent costs, which rise by about 25 and 195 Birr respectively, but we find no evidence of declines in other categories of spending. These results suggest that commuting is feasible, job networks remain intact, and improved living conditions may enhance labor supply.

Finally we turn to endogeneous amenities. The results suggests that social isolation does not inevitably lead to broader environmental collapse. We find a statistically significant 0.5 standard deviation improvement in an index of housing quality, a 0.6 standard deviation increase in an index of environmental amenity, reflecting clean and quiet neighborhoods. Overall perceptions of safety remain unchanged, although there is a small uptick in perception of violent crime. There is no evidence here of a collapse in neighborhood quality due to a decline in the quality of social

cohesion. Together, these results reinforce that amenity gains are possible, but not automatic. The loss in access to public goods are not inherent to relocation and could be addressed through better planning.

We next examine how the average effects documented above differ across two distinct tenure groups, which together make up our study population. This heterogeneity is informative because the two groups correspond to the most common tenure arrangements found in slums worldwide, and because they received very different forms of compensation.

The first group—the majority of our sample—comprises long-standing residents of nationalised *kebele* plots who, for decades, paid no rent for centrally located land and basic services. This zero price was an implicit housing entitlement: an in-kind transfer whose market value rose sharply once the land was cleared and formalised. Clearance removed this entitlement, and the state compensated them with an offer that mirrored its size: heavily subsidised ownership of newly built condominiums on the urban edge, or larger rent-free slum units slightly further from the centre. These compensated households experienced larger gains in dwelling quality, housing costs that rose (but remained below market levels), and particularly strong increases in earnings and life satisfaction. The main cost was to social networks: the effects on long-standing social connections were particularly pronounced in this group. However, plausibly exogenous variation in proximity to former neighbours within the new housing shows that these social losses were substantially mitigated when neighbours were relocated nearby, suggesting that better relocation policy could preserve network benefits.

The second group consists of market renters who had been paying private landlords—often owners with leasehold title—for slum accommodation in the same neighbourhoods. Because these households never enjoyed a housing subsidy, clearance did not strip away an entitlement, and they received no monetary or in-kind compensation. Relative to the average effects, their welfare impacts were muted: after eviction they re-entered the private rental market, typically settling somewhat farther from the centre but in safer neighbourhoods and slightly better dwellings. Consistent with the absence of a lost entitlement, their commutes lengthened only modestly, housing quality improved slightly, and neither life satisfaction nor social connectedness declined

appreciably. This pattern suggests that access to central rental units is somewhat rationed, but that proximity to the CBD is not essential to the welfare of these households.

We also measure and account for spillovers. Using geo-coded data, we construct for each control (unevicted) household an exposure index—the weighted number of nearby evicted households—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 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 therefore overcome a potential SUTVA violation. Overall, we find limited evidence of spillovers of evictions on households in slums nearby, with exception of some loss of amenity and increase in crime.

The central contribution of our paper is to provide large-scale causal evidence on the value of access to low-quality central housing in developing-country cities. While the debate between preservers and clearers has been marked by strong priors on both sides, we lack robust evidence that directly adjudicates between them. Despite a growing number of quantitative studies of relocation programs, none to date has combined the necessary features to provide a clean emperical test. To adjudicate the debate, relocation must be compulsory, include some form of compensation, and be evaluated relative to the slum status quo. Most existing studies on related questions examine programs that fall short on one or more critical dimensions.

Most notable are programs that offer *voluntary* relocation to government housing (Barnhardt et al., 2017; Belchior et al., 2023; Franklin, 2025) Since take-up likely reflects selection on gains and households move only if they move to specific new housing sites; they cannot relocate to a location of their choice. These designs (where households are 'pulled" rather than "pushed" out of slums) tell us more about the quality of the new housing sites than about the value of the slums themselves. At the other extreme, punitive clearances offer little or no compensation and reflect political repression rather than urban planning (Carrillo et al., 2023). Rojas-Ampuero (2022) studies a program where some households receive improved housing in the centre while others are relocated further away. This isolates location effects (e.g., distance to the centre) but cannot

evaluate the broader bundle of costs and benefits involved in clearance, since the comparison group no longer resides in slums.

A related literature looks at the place-based consequences of urban renewal projects, including in the US (Collins and Shester, 2013; LaVoice, 2024), and slum upgrading and planning interventions developing countries (Harari and Wong, 2024; Gechter and Tsivanidis, 2020; Michaels et al., 2021). These studies do not look at the outcomes of households directly displaced by urban interventions. Our results are also consistent with a broader literature showing that unexpected mobility shocks can positive impacts (Nakamura et al., 2022; Deryugina et al., 2018), including out of low-income public housing and neighbourhoods 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 set the program's goal as 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." Some redevelopment schemes originally planned to accommodate evicted households back on-site in newly built government housing, but by our endline data collection, such redevelopment had not been realized. Additionally, due to the piecemeal and incomplete nature of the implementation—and its suspension in 2019—several designated eviction sites remained untouched, which we exploit as "placebo eviction sites" in our analysis.

Critically, households received little to no prior notice of the eviction plans. Our baseline data confirm that, for most eviction sites, 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.

Compensation depended on household's initial legal status. Broadly, there were three relevant types of status. First, many central residents lived in *kebele* housing—government-owned, low-quality dwellings rented at nominal rates. Residents of kebele housing were typically offered the chance to purchase government-built condominiums on the city's periphery, with subsidized mortgages and available financing. Those unable or unwilling to buy condominiums were usually given alternative kebele housing elsewhere in the city. However, there was substantial variation in the implementation of this policy, and not all kebele residents received alternative housing options as intended. Though initially viewed as temporary, the halt of redevelopment after 2019 meant most relocated households remain in these new kebele locations.

Second, 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. Third, renters and illegal squatters received no compensation.

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.

2 Data and Outcome Creation

Our data allows us to make progress on key identification problems that have prevented the impact evaluation of these types of slum clearing programs in other contexts. First, our geo-located data, spread across boundaries of cleared and non-cleared areas, allows us to implement a regression discontinuity approach to establish causality, the validity and interpretation of which we discuss in detail below. Importantly, because we have data both prior to and after the program, we are able to conduct analyses to show that there was no discontinuities at the boundary prior to the evictions. Second, we collected data on households located on either side of boundaries for planned evictions that never took place, which we refer to as placebo eviction sites. This

allows us to test for the extent of spillovers from slum clearance under a set of parallel trends and SUTVA assumptions that we motivated below. Finally, relative to applications that make use of existing data, our bespoke data set allows us to estimate causal impacts on a wide range of relevant welfare outcomes that theory predicts may be disrupted by evictions.

2.1 Redevelopment site data and maps

The construction of our dataset of relocated areas and the corresponding maps constituted a large part of the for this paper. We had to build this dataset using a range of government documents from the central planning agency and discussions with local officials.

We started by compiling a list of Local Development Plans (LDPs) from the Addis Ababa City plan office and Land Development and Urban Renewal Agency. Each plan involved a scoping study and mapping of a particular local area, and a proposed land-use plan. In almost all cases, the land-use plan completely overrides the old land-use situation, implying that eviction, demolition, and redevelopment is required to execute the plan. We received a large data repository of those LDP plans. We created a digitized lists of LDPs, and created a shapefiles covering the footprints of each LDP. Together, these cover a large share of central Addis Ababa. From that, we identified the subset of LDPs that were given official planned years of development. In many cases, direct discussions with the Land Development and Urban Renewal Agency helped to inform when different LDPs planned for execution, and sometimes visits to local areas and local government offices to confirm that these plans were finalised. This gave us our list of proposed redevelopment areas. From that we then identified those that were actually executed during the period 2017 to 2019, and confirmed this by direct observation using satellite data.

In addition, we 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 human settlements.



Figure 1: Endline Sampling - Oversampling Around Eviction Sites

2.2 Baseline and Endline Surveys

Our baseline data comes from a large spatial survey of Addis Ababa undertaken in early 2016. The aim of the survey was to select a representative sample of poor households, for an evaluation of the UPSNP (Franklin et al., 2023). A survey of 30,000 households was drawn from a random sample of all households in the city, using random walk sampling starting from randomly selected points across the city. GPS coordinates were recorded. From this sample of 30,000 households, approximately 6,000 were selected to undertake a much more detailed version of the baseline survey. These households were selected on the basis of a proxy means test (PMT).

For the endline survey, we selectively sampled 5,000 households from the original 30,000 households. First, we combined our baseline survey with our maps of the planned evicted sites, and generated distances between each household and each site. We then sampled all households inside of 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

1 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 ark blue. We collected endline data in the middle of 2021.

2.3 The Sample

Our resulting sample, restricted to 0.18 km on either side of the eviction boundary, has 2,069 households. Roughly 47 percent of households in our sample have been evicted, defined as living within a boundary of a completed eviction site at baseline. 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.

Housing is predominantly rented 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. Houses are primarily constructed with cement floors, corrugated iron sheet roofs, and mud or wooden walls. Households predominantly rely on shared pitted latrine toilets (with or without ventilation) or public toilets. To access water, most households use piped water through private or shared taps. For cooking fuel, households most commonly use charcoal or electricity, highlight an imperfect access to electricity in these areas.

Table 1: Summary Statistics of Sample at Baseline

	Mean	SD
People living in household	4.39	1.91
Average age at baseline- adults	28.37	7.16
Employed head of household	0.56	0.50
Education of most educated HH member	10.77	3.64
Owned home	0.14	0.34
Rented from Kebele	0.59	0.49
Rented privately	0.17	0.38
Mud/dung floor	0.37	0.48
Cement screed floor	0.55	0.50
Corrugated iron sheet roof	0.95	0.21
Mud/wood walls	0.88	0.32
Flush toilet	0.01	0.08
Shared pit latrine or public toilet	0.90	0.30
Piped water	0.82	0.38
Shared water source	0.47	0.50
Cook with charcoal	0.43	0.50
Cook with electricity	0.46	0.50
Observations	2069	

3 Identification

3.1 The Causal Effect of Eviction on Differences Between the Evicted and Non-evicted

We use a spatial regression discontinuity design to compare households just inside the boundaries 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:

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

Where $y_{is,t}$ is the outcome for household i, closest to eviction site s, and t is the period of our endline data, and where $D_{is,t-1}$ is the distance of that household to that nearest site, in the baseline period. We include household baseline controls X, including other measures of distance such distance from the city centre and distance to main roads, and site fixed effects.

We use local a functional form that is locally linear in distance, and then test the robustness of

our results to different bandwidths. Our preferred bandwidth is 180 metres: 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, and no more. But we test robustness to a set of other bandwidths, including the use of the optimal bandwidth selection procedure of Calonico et al. (2019).

β_1 identifies the causal effect of eviction on differences between evicted and non-evicted households, under the assumption that any discontinuity that we identify at the boundary would not have existed in the absence of the program. Therefore, the key identifying assumption is the continuity of the outcomes of interest in the distance across the boundaries in the absence of the evictions. We can test for this in two ways. First, we are in the somewhat rare position of being able to test for jumps at the boundaries using our baseline data. Those tests are presented in Table E.1. We also present tests of the continuity at baseline at for our planned but not completed sites (placebo sites) in Table E.2. Although our estimates or the discontinuity at the boundary is significantly different from zero at the 10% or 5% level for a few outcomes, this is hardly suprising given the large number of tests conducted. In all main regressions, we control for baseline variables that show imbalance at baseline.

Second, we can test for continuity of the outcomes of interest across the boundaries of placebo sites at *endline*. Even if placebo sites were non-randomly selected, if we only assume that the way that they were *planned* in the same way, an absence of discontinuities at placebo site boundaries is reassuring that discontinuities across completed boundaries are likely to be the result of the evictions themselves.[4] We present these tests in panels below our main results.

A second identifying assumption is the non-manipulation of the running variable around the boundaries. In practical terms, this means that households did not change their place of residence from one side of the boundary to another or just didn't move away from the area entirely at differential rates across the boundary before our baseline because they knew that evictions were going to happen. We think that this is unlikely to be the case as we find no evidence in the baseline

[4]Note that we do not use placebo sites as a control group, which would require the arguably strong assumption that completed eviction sites were selected from potential eviction sites in a way that is orthogonal to the outcomes of households in those sites.

data that households anticipated the evictions. Furthermore, we can test for independence of the running variable at the boundaries by implementing a manipulation testing procedure following Cattaneo et al. (2018) using local polynomial estimators of the density functions of each distance variable. The unrestricted manipulation test rejects the null of continuity of the density function at the cutoff (Appendix D).

In order to interpret our estimates as the causal effect only of being evicted relative to the counterfactual of there being no evictions at all, our identification strategy requires a SUTVA assumption: that households that fall just outside of evicted boundaries are not affected by having their neighbours evicted. The magnitude of such potential spillovers is also a direct object of interest. We require a completely different empirical strategy to estimate spillovers, which we now present.

3.2 Spillover effects

We estimate the spillover effects of proximity to evicted sites with the following specification:

$$Y = \alpha + \delta_s E_i^r + \delta_p E_i^p + \delta X_i + \eta_{is} + \epsilon_i \tag{2}$$

Where E_i^r is our measure of *realised* exposure to spillovers (the share of the area or population within radius that was cleared during the evictions), while E_i^p is the potential exposure: the exposure that household i would have experienced had our placebo sites being cleared in addition to the sites that were actually cleared. X_i is a set of household controls, including geographic controls for distance to the city centre, latitude and longitude. η_{is} are site fixed effects where s is the site closest to household i.

Our preferred measure of spillovers has the intuition of a distance-weighted share of neighbouring areas or population that were evicted:

$$E_i^r = \frac{\sum_k T_k A_k / d_{ik}}{\sum_k A_k / d_{ik}} \tag{3}$$

where k denotes a unit of spatial division of the city (in our case we use enumeration areas, which

is the smallest level of geographic measure for which we have population data. A_k is the area or population of enumeration area k and d_{ik} is the distance between household i and enumeration area k. T_k is a dummy equal to one if the enumeration area was demolished as part of the evictions. Writing C_k as a dummy indicating that the area was in an area that was planned for eviction but where the eviction didn't happen, we define potential exposure as:

$$E_i^p = \frac{\sum_k (T_k + C_k) A_k / d_{ik}}{\sum_k A_k / d_{ik}} \tag{4}$$

In the appendix, we discuss alternative approaches that we use as robustness checks. For example we could define exposure as the share of nearby areas that were evicted within a fixed radius R, and to iterate of different values of R, which is another common approach in the literature.

For ease of interpretation and use in the SUTVA correction procedure, we normalize E^r such that it is approximately equal to 1 at the boundary of treated eviction sites, on average.

3.3 Causal effect of eviction on the evicted

Our regression discontinuity estimates do not estimate the causal effect of evictions on the evicted households in the presence of spillover effects of evictions on households living outside of evicted areas. Because these households constitute our control group for evicted households, spillovers imply a violation of the Stable Unit Treatment Values Assumption (SUTVA).

With our estimates of the spillovers from Section 3.2 in hand, we can adjust our RD estimates for this SUTVA violation to recover the counterfactual for nearby (un-evicted) households in the absence of the evictions and therefore the true effect of being evicted on the evicted.

Formally, we calculate $\hat{\beta}_e = \hat{\beta}_1 + \hat{\delta}_s \bar{E}^r$, where β_1 is estimated from Equation 1 and $\bar{E}^r$ is the average exposure to spillover at the boundary. In practice, we normalise E^r such that $\bar{E}^r = 1$ before estimating equation 2. 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 Figure B3). We present our estimates of β_e below our main RD and spillover estimates for all outcomes, with standard errors calculated by assuming independence between β_1 and δ_s.

4 Results

4.1 Direct Effects of Clearance

Table (2) reports RD estimate of the direct impact of the clearance program. These results largely reflect the choices that were made as part of the government program, rather than any endogenous changes in outcomes. Results are consistent with the ground that is conceded by both critics and proponents of slum clearance.

Clearance leads to increased spatial isolation. Column 1 shows that those affected move about 6KM from their initial location, and where they end up is about 5km or 18 minutes further from the center of the city. These impacts are strongly statistically significant.

This physical isolation leads to a potential increase in social isolation. Cleared households live about 6km further way from their initial close neighbours than do those who were not cleared, and they are moved to locations that have a lower population density as measured in the 2007 Ethiopian census. Again, these results are strongly statistically significant.

Finally, there is evidence that the government program only takes partial advantage of the opportunity to improve housing and environmental amenity. Column 8 shows that our index of housing quality (which measures outcomes such as improved floors, roofs, toilets, piped water and access to electricity) improves by 0.5 standard deviations a result that is strongly statistically significant. On the other hand, Column 7 shows a statistically significant 0.5 standard deviation reduction in an index measuring access to public goods such as schools. The government program could have been better designed to avoid this negative.

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	Housing Quality
Panel A: RDD-Estimated Treatment Effects								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD_Estimate	6.364	5.274	18.106	6.232	4.967	-9942.913	-0.518	0.552
	(0.498)***	(0.458)***	(2.541)***	(0.302)***	(0.291)***	(1801.068)***	(0.093)***	(0.096)***
Control Mean	0.65	3.86	32.58	1.38	1.63	33799.68	-0.02	-0.07
Observations	1792	1792	1787	1789	1805	1792	1792	1806
Panel B: Placebo Treatment Effects								
RD_Estimate	0.440	0.305	-0.267	0.728	0.726	-3491.338	0.130	-0.037
	(0.355)	(0.325)	(2.283)	(0.338)**	(0.325)**	(2548.276)	(0.124)	(0.113)
Control Mean	0.80	2.67	25.44	1.51	1.48	31959.04	0.06	-0.18
Observations	843	843	842	818	835	843	843	843

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 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 distane. 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4.2 Endogenous Social Networks

The results above suggest that social networks are at least disrupted by clearance, people live further away from their former neighbours. Where critics and proponents disagree, however, is on the medium to long run implications of these initial effects with critics predicting ongoing impacts and proponents suggesting repair. Table (4.2) shows impacts on these endogenous medium to long run social outcomes, and suggests that, consistent with critics areguments, repair is not automatic.

Column 1 reports a statistically significant reduction of 0.6 standard deviations in our index of social networks. This includes a 0.3 standard deviation increase in loneliness, and a 0.7 reduction in network satisfaction. On average, the program has ongoing negative impacts on social networks, and increases social isolation.

Impacts on social networks are, however, controlled to some extent by the policy implementation. A government could try to collect neighbours together and move them to geographically close neighbourhoods. Luckily 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. Which condo home they were offered, and hence its geographic location, was effectively random. This creates exogenous variation in the distance between cleared households and their ex-ante neighbours.

Panel A in Table (4.2) reports the results of a regression that restricts to the sample of households that were in Kebele housing in the baseline, and that were evicted. With this sample, we regress social network outcomes at endline on the share of co-evicted neighbours that end up within one KM. The result in column 1 shows that, moving from complete isolation (no ex-ante neighbours within 1km) to no isolation (all neighbours within 1KM) increases our index of social network quality by about 1.2 standard deviations. We see similar large positive effects on other measures, although we lack precision for some outcomes.

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	-0.648	-2.528	-3.221	-0.726	-0.336	-0.248
	(0.098)***	(1.997)	(1.787)*	(0.095)***	(0.094)***	(0.053)***
Control Mean	0.05	14.95	9.92	0.08	-0.01	0.02
Observations	1806	1805	1806	1792	1792	1792
Panel B: Placebo Treatment Effects						
RD_Estimate	-0.053	-0.234	-0.580	-0.008	-0.109	-0.025
	(0.118)	(2.858)	(1.542)	(0.127)	(0.126)	(0.078)
Control Mean	0.15	17.98	9.87	0.10	0.10	0.10
Observations	843	843	843	843	843	843
Panel C: Spillover Effects						
Exposure	-0.010	0.045	0.098	-0.003	-0.032	-0.007
	(0.009)	(0.195)	(0.150)	(0.009)	(0.009)***	(0.005)
Control Mean	0.11	14.91	9.91	0.11	0.08	0.06
Observations	3489	3489	3489	3489	3489	3489
Panel D: Spillover-Corrected Treatment Effects						
RD_Estimate	-0.657	-2.483	-3.122	-0.730	-0.368	-0.255
	(0.098)***	(2.007)	(1.793)*	(0.096)***	(0.094)***	(0.054)***
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. 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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 of Co-evicted Neighbours 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 4.2. 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.

4.3 Endogenous Economic Networks

We have established that the program as implemented lead to some ongoing social isolation, but that the negative impacts could have been ameliorated with careful policy design. We now loook 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 (4.3) reports the results. Column 1 shows a small positive, but statistically insignificant, improvement in an index of the quality of economic networks. Looking at the consistuent parts of this index the message is a little mixed. Most outcomes see no statistically significant change, but we do see a statistically significant reduction in financial networks size with evicted household's reporting about 0.5 fewer people in their financial networks from a base of about 1.8 people. This reduction in the number of peole i the network does not seem to affect the ability of evictees to access network finance, with evicted households reporting that they are about 4 percentage points more likely to have a network loan.

Overall, these results provide little support to critics claims of ongoing isolation from economic networks, and suggest that, inline with proponents view economic networks are relatively robust to eviction.

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	0.065	-0.395	-0.543	0.047	-0.003	1572.114	821.886
	(0.098)	(0.513)	(0.293)*	(0.023)**	(0.004)	(1571.476)	(746.605)
Control Mean	-0.03	3.68	1.80	0.06	0.00	571.51	3512.03
Observations	1806	1702	1702	1806	1806	1806	1806
Panel B: Placebo Treatment Effects							
RD_Estimate	0.182	0.396	1.232	-0.009	0.000	1325.124	414.102
	(0.122)	(0.852)	(0.569)**	(0.033)	(0.004)	(1336.140)	(974.034)
Control Mean	-0.02	3.49	1.63	0.07	0.00	723.35	4152.47
Observations	843	798	798	843	843	843	843
Panel C: Spillover Effects							
Exposure	-0.006	-0.085	-0.001	-0.002	-0.000	30.078	27.559
	(0.009)	(0.054)	(0.037)	(0.002)	(0.001)	(81.009)	(72.432)
Control Mean	-0.03	3.07	1.58	0.07	0.00	668.42	4225.53
Observations	3489	3319	3319	3489	3489	3489	3489
Panel D: Spillover-Corrected Treatment Effects							
RD_Estimate	0.059	-0.480	-0.543	0.045	-0.003	1602.192	849.445
	(0.098)	(0.516)	(0.295)*	(0.023)*	(0.004)	(1573.563)	(750.110)
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. 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4.4 Economic Outcomes: Employment, Earnings and Expenditures

Ultimately, the reason we care about spatial isolation and isolation from economic networks is concerns that economic outcomes, such as employment, earnings and hence expenditures may be negatively impacted. We have shown above that there does not seem to be a strong and ongoing impact on economic networks, but evicted households are definitely less central than they were, which may have negative impacts on labor market access if labor markets are spatially aglmoerated in the center of the city and commuting options are limited.

Table (4.4) shows medium term impacts of eviction on economic outcomes. Column 1 reports that evictions leads to a 6 percentage point increase in the share of working age household members that report being in work. Column 2 reports a 200 Birr increase in earnings per household member, from a base of around 950 Birr, implying about a 20% 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. Overall, we see these outcomes as supporting the claims of clearance proponents: economic networks are robust, and commute networks are good enough to ensure economic opportunity is available in more spatially isolated locations.

Column 3 shows that these earnings increases are translated almost entirely into increases in expenditure, which rises about 200 Birr per household member. Breaking this down, columns 4 and 5 together show that this 200 Birr is spent on more transport (consistent with commuting to work and for shopping and leisure) and an increase in rent, consistent with the higher quality homes people are living in. Importantly, we see no evidence that households see a reduction in expenditures in other categories, suggesting that after they pay for more commuting and rent they have just as much left over for other expenditures as they would have prior to clearance. Again, we suggest these results are more inline with proponents views.

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

	Labor Market		**Expenditure Per Capita**				
	Share Working	Earnings Per Capita	Total	Transport	Rent	Food	Food Price Index
Panel A: RDD-Estimated Treatment Effects							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RD_Estimate	0.064	229.967	197.289	27.991	198.588	27.594	0.090
	(0.029)**	(94.717)**	(79.934)**	(7.143)***	(30.605)***	(32.555)	(0.094)
Control Mean	0.55	960.27	1267.60	56.30	55.73	756.00	-0.05
Observations	1762	1806	1806	1792	1792	1792	1775
Panel B: Placebo Treatment Effects							
RD_Estimate	-0.001	-124.886	-12.095	-16.837	-22.256	-47.931	-0.141
	(0.043)	(121.728)	(117.030)	(9.195)*	(37.000)	(45.990)	(0.109)
Control Mean	0.56	953.27	1421.08	56.60	109.44	792.01	-0.01
Observations	829	843	843	843	843	843	841
Panel C: Spillover Effects							
Exposure	-0.006	-12.245	-6.646	0.421	-1.473	-3.183	-0.000
	(0.003)*	(9.407)	(7.913)	(0.638)	(2.075)	(3.362)	(0.009)
Control Mean	0.57	1007.78	1369.93	55.07	77.62	780.56	-0.02
Observations	3412	3489	3489	3489	3489	3489	3477
Panel D: Spillover-Corrected Treatment Effects							
RD_Estimate	0.059	217.721	190.643	28.412	197.115	24.411	0.090
	(0.029)**	(95.183)**	(80.325)**	(7.171)***	(30.676)***	(32.728)	(0.095)
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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

4.5 Endogenous Amenity

The negative social network impacts that we report above are direct concern, but they may also impact location amenity. 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 and critics assert that cohesion will decay, while proponents suggest that community cohesion will recover.

Table (4.5) show medium term impacts on our measures of location amenity and social cohesion. Column 1 shows a 0.6 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 households reporting the smell of sewerage, and a reduction in reports of litter.

Once again we conclude that there is little support or the arguments of critics. While social networks are harmed, this does not lead to community decay in our sample.

5 Compensation

Compensation provided for evicted households was determined by their existing legal status. Households in Kebele housing (68% of our sample) were offered the option to buy a "condominium": one of the newly built government houses built on the outskirts of the city and sold to eligible households with a subsidised mortgage with access to loans. Households who could not make the required down payment for a condominium house, or simply did not want to buy one, were offered alternative Kebele housing nearby. This alternative Kebele housing was located all over the city. Second, households that owned their own houses in evicted areas (12% of our sample) 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. Third, households that were privately renting in evicted areas or squatting illegally on public land (19% of our sample) were not offered any compensation at all.

The group offered no compensation at all are particularly interesting to us, as they were renting

Table 7: Effects on Endogeneous Amenities

	Environ-mental Amenities	Smell of sewerage (-)	Litter	Safety	Bur-glary/theft	Violent Crime
Panel A: RDD-Estimated Treatment Effects						
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.633	0.669	0.169	-0.038	0.045	-0.201
	(0.090)***	(0.092)***	(0.094)*	(0.092)	(0.093)	(0.085)**
Control Mean	-0.20	-0.10	-0.08	-0.03	-0.01	-0.01
Observations	1792	1792	1792	1792	1792	1792
Panel B: Placebo Treatment Effects						
RD_Estimate	-0.083	-0.084	-0.003	-0.175	-0.093	-0.190
	(0.130)	(0.125)	(0.139)	(0.130)	(0.127)	(0.124)
Control Mean	0.00	-0.07	-0.05	-0.00	-0.11	0.09
Observations	843	843	843	843	843	843
Panel C: Spillover Effects						
Exposure	-0.031	-0.016	-0.004	-0.028	-0.026	-0.011
	(0.009)***	(0.009)*	(0.009)	(0.009)***	(0.009)***	(0.009)
Control Mean	-0.05	-0.06	-0.04	0.02	-0.03	0.02
Observations	3489	3489	3489	3489	3489	3489
Panel D: Spillover-Corrected Treatment Effects						
RD_Estimate	0.601	0.653	0.165	-0.065	0.019	-0.211
	(0.090)***	(0.093)***	(0.095)*	(0.093)	(0.093)	(0.086)**
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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

on the private market to begin with and had to find alternative rental arrangements after being evicted. In an efficient equilibrium, we would expect them to relocate somewhere nearby with similar characteristics to the place they lived before. They might pay some disruption cost of having to move and minimal loss of networks (sine they would, presumably, relocate nearby to their old homes).

Table 8: First Stage Results by Original Tenure.

	Moved at all	Distance to Centre (km)	Distance Moved	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	0.792	5.248	6.310	0.230	-0.360	0.160	-0.030
	(0.028)***	(0.454)***	(0.494)***	(0.028)***	(0.036)***	(0.035)***	(0.025)
Control Mean	0.16	3.86	0.65	0.02	0.64	0.18	0.16
Observations	1805	1805	1805	1805	1805	1805	1805
Panel B: Kebele Housing at Baseline							
RD_Estimate	0.830	5.140	6.310	0.286	-0.494	0.173	0.035
	(0.030)***	(0.509)***	(0.562)***	(0.037)***	(0.046)***	(0.042)***	(0.026)
Control Mean	0.11	3.57	0.57	0.02	0.85	0.10	0.03
Observations	1258	1258	1258	1258	1258	1258	1258
Panel C: Housing Privately Rented at Baseline							
RD_Estimate	0.517	3.391	4.014	0.120	-0.024	-0.038	-0.058
	(0.089)***	(1.288)***	(1.412)***	(0.057)**	(0.066)	(0.099)	(0.059)
Control Mean	0.54	4.57	1.66	0.04	0.16	0.75	0.05
Observations	302	302	302	302	302	302	302
Panel D: Housing Privately Owned at Baseline							
RD_Estimate	0.899	5.605	6.129	0.044	0.040	0.151	-0.235
	(0.069)***	(1.180)***	(1.221)***	(0.045)	(0.070)	(0.092)*	(0.106)**
Control Mean	0.05	4.26	0.21	0.00	0.05	0.08	0.88
Observations	245	245	245	245	245	245	245
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	0.000	0.312	0.172	0.000	0.000	0.061	0.000

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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

By estimating the RDD separately for each of these three different types of households, we can compare how different compensation schemes shaped the outcome variables we discussed in the main effects section. We report the heterogeneous effects for a set of variables that had significant pooled treatment effects. Panel A reports the pooled treatment effect with Panels B-D implementing the same spatial RDD but for a specific baseline housing type.

Table 5 shows the effect of evictions on location and housing type, by baseline tenure status. Panel A shows how eviction moved households further from the city centre. It also moved households out of Kebele housing and into privately rented households. These changes in housing type are explained by differences in compensation. Households renting kebeles are evicted from them and find alternative housing in three different ways: mainly, they either purchase and occupy condominiums built by the government, or they accept Kebele housing elsewhere. The remainder, presumably not happy with either offer, chose to rent privately. This leads to a large increase in their distance from the city centre. Suggestively, this is driven both by being moved to condominiums very far from the centre and by being moved to Kebele housing that is further from the centre than they were in before.

The group who were privately renting at baseline are highly mobile, even in the absence of evictions: 54% moved home in the six years between baseline and endline. When evicted they generally move into an alternative private rental unit (although it does seem that a small number do manage to gain access to condominium housing). When they rent somewhere else it is almost twice as far from the city centre as the control group, but they move less far than the other two groups who are constrained to move very far by the type of compensation that they receive.

Finally, the group who owned their homes at baseline do not get condominiums. They are offered cash to build new homes on land that they own on far from the city, which many of them take up. A minority do not take up that offer and instead choose to rent on the private market.

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	224.883	-24.820	196.627	0.275	0.368	-0.308	0.081
	(95.345)**	(65.218)	(29.859)***	(0.045)***	(0.051)***	(0.049)***	(0.090)
Control Mean	961.25	1146.37	55.73	-0.07	-0.11	2.45	-0.07
Observations	1805	1805	1805	1805	1805	1805	1805
Panel B: Kebele Housing at Baseline							
RD_Estimate	190.621	-71.482	180.457	0.343	0.347	-0.317	0.229
	(107.353)*	(77.673)	(32.726)***	(0.052)***	(0.060)***	(0.060)***	(0.105)**
Control Mean	917.80	1128.49	12.97	-0.09	-0.13	2.46	-0.12
Observations	1258	1258	1258	1258	1258	1258	1258
Panel C: Housing Privately Rented at Baseline							
RD_Estimate	388.492	46.311	251.220	0.038	0.234	-0.080	-0.129
	(249.700)	(163.796)	(90.079)***	(0.106)	(0.123)*	(0.119)	(0.195)
Control Mean	1104.50	1159.62	311.40	-0.11	-0.04	2.37	-0.09
Observations	302	302	302	302	302	302	302
Panel D: Housing Privately Owned at Baseline							
RD_Estimate	121.904	133.056	186.389	0.200	0.446	-0.570	-0.358
	(257.602)	(170.567)	(81.291)**	(0.120)*	(0.138)***	(0.144)***	(0.220)
Control Mean	1102.76	1203.67	25.63	0.07	-0.11	2.47	0.26
Observations	245	245	245	245	245	245	245
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	0.693	0.326	0.631	0.005	0.510	0.028	0.005

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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Unlike the other households who received place-based compensation (ie: condominiums or plots of land), which ensured high rates of relocation to the city's periphery, households who rented at baseline did not relocate as far from the city centre. Households across all three types experienced an increase in total household earnings to cover their increased rent expenditure. Those who owned their housing prior to eviction see the largest rent increase, which reflects the fact that a substantial share chose to rent housing despite receiving land along the periphery. The positive effect on amenities from forced relocation is driven by households who rented privately or lived in Kebele housing at baseline. However, when looking specifically at the housing quality sub-index, we see a different relationship. It's the households that owned or lived in Kebele housing at baseline that see substantial improvements in housing quality while those renting privately did not.

Ultimately, the households living in Kebele housing at baseline who fare the best. Most are given the option to live in condominiums but many choose to rent privately. The combination of an upgraded-version of public housing and a somewhat efficient rental market allow these households to ensure against the negative economic impacts of eviction while improving their amenities, including housing quality, and self-reporting a higher level of life-satisfaction. Comparatively, those who owned or rented privately see more mixed results in terms of amenities and life-satisfaction. These households were either compensated with land or nothing respectively. These results suggest that, while households are largely resilient economically to forced relocation, the type of compensation matters for overall welfare.

5.1 Cost–benefit accounting of the compensation scheme

The programme compensates each evicted household with the offer of a newly built 40m^2 flat. These units cost the city approximately \$22,000 to construct but are sold to beneficiaries for just \$10,500.[5] The implicit subsidy is thus \$11,500.

What do evicted households lose? They previously lived rent-free in the city centre—not with formal property rights, but with an entitlement to a permanent stream of access. In 2017, our survey records the average rent for similar-quality, centrally located slum housing at 1,276 birr per

[5]See Franklin (2025) for detailed calculations.

month, or about $720 annually. Capitalising this stream with a price-to-earnings ratio of 15 yields a valuation of around $11,000—almost exactly matching the subsidy on the formal housing. The compensation is therefore *wealth-neutral*: the present value of what households lose is roughly equal to what they receive.

The logic is similar for households that move to alternative *kebele* housing (including those that reject the condominium): they do not gain property rights, but continue to live rent-free. These units are farther from the centre but of higher quality, with comparable implied rents.

From the city's perspective, the arithmetic looks different. Auction prices for consolidated, centrally located plots averaged 22,000–25,000 birr per m^2 in 2013—or about $1,100 per m^2.[6] With a median slum-house footprint of $30m^2$, the land under each household is worth roughly $33,000. The $11,500 compensation therefore returns only about one-third of the land's market value to the evicted household; the remaining two-thirds accrues to the government or the eventual developer.

Urban renewal of this kind creates substantial value by converting centrally located land to higher-value use, consistent with Henderson et al. (2021). Because the land value gain exceeds the value of what households forfeit, the city can, in principle, finance compensation that preserves the net wealth of the displaced while still capturing significant surplus.

[6] Own calculations from city land-auction data.

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A Global Evictions Around the World

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	15,000	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

B Additional Figures and Tables



Figure B1: Teklhaymanout Site - before clearing in 2016 (left) and after in 2024 (right)



Figure B2: Gibe Site - before clearing in 2016 (left) and after in 2024 (right)

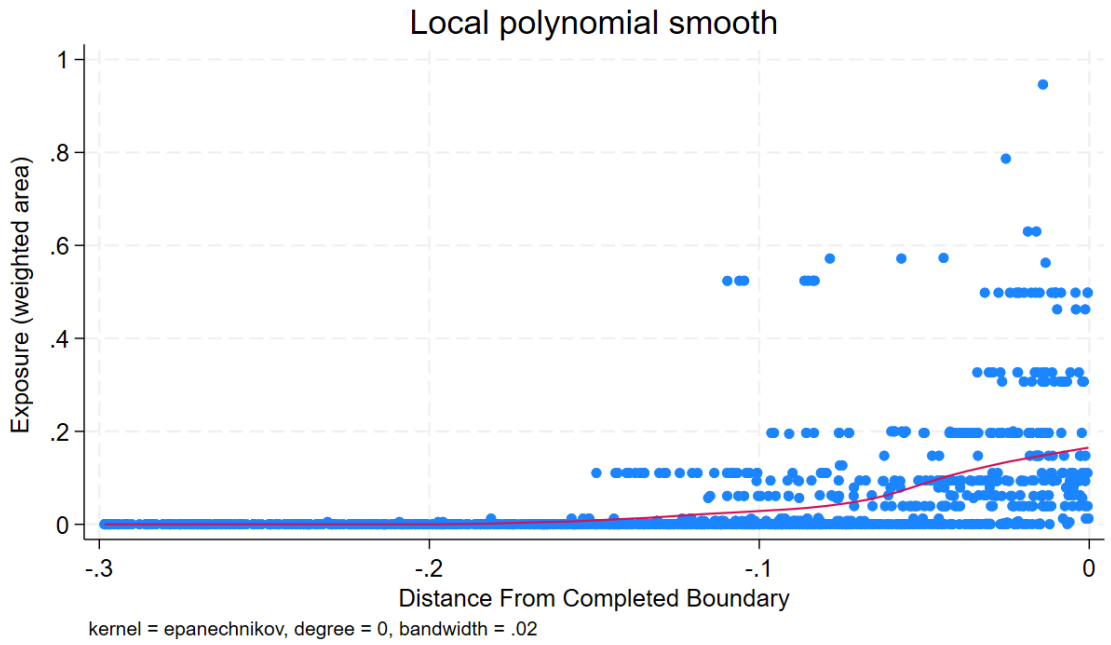


Figure B3: Exposure to spillovers as function of distance to treated areas among untreated households

Table B2: Main Sample by Treatment Status and Project Site

	Outside Completed Project		Inside Completed Project		Total	
	Count	Percent	Count	Percent	Count	Percent
Addis Ababa University	101	5.60	279	15.46	380	21.05
Akaki	118	6.54	19	1.05	137	7.59
American Gibe	175	9.70	155	8.59	330	18.28
Deafrique	23	1.27	6	0.33	29	1.61
Lideta Hull	116	6.43	3	0.17	119	6.59
Megengna	25	1.39	8	0.44	33	1.83
Nazret	24	1.33	30	1.66	54	2.99
Peacock Road	74	4.10	44	2.44	118	6.54
Sengetera	5	0.28	42	2.33	47	2.60
Shola Megengna	23	1.27	14	0.78	37	2.05
Sidist	77	4.27	19	1.05	96	5.32
Teklhaymanot	215	11.91	210	11.63	425	23.55
Total	976	54.07	829	45.93	1805	100.00
N	1805					

Table B3: Restricted Placebo Sample by Treatment Status and Project Site

	Outside Completed Project		Inside Completed Project		Total	
	Count	Percent	Count	Percent	Count	Percent
Aratkilo Aau	8	0.95	10	1.18	18	2.13
Aware	17	2.01	5	0.59	22	2.60
Bulgaria	84	9.94	2	0.24	86	10.18
Datsun	44	5.21	22	2.60	66	7.81
Felege Zordanos	35	4.14	50	5.92	85	10.06
Genet	9	1.07	14	1.66	23	2.72
Kazanchis	56	6.63	193	22.84	249	29.47
Kirkos	172	20.36	56	6.63	228	26.98
Sertegna Sefer	7	0.83	61	7.22	68	8.05
Total	432	51.12	413	48.88	845	100.00
N	845					

C Additional Regression Tables

Table C4: 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	-9942.913	-0.001	0.269	-3.012	-0.422	0.135	-0.031	0.013
		(0.012)	(0.075)***	(0.249)***	(0.168)**	(0.030)***	(0.023)	(0.019)
	(1801.068)***							
Control Mean	33799.68	0.40	1.80	26.94	2.72	0.44	0.12	0.17
Observations	1792	1791	1783	1792	1694	1783	1783	1792
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 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 C5: Effects on Financial Situation

	Total Savings	Cash Available	Has Savings Account	Has Loan	Has Informal Loan	Loan Amount
Panel A: RDD-Estimated Treatment Effects						
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	1950.455	33.587	0.002	0.050	0.047	15407.525
		(57.362)	(0.042)	(0.031)	(0.023)**	
	(933.427)**					(8622.367)*
Control Mean	4876.51	468.75	0.65	0.11	0.06	16980.51
Observations	1806	1806	1806	1806	1806	302
Panel B: Placebo Treatment Effects						
RD_Estimate	2300.829	62.363	0.117	0.006	-0.009	20027.169
	(1526.338)	(68.227)	(0.053)**	(0.046)	(0.033)	
						(11322.245)*
Control Mean	7790.60	334.60	0.77	0.15	0.07	20674.32
Observations	843	843	843	843	843	151
Panel C: Spillover Effects						
Exposure	-16.939	8.843	-0.005	-0.002	-0.002	-534.915
	(101.080)	(6.027)	(0.004)	(0.003)	(0.002)	(849.304)
Control Mean	6501.93	437.19	0.70	0.13	0.07	18294.71
Observations	3489	3489	3489	3489	3489	556
Panel D: Spillover-Corrected Treatment Effects						
RD_Estimate	1933.517	42.431	-0.004	0.048	0.045	14872.610
		(57.678)	(0.042)	(0.031)	(0.023)*	
	(938.884)**					(8664.094)*
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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C.1 Main Table Indices and Components

Table C6: Effects on Neighborhood Quality

	Environ-mental Amenities	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	0.633	0.491	0.669	0.169	0.299	0.351
	(0.090)***	(0.090)***	(0.092)***	(0.094)*	(0.089)***	(0.086)***
Control Mean	-0.20	-0.12	-0.10	-0.08	-0.14	-0.17
Observations	1792	1792	1792	1792	1792	1792
Panel B: Placebo Treatment Effects						
RD_Estimate	-0.083	-0.052	-0.084	-0.003	-0.044	-0.105
	(0.130)	(0.120)	(0.125)	(0.139)	(0.136)	(0.128)
Control Mean	0.00	0.04	-0.07	-0.05	0.06	0.11
Observations	843	843	843	843	843	843
Panel C: Spillover Effects						
Exposure	-0.031	-0.030	-0.016	-0.004	-0.028	-0.031
	(0.009)***	(0.009)***	(0.009)*	(0.009)	(0.009)***	(0.009)***
Control Mean	-0.05	-0.00	-0.06	-0.04	-0.00	-0.00
Observations	3489	3489	3489	3489	3489	3489
Panel D: Spillover-Corrected Treatment Effects						
RD_Estimate	0.601	0.461	0.653	0.165	0.271	0.320
	(0.090)***	(0.090)***	(0.093)***	(0.095)*	(0.090)***	(0.087)***
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 C7: Effects on Housing Quality

	Housing Quality	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	0.552	0.225	0.218	-0.001	2.638	0.115	0.022	0.254	0.130	0.078
	(0.096)***	(0.073)***	(0.063)***	(0.023)	(1.134)**	(0.043)***	(0.042)	(0.036)***	(0.044)***	(0.032)**
Control Mean	-0.07	1.87	0.82	0.71	-9.42	0.53	0.69	0.13	0.44	0.81
Observations	1806	1806	1792	1753	1216	1792	1806	1806	1806	1806
Panel B: Placebo Treatment Effects										
RD_Estimate	-0.037	-0.026	-0.112	-0.017	0.324	-0.093	-0.021	0.005	0.057	0.004
	(0.113)	(0.107)	(0.083)	(0.029)	(1.460)	(0.059)	(0.055)	(0.035)	(0.062)	(0.044)
Control Mean	-0.18	1.84	0.82	0.71	-10.48	0.45	0.71	0.08	0.46	0.83
Observations	843	843	843	841	720	843	843	843	843	843
Panel C: Spillover Effects										
Exposure	-0.010	-0.004	0.007	-0.003	-0.496	0.005	0.001	0.006	-0.003	-0.003
	(0.008)	(0.008)	(0.006)	(0.002)	(0.092)***	(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	0.83
Observations	3489	3489	3489	3471	2933	3489	3489	3489	3489	3489
Panel D: Spillover-Corrected Treatment Effects										
RD_Estimate	0.542	0.222	0.224	-0.004	2.142	0.120	0.023	0.260	0.126	0.075
	(0.096)***	(0.074)***	(0.063)***	(0.023)	(1.137)*	(0.043)***	(0.042)	(0.037)***	(0.044)***	(0.032)**
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.

Table C8: Effects on Safety Measures

	Safety	Bur-glary/theft	Violent Crime	Unsafe at Night
Panel A: RDD-Estimated Treatment Effects				
	(1)	(2)	(3)	(4)
RD_Estimate	-0.038	0.045	-0.201	0.039
	(0.092)	(0.093)	(0.085)**	(0.100)
Control Mean	-0.03	-0.01	-0.01	-0.05
Observations	1792	1792	1792	1792
Panel B: Placebo Treatment Effects				
RD_Estimate	-0.175	-0.093	-0.190	-0.138
	(0.130)	(0.127)	(0.124)	(0.133)
Control Mean	-0.00	-0.11	0.09	0.03
Observations	843	843	843	843
Panel C: Spillover Effects				
Exposure	-0.028	-0.026	-0.011	-0.026
	(0.009)***	(0.009)***	(0.009)	(0.009)***
Control Mean	0.02	-0.03	0.02	0.06
Observations	3489	3489	3489	3489
Panel D: Spillover-Corrected Treatment Effects				
RD_Estimate	-0.065	0.019	-0.211	0.013
	(0.093)	(0.093)	(0.086)**	(0.101)
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.

Table C9: Effects on Quality of Public Goods

	Public Goods	Street Lights	Public Space	Primary School	Hospital
Panel A: RDD-Estimated Treatment Effects					
	(1)	(2)	(3)	(4)	(5)
RD_Estimate	-0.518	-0.319	-0.393	-0.285	-0.254
	(0.093)***	(0.091)***	(0.092)***	(0.092)***	(0.095)***
Control Mean	-0.02	-0.03	-0.04	0.02	0.00
Observations	1792	1792	1792	1792	1792
Panel B: Placebo Treatment Effects					
RD_Estimate	0.130	0.231	-0.036	0.160	-0.075
	(0.124)	(0.121)*	(0.139)	(0.133)	(0.109)
Control Mean	0.06	-0.01	0.05	-0.03	0.13
Observations	843	843	843	843	843
Panel C: Spillover Effects					
Exposure	-0.017	-0.025	-0.006	-0.003	-0.004
	(0.009)*	(0.009)***	(0.009)	(0.009)	(0.009)
Control Mean	0.09	0.06	0.05	0.05	0.04
Observations	3489	3489	3489	3489	3489
Panel D: Spillover-Corrected Treatment Effects					
RD_Estimate	-0.535	-0.344	-0.399	-0.288	-0.258
	(0.094)***	(0.092)***	(0.093)***	(0.092)***	(0.095)***
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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D Validation of Regression Discontinuity Design

Regression Discontinuity Designs (RDD) rely on a continuity assumption of the running variable around the cutoff point to non-parametically estimate an Average Treatment Effect (ATE). However, this identify assumption is fundamentally untestable. Lee (2008) employs a slightly stronger identification assumption than traditional Regression Discontinuity Designs (RDD), which has the advantage of providing two testable implications. Lee assumes that the choice of location around the cutoff (the value of the running variable) is not entirely deterministic – baseline covariates are independent from the treatment status in the neighborhood of the cutoff.

The resulting testable implications from this local independence assumption are:

I. The density of the running variable is continuous around the cutoff.

II. The treatment is locally randomized around the cutoff and thus predetermined (baseline) covariates should be continuously distributed around said cutoff.

The successful test of these two implications in generally perceived as a sufficient condition for the empirical application of a regression discontinuity design.

D.1 Testing Implication I: Continuity of the Running Variable around the Cutoff

If Implication I holds, then the distribution of the running variable will be continous across the cutoff. In our context, this implies that households do not have perfect control over their location on either side of the eviction boundary.

We can test this using Manipulation Testing as proposed by McCrary (2008) to determine continuity of the running variable on either side of the cutoff. The null hypothesis is that the limit of the running variable's PDF $f(x)$ from below the cutoff is equal to the limit from above the cutoff.

$$H_0 : lim_{x \uparrow \hat{x}} f(x) = lim_{x \downarrow \hat{x}} f(x)$$

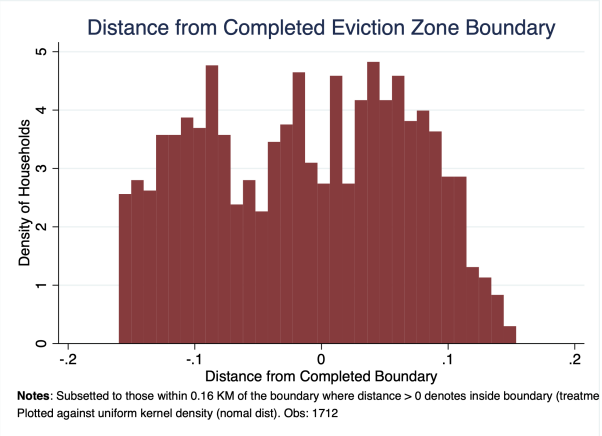
We select a window $[-0.16, 0.16]$ for graphical representation based on the following table,

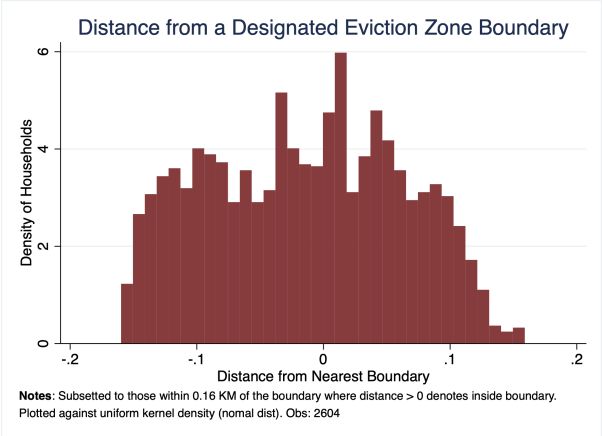
detailing the key statistics of the running variable: *distance from a completed project boundary*. (Although this window holds for the more generalized variable *distance from a designated project boundary*.)

Table D10: Distance from Completed Eviction Zone Boundary

	Mean	Median	SD	Min	Max
Distance From Completed Boundary	-0.01	-0.01	0.08	-0.16	0.15
Observations	1740				

First, we plot the distribution of the two distance variables. It is not clear whether there is continuity at the boundary due to the binning of observations.

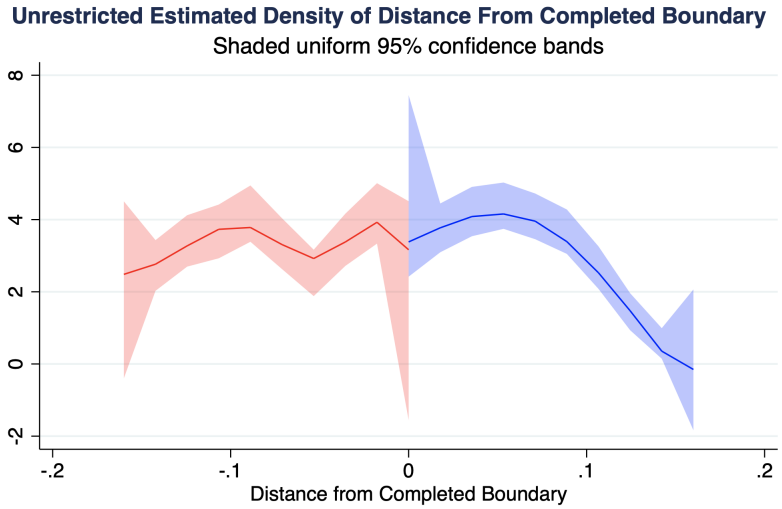




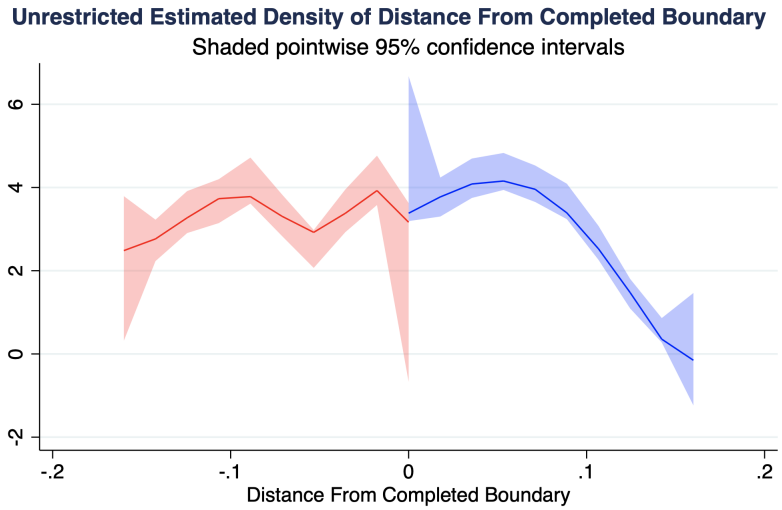
Next, we implement a manipulation testing procedure following Cattaneo et al. (2018) using local polynomial estimators of the density functions of each distance variable.

D.1.1 Unrestricted Manipulation Test

The unrestricted manipulation test rejects the null of continuity of the density function at the cutoff. We do observe a bunching to the left of the cutoff. In our setting, households are clustered at the edge on the inside edge of the boundary for completed evicted zones. If we believe this is a concious choice by households, then they must view some value to being inside the boundary and thus being evicted. It could also be explained that the population density is higher due to slum-like conditions and the boundaries often coinciding with main roads.



Notes: Default options of rddensity used except for choice of uniform kernel (no weighting) to construct the local polynomial estimators. Distance > 0 denotes inside boundary (treatment).



Notes: Default options of rddensity used except for choice of uniform kernel (no weighting) to construct the local polynomial estimators. Distance > 0 denotes inside boundary (treatment).

Table D11: Unrestricted Manipulation Test for Distance from Completed Boundary

	Outside Boundary (Left of Cutoff)	Inside Boundary (Right of Cutoff)
Sample Size	911.000	829.000
Sample Size in Bandwidth	190.000	261.000
Bandwidth Used	0.029	0.039
Bias-Corrected Density Estimate	1.425	5.069
Bias-Corrected T-Statistic	2.693	
Corresponding P-Value	0.007	

D.2 Testing Implication II: RD Balance Check

In the companion document *RD Descriptive Analysis for Eviction Project*, we provide regression discontinuity plots of various baseline covariates to provide descriptive evidence of continuity. In this section, we provide formalized results using RDD regressions and the Canany and Kamat test.

D.2.1 RDD Regressions of Baseline Covariates

As suggested by Cattaneo et al. (2019), we regress each of the baseline covariates against the running variable: distance from a completed project boundary. The results in the table below support the continuity of the distribution of the various baseline covariates around the cutoff.

Table D12: RDD Regression Results for Baseline Covariates

	MSE-Optimal Bandwidth	RD Estimator	Bias-Corrected RD Estimator	Robust SE	Eff. Nur Observ
Asset index	0.031	-0.040	-0.017	0.113	412.
Weekly food expenditure	0.044	11.763	30.639	72.121	568.
Monthly rent	0.037	126.650	107.024	116.999	375.
Monthly rent (excl. rent from Kebele)	0.043	315.012	397.790	343.457	67.(
HH registered at Woreda	0.029	-0.024	-0.024	0.049	372.
Head of household has a job	0.038	-0.017	-0.009	0.106	492.
Owned home	0.023	-0.113	-0.142	0.099	314.
Rented from Kebele	0.021	0.249	0.296	0.131**	279.
Rented from landlord	0.031	0.038	0.015	0.077	403.
Temporary shelter from local govt	0.042	-0.025	-0.020	0.042	555.
Combined govt provided housing	0.019	0.186	0.234	0.137*	265.

Using Mean-Squared Error as the bandwidth selection criteria ensures that the optimal bandwidth (and thereby effective number of observations) will vary based on the dependent variable. This variation does not seem to be a concern in the literature.

D.2.2 Canany and Kamat Permutation Test

Canay and Kamat (2017) proposes a novel method for testing the second implication of the local continuity assumption – the baseline covariates are approximately distributed the same on either

side of the cutoff.

The null hypothesis is that the distributions of the baseline covariates at the cutoff are continuous. We find minimal evidence to reject the null, supporting the validity of using a Regression Discontinuity Design in our setting.

Table D13: Canany and Kamat Test for Baseline Covariates

	P-Value of Canany-Kamat Test
Asset index	0.949
Weekly food expenditure	0.184
Monthly rent	0.654
Monthly rent (excl. rent from Kebele)	0.039**
HH registered at Woreda	1.000
Head of household has a job	0.798
Owned home	0.527
Rented from Kebele	0.319
Rented from landlord	1.000
Temporary shelter from local govt	0.230
Combined govt provided housing	0.779
Baseline Covariates Jointly	0.117

E Balance Tests

All balance tests include project fixed effects.

E.1 RDD - Treatment and Control

Table E14: RDD Balance Test for Main Baseline Variables

Baseline Variable	RD Estimate	Std. Error	Control Slope	Treat Slope	Effective Obs
People living in household	-0.107	0.263	-650.810	-650.916	1,819
Male Head of Household	-0.031	0.066	-47.979	-48.010	1,819
Number of kids under 5	0.112	0.081	-699.439	-699.327	1,819
Number of kids between 5 and 13	-0.001	0.087	-1,249.155	-1,249.156	1,819
Number of kids between 13 and 18	0.032	0.096	523.370	523.403	1,819
Average age at baseline- adults	-0.912	1.197	-9,533.425	-9,534.337	1,819
Employed head of household	0.085	0.066	-567.884	-567.800	1,819
Permanent Employment	0.034	0.049	-103.991	-103.957	1,819
Education of most educated HH member	-0.104	0.467	2,616.938	2,616.834	1,819
Privately Owned	0.031	0.047	-1,201.368	-1,201.337	1,819
Privately Rented	0.087*	0.049	-276.140	-276.053	1,819
Mud/dung floor	0.070	0.063	-1,010.097	-1,010.026	1,819
Cement screed floor	-0.097	0.065	2,192.916	2,192.819	1,819
Corrugated iron sheet roof	-0.022*	0.013	258.280	258.258	1,819
Mud/wood walls	0.050	0.034	413.118	413.167	1,819
Flush toilet	-0.002	0.011	90.009	90.008	1,819
Shared pit latrine or public toilet	0.025	0.036	320.121	320.146	1,819
Piped water	-0.074	0.047	-41.748	-41.822	1,819
Shared water source	0.055	0.063	-1,500.128	-1,500.073	1,819
Cook with firewood or charcoal	0.074	0.068	2,311.568	2,311.641	1,819
Weekly food expenditure	-26.389	46.692	-92,742.757	-92,769.146	1,819
Asset index	-0.135**	0.066	155.950	155.816	1,819
Ethnicity - Amahara	0.086	0.064	-522.902	-522.816	1,819
Ethnicity - Oromo	-0.004	0.056	1,110.438	1,110.434	1,819
Ethnicity - Tigrayan	-0.024	0.036	-1,326.144	-1,326.169	1,819

E.2 RDD - Placebo Treatment and Placebo Control

Table E15: RDD Placebo Balance Test for Key Baseline Variables

Baseline Variable	RD Estimate	Std. Error	Control Slope	Treat Slope	Effective Obs
People living in household	0.061	0.325	5.280	5.341	844
Male Head of Household	0.124	0.095	0.383	0.507	844
Number of kids under 5	0.026	0.128	0.459	0.486	844
Number of kids between 5 and 13	-0.259*	0.149	0.721	0.462	844
Number of kids between 13 and 18	0.076	0.134	0.545	0.621	844
Average age at baseline- adults	-0.170	1.651	28.455	28.285	844
Employed head of household	0.159*	0.094	0.379	0.538	844
Permanent Employment	0.058	0.077	0.116	0.174	844
Education of most educated HH member	0.374	0.642	11.960	12.333	844
Privately Owned	-0.025	0.043	0.071	0.046	844
Privately Rented	-0.023	0.066	0.128	0.105	844
Mud/dung floor	-0.013	0.096	0.199	0.186	844
Cement screed floor	-0.014	0.101	0.754	0.740	844
Corrugated iron sheet roof	-0.008	0.011	0.964	0.956	844
Mud/wood walls	-0.175**	0.075	0.710	0.535	844
Flush toilet	-0.036	0.025	0.056	0.020	844
Shared pit latrine or public toilet	0.001	0.055	0.877	0.878	844
Piped water	0.025	0.065	0.998	1.023	844
Shared water source	-0.058	0.096	0.522	0.464	844
Cook with firewood or charcoal	0.192**	0.097	0.344	0.536	844
Weekly food expenditure	28.898	45.728	437.626	466.524	844
Asset index	0.023	0.084	0.138	0.161	844
Ethnicity - Amahara	-0.113	0.104	0.473	0.360	844
Ethnicity - Oromo	0.065	0.080	0.091	0.156	844
Ethnicity - Tigrayan	-0.015	0.048	0.046	0.031	844

F Robustness of Bandwidth Selection

F.1 First Stage

Table F16: 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	6.121				
	[0.502]***				
Bias-corrected		5.026	5.270	4.266	3.198
		[0.498]***	[0.499]***	[0.567]***	[0.922]***
Robust		5.026	5.270	4.266	3.198
		[0.672]***	[0.677]***	[0.774]***	[1.093]***
Constant	-2.520				
	[1.372]*				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	0.564	0.653	0.564	0.628	1521.211
Bandwidth		0.18	0.15	0.10	0.04
Treatment Slope	.	-0.57	3.81	4.54	26.07
Control Slope	.	-10	-9	-0	92
Treatment Obs.		965	863	571	368
Control Obs.		827	825	691	482
Kernel		Uniform	Uniform	Uniform	Uniform
Bandwidth Method		Manual	Manual	Manual	mserd
Obs.	1690	1847	1847	1847	1847

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 F17: Effect of Bandwidth Specification on RDD Results Using Distance to Centre (km)

	OLS with 0.15km	Bandwidth - 0.18km	Bandwidth - 0.15km	Bandwidth - 0.10km	Optimal Bandwidth
	(1)	(2)	(3)	(4)	(5)
Treatment Status	5.097				
	[0.454]***				
Bias-corrected		4.220	4.382	3.447	2.198
		[0.458]***	[0.459]***	[0.524]***	[0.851]***
Robust		4.220	4.382	3.447	2.198
		[0.619]***	[0.623]***	[0.706]***	[1.010]**
Constant	2.127				
	[1.240]*				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	3.905	3.862	3.905	3.907	1521.211
Bandwidth		0.18	0.15	0.10	0.04
Treatment Slope	.	-1.29	1.52	1.99	27.75
Control Slope	.	-10	-10	-2	94
Treatment Obs.		965	863	571	365
Control Obs.		827	825	691	479
Kernel		Uniform	Uniform	Uniform	Uniform
Bandwidth Method		Manual	Manual	Manual	mserd
Obs.	1690	1847	1847	1847	1847

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 F18: Effect of Bandwidth Specification on RDD Results Using Time to centre

	OLS with 0.15km	Bandwidth - 0.18km	Bandwidth - 0.15km	Bandwidth - 0.10km	Optimal Bandwidth
	(1)	(2)	(3)	(4)	(5)
Treatment Status	17.721				
	[2.623]***				
Bias-corrected		11.387	10.257	7.287	7.282
		[2.541]***	[2.576]***	[3.002]**	[4.583]
Robust		11.387	10.257	7.287	7.282
		[3.836]***	[3.906]***	[4.632]	[5.351]
Constant	10.461				
	[7.171]				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	32.617	32.579	32.617	32.116	1521.211
Bandwidth		0.18	0.15	0.10	0.04
Treatment Slope	.	-24.98	-5.52	51.74	120.31
Control Slope	.	20	18	33	214
Treatment Obs.		962	860	570	434
Control Obs.		825	823	689	561
Kernel		Uniform	Uniform	Uniform	Uniform
Bandwidth Method		Manual	Manual	Manual	mserd
Obs.	1685	1842	1842	1842	1842

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 F19: 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	5.319				
	[0.385]***				
Bias-corrected		3.828	4.018	3.234	3.308
		[0.396]***	[0.396]***	[0.450]***	[0.566]***
Robust		3.828	4.018	3.234	3.308
		[0.530]***	[0.534]***	[0.606]***	[0.663]***
Constant	1.389				
	[1.051]				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	1.615	1.661	1.615	1.973	1521.211
Bandwidth		0.18	0.15	0.10	0.05
Treatment Slope	.	9.94	16.98	22.85	10.15
Control Slope	.	4	4	14	57
Treatment Obs.		965	863	571	509
Control Obs.		827	825	691	646
Kernel		Uniform	Uniform	Uniform	Uniform
Bandwidth Method		Manual	Manual	Manual	mserd
Obs.	1690	1847	1847	1847	1847

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 F20: Effect of Bandwidth Specification on RDD Results Using Pop. Density

	OLS with 0.15km	Bandwidth - 0.18km	Bandwidth - 0.15km	Bandwidth - 0.10km	Optimal Bandwidth
	(1)	(2)	(3)	(4)	(5)
Treatment Status	-7984.221				
	[2302.019]***				
Bias-corrected		-5591.596	-7536.024	-6668.466	-12605.793
		[1801.068]***	[1844.589]***	[2087.654]***	[3155.155]***
Robust		-5591.596	-7536.024	-6668.466	-12605.793
		[2456.285]**		[2980.028]**	
			[2548.201]***		[3522.898]***
Constant	42146.531				
	[6289.534]***				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	33578.914	33799.681	33578.914	30264.024	1521.211
Bandwidth		0.18	0.15	0.10	0.03
Treatment Slope	.	-66870.30	-104166.14	-96818.47	198582.07
Control Slope	.	-26128	-15102	8238	-78171
Treatment Obs.		965	863	571	361
Control Obs.		827	825	691	466
Kernel		Uniform	Uniform	Uniform	Uniform
Bandwidth Method		Manual	Manual	Manual	mserd
Obs.	1690	1847	1847	1847	1847

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 F21: Effect of Bandwidth Specification on RDD Results Using Num. Rooms

	OLS with 0.15km	Bandwidth - 0.18km	Bandwidth - 0.15km	Bandwidth - 0.10km	Optimal Bandwidth
	(1)	(2)	(3)	(4)	(5)
Treatment Status	0.241				
	[0.084]***				
Bias-corrected		0.195	0.144	0.274	0.443
		[0.075]***	[0.077]*	[0.088]***	[0.128]***
Robust		0.195	0.144	0.274	0.443
		[0.104]*	[0.107]	[0.127]**	[0.148]***
Constant	1.698				
	[0.230]***				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	1.819	1.800	1.819	1.798	1521.211
Bandwidth		0.18	0.15	0.10	0.04
Treatment Slope	.	1.40	1.08	3.25	-4.96
Control Slope	.	-1	-1	-0	-5
Treatment Obs.		963	861	571	430
Control Obs.		820	818	687	555
Kernel		Uniform	Uniform	Uniform	Uniform
Bandwidth Method		Manual	Manual	Manual	mserd
Obs.	1681	1838	1838	1838	1838

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 F22: Effect of Bandwidth Specification on RDD Results Using Predicted Poverty Rate

	OLS with 0.15km	Bandwidth - 0.18km	Bandwidth - 0.15km	Bandwidth - 0.10km	Optimal Bandwidth
	(1)	(2)	(3)	(4)	(5)
Treatment Status	0.007				
	[0.014]				
Bias-corrected		-0.010	-0.021	-0.009	-0.048
		[0.012]	[0.012]*	[0.014]	[0.022]**
Robust		-0.010	-0.021	-0.009	-0.048
		[0.017]	[0.017]	[0.020]	[0.026]*
Constant	0.301				
	[0.037]***				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	0.400	0.398	0.400	0.378	1521.211
Bandwidth		0.18	0.15	0.10	0.03
Treatment Slope	.	-0.17	-0.31	0.03	-0.35
Control Slope	.	-0	-0	0	1
Treatment Obs.		965	863	571	311
Control Obs.		826	824	690	373
Kernel		Uniform	Uniform	Uniform	Uniform
Bandwidth Method		Manual	Manual	Manual	mserd
Obs.	1689	1846	1846	1846	1846

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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

F.2 Summary Results

Table F23: 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	251.958				
	[89.587]***				
Bias-corrected		377.945	377.800	483.362	-262.935
		[79.661]***	[81.682]***	[97.198]***	[194.007]
Robust		377.945	377.800	483.362	-262.935
		[114.769]***	[118.893]***	[138.081]***	[212.963]
Constant	1550.809				
	[244.767]***				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	1282.133	1268.894	1282.133	1301.994	1521.211
Bandwidth		0.18	0.15	0.10	0.02
Treatment Slope	.	-627.50	-1168.85	-2216.47	22582.31
Control Slope	.	461	672	132	43410
Treatment Obs.		965	863	571	265
Control Obs.		827	825	691	295
Kernel		Uniform	Uniform	Uniform	Uniform
Bandwidth Method		Manual	Manual	Manual	mserd
Obs.	1690	1847	1847	1847	1847

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 F24: Effect of Bandwidth Specification on RDD Results Using Value of Food Per Capita

	OLS with 0.15km	Bandwidth - 0.18km	Bandwidth - 0.15km	Bandwidth - 0.10km	Optimal Bandwidth
	(1)	(2)	(3)	(4)	(5)
Treatment Status	36.542				
	[36.747]				
Bias-corrected		94.246	101.575	118.120	-234.251
		[32.555]***	[33.259]***	[39.030]***	[83.165]***
Robust		94.246	101.575	118.120	-234.251
		[46.786]**	[48.436]**	[57.474]**	[91.931]**
Constant	856.054				
	[100.399]***				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	758.236	756.005	758.236	770.532	1521.211
Bandwidth		0.18	0.15	0.10	0.02
Treatment Slope	.	-169.74	-207.01	-574.04	10804.20
Control Slope	.	-130	-103	-706	22648
Treatment Obs.		965	863	571	255
Control Obs.		827	825	691	281
Kernel		Uniform	Uniform	Uniform	Uniform
Bandwidth Method		Manual	Manual	Manual	mserd
Obs.	1690	1847	1847	1847	1847

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 F25: 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	188.733				
	[103.914]*				
Bias-corrected		319.441	375.416	272.286	477.862
		[94.388]***	[95.856]***	[110.397]**	[159.573]***
Robust		319.441	375.416	272.286	477.862
		[133.435]**	[137.494]***	[158.798]*	[183.221]***
Constant	816.748				
	[283.911]***				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	944.521	961.253	944.521	947.030	1521.211
Bandwidth		0.18	0.15	0.10	0.04
Treatment Slope	.	-1372.77	-895.14	-2592.44	-10991.16
Control Slope	.	42	-60	-2587	-2504
Treatment Obs.		965	863	571	386
Control Obs.		827	825	691	521
Kernel		Uniform	Uniform	Uniform	Uniform
Bandwidth Method		Manual	Manual	Manual	mserd
Obs.	1690	1847	1847	1847	1847

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 F26: 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	0.269				
	[0.047]***				
Bias-corrected		0.271	0.269	0.254	0.258
		[0.045]***	[0.046]***	[0.054]***	[0.101]**
Robust		0.271	0.269	0.254	0.258
		[0.067]***	[0.069]***	[0.082]***	[0.115]**
Constant	-0.715				
	[0.129]***				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	-0.066	-0.074	-0.066	-0.070	1756.125
Bandwidth		0.18	0.15	0.10	0.03
Treatment Slope	.	0.07	-0.07	-0.34	-0.37
Control Slope	.	-1	-1	0	-1
Treatment Obs.		965	863	571	317
Control Obs.		827	825	691	390
Kernel		Uniform	Uniform	Uniform	Uniform
Bandwidth Method		Manual	Manual	Manual	mserd
Obs.	1690	1847	1847	1847	1847

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 F27: 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	0.362				
	[0.056]***				
Bias-corrected		0.257	0.260	0.153	0.147
		[0.051]***	[0.053]***	[0.060]**	[0.117]
Robust		0.257	0.260	0.153	0.147
		[0.072]***	[0.075]***	[0.090]*	[0.131]
Constant	-0.224				
	[0.154]				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	-0.118	-0.114	-0.118	-0.170	1513.509
Bandwidth		0.18	0.15	0.10	0.02
Treatment Slope	.	-1.03	-1.21	0.21	-1.53
Control Slope	.	-2	-2	-2	7
Treatment Obs.		965	863	571	296
Control Obs.		827	825	691	350
Kernel		Uniform	Uniform	Uniform	Uniform
Bandwidth Method		Manual	Manual	Manual	mserd
Obs.	1690	1847	1847	1847	1847

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 F28: 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	-0.291				
	[0.061]***				
Bias-corrected		-0.233	-0.242	-0.285	0.149
		[0.057]***	[0.058]***	[0.067]***	[0.141]
Robust		-0.233	-0.242	-0.285	0.149
		[0.082]***	[0.084]***	[0.100]***	[0.157]
Constant	-0.102				
	[0.166]				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	0.009	-0.013	0.009	0.036	1509.063
Bandwidth		0.18	0.15	0.10	0.02
Treatment Slope	.	1.08	0.53	-0.10	-4.75
Control Slope	.	1	1	1	-30
Treatment Obs.		965	863	571	306
Control Obs.		827	825	691	367
Kernel		Uniform	Uniform	Uniform	Uniform
Bandwidth Method		Manual	Manual	Manual	mserd
Obs.	1690	1847	1847	1847	1847

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 F29: 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	-0.291				
	[0.061]***				
Bias-corrected		-0.233	-0.242	-0.285	0.149
		[0.057]***	[0.058]***	[0.067]***	[0.141]
Robust		-0.233	-0.242	-0.285	0.149
		[0.082]***	[0.084]***	[0.100]***	[0.157]
Constant	-0.102				
	[0.166]				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	0.009	-0.013	0.009	0.036	1509.063
Bandwidth		0.18	0.15	0.10	0.02
Treatment Slope	.	1.08	0.53	-0.10	-4.75
Control Slope	.	1	1	1	-30
Treatment Obs.		965	863	571	306
Control Obs.		827	825	691	367
Kernel		Uniform	Uniform	Uniform	Uniform
Bandwidth Method		Manual	Manual	Manual	mserd
Obs.	1690	1847	1847	1847	1847

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 F30: 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	-0.310				
	[0.053]***				
Bias-corrected		-0.297	-0.271	-0.274	-0.050
		[0.050]***	[0.051]***	[0.060]***	[0.110]
Robust		-0.297	-0.271	-0.274	-0.050
		[0.073]***	[0.076]***	[0.093]***	[0.121]
Constant	2.213				
	[0.146]***				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	2.443	2.448	2.443	2.447	1521.211
Bandwidth		0.18	0.15	0.10	0.03
Treatment Slope	.	-0.48	-0.20	0.04	-9.25
Control Slope	.	1	1	-0	-6
Treatment Obs.		965	863	571	383
Control Obs.		827	825	691	516
Kernel		Uniform	Uniform	Uniform	Uniform
Bandwidth Method		Manual	Manual	Manual	mserd
Obs.	1690	1847	1847	1847	1847

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 F31: Effect of Bandwidth Specification on RDD Results Using Life Satisfaction

	OLS with 0.15km	Bandwidth - 0.18km	Bandwidth - 0.15km	Bandwidth - 0.10km	Optimal Bandwidth
	(1)	(2)	(3)	(4)	(5)
Treatment Status	0.085				
	[0.093]				
Bias-corrected		0.110	0.111	0.114	0.369
		[0.089]	[0.092]	[0.110]	[0.211]*
Robust		0.110	0.111	0.114	0.369
		[0.134]	[0.139]	[0.171]	[0.239]
Constant	-0.060				
	[0.253]				
Project Site FEs	Yes	Yes	Yes	Yes	Yes
Control Mean	-0.058	-0.068	-0.058	-0.006	1842.046
Bandwidth		0.18	0.15	0.10	0.03
Treatment Slope	.	0.07	0.11	-1.40	8.12
Control Slope	.	0	0	2	-22
Treatment Obs.		965	863	571	309
Control Obs.		827	825	691	371
Kernel		Uniform	Uniform	Uniform	Uniform
Bandwidth Method		Manual	Manual	Manual	mserd
Obs.	1690	1847	1847	1847	1847

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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.