Evaluating Evictions: Causal Impacts of a Forced Relocation Program in Addis Ababa

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Abstract

We study the economic consequences of large-scale evictions of slum areas in the centre of Addis Ababa. Using a spatial regression discontinuity (RDD) approach, and rich primary survey data collected before and after 40,000 households were evicted from 20 different neighbourhoods, we estimate the effect of being evicted on evicted households and spillovers to non-evicted households nearby. We use extensive data on social networks, housing amenities, local amenities and travel times, and combine it with model-based money metrics to understand the welfare effects of evictions. Although households live considerably further from the city centre after their eviction, we find that they move to better neighbours with better amenities and housing quality. We can rule out large effects on household earnings and consumption. Turning to spillovers from *exposure* to nearby evictions for households that were not evicted, we find reductions in neighbourhood quality and social networks. Correcting the RDD results for this SUTVA violation changes the magnitude but not the sign of our main estimates, suggesting that failing to account for spillovers would have painted a too-positive picture of the effects of being evicted. We discuss how different types of compensation provided to evicted households drive our results.

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1 Introduction

A future of sustainable development will rely heavily on the successful management of cities in the Global South. Urbanization is predicted to increase rapidly. While well-organized cities can be a force for growth and a safe harbor in which to weather climate change, poorly governed cities have the potential to become havens for crowding, congestion, crime and violence. A key part of the policy challenge is managing informal housing. Evidence suggests that informal housing often sits on misallocated land (Henderson et al. 2021; Harari and Wong 2021), increases the cost of public good provision (Marx et al. 2013), and is at greater risk from climate change – all of which suggest aggregate gains from rehousing. Yet it is essential that we also understand the potential costs of rehousing and redeveloping misallocated land, particularly if we want to correctly compensate the households that would be affected by such policies. Forced relocation programs have been a popular policy option for many years.[1] Those who live in informal housing are reluctant to leave, and opponents cite the loss of social networks, decreased proximity to jobs and higher transport costs as strong negatives of any forced resettlement program. Theory does little to reconcile these views. If markets work well and most households have a closely substitutable housing option then resettlement could be relatively costless. In contrast, if markets do not work well, for example because network relationships create the need for coordinated movement, then relocation costs could be very large. There is a clear need to understand the empirical costs and benefits of different approaches to relocation.

Despite the importance of the question, we know little about the impact of forced eviction programs in the world's poor cities. A small number of papers study the impact of voluntary lottery-based access to relocation (e.g., Barnhardt et al. 2017; Franklin 2019) and recent work tries to separate out the impacts of the forced relocation from the change in housing associated with it (Rojas-Ampuero 2022),[2] but we are not aware of any evaluations of large developing country forced resettlement programs that make use of modern approaches to causal inference.[3]

[1] Appendix Table A provides a partial list of slum clearance programs around the world since 2004.

[2] Rojas-Ampuero's sample consists of the children of households that saw their informal housing redeveloped. She compares outcomes of those who were resettled elsewhere to those whose housing was upgraded, finding negative effects of resettlement. While very valuable, this study does not tell us the entire impact of forced resettlement, which is what we could need to know if it is not possible to redevelop an informal settlement, for example, because it is in a flood plane.

[3] A nascent literature uses modern methods and data to study eviction in the developed world (e.g., Collinson et al.

We partially fill that gap by studying the impact of a large-scale forced resettlement program undertaken in Addis Ababa starting in 2017, concentrating on the impacts of the program on those who were resettled.

The program we study evicted an estimated 40,000 households from the urban slums of central Addis Ababa between 2017-2019. These evictions were part of the government's centralized master plan for urban renewal, which aimed to clear all slums in Addis Ababa and transform the city center into a modern dense urban space. Prior to this program's enactment in 2017, 80% of Addis Ababa's city center was classified as a slum. Our evaluation is based on bespoke data collected by the research team, and has several characteristics designed to overcome three challenges in evaluating forced displacement programs: establishing causality; accounting for potential spillovers; and measuring welfare effects.

We overcome the identification challenge using a spatial regression discontinuity (RD) approach, combined with baseline and placebo treatment data that allows testing the key continuity assumption. This approach is made possible by detailed geo-referenced housing panel data that we collected for this study, which is rarely available for developing country cities where evictions happen, nor for that matter in rich-country cities, where forced evictions happened historically. We combine this household data with carefully digitized maps of eviction areas, including "placebo" sites: those sites that were planned for eviction but where the plans were never executed after the prime minister announced a temporary moratorium on evictions across the city. Drawing from a baseline survey of 30,000 households collected prior to the announcement of the program, we use precise geo-locations to build a panel survey sample of 5,000 households that lived in or close to the boundaries of eviction areas before the program announcement. We then collected endline data for these households five years after the evictions took place. The spatial granularity of our data enables us to construct a spatial RD using the precise boundaries of eviction sites and households' Euclidean distance to the boundary. Comparing evicted households and their counterparts outside the boundary allows us to estimate of impacts of resettlement, overcome the main identification challenge in evaluating the impact of slum clearance policies.

2022 studies eviction of low income renters, while Diamond et al. 2020 studies foreclosure in the US) but these studies are not specifically directed at the management of informal housing.

Importantly, we are able to use our baseline data to provide unique evidence in favour of the key continuity assumptions required for an RD specification, by showing that there is no suggestion of a discontinuity at the baseline for key household characteristics. Moreover, the baseline data enables us to control for selection and differential attrition while also speaking to heterogeneous treatment effects due to variation in baseline housing types. Further, we households on either side of the boundaries of our placebo sites to confirm that there is no discontinuity around these borders, either in the baseline or endline data.

Our second innovation is our approach to measuring and accounting for spillovers in our estimates. We measure spillovers through exposure to evictions, controlling for potential exposure had the placebo site plans gone ahead, and thus constructing a sort of difference-in-difference estimate. Specifically, using our geo-coding, we can create a measure of exposure to treatment for all control households in our data set, as the weighted average of the number of treated households within a given distance. This measure is a close counterpart to how we would expect a spillover to work: the impact of eviction on controls comes from the loss of interactions with the evicted through labor, social or retail markets, and could be thought of as an analogue to the usual market access measures used in the trade literature. Of course, this measure is also correlated with distance from a slum area, and so it is likely to be correlated with other characteristics. To overcome this difficulty, we adopt an approach similar to that suggested by Borusyak and Hull (2023). We control in our regressions for a similarly constructed measure for potential exposure, which is the sum of the realised and placebo treatment areas. This can be interpreted as a DID: if outcomes change with exposure to treatment more than they do with respect to placebo treatment, then we would interpret this as evidence of a spillover. Importantly, the identification of any spillovers imply a violation of the SUTVA assumption in our main RDD estimates, as households just outside of eviction sites are the most exposed to spillovers. With our estimates of spillovers in hand, we are able to correct our RDD estimates to recover the true effect of being evicted.

Finally, we address the question of welfare effects of such policies by collecting extensive data on social networks, housing amenities, local amenities and travel times, and combining it with model-based money metrics. While the first-order effects of displacement probably lie in the job market and expenditure, there are a host of other dimensions that likely change when someone moves

across space. Rents, transport times and costs, prices, neighbourhood and housing amenities and social networks seems key to us. We construct aggregated indices using our unique data to measure the effects of evictions on amenities broadly defined as well as social well-being. We are again in the unusual position of being able to observe all of these outcomes through the bespoke household survey that we designed to measure them.

Our data also allows us to shed some light on how evicted households should be compensated for their displacement. We survey a combination of households who live in different types of housing at baseline – public housing, private rental housing and home owners – a variable that is strongly predictive of how evictees were subsequently compensated. Those in public housing tended to be relocated to public housing further from the city center, those who were home owners were given land on the city's periphery and sometimes cash, while those who were in private rentals were mostly provided no compensation. Everyone was offered a nominal fee for relocation costs. This variation allows us to understand whether these different kinds of compensation led to importantly different causal effects.

In the aggregate, we find that eviction is neutral to positive from an economic perspective, but with important social downsides. As a first stage, evictees move quite a distance, on average living 5 Km further from the centre of Addis Ababa. This change in location is accompanied by a 6.6 percentage point increase in the probability that a given householder is working, and an increase in commuting time of about 13 minutes per day. The increase in work leads to a small increase in earnings per capita of about 200 Birr per week from a base of about 950 Birr per week. Households spend roughly the entire increase in their income, and this can be decomposed into a 30 birr increase in transportation costs per day, and about 190 birr increase in rent per month (both per capita). Food and other categories of expenditure do not change significantly. In summary, forced displacement leads to movement away from the center and an increase in earnings that is spent on rent and transport.

Whether this combination of effects should be seen as a welfare increase or decrease, depends in large part on whether the increase in rent comes with a large enough increase in neighbourhood and housing amenity. We see strong evidence of an increase in housing quality. Households have

more rooms in their houses, are more likely to have a flush toilet, less likely to share a toilet, more likely to have a separate kitchen, and more likely to have piped water. An index combining these and other housing features increases by nearly 0.3 standard deviations, indicating that the median treated household overtakes about 12 percent of the control households with respect to this metric. Overall neighbourhood quality similarly improves. Household report being less likely to smell trash and sewerage, less likely to see litter and less noise in their environment. Together, they report a 0.35 standard deviation increase in the quality of their neighbourhoods. In contrast to the increase in local amenities, we see a strong decrease in the quality of local public good provision in the outskirts of the city, which is where these households have relocated. Households report that they are far less likely to have street lights, have less publish space, and have less access to primary schools or hospitals. Overall, an index of public goods access decreases by about 0.32 standard deviations.

Additionally, we see important impacts on social networks. Many authors suggest that relocation can destroy social networks that bring either labor market or other advantages. The increase that we see in earnings suggests that there are no strong negative impacts on networked access to labor markets. We do, however, see negative impacts on the quality of their social networks more generally. In both objective measures of network size and subjective measures of network strength, households experience negative effects. They report a 0.27 and 0.22 standard deviation reduction in our indices of objective and subjective network quality, respectively. [4]

Turning to spillovers, we find no evidence of important economics spillover effects onto households left behind, but do find that there are negative spillovers on local amenities (possibly due to the disruption of demolitions or the precise of vacant land nearby to households left behind). We also find negative effects on social network outcomes for those left behind, consistent with them having important ties to households that were evicted. These spillover effects imply an important SUTVA violation, as the households left behind formed the control group for our RDD estimates. We adjust our RDD estimates for this bias. The positive effects on neighbourhood quality are lower but still meaningful after the adjustment, while the negative effects on social networks are

[4]The objective network quality index is a composite of network size, job network size, and financial network size. Comparatively, the subjective network quality index is a composite of network satisfaction, incidence of loneliness, incidence of difficult situations, and being without someone to talk to.

30% larger than the naive RDD estimates suggested.

We also look at heterogeneity by baseline housing type, which predicts compensation type. We find that households that privately owned or lived in public housing move roughly 2km further from the city center than those that privately rented at baseline. This is consistent with households that privately owned being offered land and those in public housing being offered new public housing both on the outskirts of the city, while those that private rented had no compensation that led them to relocate further away.

In general, we find those that were in public housing at baseline experience the largest improvements. While they paid more in rent, they see an improvement in their local amenities (as measured by our amenity index), housing quality, and overall life satisfaction. Comparatively, those that owned their housing at baseline see an even larger increase in rent (as many moved from being owners to renters). Despite paying high amounts in rent after eviction, they do not have statistically significant gains in local amenities, show some gains in housing quality, and report a (statistically insignificant) negative effect on life satisfaction. Finally, those that rented privately exhibit the most heterogeneous outcomes following displacement and thus have noisier results. These households pay more in rent at similar level increase to the households living in public housing but start with a much larger control mean. They report a positive but statistically insignificant increase in amenities, a negligible change in housing quality, and a negative but statistically insignificant effect on life satisfaction. Overall, our heterogeneity analysis shows that compensation types matter substantially for households' welfare following forced displacement.

The remainder of the paper is structured as follows. Section 2 describes the eviction program, section 3 describes our data and how we created welfare indices, section 4 explains how we identify direct, spillover and announcement effects. We present our primary results in section 5, which include the initial RDD specification for treatment effects, spillover effects, and SUTVA-corrected treatment effects that account for spillovers. To shed light on the mechanisms driving the types of treatment effects observed, we study variation in compensation types among those evicted in section 6. Finally, we offer some conclusions.

2 The Program

A new "Addis Ababa City Structure Plan" was approved in 2017. The plan outlined new land-use plans for a large share of central neighbourhoods in the city, as well the construction of large connecting roads. In many cases, the new plans proposed dramatic changes in land use from informal (slum) residential housing to more formalized, planned neighbourhoods, with provision for commercial property, private housing and government "condominium" housing.

These plans were then implemented in a piecemeal fashion across the city during 2017 and 2018. The execution of these plans precipitated the eviction of the existing slum residents and the wholesale demolition of their houses. We call the locations where redevelopment plans were executed "completed eviction sites". For some sites, the plans were to rehouse some evicted households on the original site in new government housing, however, these redevelopments had not happened at the time of our endline data collection. Moreover, not all sites designated for clearing were completed due to the piece-meal nature of the redevelopment plan's implementation and its ultimate suspension after 2019. We leverage these sites as placebos for the sites that were in fact completed.

Households were generally not warned that the eviction programs were coming. Our baseline data shows that for *most* eviction sites, the expected probability of being evicted was not discontinuously higher inside of evicted areas compared to areas just outside. Media coverage and other academic work on these evictions largely confirms the idea that these evictions came as a total surprise to evicted households.

Compensation provided for evicted households was determined by their existing legal status. Broadly, there were three types of households. First, a large share of the residents of central Addis Ababa live in *kebele* housing. This is slum-like (mud walls and floors) housing owned by the government and rented out to the residents for nominal (close to zero) fee. Households in kebele housing were supposed to be 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 loan. 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.[5] This alternative kebele housing was located all over the city.[6] Second, households that owned there own houses in evicted areas were given new land, usually further away from city, and a relatively large cash endowment (equivalent to 2 years of household income, on average) as compensation to finance the rebuilding of their home on this land. Third, households that were renting in evicted areas, or squatting illegally on public land, were not offered any compensation at all.

By combining maps of the eviction sites and Census data from 2007, we estimate that 37,968 individuals were evicted from their homes in 2017 and 2018, though is likely an underestimate as the populations of these areas likely increased from 2007 to 2017.

3 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.

[5] In practice, we find that there was some variation in the application of these rules across local areas: in some areas households previously in kebele housing report that they were not offered the choice of alternative kebele housing.

[6] Although this alternative housing was intended as a temporary form of housing – because the Master plan implied that *all* kebele housing areas would eventually be demolished– the suspension of redevelopment plans after 2019 mean that in most cases, households rehoused in this way, have remained there since.

3.1 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.

3.2 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



Figure 1: Endline Sampling - Oversampling Around Eviction Sites

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.

4 Identification

4.1 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 Appendix B. Second, we can test for continuity of the outcomes of interest across the boundaries of planned but not completed sites (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.[7]

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 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 B).

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.

4.2 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

[7] 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.

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.

4.3 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 4.2 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 D1). 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.

5 Main Results

This section consists of three main subsections corresponding to our three main areas or outcomes: the first stage effects of where people live, effects on earnings and consumption, and effects on neighbourhoods and social networks.

For each outcome, we present the "naive" estimates RDD estimate which identifies the effect of evicions on the difference between evicted and unevicted households, as in Section 4.1. We also run the same regression discontinuity design over placebo eviction sites to rule out alternative explanations for the treatment effects we see among those evicted from completed sites. Then we present our estimates of the spillover effects of evictions using our specification in Section 4.2, and finally, the SUTVA-corrected estimates of the effects of being evicted, as in Section 4.3.

5.1 First stage

Our first stage results provide strong evidence that a household's location to the boundary of eviction sites determined whether or not a household was evicted (Table 1). Treated households, those located inside a completed eviction site boundary, were 79.2% more likely to have moved at all after the sites were cleared. Treated households move on average 5.25km away from the

Table 1: First Stage Regression Discontinuity Results

	Moved at all	Distance Moved	Distance to Centre (km)	Log Distance to Centre	Time to centre	Avg. Commuting Time (in min)
Panel A: RDD-Estimated Treatment Effects						
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.803	6.364	5.274	0.987	18.106	12.839
	(0.028)***	(0.498)***	(0.458)***	(0.075)***	(2.541)***	(2.435)***
Control Mean	0.16	0.65	3.86	0.99	32.58	23.10
Observations	1792	1792	1792	1792	1787	1488
Panel B: Placebo Treatment Effects						
RD_Estimate	0.038	0.440	0.305	0.027	-0.267	-3.179
	(0.047)	(0.355)	(0.325)	(0.056)	(2.283)	(2.724)
Control Mean	0.22	0.80	2.67	0.74	25.44	24.90
Observations	843	843	843	843	842	702

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

city centre. This is in large part due to the location of condominimums on the outskirts of Addis Ababa.

Reassuringly, we find null effects on the placebo treated households: there is no discontinuity in the probability of moving or being evicted at the boundaries of sites that were planned but not actually evicted. This remains true for almost all outcomes reported in this section, providing further evidence that the selection of boundaries of planned eviction sites was orthogonal to household outcomes in those areas.

These average location changes are driven by heterogeneous changes to housing type and tenure induced by the program, which in turn depend in large part on the type of compensation offered by the government. For example, some households were given the opportunity to buy high quality housing far from the city centre, while others had no choice but to find a place to rent nearby. We discuss these changes in housing type in more detail in Section 6 where we look at heterogeneous treatment effects by compensation type. For the remained of this section.[8] For the remainder of this section, we focus on the average treatment effects of eviction.

5.2 Income and expenditure

We find that members of evicted households work and earn slightly more than those left behind.

Evicted household spend more per capita by an amount that is very similar to the increase in earnings, but this increase is driven entirely by increases in rent and transport costs. This increase in rent is perhaps surprising given that households live significantly further from the city centre. Our estimate of housing rent includes the costs of paying mortgages among households that received the chance to by a government condominium as part of their compensation, a point that we will return to later. Food consumption is not affected, and neither is the residual of other forms on consumption (not shown here). We find no effect on local food prices.[9]

Turning to spillover effects, we find no evidence of spillovers on earnings and expenditure.

[8] Note that, as shown in Table 6, eviction moves all households further from the city centre regardless of compensation type.

[9] We have food price data by combining expenditures and quantities, which are reasonably well measured for food items, from our household expenditure model.

Therefore, after we adjust for the bias induced by these spillover effects, our corrected estimates for the eviction of being evicted are largely unchanged from the RDD estimates, although the effect of evictions on per-capita earnings is no longer significant. Certainly, we can rule out that being evicted dramatically decreased earnings or consumption of either evicted or left-behind households.

Table 2: 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	0.062	202.426	210.139	27.991	198.588	27.594	0.021
	(0.029)**	(94.388)**	(79.661)***	(7.143)***	(30.605)***	(32.555)	(0.022)
Control Mean	0.55	961.25	1268.89	56.30	55.73	756.00	-0.04
Observations	1748	1792	1792	1792	1792	1792	1775
Panel B: Placebo Treatment Effects							
RD_Estimate	-0.001	-124.886	-12.095	-16.837	-22.256	-47.931	-0.033
	(0.043)	(121.728)	(117.030)	(9.195)*	(37.000)	(45.990)	(0.026)
Control Mean	0.56	953.27	1421.08	56.60	109.44	792.01	-0.03
Observations	829	843	843	843	843	843	841
Panel C: Spillover Effects							
Exposure	-0.033	-67.950	-36.812	2.324	-8.143	-17.709	-0.001
	(0.017)*	(52.254)	(43.953)	(3.541)	(11.528)	(18.673)	(0.012)
Control Mean	0.57	1008.06	1370.31	55.07	77.62	780.56	-0.03
Observations	3412	3489	3489	3489	3489	3489	3477
Panel D: Spillover-Corrected Treatment Effects							
RD_Estimate	0.030	134.475	173.327	30.315	190.445	9.885	0.021
	(0.034)	(107.887)	(90.982)*	(7.973)***	(32.705)***	(37.530)	(0.025)
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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 delves deeper into the labour market outcomes, by looking at individual labour market outcomes by gender. On average, the individual effects are very much in line with the household level aggregates. We find that men in evicted households switch from self-employment to wage work with a net effect on employment that is positive but not significant. Women, by contrast, are more likely to be self-employed, driving an overall significant increase in employment. The effects on earnings are larger for men, though our estimates are noisier in the individual data. Consistent with the literature on limits of women's mobility in developing countries (REF), we find that women's commutes increase by significantly less than men. Notably, we find that hours worked increased, even conditional on employment, for men.

Table 3: Effects on Individual Labor Market Outcomes by Gender

	Employed	Wage work	Self employed	Hours work	Hours work (cond.)	Earnings	Commuting time	Commuting cost
Panel A: Average Treatment Effects								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD_Estimate	0.053	0.055	-0.008	2.712	0.841	178.845	6.842	63.278
	(0.028)*	(0.025)**	(0.017)	(1.457)*	(1.632)	(114.304)	(1.305)***	(14.773)***
Control Mean	0.50	0.34	0.11	21.73	43.31	1273.89	10.86	63.44
Observations	5249	5249	5249	5249	2634	5249	5249	5249
Panel B: Men								
RD_Estimate	0.033	0.084	-0.045	3.588	4.005	258.225	8.287	103.797
	(0.038)	(0.037)**	(0.026)*	(2.090)*	(2.241)*	(205.716)	(1.980)***	(24.040)***
Control Mean	0.54	0.38	0.12	24.68	45.91	1718.08	12.73	82.51
Observations	2336	2336	2336	2336	1280	2336	2336	2336
Panel C: Women								
RD_Estimate	0.063	0.008	0.041	1.748	-1.831	56.998	5.229	30.083
	(0.035)*	(0.032)	(0.021)*	(1.780)	(2.046)	(107.895)	(1.612)***	(22.392)
Control Mean	0.47	0.31	0.10	19.60	41.33	956.21	9.62	51.64
Observations	2913	2913	2913	2913	1354	2913	2913	2913
Bandwidth	0.18	0.18	0.18	0.18	0.18	0.18	0.18	0.18
Project Site FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Male=female p	0.561	0.118	0.010	0.503	0.055	0.386	0.231	0.025

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

5.3 Amenities and public goods

To understand whether or not increased income and expenditure translates to better well-being, we look at how evictions changed the local amenities households experience. Each index is an Anderson index (also known as an inverse-covariance index) of a set of z-scored outcomes.

In Table 5.4, we see that evicted households have higher household quality by 0.265 standard deviations. There are no spillover effects on housing consumption, such that the corrected estimate is hardly changed. For detailed housing outcomes, see Appendix C. We find that evicted households live in houses with more rooms, bedrooms, and share toilets with fewer households.[10] Evicted Households also have a higher probability of having a separate kitchen, a flush toilet, piped water, and non-mud floors (usually tile or cement).

Turning to neighbourhood amenities, evicted households benefit from improved environmental amenities, which are related to the condition of their neighbourhood: cleanliness, sewerage in the streets, smells, noise and litter.[11] This suggests that the increased expenditure in rent (at locations further from the city centre) is at least partly compensated by the amenity value of their new housing. However, we find evidence of negative spillovers to untreated households: households left behind experience worse environmental outcomes. This means that we were slightly overestimating the positive effects of being evicted on amenities: these are about 30 percent smaller after the correctly but still sizeable and significant.

On the less positive side, when looking at safety outcomes – an index measuring experiences of crime, theft, and feeling unsafe at night– our naive RDD estimate shows no effects. However, we do find a negative spillover effect on this outcome. This is plausibly due to the fact that unevicted households now live next to empty lots, with fewer "eyes on the street" and easier places for potential criminals to hide. Once we correct for this spillover in Panel B, it suggests that safety is significantly harmed for evicted households, although this effect is not strong. We also find that evicted households report lower quality and access to public goods like schools and health

[10] For clarification, the variable number of households sharing a toilet (column 5) is oppositely coded such that a positive coefficient corresponds to a decrease in the number of households sharing so that a positive coefficient signifies an improvement in housing quality.

[11] For full set of indicators in enivronmental amenities, see Appendix Table C.1

services, driven mostly by the lower density of these in the more remote areas that they are moved to. We also find evidence of negative spillovers on public goods too. But these are driven solely by negative effects on working street lights nearby: suggesting that these were also removed during evictions. Fewer houses on those streets may also contribute to a feeling of having less well-lit public spaces. This in turn may explain the effects on crime discussed above. Perhaps not surprisingly, there are no spillover effects on other public goods like schools and hospitals.

5.4 Social well-being

A common concern of forced relocation programs is the negative effect on social cohesion and social well-being. Consistent with this view, we find negative effects on our aggregate social quality index, in Columns 5 and 6 of Table C. This is driven primarily by evicted households having weakened social networks. Households express lower satisfaction with their social networks, report they are less likely to be able to turn to their neighbours for advice, and are more likely to feel lonely. Unsurprisingly, this effect is symmetric: households that are left behind also experience some loss of networks as some of their neighbours were moved away.

The effects on the size of their objective network are small and non significant, which is perhaps not surprising given that the total number of people who were moved away would have been small relative to the total population, even right next to a boundary (we estimate that this is, at most, 40% of households within a 5 minute walk for the households most exposed to spillovers). However, households feel like the have lost networks as evidenced by a small but significant negative effect of their perceived network quality. In all this suggests that our naive RDD results underestimated the negative effects being evicted by around 30%.

Table 4: Effects on Amenities and Social Networks

		Neighborhood Amenities			**Social Networks**	
	Housing Quality	Environ-mental Amenities	Safety	Public Goods	Objective Network Quality	Subjective Network Quality
Panel A: RDD-Estimated Treatment Effects						
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.265	0.360	-0.029	-0.315	-0.276	-0.219
	(0.045)***	(0.051)***	(0.072)	(0.057)***	(0.053)***	(0.066)***
Control Mean	-0.07	-0.11	-0.02	-0.01	0.05	-0.02
Observations	1792	1792	1792	1792	1792	1792
Panel B: Placebo Treatment Effects						
RD_Estimate	-0.017	-0.047	-0.137	0.079	0.067	-0.041
	(0.053)	(0.074)	(0.102)	(0.075)	(0.077)	(0.091)
Control Mean	-0.13	0.00	-0.00	0.03	0.06	0.13
Observations	843	843	843	843	843	843
Panel C: Spillover Effects						
Exposure	-0.025	-0.098	-0.120	-0.058	-0.012	-0.075
	(0.021)	(0.029)***	(0.038)***	(0.031)*	(0.030)	(0.035)**
Control Mean	-0.08	-0.03	0.02	0.05	0.03	0.04
Observations	3489	3489	3489	3489	3489	3489
Panel D: Spillover-Corrected Treatment Effects						
RD_Estimate	0.240	0.261	-0.149	-0.373	-0.287	-0.295
	(0.049)***	(0.059)***	(0.081)*	(0.064)***	(0.060)***	(0.075)***
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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6 Heterogeneous Effects

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 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 5: Heterogeneous Results for First Stage

	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 6 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 6: Summary of Heterogeneous Results

	Earnings Per Capita	Expenditure	Rent	Housing Quality Index	Environmental Amenities Index	Social Quality Index	Life satisfaction today
Panel A: Average Treatment Effects							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RD_Estimate	224.883	199.330	196.627	0.275	0.368	-0.251	0.081
	(95.345)**	(79.391)**	(29.859)***	(0.045)***	(0.051)***	(0.045)***	(0.090)
Control Mean	961.25	1268.89	55.73	-0.07	-0.11	0.03	-0.07
Observations	1805	1805	1805	1805	1805	1805	1805
Panel B: Kebele Housing at Baseline							
RD_Estimate	190.621	138.333	180.457	0.343	0.347	-0.267	0.229
	(107.353)*	(93.184)	(32.726)***	(0.052)***	(0.060)***	(0.056)***	(0.105)**
Control Mean	917.80	1202.17	12.97	-0.09	-0.13	0.04	-0.12
Observations	1258	1258	1258	1258	1258	1258	1258
Panel C: Housing Privately Rented at Baseline							
RD_Estimate	388.492	340.369	251.220	0.038	0.234	-0.058	-0.129
	(249.700)	(217.411)	(90.079)***	(0.106)	(0.123)*	(0.101)	(0.195)
Control Mean	1104.50	1550.66	311.40	-0.11	-0.04	-0.04	-0.09
Observations	302	302	302	302	302	302	302
Panel D: Housing Privately Owned at Baseline							
RD_Estimate	121.904	311.501	186.389	0.200	0.446	-0.354	-0.358
	(257.602)	(203.483)	(81.291)**	(0.120)*	(0.138)***	(0.122)***	(0.220)
Control Mean	1102.76	1313.71	25.63	0.07	-0.11	0.01	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	0.371	0.631	0.005	0.510	0.109	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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Turning to the main heterogeneous treatment effects in Table XX. Households renting from the Kebele at baseline far significantly better, on average, than the other groups....

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.

7 Conclusions

Evicting households from slums for the sake of urban redevelopment has been a popular policy from European cities during the industrial revolution to modern-day cities across the Global South. Despite their widespread adoption and the substantial amount of policy and qualitative work studying them, we find no work that has quantified the causal impacts of these schemes on the

households that experience eviction. This paper aims to fill this absence of quantitative evidence by implementing a spatial RD approach on a forced relocation program of slum inhabitants in central Addis Ababa. Using well-defined geographic boundaries of evictions sites and bespoke data, we are able to causally identify the effects of evictions along a wide set of welfare dimensions, including first-order economic outcomes such as expenditure and employment, and less commonly measured outcomes such as commuting costs, amenities, and social networks. We find that households are able to sustain paying higher rents and more on transportation costs through increasing their earnings and employment rates. Higher rents map to better amenities, particularly in housing quality and neighborhood quality. However, evicted households do suffer from a weakened social network. Ultimately, more research needs to be done to build on our heterogeneity analysis, which scratches the surface of understanding how different compensation schemes impact these effects. If governments around the world continue to implement wide-scale evictions, policymakers need evidence on what negative outcomes can or cannot be overcome with different compensation types.

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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 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.

B.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.

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

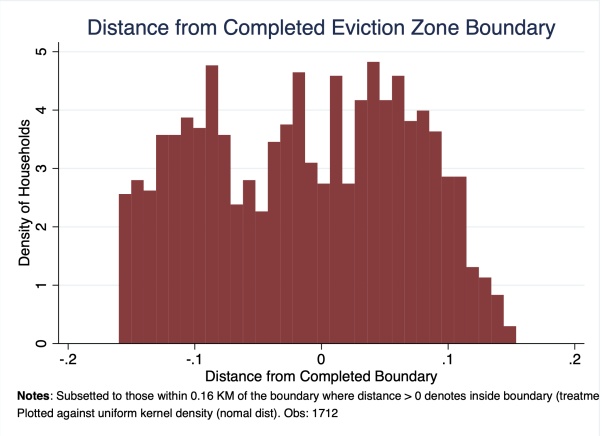
We select a window $[-0.16, 0.16]$ for graphical representation based on the following table,

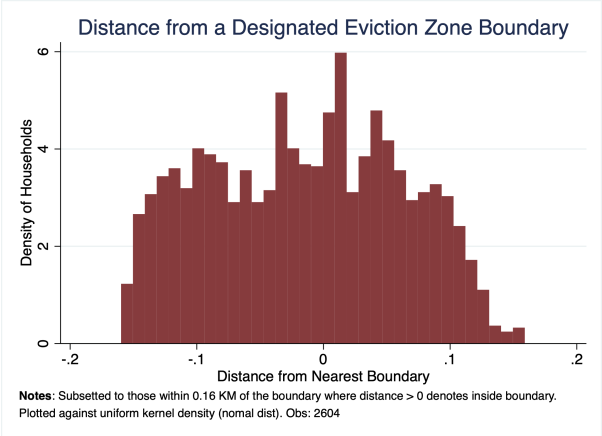
detailing the key statistics of the running variable: *distance from a completed project boundary*. (Although this window holds for the more generalized variable *distance from a designated project boundary*.)

Table B2: Distance from Completed Eviction Zone Boundary

	Mean	Median	SD	Min	Max
Distance From Completed Boundary	-0.62	-0.44	0.82	-13.47	0.15
Observations	4518				

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.

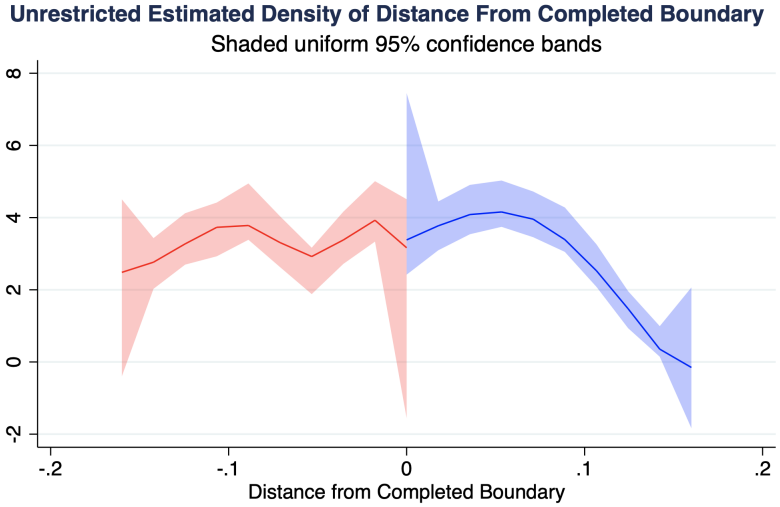




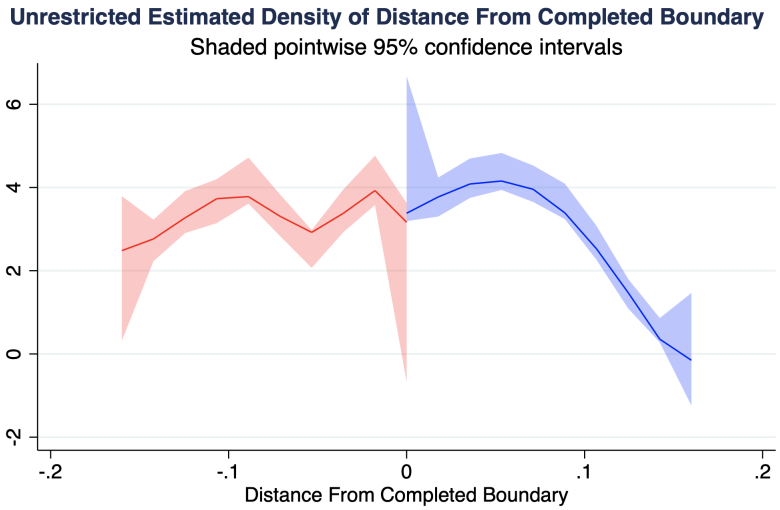
Next, we implement a manipulation testing procedure following Cattaneo et al. (2018) using local polynomial estimators of the density functions of each distance variable.

B.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 B3: Unrestricted Manipulation Test for Distance from Completed Boundary

	Outside Boundary (Left of Cutoff)	Inside Boundary (Right of Cutoff)
Sample Size	912.000	800.000
Sample Size in Bandwidth	200.000	280.000
Bandwidth Used	0.031	0.042
Bias-Corrected Density Estimate	1.479	4.939
Bias-Corrected T-Statistic	2.431	
Corresponding P-Value	0.015	

B.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.

B.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 B4: RDD Regression Results for Baseline Covariates

	MSE-Optimal Bandwidth	RD Estimator	Bias-Corrected RD Estimator	Robust SE	Eff. Number of Observations
Asset index	0.041	0.015	0.017	0.021	523
Weekly food expenditure	0.050	-1.351	7.388	74.242	637
Monthly rent	0.032	147.538	132.392	137.275	319
Monthly rent (excl. rent from Kebele)	0.044	571.684	672.821	445.361	67
HH registered at Woreda	0.042	-0.009	0.002	0.050	535
Head of household has a job	0.030	0.051	0.083	0.128	368
Owned home	0.028	0.078	0.096	0.111	339
Rented from Kebele	0.021	0.091	0.136	0.154	250
Rented from landlord	0.040	0.054	0.041	0.072	511
Temporary shelter from local govt	0.040	-0.006	-0.004	0.046	511
Combined govt provided housing	0.025	-0.082	-0.047	0.139	310

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.

B.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 B5: Canany and Kamat Test for Baseline Covariates

	P-Value of Canany-Kamat Test
Asset index	0.870
Weekly food expenditure	0.649
Monthly rent	0.311
Monthly rent (excl. rent from Kebele)	0.688
HH registered at Woreda	1.000
Head of household has a job	0.103
Owned home	1.000
Rented from Kebele	0.215
Rented from landlord	1.000
Temporary shelter from local govt	0.058*
Combined govt provided housing	1.000
Charcoal	0.264
Electricity	0.150
Pit latrine shared - ventilated	0.543
Pit latrine shared - not ventilated	1.000
Public Toilet	0.080*
Baseline Covariates Jointly	0.619

C Additional Main Regression Tables

Table C6: 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C.1 Amenity Index Sub-Indices and Components

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	0.265	0.241	0.218	-0.001	2.638	0.115	0.027	0.259	0.138	0.076
	(0.045)***	(0.073)***	(0.063)***	(0.023)	(1.134)**	(0.043)***	(0.042)	(0.037)***	(0.044)***	(0.033)**
Control Mean	-0.07	1.87	0.82	0.71	-9.42	0.53	0.69	0.13	0.44	0.81
Observations	1792	1792	1792	1753	1216	1792	1792	1792	1792	1792
Panel B: Placebo Treatment Effects										
RD_Estimate	-0.017	-0.026	-0.112	-0.017	0.324	-0.093	-0.021	0.005	0.057	0.004
	(0.053)	(0.107)	(0.083)	(0.029)	(1.460)	(0.059)	(0.055)	(0.035)	(0.062)	(0.044)
Control Mean	-0.13	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	-0.025	-0.020	0.037	-0.015	-2.755	0.026	0.003	0.033	-0.019	-0.017
	(0.021)	(0.045)	(0.032)	(0.011)	(0.512)***	(0.025)	(0.023)	(0.016)**	(0.024)	(0.019)
Control Mean	-0.08	1.88	0.83	0.71	-8.95	0.49	0.70	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	0.240	0.221	0.254	-0.017	-0.117	0.141	0.030	0.292	0.119	0.060
	(0.049)***	(0.086)**	(0.070)***	(0.026)	(1.244)	(0.049)***	(0.048)	(0.040)***	(0.050)**	(0.038)
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 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	0.360	0.491	0.669	0.169	0.299	0.351
	(0.051)***	(0.090)***	(0.092)***	(0.094)*	(0.089)***	(0.086)***
Control Mean	-0.11	-0.12	-0.10	-0.08	-0.14	-0.17
Observations	1792	1792	1792	1792	1792	1792
Panel B: Placebo Treatment Effects						
RD_Estimate	-0.047	-0.052	-0.084	-0.003	-0.044	-0.105
	(0.074)	(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	-0.098	-0.167	-0.088	-0.021	-0.156	-0.173
	(0.029)***	(0.050)***	(0.051)*	(0.051)	(0.052)***	(0.051)***
Control Mean	-0.03	-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	0.261	0.325	0.581	0.148	0.143	0.178
	(0.059)***	(0.103)***	(0.105)***	(0.107)	(0.103)	(0.100)*
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 C9: 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	-0.029	0.045	-0.201	0.039
	(0.072)	(0.093)	(0.085)**	(0.100)
Control Mean	-0.02	-0.01	-0.01	-0.05
Observations	1792	1792	1792	1792
Panel B: Placebo Treatment Effects				
RD_Estimate	-0.137	-0.093	-0.190	-0.138
	(0.102)	(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	-0.120	-0.143	-0.060	-0.146
	(0.038)***	(0.050)***	(0.049)	(0.048)***
Control Mean	0.02	-0.03	0.02	0.06
Observations	3489	3489	3489	3489
Panel D: Spillover-Corrected Treatment Effects				
RD_Estimate	-0.149	-0.099	-0.261	-0.107
	(0.081)*	(0.105)	(0.098)***	(0.111)
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 C10: 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	-0.315	-0.319	-0.393	-0.285	-0.254
	(0.057)***	(0.091)***	(0.092)***	(0.092)***	(0.095)***
Control Mean	-0.01	-0.03	-0.04	0.02	0.00
Observations	1792	1792	1792	1792	1792
Panel B: Placebo Treatment Effects					
RD_Estimate	0.079	0.231	-0.036	0.160	-0.075
	(0.075)	(0.121)*	(0.139)	(0.133)	(0.109)
Control Mean	0.03	-0.01	0.05	-0.03	0.13
Observations	843	843	843	843	843
Panel C: Spillover Effects					
Exposure	-0.058	-0.137	-0.033	-0.014	-0.021
	(0.031)*	(0.050)***	(0.050)	(0.051)	(0.051)
Control Mean	0.05	0.06	0.05	0.05	0.04
Observations	3489	3489	3489	3489	3489
Panel D: Spillover-Corrected Treatment Effects					
RD_Estimate	-0.373	-0.456	-0.426	-0.299	-0.275
	(0.064)***	(0.104)***	(0.105)***	(0.105)***	(0.108)**
RDD Bandwidth	0.18	0.18	0.18	0.18	0.18
Project FEs	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes

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

Table C11: Effects on Objective Measures Social Network Quality

	Objective Network Quality	Network satisfac-tion	Job network size	Financial network size	Neigh-bours - help fix	Neigh-bours - caring	Neigh-bours - advice
Panel A: RDD-Estimated Treatment Effects							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RD_Estimate	-0.276	-0.610	-0.395	-0.543	-0.125	-0.280	-0.306
	(0.053)***	(0.080)***	(0.513)	(0.293)*	(0.078)	(0.088)***	(0.087)***
Control Mean	0.05	3.36	3.68	1.80	1.95	1.84	1.65
Observations	1792	1792	1702	1702	1792	1792	1792
Panel B: Placebo Treatment Effects							
RD_Estimate	0.067	-0.007	0.396	1.232	0.019	-0.076	-0.032
	(0.077)	(0.106)	(0.852)	(0.569)**	(0.114)	(0.117)	(0.124)
Control Mean	0.06	3.37	3.49	1.63	2.03	1.78	1.59
Observations	843	843	798	798	843	843	843
Panel C: Spillover Effects							
Exposure	-0.012	-0.015	-0.470	-0.004	-0.028	0.071	0.069
	(0.030)	(0.041)	(0.301)	(0.204)	(0.044)	(0.049)	(0.050)
Control Mean	0.03	3.38	3.07	1.58	1.98	1.82	1.59
Observations	3489	3489	3319	3319	3489	3489	3489
Panel D: Spillover-Corrected Treatment Effects							
RD_Estimate	-0.287	-0.625	-0.865	-0.546	-0.153	-0.210	-0.237
	(0.060)***	(0.090)***	(0.595)	(0.357)	(0.090)*	(0.101)**	(0.101)**
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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C12: Effects on Subjective Measures of Social Network Quality

	Subjective Network Quality	Network size	Loneliness	Difficult situations	Without someone to talk to
Panel A: RDD-Estimated Treatment Effects					
	(1)	(2)	(3)	(4)	(5)
RD_Estimate	-0.219	-2.461	-0.367	-0.292	-0.309
	(0.066)***	(2.017)	(0.103)***	(0.099)***	(0.097)***
Control Mean	-0.02	14.96	3.09	2.93	3.19
Observations	1792	1791	1792	1792	1792
Panel B: Placebo Treatment Effects					
RD_Estimate	-0.041	-0.234	-0.119	-0.107	0.059
	(0.091)	(2.858)	(0.137)	(0.138)	(0.120)
Control Mean	0.13	17.98	3.21	3.14	3.36
Observations	843	843	843	843	843
Panel C: Spillover Effects					
Exposure	-0.075	0.270	-0.192	-0.123	-0.172
	(0.035)**	(1.081)	(0.054)***	(0.057)**	(0.050)***
Control Mean	0.04	14.92	3.19	3.07	3.30
Observations	3489	3489	3489	3489	3489
Panel D: Spillover-Corrected Treatment Effects					
RD_Estimate	-0.295	-2.191	-0.559	-0.415	-0.481
	(0.075)***	(2.288)	(0.116)***	(0.114)***	(0.109)***
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 Additional figures

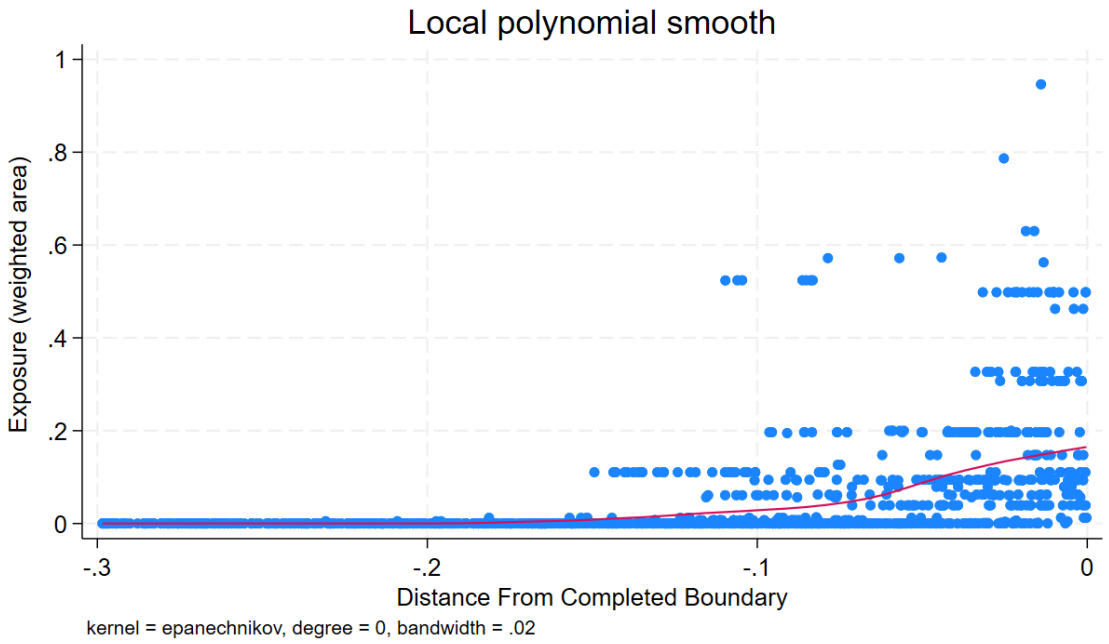


Figure D1: Exposure to spillovers as function of distance to treated areas among untreated households