There is No Free House.

Low-cost Housing & Labor Supply: Evidence from Urban South Africa[1]

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PRELIMINARY & INCOMPLETE

Abstract: Housing relocation programs such as the Moving-to-Opportunity program in the US are a common policy choice for governments in developing countries. This paper is the first to estimates the effect of this type of programs on the labor market outcomes and housing-related welfare indicators for low-income households across major cities in a large developing country, South Africa. I use four waves of panel micro data collected between 2008 and 2014, and I exploit the arbitrary eligibility rules of the policy with a fuzzy regression discontinuity design to obtain causal estimates. In the short-term, the labor supply of recipient households at cutoff decreases by between half to one standard deviation, driven mostly by a decrease in female participation. An income effect together with spatial dislocation dynamics explain these results. The effect on other dimensions of urban welfare is ambiguous.

Keywords: Low-Cost Housing, Labor Supply, Urban Areas, South Africa

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1. Introduction

The rapid pace of urbanization in developing countries emphasizes the need for robust evidence on the successes or failures of the public policies aimed at addressing the negative externalities of agglomeration forces and unplanned cities' expansions. Large rural-urban migration flows place significant strain on housing and serviced land, and more often than not result in fast-growing slum populations at the peripheries of cities of the developing world. Almost a quarter of the world's urban population lives in slums, and by 2030 close to 40% will lack proper housing and access to basic infrastructure and services (UN-Habitat 2014). Further, far from being a temporary phenomenon of migration to cities, informal settlements have become a full form of housing where millions of poor reside for decades (Marx et al 2013). Given the social and political imperatives of improving access to formal affordable housing, understanding the efficiency of housing policies is quickly becoming a priority for cities authorities around the world. In this paper, I provide the first quasi-experimental evidence of the effect of a subsidized low-cost housing program on the labor outcomes and the housing-related welfare of beneficiary households across main metropolitan areas of a developing country.

Housing relocation programs, through which governments provide low-cost housing at cities' peripheries, are a popular policy choice for governments of the developing world (Barnhardt et al. 2015). Most of these programs follow a model of subsidized housing provision managed by local authorities, by which housing is directly attributed to eligible households through lotteries or waiting lists (Gilbert 2004). Despite their popularity, there is little evidence of their success. In order to increase household welfare the structures need to be supported by complementary physical infrastructure and social services (Brueckner and Lall 2015) and allow the poor to benefit from the major advantages of urbanicity such as access to affordable commutes, public services and proximity to ethnic enclaves (Lall et al. 2008). Under the current conditions and despite voluntary take-up, the benefits of relocating (i.e. safe infrastructures, cleaner environments) may well be overcome by their costs (i.e. increased commuting and services costs, loss of social networks) suggesting possibly low or inexistent social returns to expensive housing programs (Barnhardt et al. 2015).

The program I study here is South Africa's national Housing Subsidies Scheme rolled-out at the end of Apartheid under the Reconstruction and Development Program (RDP). Similar to other relocation programs, it consists of one-off capital subsidies for the construction of free-standing housing units on greenfield developments built in partnerships between municipalities, the private sector and the communities (Khan and Thring 2003), and allocated to eligible households on an ownership basis. Allocation is done through municipal waiting lists. Since 1994, 2.8 million houses were built under the program across South Africa, benefitting more than 12 million individuals (2015). The advantage of studying this program is its large national scale, permitting to look for the first time across main metropolitan areas of a large country in sub-Saharan Africa[2]. A second advantage is that eligibility relies on an arbitrary income rule, with households earning less than R$3500 per month

[2] Sub-Saharan Africa is the region with the largest share of slum population in the world (199.5 million in 2015. It is estimated to be growing at 4.5% per year (UN-Habitat).

eligible for free housing under the scheme. I exploit this discontinuity to estimate treatment effects that are as good as randomized in a neighborhood around the discontinuity threshold. A final interesting feature concerns the country historical heritage. South Africa's urban structures have been marked by almost half a century of Apartheid spatial planning, which has accentuated constraints to access labor markets for the disadvantaged low-income groups residing at cities' peripheries (Banerjee et al. 2008). Combined with very high unemployment rates, this issue has put housing policies at the top of the political agenda[3].

This study identifies the causal impacts of receiving a subsidized housing under the RDP program by comparing households just below and above the income threshold, using being below the income threshold as an instrument for receiving subsidized housing. The probability of receiving subsidized housing decreases sharply above R$3500 among otherwise eligible households. Under the assumption of no manipulation of the assignment variable this fuzzy regression discontinuity (FRD) approach ensures greater internal consistency than other quasi-experimental methods, and results are (locally) comparable to that of randomized control trials (Lee and Lemieux 2010). To estimate the effect, I use four waves of panel data from the National Income Dynamics Study (NIDS) collected between 2008 and 2014 which documents households' subsidy status for obtaining their dwelling. I find no evidence of sorting at the threshold. Overall, my results are stable across a range of specifications, bandwidths, controls and sample restrictions. With regard to the external validity of the estimates, the FRD estimand should be interpreted as a weighted average treatment effect for the subpopulation affected by the instrument (Local Average Treatment Effect or LATE), where the weights reflect the ex-ante likelihood that the households' income is near the threshold. Results are thus valid for the population induced to take-up treatment around the discontinuity.

I find that in the short period of one to four years following RDP allocation, households are not better off on a majority of urban-related welfare indicators. The labor supply of recipient households at cutoff decreases by between half to one standard deviation. Overall, the decline is driven by a large drop in total weekly hours of paid work of about 30 hours, consistent with a reduction of the number of employed members by almost one. This finding is in line with similar studies in advanced and developing economies. The gender decomposition shows that the reduction is larger for female members through a reduction at their intensive margins. While I find evidence that some degree of income effect could explain these variations, this is unlikely to be the only explanation as evidenced by an increase in the number of unemployed working age members. The size fails to account for the total reduction in labor supply, but the statistically significant increase suggests not all members drop out of the labor market. Although endogenous household compositional dynamics could still be biasing the results, I find no evidence of significant changes in overall age structures that would suggest a relocation of less mobile members to RDP housing.

I explore alternative channels. I find a significant increase in the labor supply cost of households of up to half a standard deviation at cutoff. It is driven by a surge in the average distance to CBD and employment nodes (12 to 13km). These findings support spatial dislocation theories as one of the main

[3] Many argue that the housing scheme has helped reinforce the spatial logic of Apartheid by moving the poor to low-density areas often in old township locations (Lall et al. 2012).

causes for the negative labor supply response of RDP beneficiaries. The fact that I find no significant effect on the monthly share of total household revenue spent on transport, further suggests that households are not compensating by longer (more expensive) commutes, but rather choose to reduce their overall commuting. This is consistent with the reduction at the intensive margin. The literature has found that in developing countries females rely on fewer transport choices than males (Baker et al. 2005, Venter et al. 2007), and could potentially be more affected by distance.

The effect on other dimensions of urban welfare are more ambiguous. Housing amenities do deteriorate, driven by worse housing structures (fragile walls, number of rooms). Overall, I find no impact on neighborhood conditions. All characteristics tested are insignificant. Revealing however is the insignificant but increased perception of insecurity which could result from isolation at cities' peripheries and the disruption of social networks. The loss of social networks has been raised as a major limitation of this type of relocation programs in developed and developing countries alike (Barnhardt et al 2015; Day and Cervero 2010; Mills et al 2006; Kling et al 2007). They could be a factor behind the reduction in labor supply. Consistent with this interpretation is the statistically significant impact on the willingness to move by household members younger than 30 years of age at cutoff. This contrasts with the non-significant impact on the older members who might prioritize tenure security and home-ownership over labor opportunities.

This paper mostly focuses on labor supply. It relates to the large literature that has looked at the impact of housing lotteries and vouchers on the labor supply of beneficiaries in U.S. cities, with the Moving to Opportunity (MTO) program being the most notorious. Most of these papers find null (Jacob 2004; Kling et al. 2007) or negative impacts on the labor supply of recipient households (Jacob and Ludwig 2013; Mills et al. 2006 for the first year and null afterwards). Only Chetty et al. (2015) find long-term improvements on children below 13 years of age at the time of moving. Economic theory is ambiguous regarding the expected sign of any labour supply response to means-tested housing programs, mainly for at least two reasons. The first one involves how the dynamics of these programs relate to the life cycle model of labor supply; the second relates to its in-kind nature that offers a 'take it or leave it' level of housing consumption, the effect depending on how the program constrains consumption and whether the subsidized good is a complement or substitute to leisure (Jacob and Ludwig 2013). In developing countries, this relationship is all the more complex given tenure insecurity, the predominance of informal employment, the greater constraints on transport infrastructure and residential inequalities more closely related to the spatial layouts of cities. Under these conditions, housing is a greater complement to work. In the case of South Africa, high commuting costs[4], the low spatial mobility of the labor force and the dislocation between dwellings and formal employment remains one of the main constraints to the employment for the poor (Banerjee et al. 2008). The results of this paper confirm the relevance of these obstacles.

This study also contributes to the nascent literature evaluating housing programs in cities of developing countries. Historically, this literature has focused on titling and slum upgrading programs (Fields 2007, Galiani & Schargrodsky 2010; Takeuchi et al. 2008). The partial exception is Barnhardt

[4] See section 4.2.

et al. (2015). They provide the first experimental evidence of a housing lottery in Ahmedabad, and find robust evidence that the target population was worse off in the long term on a variety of socio-economic measures, including labor supply. Still their paper focuses on slum dwellers. Despite its unique relevance and large scale, previous attempts to evaluate RDP policy have remained fairly descriptive or have been limited to certain population groups (Lall et al. 2012; Franklin 2015). Franklin (2015) estimates the causal effects of the South African RDP program on labor outcomes but focuses solely on the population of slum dwellers in the city of Cape Town[5], de facto studying the effect of titling on a subpopulation of the treated. To the best of my knowledge, this is the first paper to estimate the effect of a large housing program for all low-income households across major cities of a large developing countries. It adds to the little evidence available to guide policy discussions.

The remainder of the paper is organized as follows: Section 2 discusses the policy setting, its historical implementation and the allocation mechanism. Section 3 outlines the empirical strategy, discusses the data and the validity of the research design. Section 4 presents the results. Finally, Section 5 concludes.

2. Policy Framework

2.1. Historical Background & Main Elements

South Africa's national housing policy was formally put in place following the end of Apartheid, to respond to the housing needs of the until-then formally excluded population. In 1997, the National Housing Department estimated that 2.2 million households were facing severe housing requirements, and included the right to adequate housing in the South African constitution (Sect. 26). A long list of regulations and laws[6] further defined the building blocks of today's housing strategy, and though the standards and implementation mechanisms have been revised throughout the years, the modality of allocation and final housing concepts have remained largely unchanged. By 2014, 2.8 million dwellings had been constructed (table 1), amounting to 24% of the formal housing stock in the country[7]. According to the General Household Survey (GHS), 15.3% of South African households lived in a RDP or state-subsidised dwelling in 2014. There was a substantial increase in delivery during the period of this study, the number going from only 5.5% in 2002 to 9.4% in 2009, and 13.5% residing in RDP in 2012.

Its main components and the focus of this paper are the Housing Subsidy Schemes, which provide qualifying households with the opportunity of obtaining their first house. Defined as one-off capital subsidies, in practice these are used for the construction of freestanding houses in new developments, administered and developed by municipalities, and later transferred to beneficiaries on an ownership

[5] Only about 50% of beneficiaries come from informal settlements (GHS 2009).

[6] These include the National Housing Accord and the White Paper on Housing promulgated in 1994, the Housing Act of 1997, the Breaking New Ground Paper (2004), and the National Housing Code, 2009.

[7] The housing backlog actually increased since 1994, fueled by a reduction of the housing supply at the lower-end of the affordable market and a rise in housing prices triggered by the high demand and the small size of the formal rental market (DHS 2012).

basis. Allocation is made through waiting lists managed by municipal housing departments, which are ultimately responsible for transferring property deeds. An RDP house cannot be sold until after eight years since the time of procurement (Department of Human Settlements-DHS 2012). Since early 2000s, 6% of registered houses have been sold, but informal transactions (often without transferring titles) are estimated to be higher: for the period 2005-2011, owners unofficially traded approximately 11% of all RDP houses (Urban Landmark 2011).

The project-linked housing subsidies constitute 90% of all subsidized housing; they are popularly referred to as RDP housing projects and are the focus of this paper[8]. Given that municipalities outsource their construction, the system is based on a fixed minimum cost for the house construction tied to a minimum standard. Since 2009, the National Housing Code stipulates that all stand-alone RDP houses must at least have a minimum gross floor area of 40m², two bedrooms, combined living area and kitchen, and a separate bathroom with a toilet[9] (See Appendix figure A.1)[10]. The minimum construction costs for each unit is set at between R$100-150 thousands. The overall development needs to be 200-250m² with paved roads and electrical connection (Tissington 2011).

2.2. Allocation & Waiting lists

Eligibility to receiving a RDP housing is determined by two categories of conditions. The first ones are socio-demographic criteria: the recipient must be above 18 years of age, married or living with a partner or otherwise have financial dependents, be of South African nationality (or permanent resident), and be a first time-owner. Additionally, it may not have benefited from a housing subsidy in the past. The second one is an income-band criterion: the monthly household income must fall below R$3501, making the housing program a means-tested benefit. Given socio-demographic trends in South Africa, I consider the income criteria to be the main condition (figure A.3). Yet, even in this case, this includes a large proportion of the population. In 1994 more than 80% of households fell under the eligible RDP income category (≤R$3500). In 2012, the number decreased to 60% (DHS 2012). This dimension is reflected in the sample, overall in the period considered 45.44% of households have an income below R$3501 (18.25% ≤R$1500)[11].

Since 2005, two official income bands exist for receiving RDP housing. The first one, consists of households with monthly incomes below or equal to R$1500, and entitles the recipient to a full subsidized house without any contribution. The second band, is made out of households with monthly incomes between R$1501-3500, and stipulates a one-off contribution of R$2479 towards the purchase

[8] There are in theory different types of subsidies through which it is possible to access subsidized housing (Individual subsidies, Consolidation subsidies, Institutional and Project-linked subsidies, and the People's Housing Process establishment grants). In practice they are determined according to the same set of criteria, and because of the limitations of the low-cost housing market, more than 90% is delivered as a project-linked subsidy. The rest is classified as an individual subsidy. In 1999 only 5% of RDP houses were in situ upgrading, the rest corresponded to greenfield developments (Khan and Thring 2003). Information is not available since, but conversations with officials at the municipality lead to believe the ratio remained largely unchanged.

[9] Before then, the typical size was a 25-35m² structure, with no separations until early 2000s, and a possible flush toilet (UN-Habitat 2008).

[10] All tables and figures referenced as A.# are in the online Appendix.

[11] The median income of RDP recipients in *t-1* is R$2435 in my sample, and the mean is R$ 3441.6.

price of the property, payable to the municipality or to the provincial Housing Department. This condition was added to engender a "sense of ownership" and prevent sells at below market value. In practice, the contribution is not enforced and was progressively abandoned given the difficulty for qualifying individuals to come up with the amount required (HDA 2011). This paper uses the discontinuity in the income criteria to identify the causal effect of receiving subsidized low-cost housing. I do not distinguish between both bands and use the R$3500 threshold for the discontinuity. The main reason for this relates to the lack of enforcement of the distinction between bands in the period covered. This is reflected in the data as I do not observe a significant jump in the probability of receiving treatment at the R$1500, the latter only existing at the second threshold[12] (figure A.4).

Generally, three phases can be identified in the allocation process. The first phase concerns the completion of a form at the provincial or municipal housing department, during which eligible households present proof of qualification. Details are checked against the National Housing Subsidy Database (NHSDB) as required by the National Housing Code. This database keeps records of all subsidy applicants approved by provinces across the country, with the purpose of preventing households from receiving more than one subsidy allocation, and it is used as a verification mechanism for those that have already benefitted from a subsidy. The information is recorded against the ID number of each individual (SERI 2013). Once approved, individuals are allocated to what is commonly known as the "RDP waiting list" (second phase). The waiting list is the main mechanism through which qualifying households are granted a house. The list is administered at the municipal level, and allocation is made in date order of registration, as well as location, once a project is completed in the municipality where the application was made. Some municipalities have recently digitalized their databases, and individuals can check whether they are successfully part of one (but no information on time or rank is given). According to the General Household Survey, in 2009 the average time in the waiting list was 5 years. Because of the long waiting periods, eligibility criteria are re-assessed after successful allocation to a project. If still compliant, households are given a property (third phase).

The application procedure for accessing RDP housing is often characterized as long and cumbersome. This is partly due to a generalized take-up, with demand far exceeding supply throughout the entire existence of the program. In 2009, 13.5% of households had at least one member on a demand database. The proportion of households from informal settlements with a member in the RDP waiting list is higher, at 39% for the same year. I discuss the implications for identification related to the allocation mechanism and how I deal with these in the next section.

2.3. Implications for identification

The allocation mechanism has two main implications for identification. The first one concerns the existence of crossovers, i.e. households receiving RDP above the income cutoff. One reason for this relates to the discretion that in practice municipalities have to allocate a proportion of the new houses to qualifying households from priority groups and catchment areas around the projects, depending on

[12] The decline is only of 2-3% at R$1500.

special needs (i.e. fire of informal settlement, community decision)[13]. In principle, these will still be from the database and still in date order (Western Cape Government 2013), but the proportion varies by municipality, and because of urgency, checks are not always conducted. A second reason for observing crossovers relates to the likelihood of subversion. The authorities have recorded different methods as means to 'jumping the queue'. These include land invasions, protest actions and violence, as well as bribes and connections. While potentially more worrisome for identification, as subversion implies selection into treatment, random subversion does not compromise the validity of the RD design. As long as subversion results from idiosyncratic errors the RD design isolates treatment variation that is "as good as random" (Lee and Lemieux 2010). Further, random subversion and crossovers are easily accommodated with Wald-type estimators of fuzzy RD designs, as used here. There is no reason to believe systematic non-random subversion through the manipulation of the income receipts exists. While incentive to cheat are high, the relatively low level of the threshold – with 2/3 of the income distribution falling within the qualifying band, reduces the probability of large manipulations. Evidence in the dataset supports this: I find no bunching in the assignment variable at cutoff. I discuss this further in section 3.2.

The second implication concerns the time dimension, related to the long waiting period from application to assignment into new RDP houses. This can complicate identification in two ways. The first one through behavioural mechanisms that could bias the results, such as the anticipation of receiving a house in the future or the strong disincentive to earn above the income threshold to remain eligible once a house is assigned. The long waiting periods and the strong uncertainty related to actually benefitting from the subsidised program reduces the likelihood of an anticipation bias. The same logic is true with the disincentive to apply effort. The second issue with the time dimension is due to the fact that I do not observe the date at which households applied and registered in the waiting list of the housing database. To tackle this problem, I only use a subsample of the entire dataset available: I keep at baseline[14] only those households who fall under the socio-demographic conditions to receive RDP in the subsequent time periods. This also supposes the exclusion of any household that received RDP by the first year (i.e. I exclude any household treated at baseline). I then use the lagged monthly household income as assignment variable to best proxy for the time of application (and moving), which is likely to have happened prior to the period at which I observe a change in RDP status. As a robustness check I run regressions separately for households whose income during the entire period crosses-over the cutoff and for those whose income always remain on one side[15]. The proportion of income crossovers is small. Only 185 households crossover above the RDP threshold, among these only 45 are RDP beneficiaries. The opposite dynamic only concerns 25 households.

[13] The proportions are ad-hoc and not publicly available.

[14] Baseline is defined as first time a household is registered in dataset, 2008 for 92% of households. Only 3% in 2012.

[15] Results are unchanged in terms of sign of the point estimates but statistical power is significantly reduced when partitioning the sample. These results should be considered with caution as I impose selection into the sample and could potentially be excluding households that are better off as a result of treatment. They were circulated in an earlier version and are now available upon request.

3. Empirical Strategy & Data

This section presents the methods and data employed to estimate the FRD. It also discusses the validity of the research design, potential sources of error and their implication.

3.1. The FRD Design

The main threat to empirically identifying the causal impact of low-cost housing on labor outcomes is non-random selection into the program. To address this issue, I exploit the arbitrary discontinuity income rule with a fuzzy regression discontinuity (FRD) design.

Given that I only observe the change in RDP status in the subsequent period, to account for the difference between the time of application and that of allocation, I use the two year lagged monthly household income ($X_{hm,t-1}$) as the assignment variable, with X=3500 at cutoff (c). If common RD assumptions hold, and all and only eligible households before the cutoff obtained RDP housing, then the causal effect of subsidized housing would be given by the difference in outcomes Y_{hmt} between those just above and just below R$3500:

$$\lim_{x \to c-} E[\,Y_{hmt}|X_{hmt-1} = c] - \lim_{x \to c+} E[\,Y_{hmt}|X_{hmt-1} = c] = E[Y_{hmt}0 - Y_{hmt}1|X_{hmt-1} = c] \tag{1}$$

In the case considered here, receiving a subsidy D_{hmt} is not deterministically related to crossing the threshold. For reasons discussed (i.e. crossovers), the jump in the probability of treatment at c does not go from zero to one (figure 1). In this 'fuzzy' RD setting, the causal effect is retrieved by dividing the jump in the relationship between Y_{hmt} and the assignment variable $X_{hm,t-1}$ at c by the fraction induced to take-up the treatment at the threshold (Hahn et al. 2001) [16] :

$$T_{FRD} = \frac{\underset{x \to c-}{lim}\, E[\,Y_{hmt}|X_{hmt-1} = c] - \underset{x \to c+}{lim}\, E[\,Y_{hmt}|X_{hmt-1} = c]}{\underset{x \to c-}{lim}\, E[\,D_{hmt}|X_{hmt-1} = c] - \underset{x \to c+}{lim}\, E[\,D_{hmt}|X_{hmt-1} = c]} \tag{2}$$

The ratio (2) can be estimated using both parametric and non-parametric Wald-type estimators as long as the order of polynomial in the forcing variable and the data window are the same for the first and second stage outcomes. I estimate (2) by two-stage least squares, with the basic reduced-form and first-stage estimating equations respectively given by:

$$Y_{hmt} = \beta_0 + \beta_1 . Below_{hmt-1} + \sum_{s=1}^{p} \beta_s . \left(\tilde{X}_{hmt-1}\right)^s + Below_{hmt-1} . \sum_{s=1}^{p} \pi_s \,.\, (\tilde{X}_{hmt-1})^s + v_{hmt} \tag{3}$$

[16] They show that the FRD can be conceptualized as a local IV, and that the interpretation of the ratio for a causal effect requires the same assumptions as a regular IV at the local threshold, i.e. monotonicity and excludability.

$$D_{hmt} = \alpha_0 + \alpha_1 . Below_{hmt-1} + \sum_{s=1}^{p} \alpha_s . (\tilde{X}_{hmt-1})^s + Below_{hmt-1} . \sum_{s=1}^{p} \gamma_s . (\tilde{X}_{hmt-1})^s + \varepsilon_{hmt} \tag{4}$$

where, (as in the above) *h* indexes for household in metro-area *m* (1…6) at period *t* (1…4). The forcing variable is now $\tilde{X}_{imht-1}$ - the normalized value of the lagged monthly income with respect to the cutoff – so that the discontinuity occurs at zero; the variable Below is a dummy variable equal to one when the household is below the threshold in the previous period. I include *s-th* order polynomials in the assignment variable, and I allow the relationship between Y_{hmt} and $\tilde{X}_{hmt-1}$ to have different slopes on either side of the discontinuity. Other parameters are as previously defined. Following Gelman and Imbens (2014), I focus on first and second order polynomials in $\tilde{X}$ in the main specifications, but explore the sensitivity of the results to using any polynomial degree between zero and four[17]. To avoid extrapolation bias in these global polynomial regressions, I restrict the sample by trimming off the tails of the income distribution (at 1 and 5 percent). Baseline estimates include time and metropolitan-area fixed effects, and I test the results to the inclusion of standard controls. These include the gender, age, population group, and education level of the household head, the number of members below 14 and above 68 years old. I cluster standard-errors at the household level.

As is best-practice, I also compute (2) using a non-parametric local linear regression specification with rectangular kernel weights and smaller bandwidths about the discontinuity[18]. For these, the property of the estimator depends crucially on the choice of bandwidth. I use both Imbens and Kalyanaraman (2012) and Cattaneo et al. (2014) bandwidths. Results are provided in the online Appendix in tables A.14 to A.21 and figure A.5. The global polynomials approximations are my preferred estimates as RDP recipients are not contained near the threshold ; including them reduces asymptotic variance.

3.2. Data

This study utilizes data drawn from the first four waves (2008, 2010, 2012, and 2014) of the National Income Dynamics Study (NIDS) panel, conducted by Southern Africa Labour and Development Research Unit (SALDRU) at the University of Cape Town[19]. The panel collects information on demographic characteristics, dwellings, income, employment, health and wellbeing of the respondents. A two-stage cluster sample design was used to randomly select near 7300 households across 400 primary sampling units for the first wave, stratified by district council.

[17] The Akaike information criteria (AIC) confirms that linear and second order polynomial are the best specifications at nearly all bandwidths considered. Results using larger order polynomials are available upon request.

[18] Lee and Lemieux (2010) argue that ultimately there is no much difference between both methods. In the end, they both have their source of bias and the best fit ultimately depends on the data. Here, I do not weight the data differently according to distance from the discontinuity, using rectangular kernels for the non-parametric estimations. The setting justifies this approach, as 25% of RDP recipients have an assignment variable below R$1500.

[19] Southern Africa Labour and Development Research Unit. National Income Dynamics Study 2008-2014, Waves 1 to 4 [dataset]. Cape Town: Southern Africa Labour and Development Research Unit, 2016 [producer]. Cape Town: Data First [distributor], 2016.

NIDS is a panel of continuing sample members (CSMs, i.e. individuals that were residents in participating households in wave 1); co-residents from wave 2 onwards are also re-interviewed as long as they do not abandon the household. Attrition is moderate for continuing sampling members (19% from wave 1 to 2 and 16% from wave 2 to 3), with the primary reason for no response being refusal and no contact. Tables A.1 and A.2 show the original sample of households' residents, and the redefinition of the sample with respect to the socio-demographic qualifying criteria of the RDP policy and to urban areas in the six largest metropolitan areas. These are the City of Cape Town, Ekurhuleni, eThekwini, Johannesburg, Nelson Mandela Bay, and Tshwane; South Africa's largest metro areas and the only ones with populations above 1 million (Census 2011). The choice of restricting the analysis to these large urban areas is made for simplicity reasons. Firstly, the mechanisms are likely different in rural zones with different commuting behaviors and a higher likelihood of home agricultural production. Second, it would be very difficult and time consuming to obtain information on employment nodes and calculate distances from the home addresses to CBD for every secondary city in the sample. South Africa's six principal metro areas amount to 40% of the working age population, and receive more than 50% of RDP housing subsidies (Urban Landmark 2011). It is nonetheless important to keep in mind that results may well differ in other contexts. My final sample is at the household level. It leaves me with a total of 2,984 observations. I use this level of analysis given that members' decisions to participate in the labor market are collective decisions, and receiving a subsidized house is likely to have an impact on all members in the household[20].

Information on whether a household received RDP housing in any of the subsequent periods is derived from the Household Questionnaire, which asks households if they received a government-housing subsidy to obtain the dwelling at each wave of NIDS. To correct for possible measurement error, I further refine recipients by crosschecking with the reported estimated subsidy amount and the estimated market value of the dwelling. I only keep as RDP those that report the market value at below R$300-450 thousands depending on district councils' differences in the distribution of housing prices[21]. The highest estimated value for RDP houses is R$200 thousands (at R$50 thousand). While the upper-end of the 'affordable' housing market in South Africa is capped at dwellings below R$250 thousand, and some noise may exist from the larger ceiling chosen, it also prevents Type II errors (i.e. the exclusion of treated observations due to misreported households' estimations). As discussed, RDP eligibility is derived at the baseline. Nearly 15% of households in my final sample received RDP housing following the baseline period, in line with national averages. The proportions are stable across years.

Distance to CBD and employment nodes are calculated as simple Euclidean distances between the household's address and the CBD or the main economic nodes' centroids. In the absence of supply-side data across cities, I use the 2013 South African National Household Travel Survey (NHTS) to identify main employment nodes by metropolitan area. The survey is designed to assess travel patterns

[20] I consider a household to be the same across time when the household head does not change, or when after they die, their spouse becomes the new head. Given this, I construct household identifiers that are unique in time by keeping unchanged the identifiers of the first wave when this condition holds. Using unique household identifiers per year slightly changes the standard errors but estimates remain unchanged.
[21] This depends on the district council distribution of housing prices, the benchmark being the value of the 75th percentile.

in South Africa by Transport Analysis Zones (TAZ)[22]. I identify the main economic nodes by the density of daily commutes to each TAZ by metro area. I then create the weighted average distance to the primary (65%) and secondary (35%) 'destination for work' TAZ. I use the same weights across all metros, except for eThekwini where three nodes have the same weight as per the polycentric nature of the city.

Table 2 contain basic summary statistics of households' socio-demographic and residential characteristics. Worth mentioning is the fact that most residential characteristics reflect the higher living standards of large urban areas, with 88% of households that report having access to electricity, and only 13% with no access to refuse collection. Informality is high with close to 30% of dwellings identified as informal (it includes backyard dwellings). Distances to CBD in minutes by mode of commute[23] and km are also high (26.35 km) and in line with national averages.

Tables A.3 and A.4 display baseline outcome means. I consider as employment any type of paid work (30% of the employed have no written contract and can be considered informal). Less than 50% of working age individuals are employed within households. This is not surprising given South Africa's high unemployment (on average 25% across metro areas in the period) and relatively low labor force participation (55% in 2010). I follow Barnhardt et al. (2015) and group outcomes into thematic indices: labor supply and labor supply cost, amenities and neighborhood quality. Each index is the simple average of the z-scores of their respective components. The labor supply cost index aims at quantifying the cost for households to participate in the labor market. The measure is imperfect but the sign of the estimated coefficient will give an idea of the commuting burden following the assignment to an RDP dwelling. The housing amenities index is computed to understand additional welfare dynamics related to low-cost housing. On average, the number of rooms per household is 3.6 and households rate their dwellings as structurally sound but requiring maintenance; 77% of households have access to flush toilets. Neighborhood quality is defined by dummies of functioning streetlights and regular refuse collection, as well as a measure on the frequency of robberies and thefts in the neighborhood. 71% of urban households declare having functioning streetlights and half of households perceive thefts as being 'common' in their neighborhoods.

3.3. Discussion of validity

Identification requires three conditions. First is the absence of manipulation of the assignment variable around the cutoff, which can be formally tested by examining the density of its distribution about the discontinuity. Figure 2 shows the McCrary density plots (McCrary 2008) for wave one and subsequent waves of the panel. The test runs kernel local linear regressions of the log of the density separately on both sides of the threshold. I find no evidence of sorting, with the discontinuities statistically insignificant. Further, I compute local polynomial density estimates (Cattaneo, Jansson and Ma 2015) to test for the null hypothesis that the density of the assignment variable is continuous at

[22] Defined by Statistics South Africa as relevant travel micro-areas created from the aggregation of Census EAs (2011).
[23] I proxy time of commute by the average minutes per km of the main mode of transport used at corresponding TAZ of the household's residence, conditional on their population group and income-decile..

cutoff. I confirm that there is no evidence of manipulation (table A.5). Appendix Box A.1 addresses concerns regarding the fact that the assignment variable relies on self-reported income data. Even absent intentional misreporting or manipulation, self-reported income data is noisy and it is important to discard biased estimates that could result from bunching in the assignment variable due to its self-reported nature, i.e. it is easier for individuals to round the income they report (Barreca, Lindo and Waddell 2015). I find no evidence of systematic bunching at round numbers.

The second related key condition of a valid RD design is that 'all other factors' are continuous with respect to the threshold. If there was non-random sorting, we could expect some of these characteristics to differ systematically between households immediately above and immediately below a given income threshold. Graphical tests using local linear polynomial regressions on lagged household characteristics are supportive of the identifying assumption (figure 3). Dashed lines show 95% confidence intervals, each point plots an average within a bin. I conduct a formal balance test by replacing the dependent variable in equations (3) and (4) with relevant observed lagged households' characteristics. The results indicate that these are well balanced on both sides of the cutoffs. The coefficients on below are typically small and statistically insignificant, with the few exceptions notably on informal dwelling which I include as control in relevant specifications (table 3).

Third, identification requires a strong relationship between the assignment and treatment variables, i.e. the conditional probability of receiving treatment should change discontinuously at the threshold. I examine graphically the first-stage relationship between being below the threshold and receiving RDP housing. Figure 1 shows local averages and linear fits at narrow bandwidths to plot RDP assignment in *t* against the monthly household income in *t-1*. I do not include controls or fixed-effects to transparently display the raw data. The likelihood of obtaining RDP decreases discontinuously at zero. Table 4 displays strong first-stage estimates for the preferred specifications, with F-Stats of up to 30.26. Figure 4 graphically explore the reduced-forms of selected outcome variables repeating the exercise done for assessing the continuity of control variables.

Some limitations of the identification strategy deserve discussion. As with many RD studies statistical power is an issue and weak identification when decreasing the sample size remains a problem due to the loss of efficiency and asymptotic size distortions in the standard errors (Marmer et al. 2014) While the presence of crossovers is well accommodated with the fuzzy design, it weakens the power of the identification with non-parametric estimates at narrow bandwidths. Figure A.5 plots estimated non-parametric coefficients for different bandwidths: asymptotic size distortions are graphically visible but signs and sizes are consistent. I consider non-parametric estimates to be robustness checks. A second limitation concerns the external validity. As is the case with LATE, the estimated effects are based on *compliers* at cutoff and results are valid for the subpopulation affected by the instrument.

4. Results

4.1. Households' Labour Supply

In tables 5 to 8 I examine how receiving RDP subsidies influenced the labor supply of households. Each row reports estimates of ratio (2) using parametric regressions and a polynomial of degree two. For robustness, the same estimates are compared to polynomials of degree one (tables A.6 to A.9) and non-parametric local linear regressions (tables A.14 to A.17). OLS results can be found in Appendix A.22-A.23. The same exercise is done for subsequent outcome variables.

I begin by examining the main components of household labor supply (table 6). I find a large decline in total household weekly hours of paid work of about 30 hours, equivalent with a reduction of 2-3 days of weekly work. The sign and size are robust across specifications, with statistical significance varying from 10 to 5% levels in parametric regressions. This is consistent with a decline in the overall number of employed household members (columns 4-5) by 1 to 0.7 members, statistically significant at 5% levels. The labor supply index in table 5 confirms the overall negative effect. The index is calculated as the average of the z-scores of weekly labor hours per working age member and employed per working age member. In a time period of 1 to 4 years, the labor supply of households receiving RDP housing, declines by between 0.52 and 0.95 standard deviations. Point estimates are statistically significant at 10% level and driven by the negative effect on the intensive margin. I study the variation in impact across household members by gender. Table 7 contains the results of these separate regressions; these are identical as the previous ones except for the inclusion of gender composition as control variables. The estimates reveal that most of the decline in labor supply happens through a reduction of total female hours. The effect is also larger (twice or three times as large) for females between 0.8-0.5 on the extensive margin but almost never statistically significant.

I also look at the effect on unemployment (table 8). I use a large definition of unemployment - those actively looking for work and discouraged searchers. The coefficients for the total number of unemployed are positive but non-significant. The size of the coefficients fails to compensate the overall decline in labor supply. The number of unemployed per working age member display an increase of about 0.2-0.25 for households below the threshold, statistically significant at 10 percent level. Though small in size, these results suggest that while some members are dropping out of the labor market, working age members are more likely to keep looking for work.

These findings are in line with the recent literature and the findings by Barnhardt et al. (2015) on the housing lottery in Ahmedabad. They also align with the conclusions of the U.S. literature that finds temporarily negative or no effect on the labour market outcomes of households benefitting for public housing programs (Mills et al 2006; Ludwig and Jacob 2012). The fact that the bulk of the negative labor supply effect concentrates on female members may reflect a switch to home-based work. While the employment variable includes informality, it is likely to be under-reported. Still, informal work in South Africa is small compared to countries with similar levels of GDP per capita (35% in 2014), and estimates on home agricultural production are close to zero and fail to find any consistent effect.

Standard labor supply models would explain this reduction from the new wealth shock or the income effect associated from no longer paying rent. Given the South African practice for low-income households to rent a property (*shack*) in their backyard, it could also arise from new rental income. In the case of informal settlements this new rental income could also result from the rental of the previous dwelling. In the 2011 census, 40% of individuals living in shacks reported 'owning' their unit and 17%

renting it. The percentage is larger in the sample with 40% of households in informal housing declared as renters, and 53% as 'owners'. In columns (1) and (2) of tables 13 and A.21 I show that households receiving RDP are more likely to collect rental income by between 0.25-0.55 percentage points, suggesting some income effect may be at play. If this effect dominates, the reallocation of female work to within the household may reflect South African social values. At the same time, the predictions of the basic static labour supply model should be considered carefully as there may be non-linearities resulting from the in-kind nature of housing subsidies in the budget constraint (Moffit 2002). For instance, it is rational to expect that for many households RDP subsidies imply higher levels of consumption of housing – as households are expected to pay for public services such as water and electricity. In these cases, rental incomes could be coping mechanisms from the reduction in labor supply or insufficient labor incomes. The positive impact on unemployment suggests able adult members do not completely drop out of the labor market and are still looking for jobs. This would be less likely if a pure income effect dominated.

I explore alternative hypotheses in the next sections. The decline could be driven by an increase in the cost of participating in the labor market following relocation to a new neighborhood. It is reasonable to expect that this cost could disproportionately affect female members (section 4.2). The impact of the subsidized dwelling on other elements of urban welfare, such as neighborhood quality, security and amenities also help to shed light on the above mechanisms (section 4.3). Further, endogenous changes in household composition could still be biasing the results. If in anticipation of the new location, households choose to move their less mobile members to RDP housing, the negative effect could partially reflect the change (section 4.4).

4.2. Urban Distances

Next, I examine the impact of obtaining RDP housing on commuting costs. RDP houses are often built at the outer edges of cities, even further from employment opportunities than informal settlements (SERI 2013). On average, in the province of Gauteng where three of the metropolitan areas in the study are located, RDP houses have been built at a distance of between 10 to 45 km from employment centres (figure A.2). Most South African cities are marked by the heritage of Apartheid planning, which reinforces spatial inequalities by pushing low-income households at the peripheries of cities. For poor households, the cost of participating in the labor market is closely related to their commuting expenses. A large literature has studied the linkages between spatial dislocation and the labor market participation of disadvantaged populations (Gobillon and Selod 2014). South Africa's average commuting times are extremely high for all population groups. On average in 2013, daily commutes took up to 100 minutes (Kerr 2015). In the sample, average daily commutes to CBD were even higher (117 minutes). Coupled with inadequate public transport infrastructure, distances tax the poor the most for their daily trip to work (Kerr 2015). In 2013, less than 9% of commuters used publicly-subsidized transport (train and buses) across South Africa, with nearly 50% choosing cars and minibus taxi (informal transportation). Further, a nascent literature has shown that in poor settings the cost of commuting is higher for females who disproportionately choose to walk, limiting the size of their accessible labor market (Baker et al. 2005). At baseline, I do not observe statistically different average commuting distances for female (26.615 km) and male (26.068 km) headed households. Female-headed households do display statistically higher daily commuting times on average (120.439 minutes vs. 115.340 minutes for men headed households).

Table 9 (A.10 and A.18) formally examines the impact of receiving RDP housing on distances to CBD and employment nodes in km and time of commute (minutes). Metropolitan-area and time fixed-effects should absorb any differences and changes in infrastructure and city-structures.

I begin by examining the overall effect in the labor supply cost index (column 1), calculated as the average of the z-scores for distance to CBD in km and two-way time of commute in minutes, both as previously defined. I find that there is an increase of the labor supply cost of between 0.468 to 0.532 standard deviations for households at cutoff, significant at 10 percent levels. The effect is largely driven by the positive increase in distances to CBD and employment nodes (also positive and significant at 1 levels). The surge in km is relatively large even for South African distances (12 to 13 km), just below half of the average at baseline. The sign and size are robust across global specifications, but double in size in nonparametric estimates suggesting some level of measurement error. There is no significant effect on the time of commute[24] or in the monthly share of total household revenue spent on transport. These results are stable in all specifications[25].

The fact that increased distances do not translate into larger monthly expenses on transport suggests that households are not compensating by longer (more expensive) commutes, but rather choose to reduce their overall commuting. This is consistent with the reduction in the intensive margin of their labor supply. Having to compensate for larger distances, household members may choose to reduce their hours of work outside of the household without completely dropping out of the labor market. Further, the literature has shown that in developing countries, the commuting choices of females differ from that of males in that they disproportionally choose to walk (Baker et al 2005). While there is no evidence on South African commuting choices by gender, this explanation could be put forward as to why I observe a larger decline in the labor supply of female members. A related hypothesis concerns the possible isolation of households due to the larger distances, with the loss of social networks disproportionally affecting female members (i.e. one can imagine informal networks for day care provision, for instance). While I cannot be conclusive regarding the channel through which increased distances may or may not be responsible for the decline in the labor supply of RDP recipient household members, results in the next sections are supportive of the key role played by the spatial dislocation in explaining these results.

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[24] As explained earlier, to obtain the time of commute I proxy the household mode by the preferred mode in the travel zone (TAZ) of the household residence, conditional on their monthly income and population group. While it may seem a rough approximation, 71% of public transport users commute using minibus taxis across South Africa (Kerr 2015). I observe an extremely small variability in terms of choice by population group, irrespective of their income. For instance white South African majoritarely prefer to drive for commuting, while African South Africans predominately use minibus taxi.

[25]The small sample size prevents me from running the regression with distance stratifications. Approximations show no particular difference between groups further or closer to original employment node.

4.3. Amenities & Neighborhood Quality

It is important to understand how housing amenities and neighborhood quality are affected by treatment. The benefits of housing programs like RDP subsidies go beyond the added fixed-asset and the possibility for low-income groups to become home-owners. They also relate to their effect on complementary dimensions of wellbeing, which may include better housing conditions (Fields 2007) and improved access to public services such as refuse collection and electricity, better access to sanitation and lower crime-rates. Despite worse work accessibility, improvements in these categories would explain the choice for taking-up the subsidy as per the standard Rosen-Roback model. Yet, it is not obvious that this type of programs achieves these objectives, and RDP subsidies have been severely criticized in terms of the quality of dwellings and the servicing of the locations (SERI 2013). Housing and neighborhood amenities are also closely related to the labor supply of households, not only for instance through health channels, but also as they help understand the effect on social networks, households' choices and their perception of opportunities.

I first look at neighborhood quality, and estimate the effect of obtaining RDP housing on the neighborhood quality index, calculated as the average of the z-scores for perceived frequency of robberies, functioning street light and refuse collection. These are partial measures of neighborhood quality but they aim at giving an idea on the overall environment for RDP dwellers. Distances to bus and train stops where only collected at baseline and cannot be used to infer the effect of the policy on accessibility to public transport. Table 10 (and tables A.11, A.19 and A.24) contain these results. The regressions in columns (1) and (2) are identical except that in column (2) I control for informal housing in *t-1*. Results are smaller but unchanged suggesting amenities are worse-off irrespective of previous dwelling conditions[26].

Overall, I find no effect on neighborhood quality, and none of its components are statistically significant. There are however telling differences regarding their signs, which suggest a consistent improvement on refuse collection. The effect on functioning street lights is ambiguous, and large standard-errors do not allow me to draw any conclusive remark. More interesting, pulling the average on the other direction, is the steady perceived increase in thefts and robberies in the neighborhood of about half a standard deviation. Though insignificant, the perception of crime security matters. The *deterioration* is revealing of an increase in the sense of insecurity that may result from isolation, but also from the disruption of social networks in close-knit communities. Because of the design of the allocation mechanism it is unlikely that all social networks are lost (i.e. sometimes entire communities are moved). Yet, this finding suggests that part of the decrease in labor supply could be related to the loss of informal business partners, day-care providers, and security mechanisms put in place in previous locations. Evidence from other housing mobility programs suggests this is not a farfetched hypothesis. The loss of social networks is one of the main constraints of these types of relocation programs in

[26] Point estimates in all other columns correspond to specifications that include a dummy variable for informal dwelling in *t-1*. I only use this preferred specification for the local linear regressions.

both developed and developing countries (Barnhardt et al 2015; Day and Cervero 2010; Field et al 2008; Mills et al 2006; Kling et al 2007).

Next, I look at housing amenities and estimate the effect of obtaining RDP housing on the amenities index, calculated as the average of the z-scores for a measure of dwelling quality, number of rooms, access to electricity and type of toilet facility. A measure of access to a water source would have been ideal but the variable in the NIDS panel is highly imprecise. Results are in tables 11 (and A.12, A.20, and A.24). As before, column (1) differs from the rest in its specification, and results are unchanged when controlling for the previous dwelling conditions. I focus on columns (2) to (6). Overall, I find that amenities for RDP housing beneficiaries declines by between 0.65 to 1 standard deviations. These are statistically significant at 5 and 10% levels. The effect is driven by a decline in the number of rooms and the perceived quality of the structure, and to a minor extent access to access to electricity. The quality of RDP subsidies has been the subject of heated debates. Officially, 14,5% of residents have said that the walls are weak or very weak, while 13,9% regarded the dwellings' roofs as weak or very weak (GHS 2014), but the numbers are higher in press reports and anecdotal evidence. According to Khan & Thring (2003), the fixed-cost of the housing units has pushed developers to reduce margins through lower land costs at relatively peripheral sites that often fail to include the range of necessary public facilities and amenities. The high cost of services, which many RDP recipients previously did not pay, has also led to cases of up to 80% non-payment where entire projects have been disconnected from electricity grids (SERI 2013). The results in this paper supports these reproachs. Short-term mechanisms could also be at play by which delays in servicing in recently constructed areas or expectations about the new dwelling negatively bias perceptions. Still, results are in line with the literature that has looked at longer time-spans (10 years) and finds no long-term effects on investments in durable goods (Galiani & Schargrodsky 2010; Lall et al. 2012).

Benefiting from *urbanicity* and better access to opportunities is one of the underlying objectives of the public provision of low-cost housing. Improvements on some elements of neighborhood quality and housing amenities tested here are only marginal. Tables 13 (and A.21), examine data concerning the preferences of household members to continue living in the current area. Overall, household members' preference to stay are statistically significant and negative below the threshold. There is a 0.30 to 0.88 percentage points increase in members preference to leave their current location at cutoff. When grouping the results by age groups (columns 7 and 8) it becomes clear that the preference to leave is driven by members aged 30 or below, for which point estimates are large, consistently negative and statistically significant at 10% levels. These result are supportive of the isolation hypothesis by which the loss of social networks and higher labor supply costs would be hampering access to work opportunities. In their paper, Barnhardt et al (2015) find suggestive evidence that greater average distances to employment opportunities discourage adult children from staying in their parents' household; but still find that on average adult children sacrifice geographic proximity to their families. In the short-term period analyzed here, one could imagine similar dynamics.

4.4. Household Composition

Related to the change in residential location and reduced accessibility to labor markets are possible effects on household composition. The social housing literature has put emphasis on this issue (Mills et al. 2006). In the South African context, some papers have found that social benefits are indeed altering household dynamics (Ardington et al. 2009).

Examining the effect of RDP on overall household dynamics would go beyond the scope of this paper. Here, I rather focus on understanding whether endogenous household compositional changes might be biasing the results on labor supply, in cases were less mobile members (senior, children or disabled) are reallocated to public housing due to their geographic constraints. Summary statistics do not suggest large differences in household composition for RDP-recipients and non-RDP recipients at baseline. The exception is the larger proportion of dependents and female-headed households in future RDP households, which reflects the policy of favoring vulnerable populations[27], and is the reason why I include the gender of the household head and the proportion of members below 15 and above 68 years old as basic controls in all specifications.

To explore possible changes, table 12 shows the FRD estimates on dependency ratios, the age of the household head, and on receiving other government grants. The interest of looking at dependency ratios is that they inform on overall changes in household members' age and indirect labor market abilities. Results differ by age group (columns 2-3): contrary to the aged dependency ratio (for those aged 65 and over), point estimates on child dependency ratios are positive (for those aged 14 or less). None is statistically significant. Overall, I find no effect on dependency ratios suggesting average age dynamics are unchanged. The same is true for the age of the household head, which is unaffected by treatment.

These results suggest that while some degree of household reconfiguration may exist, they do not concern the overall age and work abilities of households, at least in the short term considered here. They are supportive of a reduction on labor market outcomes resulting from the previous mechanisms discussed.

5. Conclusions

This study has focused on identifying the reduced-form impacts of a large public housing program on the labor supply and housing-related welfare of beneficiary low-income households across South African metropolitan areas. Albeit limited, findings here provide compelling causal evidence about a controversial type of housing policy.

In the short-term period of one to four years following RDP allocation, I find that households are not better off on a majority of urban-related welfare indicators. The labor supply of recipient households at cutoff decreases by between half to one standard deviation, driven by a decline on the labor supply of female members. While I find evidence that some degree of income effect could explain these variations, it is unlikely to be the only explanation. In fact, the spatial dislocation of households, as evidenced by the rise in average distance to CBD and employment nodes (12 to 13km), seems to

[27] For instance, female-headed households are more likely to receive housing subsidies (GHS 2014).

dominate. Results on neighborhood quality, housing amenities and willingness to move, let to believe that the effect is not only happening through higher commuting costs, but is also related to increased isolation and the loss of social ties. These could disproportionally be affecting female members.

Benefiting from *urbanicity* and better access to opportunities is one of the underlying objectives of the public provision of low-cost housing. It is hard to argue there are significant positive short-term effects in the case considered here, beyond homeownership and the obtention of tenure security. Even the latter needs to be properly evaluated. The Department of Human Settlements estimates that about 35% of total units built were not formally registered (2012)[28]. While it is not possible to draw general conclusions from LATE estimates, and overall dynamics migh differ in the long term - particularly when considering the effects on health outcomes or younger children -, findings in this paper add to the evidence of the limited (and negative) effects arising from relocation programs. The gender differences are also compelling: more evidence is needed to understand how commuting choices vary by gender and how these differently affect labor supply choices. Overall, findings in this paper further emphasize the importance of connectivity and social networks for the labor supply of low-income households living in large urban areas. These elements should be key components of any low-cost housing policies.

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[28] By 2011 only 8% of subsidies had been used to leverage a mortgage, which also raises the issue of the value of the house as a fixed asset.

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