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Enabled to Work: The Impact of Government Housing on Slum Dwellers in South Africa

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Abstract

This paper looks at the link between housing conditions and household income and labour market participation in South Africa. I use four waves of panel data from 2002-2009 on households that were originally living in informal dwellings. I find that those households that received free government housing later experienced large increases in their incomes. This effect is driven by increased employment rates among female members of these households, rather than other sources of income. I take advantage of a natural experiment created by a policy of allocating housing to households that lived in close proximity to new housing developments. Using rich spatial data on the roll out of government housing projects, I generate geographic instruments to predict selection into receiving housing. I then use housing projects that were planned and approved but never actually built to allay concerns about non-random placement of housing projects. The fixed effects results are robust to the use of these instruments and placebo tests. I present suggestive evidence that formal housing alleviates the demands of work at home for women, which leads to increases in labour supply to wage paying jobs.

Keywords: housing, labour supply, time allocation, home production
JEL Classifications: P14; J22; O18; D13

1 Introduction

Substandard housing conditions are considered to be one of the key deprivations suffered by the poor. Currently, it is estimated that over 860 million people live in slums in developing countries, and this number has been growing rapidly over the last decade (UN Habitat, 2003, 2010). Living in informal settlements is associated with a lack of access to running water, electricity, ventilation, security of tenure and access to economic opportunity. Improving or eradicating slums has been a key policy goal of governments, yet there is no clear consensus on what best practice should be.[1]

Slums can be thought to constitute a poverty trap (Marx et al., 2013). Living in a slum is not only an outcome of poverty, a growing body of evidence shows that slum conditions themselves have a detrimental impact on households. Yet improving housing conditions at the cost of relocating households further away from jobs and existing networks could do more harm than good (Barnhardt et al., 2014; Lall et al., 2008). Policies aimed at improving the housing conditions of the poor need to take account the effect that they have on the economic and labour outcomes of slum-dwellers.

This paper examines the links between housing conditions and labour supply. Over the past 20 years the South African government has provided over 3 million free stand-alone houses to its citizens. While the government housing policy has been praised for the extraordinary scale of delivery of housing, it has also been criticised for providing low quality homes in areas far away from jobs. I study the impact of free government housing in South Africa, and find clear evidence of increased incomes among recipient households. The evidence suggests that poor housing conditions constrain the ability of households to take wage-paying work.

I use longitudinal household data from Cape Town over four waves from 2002 to 2009 to assess the impact of government housing on household income and labour market participation.[2] I test the hypothesis that receiving a government home allows substitution from labour at home to work in the labour market, which in turn leads to increases in household income.

I use the allocation procedure used by the local government to award housing to households as a natural experiment. Recipients of housing were selected because of their proximity to the new housing projects. I proceed in two steps: firstly I use the distance between households' original place of living and locations of newly built housing projects to instrument for individual selection into treatment. To do this I develop a unique maximum likelihood estimator which predicts the probability of receiving government housing from any one of multiple nearby housing projects.

Secondly, I control for non-random locations for the selected sites of new housing projects. I use a set of housing projects that were approved and planned but cancelled for reasons unrelated to the communities they were intended to benefit. I exclude from the sample households that had no projects planned nearby and use only those households that had planned but incomplete projects nearby as a control group. I then repeat the main estimates using only completed projects as instruments.

Both household fixed effects and instrumental variable estimates show that households receiving government housing experience large increases in income, relative to households that do not receive housing. These findings are robust to tests using the cancelled projects. In general the IV

[1] For an overview of some of the debates in this literature see Marx et al. (2013); Collier and Venables (2013); Davis (2007); Werlin (1999).

[2] It is estimated that between one quarter and one fifth Cape Town's entire population benefited from government housing since 1994 (Seekings et al., 2010).

results are larger than the fixed-effects results. This is consistent either with measurement error, or a story whereby households with the greatest needs (those struggling economically) within communities are awarded the housing, leading to downward bias in the fixed-effect estimates.[3]

I investigate the channels through which improved housing increases household incomes. I find that the rise in household income is due to wage employment, rather than increases in income from rent or self-employment. The effect seems to be driven by increased female labour force participation. Women in treated households are more likely to be working in wage labour. These effects are not present for male households members. However, I do find a significant treatment effect on both both female and male household members' wage earnings.

The concentration of the effect on female labour supply suggests that poor housing conditions place particular burdens on the time use of women. I show that that women in South Africa, particularly in informal settlements, allocate significant time to housework and care. This is consistent with evidence from other developing countries (Berniell and Sánchez-Páramo, 2012). Due to data limitations, I cannot conclusively show that this is the main channel driving the results. I do find that government housing significantly increases electrification, direct access to running water, and modern home appliances, all of which could be saving significant amounts of time for women. In addition, living in the South African slums is hazardous- shack fires are common because of low quality stove and heating devices. I speculate that improved housing could reduce time mending and rebuilding after these kinds of disasters.

Field (2007) argues that improved tenure security frees up time that otherwise would have been spent at home defending the home from expropriation. Most South African households in informal settlements already enjoy de-facto tenure security (Payne et al., 2008), and eviction risks that do exist are unlikely to be bolstered by time spent at home.[4] I argue that these results are not driven by tenure security. Instead, South African households face a high rate of crime, and may need to spend time at home to deter intruders. I show that receiving government housing significantly increases feelings of safety in the home.

I contribute to the literature on the relationship between physical living conditions and female labour supply. Labour saving improvements to the lives of the poor can free up time to work in the labour market (Duflo, 2012; Greenwood et al., 2005; Devoto et al., 2011). Dinkelman (2011) and Field (2007) show that home electrification and improved tenure security, respectively, increase female labour supply and earnings by freeing up time from work at home. In the context of housing projects specifically, Keare and Parris (1982) find that provision of tenure and basic services in four countries had positive impacts on employment and income generation.

A related literature looks at the impact of *where* people live on their labour outcomes (Bryan et al., 2014; Ardington et al., 2009; Barnhardt et al., 2014; Franklin, 2015). The housing project studied in this paper moved households very slightly further away from job opportunities. In a setting where distance from jobs is thought to be an important contributor to poor labour market outcomes for black South Africans (Banerjee et al., 2007), I find evidence that government housing is not improving the class and race-based segregation of the city. My study benefits from an IV estimator that estimates the impacts of housing on those that did not have to move too far. In this way my study isolates the impact of housing on household outcomes from the effect of

[3]It could be because of local average treatment effect interpretation of the results, which I discuss in detail in the paper. Briefly, the instruments identify the effect on households that didn't have to move too far to take up housing, because they were treated by virtue of their proximity to housing.

[4]Ironically, evictions that have come from large townships, as opposed to "squatter" settlements on private land, are made by the government to make way for government housing projects. While these evictions often come with the promise of replacement government housing, some households could miss out, or have been placed in temporary shelters for long periods of time.

relocation.[5]

Secondly, I contribute to the broader literature on the ways in which slum living constitutes a poverty trap. A growing literature looks at the impact of slum upgrading on health and well-being (Cattaneo et al., 2009; Galiani et al., 2014), considerable evidence shows large impacts on health of improved access to services such as sanitation and running water (Zwane and Kremer, 2007; Pitt et al., 2006; Jalan and Ravallion, 2003; Duflo et al., 2012). Marx et al. (2013) discuss three channels through which slums could act as a poverty trap: human capital and health effects, poor incentives for policy, and under-investment due to weak property rights.[6] I add a forth channel to this list by showing that poor housing conditions can constraint labour market participation.

Thirdly, this paper provides a rigorous evaluation of a large scale government program that is the subject of much debate and criticism. The scale and political sensitivities of projects like these make them difficult to randomize. A large developed country literature generally draws negative conclusions about the impacts of large scale public housing projects (Olsen and Zabel, 2014).[7] This is the first rigorous evaluation, to my knowledge, of a housing project that provides a complete housing unit free of charge in developing country context.

Indeed, delivery of housing on the scale of millions of households is usually considered infeasible or not cost-effective for most developing countries (Gilbert, 2004). Donors and researchers have tended to focus on evaluations of upgrading and land-titling programs. However, standard policy approaches have had little success at mitigating the expansion of slums (Marx et al., 2013). Housing projects of the kind of implemented by South Africa are popular with governments, because they are so popular with electorates. Ethiopia and Columbia (Gilbert, 2014) are embarking on a housing projects of a similar scale. Rigorous evaluations of projects like these are important to guide policy makers, to inform best practices for dealing with informal housing conditions.

Finally I contribute methodologically to a growing literature that attempts to estimate the effects of housing policies and urban policy more generally (Baum-Snow and Ferreira, 2014; Field and Kremer, 2006). Local area instrumental variables are harder to use in a setting of continuous population density. I extend a literature that uses proximity data to instrument for individual selection into projects (Attanasio and Vera-Hernandez, 2004; McKenzie and Seynabou Sakho, 2010). I overcome the challenge of multiple weak instruments and improve the efficiency of my first stage IV estimate by creating a unique maximum likelihood estimator for the effect to create a time-varying set of instruments top predict selection into housing.

The rest of the paper is organized as follows. In Section 2 I discuss the context of housing policy in South Africa, as well the role of work at home for women in South Africa. I present a simple model of how housing could increase labour supply for women. Section 3 describes the GIS and survey data used in the paper. In Section 4 I describe the instrumental variables strategy in detail, and show results from the first stage to show that proximity to housing predicts selection into treatment. Section 5 show the main results for household earnings. The mechanisms driving the impacts on income are discussed in Section 6, while further robustness checks are in Section 7. Section 8 concludes.

[5] If the movement induced by housing did have negative impacts on household outcomes, it seems that household incomes rise in spite of this additional distance from jobs.

[6] Galiani and Schargrodsky (2010) provide strong evidence for the last of these channels in Argentina.

[7] The flavour of these arguments are still best summarized by Jane Jacobs (1961), in her seminal text on urban planning: "The method fails. At best it merely shifts slums from here to there, adding its own tincture of extra hardship and disruption. At worst, it destroys neighbourhoods where constructive and improving communities exist and where the situation calls for encouragement rather than destruction".

2 Setting and Context

Informal settlements in South Africa grew in the context of the apartheid system of enforced segregation. Relocation of non-white populations to the periphery of cities or remote rural areas has left a persistent pattern of segregation. Poor non-white areas are located far from more prosperous city centres. Migrant labour in cities was highly regulated, making access to urban employment a constant battle, and secure rights to adequate housing in the cities almost impossible (Royston, 1998). Public investment in urban infrastructure and housing in black areas was minimal.

As the architecture of apartheid was dismantled, starting in the 1980's with the repeal of the Group Areas Act, families that had previously been prevented from doing so began to move to the cities in vast numbers. This rate of migration, combined with the poor existing housing stock and South Africa's extremely high and rising employment rate (estimated to be around 24% for those actively seeking work) has led to a housing crisis. Many new urban migrants moved into shacks built in the backyards of existing formal dwellers (Seekings et al., 2010).

When the first democratic government was elected in 1994 there were an estimated 12.5 million people without adequate housing. Only 65% of the total population was housed in formal (cement and brick) dwellings, and high household formation rates have made this problem even more acute. It is estimated that the number of informal dwellings in Cape Town grew from 28 000 in 1993 to roughly 100 000 in 2005 under the pressure of migration and urban population growth (Rodriques et al., 2006).

2.1 Housing Policy

The first democratically elected government embarked on a number of policies to improve the lives of South Africans.[8] The new South African constitution included the right to adequate housing (see Section 26, Constitution of South Africa).[9] The South African government promised to deliver 1 million houses in the 5 years between 1995 and 2000. This ambitious target was more or less met. By 2008 it was estimated that 2.3 million houses had been built, and in May 2013 the government announced that it had passed the 3 million mark (South Africa, 2013).[10]

This housing policy, originally referred to as the Reconstruction and Development Program (RDP) aimed to provide as many low cost houses as quickly as possible.[11] The value of the house provided is very small in comparison to similar projects in countries such as Chile (Gilbert, 2004).

The program gives individual capital subsidies to eligible households. However, the vast majority of the subsidies have been product linked; they had to be used to purchase houses commissioned by the government and built by private construction companies. These came to be known as the RDP houses, small stand alone units built on large empty land just outside existing informal settlements. Government housing policy was updated with the "Breaking New Ground" policy document of 2004, which placed increased emphasis on minimum build-

[8] Important policies include electrification of 1.75 million home, improved access to running water to nearly 5 million people in rural areas, the extension of free basic health care to 5 million people, and a childcare grant and pension program. Some evidence on the positive impacts of these policies are documented in Duflo (2003); Case and Deaton (1998); Dinkelman (2011).

[9] The South African constitutional court has consistently upheld individual rights to housing, and these constitutional changes have insured rights to land for millions of households that were previously categorized as "squatters". For a description of the watershed legal case involving land rights see Sachs (2003) on the Grootboom case.

[10] According to the census of 2011 there are approximately 14.5 million households in total in South Africa.

[11] This policy of the national housing subsidy scheme was outlined in "A New Housing Policy and Strategy for South Africa" (Republic of South Africa, 1994).

ing standards, *in situ* approaches to upgrading, rental housing and densification (Charlton and Kihato, 2006). But by and large, the housing scheme has continued to be characterized by the construction of large greenfields projects. Small houses on separate plots with the orange roofs that have come to characterize the South African urban landscape. For a detailed and up-to-date outline of issues relating to the housing policy, see Tissington (2011). There is no evidence to my knowledge that government housing projects have been rolled out in conjunction with other welfare or urban improvements projects.

To be eligible to receive housing an individual applicant needs to be married or otherwise supporting dependents in a household with total income of less than R3500 per month, cannot own a registered property, and must be a South African citizen (Department of Human Settlements, 2009).[12]

The success in the delivery of housing units has been a cornerstone of the African National Congress's electoral campaigns since 1994. More than 10 million people are estimated to have benefited directly from the program. Yet in a period of increasing poverty, unemployment and urbanization, the number of households living in informal housing has actually increased, especially among the African population.

The policy has been criticised for not doing enough to deal with the housing backlog, providing low quality substandard housing that hardly improves living conditions of the poor (Tomlinson, 1998; Lipman, 1998), not being accompanied with other infrastructure and neighbourhood investments (Huchzermeyer, 2003) and for not doing enough to deal informality by ensuring transfer of title deeds (Huchzermeyer and Karam, 2006). Housing programs have been said to have contributed to forced evictions (Chance, 2008), particularly to areas further away (Centre on Housing Rights and Evictions, 2009) and to have been biased towards particular racial or political groups (Seekings et al., 2010). The most pervasive criticism of the policy has been the location of housing which has often been determined by private construction companies that choose to build on the cheapest possible land. In most cities housing has been built far away from the city centres in a way that has reinforced the spatial segregation of South African cities (Huchzermeyer, 2006; Bundy, 2014; Charlton and Kihato, 2006).

In the setting where this study is conducted, the Western Cape Occupancy Study (Vorster and Tolken, 2008) finds that resale rates of housing are high, at around 20%, mostly on an informal market. Rental of these houses does not appear to be at all common, while the practice of building a small "backyard" shack or shelter is. More than one third of households had a backyard structure within a few years of receiving the house.[13]

While many studies have evaluated government housing using observational data or qualitative analysis, there is no study, to my knowledge, that attempts to estimate causal impacts of government houses on the outcomes of households receiving them.

2.2 Theoretical Framework

In this section, I provide a theoretical basis for causal links between housing and household labour participation. I provide evidence on the patterns of time use for female household mem-

[12]It has been frequently observed that these eligibility requirements were often unverified, and for the vast majority of slum dwellers, are not likely to be binding anyway. I discuss issues to do with allocation of housing to applicants when I discuss the identification strategy used in this paper in Section 4

[13]These structure were sometimes used to accommodate other members of the household that could not fit in the original structure. In the cases when the structures were occupied by non-household members, only about half of paid rent. Some households still owned their previous (informal) dwelling, and were renting it out, but in most cases their informal dwellings had been demolished when they left, or they had given it to a friend or family member.

bers in South Africa and argue that slum dwellers' time is constrained by their physical living environment. A simple model of home production predicts that upgrading housing would induce substitution of time away from home production into wage labour.

I conceive of home production as time consuming activities related to the production of goods and services consumed at home. This includes maintaining the physical structure to ensure safety, security, warmth and shelter, household activities such as cooking and cleaning, and rebuilding of structures after damage from fires or flooding.

The UN-Habitat report of 2003 (UN Habitat, 2003) outlines a full taxonomy of the basic characteristics of slums. Many of the deprivations of slum living relate to issues of home production. Cooking and bathing is likely to be considerably easier in a home with running water and electricity, as opposed to a informal dwelling where other carbon fuel sources are often collected, and water has to be fetched from communal taps. Maintaining a sanitary home environment is also likely to be far easier in a cement floored home without leaking roofs or permeable walls.[14] In Cape Town most slum dwellers have to use badly maintained communal toilets located some distance from their homes, or buckets which must be emptied outside of the home every morning. Paraffin is a common use of fuel for cooking and heating, and is known to be a cause of fires and respiratory disease (Schwebel et al., 2009). In Cape Town, as in slums around the world, formal electricity connections are rare for shack dwellers, with more than 50% having fire-prone illegal connections, or no electricity at all (City of Cape Town, 2005). Electricity greatly aides home production if it facilitates the use of fridges, stoves and microwaves.

Time use surveys of poor South African indicate that a considerable amount of time is consumed by domestic activities, particularly for female members of households, who are primarily responsible for chores at home. South African women spend on average three times as long (3.5 hours a day) as men on unpaid work (Budlender et al., 2001).[15] Crucially, the evidence suggests that these activities take far longer in informal housing. In the national accounts individuals living in informal housing in urban areas spent 25% more time on non-labour market work than other urban households (Budlender et al., 2001). Shack dwellers in Cape Town report *more than twice* as much time (17.1 hours per week) spent on housework than their formally housed counterparts (7.5 hours per week).[16]

Many of the issues related to time use are likely to do with access to labour saving appliances. In 2000 only 28% of informal dwellings in urban areas used electricity for cooking, versus 77% of urban households. Similarly they were far more likely to use gas or paraffin stoves for cooking and heating and lighting. Only 46% of shack dwellers have access to a refrigerator, compared to 90% of families in brick houses.

Households living in the slums on the Cape flats are extremely vulnerable to township fires and, during winter months, storms and flooding.[17] Fire hazards are due, in part, to the types of appliances used for cooking and heating outlined above. These events are common, and often lead to widespread destruction of housing infrastructure, which takes time and money to rebuild.

[14] Cattaneo et al. (2009) have looked at how cement floors improve health from improved sanitary conditions.

[15] These patterns are consistent estimates for other developing countries (Berniell and Sánchez-Páramo, 2012).

[16] These were calculations based on the CAPS datasets used for this paper, outlined in Section 3. Unfortunately this data was no collected for periods of the survey beyond the first wave, which makes it impossible to estimate the impact of housing on time use in this setting.

[17] In 2005 a particularly damaging fire razed over 3000 shacks in Joe Slovo informal settlement just outside of Cape Town ("Shack-dwellers have nothing left after blaze" (iolnews, January 17 2005)) The victims of the fire were promised government housing after being displaced, but many remain in temporary relocation camps years later (Centre on Housing Rights and Evictions, 2009).

Pharoah (2012) provides an overview of some of the risks facing informal dwellers in Cape Town. The greatest impacts come from the health problems and losses of days worked and at school because of the disruption caused by fires.[18] In that study 83% of shack dwellers had experienced some kind of flooding while living in Cape Town.

In addition, slum dwellers have considerably less security, since their homes can easily be broken into. This could impose limits on tenants' ability to commute into the city to look for work for fear of theft.[19]

2.3 A Model of Home Production, Work and Leisure

All told, the time burdens of living in informal dwellings are considerable. In the empirical analysis I seek to evaluate the total effect of receiving housing, which affects the way in which home production happens through many channels. In what follows, I present a simple model of how changes in housing quality could influence home production, which in turn predicts increases in labour hours due to the effect of formalized housing. I do not distinguish between the different channels through which housing could improve home production, which were outlined in the previous section.

The model I use is of the lineage of Becker (1965), since it specifies utility as a function of an unobserved home production input $H(T_h, b)$, which is produced through time at home T_h in combination with the physical housing infrastructure b. Importantly, I assume that home produced goods and services are *not* perfect substitutes with other forms of consumption, as opposed to many of the other models in this literature (Gronau, 1977, for example). This fits with the way I have conceived of home production in informal settings, where the basic needs provided for by housing cannot be taken for granted.

In this model, production at home cannot be traded on the market, it is used within the household. Household utility is a function of home production, consumption and leisure $U(H, C, L)$. Consumption is given by time spent working for wage labour T_w times the prevailing wage w. Leisure, time on home production, and time at work sum to one. With prices normalized to one, household utility is given by:

$$U(H(T_h, b), wT_w, 1 - T_h - T_w,)$$

If the household maximizes utility with respect to its allocation of time between labour, leisure and work at home, the first order conditions are simple:

$$\begin{aligned} U_H \cdot H_{T_h} &= U_L \\ U_C \cdot w &= U_L \end{aligned}$$

The optimizing household would thus choose its optimal time on work at home and labour, given by $T_h^\star = T_h^\star(b, w)$ and $T_w^\star = T_w^\star(b, w)$, respectively. I want to find $\frac{dT_w^\star}{db}$: the impact of upgrading the physical housing infrastructure on wage labour supplied. While one could speculate intuitively about the direction of impact from the FOC's, total differentiation with respect

[18]Some 20% of live in high flood risk areas, and roughly 40 000 people were directly affected by townships fires in Cape Town between 1995 and 2004.

[19]This threat of invasion seems more urgent than that of expropriation risk (Field, 2007). While security of tenure is a great issue for informal dwellers in South Africa (Royston, 2002) this risk is related more to formal eviction to make way for new housing or urban development projects, rather than contestation of property right by other private agents.

to b, gives a more complete picture, in the general case. With some manipulation this eventually yields:

$$\frac{dT_w^\star}{db}\left[U_{LL} - \frac{wU_{CC}+U_{LL}}{U_{LL}}\left(U_{LL}+U_{HH}H_T+H_{TT}U_H\right)\right] = -\left[U_{HH}H_bH_T + H_{Tb}U_H\right]$$

$$\frac{dT_w^\star}{db} = \frac{\left[U_{HH}H_bH_t + H_{Tb}U_H\right]}{wU_{CC}+\left(\frac{U_{CC}w+U_{LL}}{U_{LL}}\right)\left(U_{HH}H_T+H_{TT}U_H\right)}$$

Assuming diminishing marginal utility for all inputs into the utility function, and a diminishing marginal product of time at home, renders the denominator unambiguously negative. Turning to the numerator, the first term is clearly negative due to the diminishing marginal returns on home production and the positive returns to housing quality from time spent at home. The sign of the second term hinges on whether or not the marginal utility of time in the home increases or decreases with an improvement in housing quality. If $H_{Tb} = \frac{\partial^2 H}{\partial T \partial b} \leq 0$ the numerator would be negative, and the response of hours in the labour market would be unambiguously positive.

The sign of the H_{Tb} reflects the extent to which the returns to time spent on activities in the home increase or decrease as the home technology improves. In an setting where home production leads to income through the production of goods sold on the market, one might expect a positive sign for H_{Tb}: b acts as production technology that allows households to increase output by working at home more.

I would argue that under my definition of home production, H_{Tb} is likely to be negative in this context. Improved housing is thought to be a labour saving technology, allowing households to reach a desired level of home quality. For instance, providing a better roof and walls would reduce the value of work done on maintaining the home, because nothing really needs to be done to make the structure more secure anymore.

In a sense, the empirical results of the paper provide a test of the sign of H_{Tb}, showing that poor housing quality necessitates increased time spent at home, which could be spent more productivity somewhere else.

2.3.1 Alternative channels

I cannot rule rule out other channels that could lead to changes in household labour supply. Health could be leading to a positive impact on labour supply through the productivity of household members. There is a large literature looking at the links between health and productivity (Strauss, 1986; Strauss and Thomas, 1998). The links between housing and health are also firmly established (Pitt et al., 2006; Cattaneo et al., 2009). This channel is relevant in this setting, but I am unable to estimate the impact of housing on health using the data available. Given that the impacts of improved health are likely to accrue more to female members of households who spend the most time using the stoves and appliances that are most detrimental to health, this effect might be considered part of the full effect of informal housing on female capabilities.

In addition there could be additional effects of receiving government housing such as changes in household composition, new rental income, and household location. New household members might move into the additional space that a larger house and plot affords. These new arrivals could bring with them sources of income if they are employed, or government grants. Alternatively they could be alleviating the burden of work in the home, allowing other members of the household to seek employment. Recipients of housing could see large increases in incomes due to rental incomes- either from the shacks they have moved out of, or backyard structures constructed on their properties, which is common practice.[20] In my empirical analysis I will show that the results are not influenced by including controls for changes in household size and composition, nor are they effected by looking at per capita measures of household income and earnings. I argue that the labour supply channel fully explains the impacts on household income.

3 Data

My empirical analysis uses the CAPS panel survey of Cape Town metropolitan area, with four waves collected in 2002, 2005, 2006, and 2009.[21] The sample was randomly selected using probability proportional to size sampling and stratification by racial group using 1996 local area census data.[22]

Of the households surveyed in the first wave, roughly one third were living in informal dwellings. For the purposes of evaluating the impact of receiving a housing subsidy, it was necessary to drop all those who weren't eligible, and therefore not a valid control group. As a result I have dropped all households that were not living in an informal dwelling (or "shack" as coded in my data). I also drop the few households who report that they have already received government housing before wave one. Households who have received government housing are usually not still living in shacks, but some have moved out or lost their houses. These individuals are no longer eligible for housing and thus should not be in the sample. This leaves me with a sample of 1350 eligible households. 1097 of these are found at least once in subsequent waves.[23]

Table 1 provides an overview of my sample of houses that were living in shacks in 2002. Over a period of just 7 years nearly 40% of the sample has received a government house. I show the proportion of households that received housing for each wave of data ("Treated Here") as well as cumulative proportion that have received housing to that point. It is the latter outcome that will be used as the dependent variable in the analysis because I expect the impact of housing to be present in all periods after which it is received. More households are treated between the first and second periods (19.7%) than any other. The high proportion of households receiving housing in this data is testament to the scale of the roll out of the housing program in Cape Town.

However, it is also striking how rapidly all households improved the quality of their living and housing conditions. Some of this effect is undoubtedly due to government housing, but households that did not receive government housing managed to either improve their housing

[20] Rent and one off wealth increases from the sale of housing should not be included in measures of include, but it is possible that these were incorrectly reported by households.

[21] The Cape Area Panel Study Waves 1-2-3 were collected between 2002 and 2005 by the University of Cape Town and the University of Michigan, with funding provided by the US National Institute for Child Health and Human Development and the Andrew W. Mellon Foundation. Wave 4 was collected in 2006 by the University of Cape Town, University of Michigan and Princeton University. Major funding for Wave 4 was provided by the National Institute on Aging through a grant to Princeton University, in addition to funding provided by NICHD through the University of Michigan (Lam et al., 2006). Further information can be found on the CAPS website at http://www.caps.uct.ac.za

[22] This survey was conducted with the primary motivation of tracking young adult's behaviour, sexual attitudes, labour force participation and health. However household questionnaires were also conducted, with an extensive household roster questionnaire which surveyed the entire household. However, this does impose some limitations on the analysis that can be conducted.

[23] Although in each specific follow up wave about 80% of households are reached on average, conditional on having been found at least once in the follow up waves.

conditions (move out of shacks) or gain access to important infrastructure and amenities. This could be due to both improved government service delivery during this time, and a natural process whereby new migrants to cities manage to improve their living conditions over time, consistent with a "modernization theory of slums" (Marx et al., 2013). Indeed many of the households in informal housing in the first wave of the panel were recent migrants to the city.

Table 1: Evolution of sample household characteristics

Wave	1	2	3	4
Year	2002	2005	2006	2009
Treated	0.0%	19.7%	25.9%	38.6%
Treated Here	0.0%	19.7%	8.87%	12.09%
Shack	100.0%	65.6%	62.4%	45.6%
Flush Toilet	70.8%	79.2%	85.6%	90.7%
Piped Water	12.3%	25.3%	28.4%	42.0%
% Female	54.0%	55.1%	54.0%	53.3%
Dist To City (km)	23.51	23.69	23.65	23.55
Head of Household Background				
Coloured	15.0%			
African	83.2%			
Moved to Cape Since 1985	56.2%			
Born Cape Town	19.0%			
Born Eastern Cape	75.1%			
Lived in backyard dwelling	10.6%			

The scale of rollout of housing, the effects of which are clear in my sample, provides a perfect setting in which to evaluate the effects of government housing on labour outcomes. My data includes information on housing conditions in each wave of the survey, and detailed information on labour market decisions of one young adult member of the household. Other labour data comes from the household rosters.

In Table 19 in the Appendix, I compare the sample mean for households that received housing to those that did not at both baseline and and endline (at baseline I look at househld that are going to receive housing). There are clear differences in observables between treated and control individuals. This differences are consistent with a story of housing allocation whereby poorer households were more likely to get housing, as discussed in Section 4. Backyarders (those living not in large informal settlements but in shacks in the yards of a more formal dwellings) seem far less likely to get housing, as are coloured households. Migrant status does not seem to make a significant difference. Importantly, we observe that households that were treated lived far further away from the city center, which is due to to fact that projects were built further away from the city, where there was more cheap available land. These differences in the characteristics of the population targeted by housing motivate many of the robustness checks discussed in Section 7.

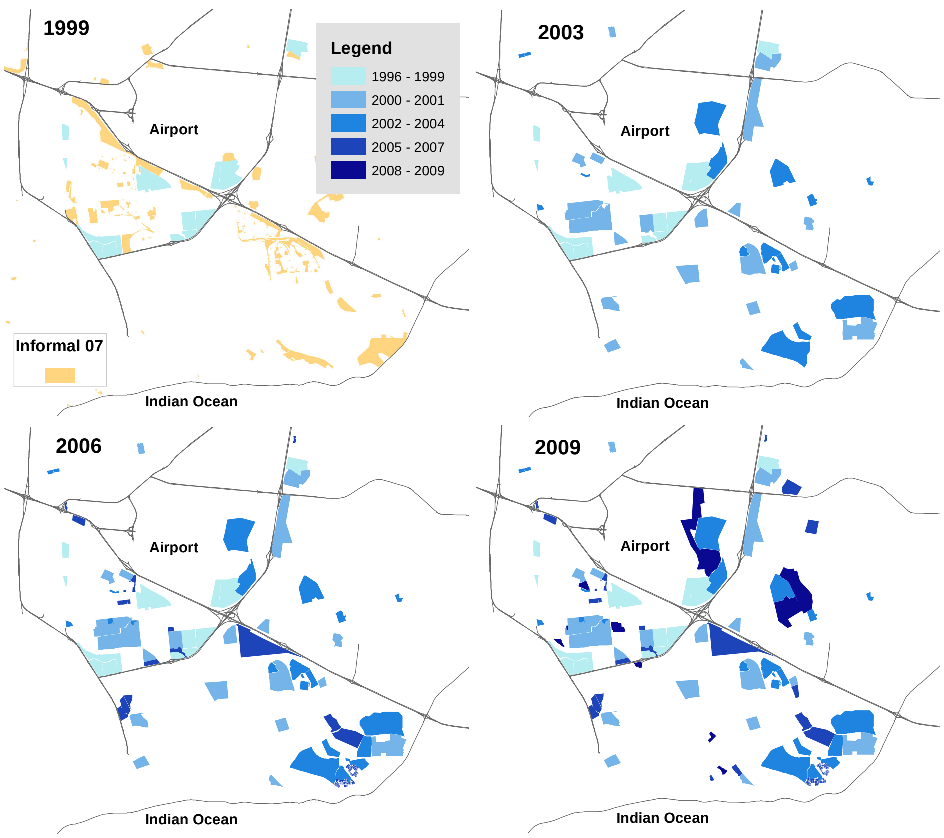
3.1 Housing Project Data

During fieldwork conducted during 2011, I gathered datasets on the rollout of government housing from the Provincial Department of Human Settlements and Local Government Planning departments in Cape Town. I built a comprehensive and accurate dataset of RDP housing roll out in Cape Town over last 15 years. I used three main sources to generate this data. The first was a database of projects that originally came from the National Housing administrative records, with geographical coordinates of the projects, along with approval status and date of approval. However I found that a great deal of the coordinates were highly inaccurate.[24] The data lists dates for when programs were proposed or approved, rather than when they were actually completed. I used project ID numbers to match this data with a second database of projects which listed more accurate dates of housing roll out, as well as a detailed breakdown of housing subsidy numbers by building date, but that lacked any geographical information.

Finally I combined this data with an invaluable geographical ArcGIS map acquired from the Cape Town City Housing Department, which provided polygons outlining the location of housing projects.[25] By linking the three datasets together I was able to generate a georeferenced panel of the number of households built per project in each year.

This data is presented in Figure 1 showing the expansion of housing projects over the years from 1999 to 2009 for areas in Cape Town where housing was built. Figure 7 in the Appendix shows a broader overview of housing projects for the whole City in 2009.

Figure 1: Housing roll out in Cape Town



I aggregated this yearly data into blocks of years corresponding to the time between waves of the CAPS data to get a measure of how many houses were built, at each location, between each wave of survey data.

[24]There were housing projects placed in the ocean or on the mountain.

[25]This dataset was built by Rehana Moorad at the local government department with great accuracy. In some cases planning department construction blueprints had been used to individually identify housing units in great detail.

3.2 Location and Proximity Measures

I used confidential datasets in order to track households as they moved.[26] I used original enumeration areas maps to locate the original living location of households in the first wave of the sample, then used household addresses from survey tracking sheets to update household locations as they moved.

I used ArcGIS maps of the original EAs sampled to map the approximate locations of the households at the start of the survey. I then used household's addresses in later waves, transcribed from the survey documents, to identify households that had changed address. I then geocoded the new addresses. In this way I tracked households throughout the four waves by their GPS coordinates.[27] I was then able to generate a range of distance and geographic outcomes for each household. In each wave of data I calculated the distance from schools, roads, the city centre, and the distance of move from the original place of living at the basline (if there was any move at all).

Summary statistics of the migration data are presented in table 2, along with the housing distance data described in the next section. Roughly 30% of the sample moved at some point during the survey.[28] The average move distance is small, under 1 km.[29] This data gives an idea of how far informal households live from the city centre- 26kms on average.

Table 2: Proximity data in wave 3 (2006)

	Mean	Min	Max	N	Control	Treat	Diff
City Distance	25.8	4.05	53.3	968	24.7	27.8	3.07***
School Distance	0.48	0.019	3.43	970	0.50	0.45	-0.041
Moved	0.36	0	1	970	0.34	0.40	0.060
Move distance	0.94	0	36.8	970	0.95	0.92	-0.025
Cumulative dist moved	1.53	0	36.8	968	1.37	1.83	0.46
Distance Proj1	0.88	0	16.7	970	1.13	0.41	-0.73***
Distance Proj2	2.41	0	28.6	970	2.82	1.69	-1.13***
Distance Proj3	3.24	0.046	31.2	970	3.60	2.57	-1.04***
Rank Proj1	0.39	0	1	970	0.35	0.48	0.13***
Rank Proj 2	0.11	0	1	970	0.089	0.16	0.068***

3.2.1 Housing Project Distances

Most importantly I was able to generate distances for each household, in each wave, to all of the government housing projects on which houses had been constructed during the years since the last survey. For the reasons that become clear in the next section, I focus only on the distance between housing projects and enumeration areas (EAs) that the household was living in the first

[26] These were provided with the help of Jeremy Seekings of the Centre for Social Science Research, University of Cape Town, and David Lam of Population Studies Center, University of Michigan, after discussions in January 2011.

[27] I used Google maps for this. Their batch geoprocessing tools could not always be used because of the considerable variation in spellings of streets and areas name, especially when in different languages, or in newly developed areas were street names had not been formalized. Most of these GPS coordinates had to be found by hand.

[28] Most of these moves were within the boundaries of the City of Cape Town, but there were a few households that moved back to rural areas in the Eastern Cape or KwaZulu Nata, some hundreds of kilometers away. For the purposes of urban relocation analysis, such outliers were excluded from the sample.

[29] This may be an underestimate because households that moved further were less likely to be found, and there were sometimes mistakes with updating address data during the fieldwork.

wave.[30] I created dummy variables for EAs that were contained within housing projects, as they were most likely to be upgraded.

In addition each EA was given a rank (among all other EAs) to each project nearby, such that each household-project distance pair had a corresponding rank assigned to it. A housing ranking might not necessarily correspond closely with its distance to a project, if is located in a densely populated area where many households are competing for treatment.

Table 2 shows these measures, the average distance from the closest housing project, then the second and third closest. I also show a dummy variable for whether the household lived in the top 3 closest EAs to the housing project nearest to that household.

4 Empirical Strategy

The paper uses three key strategies for identifying the causal effect of government housing on household outcomes. Firstly I look at OLS regressions with household fixed effects to estimate the effect of receiving housing. This gives a basic estimate for the difference-in-difference treatment effect of housing. Secondly, using a natural experiment that I will explain in detail in this Section, I instrument for individual selection into treatment (receiving a house) by using proximity to government housing projects. Thirdly, I use a set of housing projects that were planned but not built in order to control for selection at the geographic level, by dropping from the control group those areas that never had projects planned nearby.

Before turning to the formal identifying specifications and assumptions, I describe the natural experiment that I exploit as part of my identification strategy. In what follows, I use 'treated' or 'treatment households' to refer to households that received government housing as a result of the policy.

4.1 Natural Experiment: allocation by proximity

This paper uses the government's proximity-based allocation policy as a natural experiment. I focus on the procedures used by the local government in Cape Town. Observations on the workings of allocation procedures came from numerous meetings and discussions with officials in the local government in early 2011.[31] Additional policy documents, reports and research papers on the methods of allocation corroborate this story (Tshangana, 2009; Seekings et al., 2010; Tissington et al., 2013).

While the official eligibility rules for housing stipulate that households must earn less than R3500 per month to be eligible for housing, this cut off seems not to be enforced in practice.[32] Once a household has (rightly or wrongly) been deemed eligible, it joins a national housing waiting list. This list is supposed to work like first-come-first-served queue, but in reality housing construction at the local level determines the order of delivery, and even within communities there is evidence that households often jump the queue (Tissington et al., 2013).

[30] Importantly, EA centroid locations, instead of their boundaries, were used to calculate distance. This only makes a noticeable difference for those EAs right next to, or inside projects. I wanted to distinguish EAs that were completely surrounded by projects from those that simply had part of their boundary overlapping with the boundary of a housing project.

[31] I refer to discussions I had with Paul Whelan (Western Cape Provincial Department of Housing), and Heinrich Lotze (Head Housing Development Co-ordinator, City of Cape Town Government).

[32] Indeed I looked for a discontinuity in the probability of receiving housing at the cut-off in baseline income. While the probability of receiving housing was definitely lower for very wealthier households, the discontinuity at the eligibility cut-off was almost non-existent and statistically insignificant. In addition, there are relatively few individuals (only 12%) in my sample of slum-dwellers who fall above that cut off: the eligibility constraint did not find for them.

As a result of the project-by-project nature of the roll out, households were selected into projects according to catchment areas around the projects. From these areas a number of "source areas" (particular informal settlements, or communities within settlements) were selected. These stakeholders were allocated a certain quota of housing units from the project (Tshangana, 2009).[33] That group of communities would establish project committees responsible for allocating housing to their members, with the one restriction (not always enforced) that all selected candidates much be on the housing waiting lists.[34]

In this way, households that were living close to housing projects that were built between 2002 and 2009 were more likely to be treated than those living further away. It is this relationship that I exploit as an identification strategy. Of course location of housing projects itself was not exogenous, making it crucial to understand how housing site locations were selected. The location of the housing projects was not generally determined by members of communities. In most cases it was not determined by the government either. The role of private developers in the housing process meant that land availability and affordability were the main forces determining construction locations. This meant that housing projects were generally developed in areas where land was relatively abundant or cheap, or in parcels of undeveloped within the city.

In this way I argue that geographic proximity to new projects was uncorrelated with *changes* in household outcomes, except through the channel of improved housing. I will return to this argument shortly. Firstly, I use a set of the set of distance-from-project measures to predict selection into treatment, as the first stage of an instrumental variables estimator, discussed in more detail below.

4.2 Identification

The basic OLS regression of household outcome y_{it} on having government housing T_{it}, including controls for household observables X_{it} is given by:

$$y_{it} = \alpha_0 + \alpha_i + \lambda_t + X_{it}\beta + T_{it}\tau + \delta_{it} + \epsilon_{it} \tag{1}$$

This estimator likely to be biased due to correlation between household unobservables α_i and the housing treatment. In order to account for household unobservables that might be driving selection into housing, as well as outcomes of interest, I estimate a fixed effects model which estimates the difference-in-difference impact of receiving government housing:

$$\begin{aligned} y_{it} - \bar{y}_i &= \lambda_t - \bar{\lambda} + (X_{it} - \bar{X}_i)\beta + (T_{it} - \bar{T}_i)\tau + (\epsilon_{it} - \bar{\epsilon}_i) \\ \widetilde{y_{it}} &= \widetilde{\lambda}_t + \widetilde{X_{it}}\beta + \widetilde{T_{it}}\tau + \widetilde{\epsilon_{it}} \end{aligned} \tag{2}$$

where $\widetilde{y_{it}}$ represents the demeaned version of the outcome of interest. The fixed effects estimates correctly identify the effect of housing under the assumption of common trends. That is, households that were treated would have had the same changes in y over time had they not been given the housing, that is: $E(\delta_{it}|T_{it} = 1) = E(\delta_{it}|T_{it} = 0)$.[35] This requires of course that treated

[33] In some cases a certain number of units would be reserved for households outside of the catchment area, usually communities that had been waiting for a houses for a particularly long time, or had been recently relocated. An example is the Joe Slovo informal settlement near Langa, which was allocated housing in the N2 Gateway Project due to a fire that affected that community.

[34] Street committees are a common characteristic of most townships in Cape Town and are often those involved in the management of the communities housing quota allocations. Committee representatives that I met in Cape Town had a list of their community members who were eligible for housing, which they used to make allocations.

[35] For a more detailed discussion of the problem of unobserved time trends in panels, and the resulting bias of

households were not effected by different trends or shocks unrelated to housing over time.

4.2.1 Sources of bias

There are a number of reasons to doubt the assumption that treatment is uncorrelated with individual time shocks. There have been widespread reports of manipulation of the housing allocation lists, with certain individuals receiving preferential treatment based on political connections or other means to access housing (even paying bribes) (Seekings et al., 2010; Tissington et al., 2013). If households who received windfalls or good new jobs were able to leverage their increased incomes to access housing, this could bias the estimates upwards.[36]

On the other hand, it may be the case that housing allocation is more pro-poor such that housing is allocated by local politicians and communities to households that have the least ability to improve their own circumstances. Alternatively, households that suffer negative income shocks might be more likely to be awarded housing. This would bias the estimates of the impact of housing downwards, as households are less likely to experience increases in their incomes are most likely to get housing. In the data used in this paper I find that housing is more likely to go to households that were poorer at baseline.

In addition, the long waiting lists for houses could cause downward selection bias. Many of households that get treated are likely to be the ones who have remained in informal dwellings the longest, making them high up the community waiting lists. Those who were able to get out of poverty and upgrade dwellings on their own are, by definition, off the waiting lists (or at leats out of my sample of eligible individuals). Thus households would be selected into treatment due to their relative inability to improve their housing on their own. Finally, measurement error could be a source of downward bias: the extent of measurement error in the sample could be substantial, especially in the measurement of incomes, and even in the treatment variable.[37]

4.2.2 Instrumental variables estimator

I deal with non-random selection into housing at the individual level through use of an instrumental variables (IV) estimator. The natural experiment outlined in the previous section allows me to use distance from housing projects as instrument for selection into housing projects. In this way I follow McKenzie and Seynabou Sakho (2010), Attanasio and Vera-Hernandez (2004) and Ravallion and Wodon (2000), who use distance from tax registration offices, community centres and schooling project, respectively, to control for selection into social programs.[38]

Call the relevant distance instrument Z_{it} to estimate a fixed effects-two stage least squares

difference-in-difference estimators see (Bertrand et al., 2004).

[36]Given the roll-out of numerous government programs at the same time as the housing project, it is possible that households that managed to get government housing, also received other benefits simultaneously, which might improve their economic outcomes

[37]Sometimes the interviewed household member might not be able to remember if the household had received the house from the government. Alternatively households might have moved out of housing after selling it or renting it out, such that they would mistakenly report not having received government housing.

[38]This fits with a larger literature of using geographic instruments. Dinkelman (2011) and Klonner and Nolen (2010) use terrain data to instrument for the placement of electrification programs and mobile phone antennas, respectively. These papers follow a methodology pioneered in Duflo and Pande (2007) to evaluate the growth impact of dams. Similarly Banerjee et al. (2012) uses distances from major roads built across China to evaluate the impact of these roads on local growth.

(FE-2SLS) estimator, given by

$$\widetilde{y_{it}} = \widetilde{\lambda}_t + \widetilde{X_{it}}\beta + \widetilde{T_{it}}\tau + \widetilde{\epsilon_{it}} \tag{3}$$

$$\widetilde{T_{it}} = \widetilde{\lambda}_t + \widetilde{X_{it}}\pi_1 + \widetilde{Z_{it}}\pi_2 + \widetilde{\epsilon_{it}} \tag{4}$$

where Equation 4 gives the first stage prediction of the probability of switching to be treated (receiving a house) from non-treated in time period t. The fitted values for $\widetilde{T_{it}}$ are then used as regressors in Equation 4.2.3. The identifying assumption (exclusion restriction) of this model is that distance from housing projects is uncorrelated with the change in the outcome of interest: $\widetilde{Z_{it}} \perp \widetilde{\delta_{it}} + \widetilde{\epsilon_{it}}$. I turn to discuss this assumption in Section 4.4.

In this framework, fixed effects estimation addresses the problem of endogenous time invariant household unobservables, while endogenous time varying "shocks" to the household are dealt with through the instrumentation.[39]

4.2.3 First stage

It is the distance from multiple housing projects that matters for the probability of receiving government housing. This presents an econometric challenge since the distance from a single (closest) housing project is not particularly informative about the probability of treatment. It is the cumulative effect of numerous housing projects, including the number of houses built in that project, over the years that predicts selection. After all, if a household was not given a house by the closest project, it may stand a good chance of winning housing in the next closest project, especially if it was moved up the waiting list after neighboring households got houses. Furthermore the number of other households in the neighbourhood of a project will also influence the probability of receiving housing for a fixed supply of new housing.

In Section A.1 in the Appendix I discuss in more detail some of the challenges arising from this issue, including a discussion of why alternative measures summarizing the total distance of households from multiple projects are problematic in terms of the parametric assumptions that they place on the relationship between distance and selection. In the robustness checks, Section 5.3 I look at the results IV estimates where I simply use a full set of distance measures linear predictors of treatment in the first stage, and show that the results are consistent with the rest of the results in the paper, but are estimated imprecisely and with a severe problem of too many weak instruments.

Instead, I need a flexible estimator to predict selection into treatment that involves multiple nearby housing projects. I follow Wooldridge (2002) by estimating the probability of treatment by a non-parametric function $G(x, z; \rho) = P(T = 1|x, z)$, which uses multiple instruments z and a common coefficient ρ determining the impact of distance on the probability of treatment. Importantly, the fitted probabilities of the probability of treatment $\widehat{G}$ cannot be used as regressors in Equation in the usual 2SLS estimator. These are unlikely to uncorrelated with the error term as they are in the linear case.[40] Such an estimator will not be consistent. In addition, inference with this method will produce incorrect standard errors because of the non-linear form of the regressors and error correction methods would need to be applied.

[39] Murtazashvili and Wooldridge (2008) present a more thorough discussion of what I have presented here. They investigate a more general version of the model I have introduced, using time varying and permanent individual slopes, and show the conditions required for this model to give consistent estimates of the 2nd stage parameters.

[40] The only condition under which such a method would yield an efficient estimator is if data generating process is perfectly specified by G. We can never really know this and is far too strong an assumption in almost any case (Angrist and Pischke, 2008).

I use an IV estimator adapted from Wooldridge (2002) and applied to the fixed effects case. Firstly I generate fitted probabilities of treatment $\widehat{G_{it}}$ for each individual in each period using a non-linear specification based on a full set of proximity instruments. I then use those predicted values as an instrument for treatment status T_{it} in the FE-SLS given by Equation 4. In other words I use a linear projection of T_{it} onto $[x, G(x, z; \hat{\rho})]$ as the first stage of a 2SLS procedure. Wooldridge (2002) refers to this as using generated instruments as opposed to generated regressors. This linear projection will not be correlated with the error term under a valid exclusion restriction. This follows intuitively from the logic of 2SLS; if the instruments Z are informative and valid, then $G(x, z; \hat{\rho})$ will be too.

Wooldridge (2002) shows that in the IV framework, we can ignore the method of estimation of ρ in the first stage. Inference in the 2SLS with $\widehat{G_{it}}$ as instruments is consistent, and no standard error corrections are required. But this non-linear form is more efficient than the linear 2SLS, and thus more likely to provide valid inference (Newey, 1990).

4.2.4 First Stage Specification

In this section I define the function that determines selection into housing $G(x, z; \rho)$ and the estimation of ρ (the set of coefficients that capture the effect of distance on receiving housing). I use maximum likelihood methods to estimate a unique binary outcomes estimator which assume a latent variable structure for the impact of each distance instrument on the probability of treatment.

Imagine a household surrounded by a number of housing projets: the aim here is to predict treatment as a joint function of distance from all of the nearby housing projects as efficiently as possible. Firstly I use a binary outcomes model to for an expression for the probability of household i being selected by a particular project a, for each project-household pair. This is not the same as actually getting housing from that project, since a household cannot receiving housing twice. I then combine the probability of being selected by each project into an expression for the joint probability of a household being selected by any housing project. I do not observe T_{ia}: that is which households received housing from which projects. I observe only T_i, the combined effect of being selected by any project.

Here I use the set of instruments dis_{ia}- the distance between the household and a project built since the last survey wave.[41] The probability of household being *selected* housing from a specific project is given by:

$$y^\star_{ia} = x_i\beta + dis_{ia}\rho \tag{5}$$

$$T_{ia} = \mathbf{1}(y^\star > 0)$$

$$T_{ia} = \mathbf{1}(x_i\beta + dis_{ia}\rho + v_i > 0) \tag{6}$$

I assume that the error term v takes on the logistic distribution $F(y^\star) = \Lambda(y^\star) = \frac{exp(y^\star)}{1+exp(y^\star)}$ such that

$$P(T_{ia} = 1) = \Lambda(x_i\beta + dis_{ia}\rho) \tag{7}$$

Imagine the case where there are only two projects, and note that a household can receive

[41] In the application to the real data we will use a more full set of instruments, all relating to the relationship between households and individual projects. These are excluded at this point, for ease of exposition.

housing from only one project. The likelihood of a household being treated by *either* project is:

$$P(T_i = 1) = P(T_{i1} = 1) + P(T_{i2} = 1) - P(T_{i1} = 1)P(T_{i2} = 1)$$

where $P(T_{ia} = 1)$ is calculated by (7). Notice the adjustment for the fact that a household cannot be treated more than once. In this framework, the expression $P(T_{ia} = 1)$, given by (7) has to be interpreted as the project specific contribution to being treated, *not* the probability of being treated by that project. For many projects, the probability is most simply expressed as complement of the probability of being selected by none of the projects:

$$P(T_i = 1) = 1 - \prod_a^A (P(T_{ia} = 0)) \tag{8}$$

$$= 1 - \prod_a^A \Lambda(-x_i\beta - dis_{ia}\rho) \tag{9}$$

This expression, when estimated, gives a single solution to the coefficient ρ, a common effect of distance for all housing projects, no matter how many different housing projects are used in the estimation. I use this model to predict the probability of being treatment for a single period, based on the housing projects that were built in that period.

The problem is complicated further by the use panel data: we'd like efficient estimates for the probability of receiving housing in each period. Households cannot receiving housing more than once, so the predicted probability of treatment should decline in a period after a household had a high predicted probability of treatment, all things equal. We want to derive an expression for the probability that a household has received housing at point in time up until the specific period. I development a functional form that conditions the probability of receiving housing in a particular time period on the probability of having received housing in previous periods.In the interests of space, this method relegated to the Appendix, Section A.2. There I also develop a multinominal estimator that predicts, the period in which a household will most likely be treated.

In addition, I present Monte Carlo simulations using simulations with calibrated parameters for ρ which gives the effect of proximity on the probability of receiving housing. I find that the estimator developed here does a good job of recovering the true parameter value for ρ, even in the presence of considerable noise and individual fixed effects. The average predicted probabilities of treatment from this model match the rates of treatment in the simulated data.

I am able to estimate the equation given by the 8 and the time (wave) specific probability of being treated using maximum likelihood techniques. It is to the results of these estimates that I now turn.

4.3 First Stage Results

I have outlined the key elements on the first stage of my instrumental variables strategy. Taken together I will estimate a system of equations taking the following form:

$$\widetilde{y_{it}} = \widetilde{\lambda_t} + \widetilde{X_{it}}\beta + \widetilde{T_{it}}\tau + \widetilde{\epsilon_{it}} \tag{10}$$

$$\widetilde{T_{it}} = \widetilde{\delta_t} + \widetilde{X_{it}}\delta_1 + \widetilde{\widehat{G_{it}}}\pi + \widetilde{v_{it}} \tag{11}$$

$$\widehat{G_{it}} = G(X_{it}, Z_{it}; \widehat{\rho}) \tag{12}$$

In this section, I show the results for the estimates of the selection equation (12). The model is estimated by maximum likelihood, where a single likelihood function describes the probability of getting housing in each period using all housing projects built during the time period. In this section show that this method of predicting selection into housing is highly informative and efficient. I also discuss other interesting predictors of selection into treatment to shed light on the way in which allocation to housing opportunities happens in practice.

Table 3 shows the estimates for the two different models outlined in the estimation section and obtained by maximum likelihood programming methods. I show two estimates: "L" denotes the use of the binary form estimator with a different likelihood function for each time period given by Equation (13). In this case the dependent variable is T_{it}- whether the household had received a government house by time period t. By contrast the multinominal "MNL" (described in detail in Equation (14) in the Appendix) denotes the model with dependent variable TD_i - indicating the period in which the household got the house (or 0 if not at all).

In the estimation I add a range of additional project-household variables to predict treatment to the basic specification . The distance between the household and the project ($\mathbf{ProjectDist_{ia}}$), a dummy variable indicating that the household was actually located within the boundary of the project ($\mathbf{Insitu_{ia}}$), and dummy variable indicating that the household's enumeration area was ranked among the three closest EAs to the project ($\mathbf{Rank_{ia}}$).[42] I also include a square term in the distance from projects, to capture non-linearities in the effect of distance.

Figure 2: Kernel density of predicted treatment for treated and untreated groups

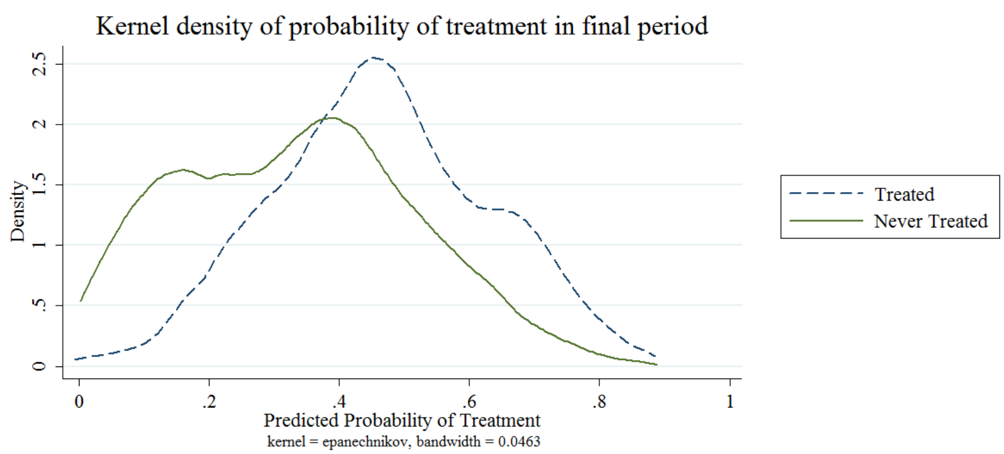


Table 3 shows large and significant impacts of distance from housing on the probability of receiving housing. Living in an area that was fulling upgraded "in situ" is also a significant predictor of treatment, as is being one of the closest three households. I discuss some marginal effects interpretation in the Appendix, Section A.2.1.

The combined effect of the distance has enormous predictive power on the probability of treatment in the data. I use the coefficients from the estimates in Table 3 to predict treatment in each time period ($\widehat{G_{it}}$). Using the coefficients from the estimation in Column 1 in Table 3, I plot the kernel density of predicted treatment by those that actually received housing, and those

[42]Such a household might have been upgraded *in situ*, which was the case for some areas in Cape Town. This dummy variable indicates that the entire enumeration area (EA) was located within a project, not just that the boundary of the EA overlapped with a project.

Table 3: Maximum likelihood estimation of treatment status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	L	L	L	L	MNL	MNL	MNL
Project Dist	-0.499***	-0.515***	-0.396***	-0.672***	-0.534***	-0.514***	-0.561***
	(0.100)	(0.105)	(0.106)	(0.142)	(0.137)	(0.135)	(0.164)
Rank	0.687***	0.735***	0.662***	0.350**	0.844***	0.897***	0.426*
	(0.126)	(0.127)	(0.130)	(0.143)	(0.203)	(0.202)	(0.245)
Project Dist sq	.0062***	.0065***	.0044***	.0079***	0.007***	0.007***	0.007***
	(.0015)	(.0015)	(.0017)	(.002)	(.245)	(.244)	(.288)
In situ	1.113***	1.149***	1.191***	1.802***	0.707***	0.761***	1.28***
	(0.176)	(0.177)	(0.186)	(.196)	(.0019)	(.0019)	(.0023)
Female Head		0.202**	0.166*	0.120		0.312**	0.269*
		(0.0895)	(0.0910)	(0.0957)		(0.136)	(0.142)
HH Size		0.0859***	0.0666***	0.0733***		0.0219	0.0178
		(0.0158)	(0.0173)	(0.0180)		(0.0272)	(0.0319)
Sex Ratio		-0.610***	-0.637***	-0.544**		-0.679**	-0.591**
		(0.197)	(0.201)	(0.212)		(0.283)	(0.295)
Age Ratio		0.153	0.205	0.0658		0.744**	0.667*
		(0.224)	(0.226)	(0.237)		(0.325)	(0.355)
City Distance			0.04***	0.05***			0.04***
			(0.00655)	(0.00699)			(0.0111)
Max Education			0.0267**	0.0319**			0.0132
			(0.0126)	(0.0134)			(0.0194)
From Cape Town				-0.515**			-0.168
				(0.239)			(0.344)
Coloured				-0.713			-0.713
				(0.643)			(0.942)
Back yard				0.0492			0.0418
				(0.183)			(0.277)
Migrant				-0.70***			-0.69***
				(0.0969)			(0.138)
Informal settlement				1.35***			0.89**
				(0.243)			(0.348)
Obs	2,694	2,654	2,648	2,648	1,074	1,074	1,074
Time	Yes	Yes	Yes	Yes	No	No	No
LL	-1430	-1390	-1364	-1284	-838.2	-829.9	-801.9

Notes: Clustered standard errors in parentheses.*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent var in "L" is a dummy for treated in current period. Dependent var in "MNL" is categorical: 0 for never treated $t = 2, 3, 4$ for treated in period.

Each coefficient on the project-household pair variables estimates the common parameter specifying the effect of that variable on the probability of being selected for a project. Project dis: the coefficient of the distance from a housing project to the household. Rank 3: dummy variable = 1 if the enumeration area of the household is among the three closest EAs to the project. Project dis sq: project distance squared. In Project: dummy variable = 1 if the enumeration area of the household was contained within a housing project that was build within the last year. Informal settlement indicates the household was contained within an area recognized as an informal settlement by the state. Age ratio is the ratio individuals under 15 to individuals over 15 living in the household.

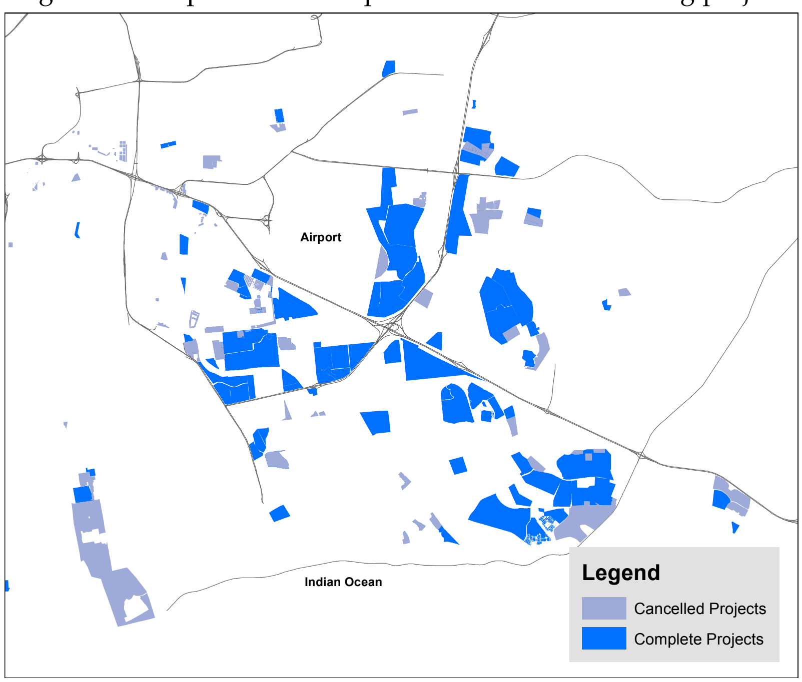
that did not, by the final wave of data from 2009. The results show a very clear right shift in the distribution for treated households.

I note a few other facts about observables that predict access to housing: female headed households are more likely to get housing, households living further away from the city are more likely to getting housing even when controlling for distance from projects, perhaps indicating that higher demand for housing closer to the city leads to housing being allocated differently.[43] Recent migrants (arrived in Cape Town in the last 5 years) are less likely to be treated, perhaps reflecting the fact that they joined the housing lists later and are there further down the waiting lists. Households living in communities classified as informal by the city government were also more likely to get housing: perhaps reflecting that formal slum recognition matters for ensuring access to services in this context.

4.4 Cancelled Projects

The identification strategy using proximity instruments attempts deals with issues of selection into treatment based on individual characteristics. It does not necessarily account for non-random selection at the geographic level.

Figure 3: Comparison of complete and cancelled housing projects

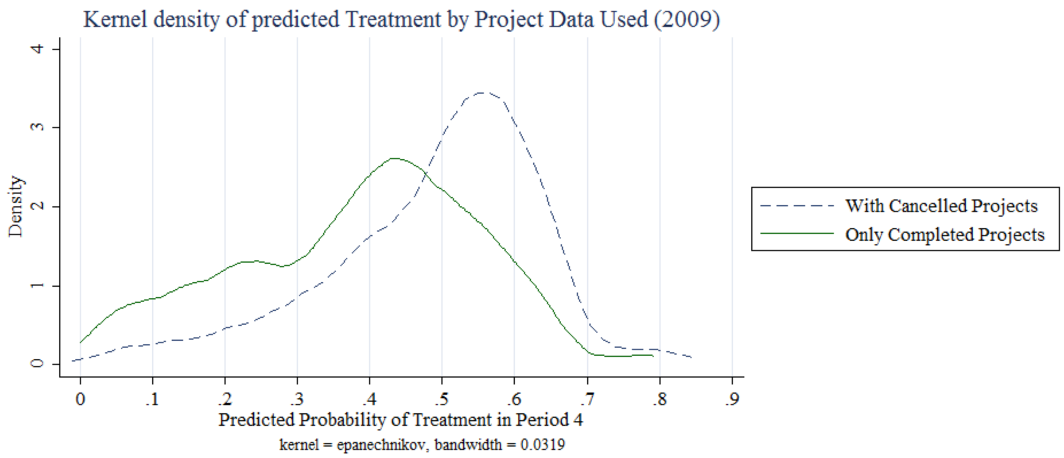


The identifying assumption of the IV strategy with fixed effects is that the chosen location of projects is not correlated with changes in households outcomes over time, except through the channel of government housing. This assumption may not hold for a number of reasons. Although all anecdotal reports suggest that communities had very little power in initiating new housing projects, which were mostly driven by the land demand needs of private construction companies, it is possible that certain connected individuals were able to lobby effectively for housing on the part of their communities. These individuals have been successful at lobbying

[43]Perhaps ineligible, wealthier households were more likely to jump the queues and get these houses.

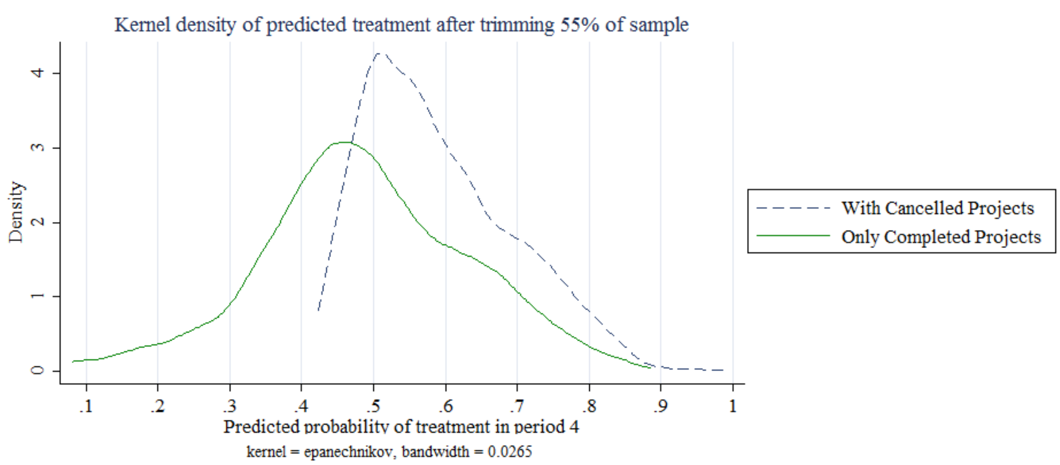
for other services or employment projects. Further, the government may have prioritized the development of certain neighborhoods or areas for political reasons, and simultaneously awarded those areas other social programs.

Figure 4: Predicted probability: comparison with and without incomplete projects



I present a set of robustness checks to show that the results are not driven by larger geographical variation in treatment: I restrict the estimation to certain areas and townships and find that the results hold within those sub-samples. Similarly I argue that the results were not driven by targeting of certain ethnic groups geographically, by restricting the analysis to certain groups in turn. I also show that receiving government housing did not lead to the receipt of other, additional government grants. Finally, I argue that housing projects did not stimulate local employment by creating construction jobs. Construction was always done by external construction companies that brought in their own permanent labour force to the sites.[44]

Figure 5: Predicted probability of treatment in the trimmed sample



Most importantly, I use a natural experiment using housing projects that were planned and

[44]In addition, my results show the biggest impact on labour supply of women, who are unlikely to have got jobs on construction sites.

approved but cancelled for bureaucratic reasons. According to discussions with City officials, this was often due to bureaucratic and budget issues, or problems with the construction companies, as opposed to something inherent in the local communities in the area.[45] Table 3 shows a map of the cancelled projects along with completed projects, which gives an idea of the variation in treatment probabilities induced by project cancellations.

The idea of using cancelled projects is to compare households that lived near planned and cancelled projects to those that lived near housing projects that were actually built, while excluding from the sample those households that lived in areas were projects were not even planned.

To do this, I generate a new dataset of distances from housing projects, *including* the projects that were cancelled.[46] This new dataset of distances, combined with coefficient estimates from Table 3, are then used to predict the probability of having been treated had *all* planned projects been completed. These predicted probabilities are the counterfactual probabilities of treatment had all housing been built.

Naturally, this new set of instruments has a smaller average distance to housing- there were a number of areas that would have been very close to housing projects had their nearby projects not been cancelled. As a result, the predicted probability of treatment (I call this the "placebo probabilities") with the completed projects is considerably higher with the cancelled projects included. Figure 4 shows the predicted probabilities of treatment with and without cancelled projects, and a clear right shift for the "placebo probabilities", and considerably less mass with probabilities less than 30%.

In order to concentrate on the differences between areas that were near cancelled projects and those near completed projects, I trim the sample by dropping those far away from both. That is, I drop individuals with a low probability of treatment with cancelled projects included. Since many individuals with a relatively high probability of treatment when cancelled projects are included have a relatively low probability of treatment with only completed projects, the distribution of the predicted treatment once the sample has been trimmed still has support over the full range of predicted probabilities. This is illustrated in Figure 5 where all households with probability of treatment less than 40% are dropped from the sample.

In the main empirical results, I will re-estimate the impacts of receiving housing using trimmed samples, by iteratively dropping quintiles of the "placebo probabilities" and estimating both the fixed effects and IV models for those restricted samples.

5 Main Results

In this section I present the main results of the impact of government housing on household outcomes.

As explained in the empirical strategy, I proceed in three steps: I estimate regular OLS models with household fixed effects, then use instruments to deal with selection on individual unobservables, and finally re-estimate both the FE and IV result with a trimmed sample that excludes areas that were far away from both cancelled and completed projects.

I start by looking at the impacts housing on log of household income. The income vari-

[45] During the time of the study, there were numerous reports of housing projects that were cancelled or put on hold because the holding companies had become bankrupt

[46] Of course, the distance instruments used until now included only distances from completed projects. I carefully verified that cancelled projects had indeed been cancelled, and that completed projects had indeed been completed, by using satellite imagery from the time.

able comes directly from the CAPS household data.[47] Throughout this section I use the label "house" to denote this coefficient of interest. Column 1 of Table shows the results from fixed effects regressions without any controls for changes in household composition, characteristics, place of living, or other sources of income shows large and significant increases in incomes for households receiving housing. This effect on total income is about 24%.

Table 4: Effects of government housing on total household income

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	FE	FE	FE	IV	IV	IV	IVbal
	lginc	lginc	lginc	lginc	lginc	lginc	lginc
house	0.245***	0.192***	0.190***	0.552**	0.617**	0.559*	0.657**
	(0.0539)	(0.0509)	(0.0512)	(0.2695)	(0.310)	(0.313)	(0.313)
femalehd		-0.188***	-0.188***		-0.172**	-0.174**	-0.144*
		(.0477)	(.0480)		(.0712)	(.0743)	(.0798)
hhsize		0.146***	0.122***		0.142***	0.120***	0.112***
		(.0085)	(.0126)		(.0137)	(.0177)	(.019)
sexratio		-0.174	-0.228*		-0.178	-0.231	-0.317*
		(0.114)	(0.122)		(0.140)	(0.144)	(0.166)
youngratio		-0.60***	-0.56***		-0.59***	-0.55***	-0.63***
		(0.0921)	(0.104)		(0.115)	(0.129)	(0.163)
femadults			0.0353			0.0360	0.0603*
			(0.0272)			(0.0325)	(0.0337)
citydis			-0.00705			-0.0128	-0.0212*
			(.0079)			(.0089)	(.0121)
maxedu			0.0209***			0.0203***	0.0145*
			(.0060)			(.0078)	(.0084)
maxage			0.00159			0.000842	0.000417
			(.0017)			(.0024)	(.0025)
govgrants			0.0679*			0.0744	0.0899
			(0.038)			(0.049)	(0.059)
Obs	3,590	3,590	3,570	3,572	3,572	3,547	2,526
R^2	0.191	0.284	0.291	0.1828	0.264	0.277	0.272
Groups	1,077	1,077	1,076	1,059	1,059	1,053	661
AvGroup	3.333	3.333	3.318	3.373	3.373	3.368	3.821
McKinnonF	.	.	.	2.62*	5.51***	4.31***	2.26***
WeakIVF	.	.	.	70.24	96.98	96.95	91.94

Notes: Clustered standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1. house=1 if household reported getting a subsidized house at any point in the past. Dependent variable is the log of total household income. The column IV-Bal is a replication of the results with a fully balanced panel- households that appear in very wave of the data. All IV regressions use a non-linear predicted probability of treatment as an instrument.

Columns 2 and 3 in Table 4 introduce controls for changes in household composition. I control for household size, changes in the sex and age (young-old) ratio in the house, as well as whether households were receiving household grants. Adding these controls reduces the size of

[47]This is the most comprehensive income variable which includes data from a one-shot total household income question, but excludes income from rent. Later I address issues due to sources of non-wage income that might be included in this measure.

the estimated effect of housing, but not significantly. The results are similar and still significant even when controlling for changes in household composition and size. This goes some way to showing that the effects are not driven by new incomes from new household members.

I then turn to the IV results. As outlined in the estimation section I use the predicted probabilities from Equation (13) and Table 3 (Column 1) in each time period as instruments in a two staged least squares estimator. The estimation results of this first stage of the 2SLS is presented in the Appendix 21.

All IV regressions report the Kleibergen-Paap Wald F statistic for weak instruments.[48] There is little reason to suspect a problem of weak instruments, given the very high Wald F-statistics on the test for insignificant instruments in the first stage, reported in the last row of each table.

I find that the IV results estimate large and significant effects of housing on household income. The coefficients are generally larger than the OLS results, which indicates either correlation between the probability of treatment and *negative* income shocks or a problem of measurement error.[49] As discussed in the conceptual framework, this could be due to individuals in worsening circumstances (like the aged or recently unemployed), or victims of recent shocks, being assigned houses by their communities, which leads to downward bias of the treatment effects of housing. We might have expected them to have done considerably worse without the treatment. Finally, under the LATE interpretation of my results (see Section 6.5.1) my IV results identify the effects for households who did not have to move far to get housing, and thus do better than households that were required to relocate.

Next, I confirm that these results are due to higher average wage earnings for members of the household. The data on household income used thus far came from a combination of questions, including a one-shot question on household income when individual earnings were missing. This may have contained sources of income not related to labour market activities. I use estimates of the sum of household income from earnings data from household rosters. However, the data is more often missing for these variables, which yields less precision for the estimates. Panel A shows the OLS fixed-effects results, Panel B the IV results. The coefficients on the impacts on household income in logs are significant and similar in magnitude to those in Table 4.

I also estimate the impact of housing on the per capita income of household members, to rule out that the effects on incomes were being driven by increases in household size. I find that receiving housing had a significant impact on the total salary earned by the household, and on the average earnings per person in the household. In Column 1 I estimate the impact on the total salary in levels. This is estimated with less precision, but estimates an average impact on household income of about R350. Average household earnings in the final wave around R2500.

5.1 Cancelled Projects

In this section I use the data on cancelled projects to deal with non-random selection of project sites. As outlined by the identification strategy, I use the predicted probability of receiving housing using the projects that were cancelled (I call this the "placebo probability") in order to drop

[48] This is the analogue of Cragg-Donald Wald F statistic, under the assumption of heteroskedastic or serially correlated errors. Note that in the single endogenous regressor case, such as this one, this statistic is equivalent to standard Wald test on the first stage coefficient on the single instrument (Baum and Schaffer, 2007).

[49] I report post estimation tests of endogeneity, which, in the fixed effect IV setting is the Davidson-MacKinnon F statistic. This test rejects the null that the IV estimates are the same as the OLS model. See Davidson and MacKinnon (1993). For the execution of the Fixed Effects Instrumental Variable model with clustered standard errors and 2-Step GMM I use the stata command xtivreg2, and the *dmexogxt* post estimation command for exogeneity test of OLS vs XT-IV estimation (Baum and Stillman, 1999).

Table 5: FE and IV Impacts on different earnings measures

	(1)	(2)	(3)	(4)
		log	log	log
	total salary	total salary	income/person	head's salary
Panel A: OLS with Fixed Effects Impacts on Different Income Measures				
house	348.8*	0.242***	0.199***	0.137**
	(195.5)	(0.0649)	(0.0717)	(0.0664)
Observations	2,273	1,837	2,438	1,159
R-squared	0.171	0.158	0.236	0.197
Number of hhs	574	574	636	416
Av Group Size	3.960	3.200	3.833	2.786
Panel B: Instrumental Variables Impacts on Different Income Measures				
house	568.0	0.520*	0.721**	0.547*
	(801.6)	(0.304)	(0.344)	(0.312)
Observations	2,518	1,837	2,438	1,159
R-squared	0.099	0.117	0.185	0.157
Number of hhs	637	574	636	416
Av Group Size	3.953	3.200	3.833	2.786
Weak IV F	79.21	63.30	75.94	44.08

Notes: Clustered standard errors in parentheses. All IV regressions use a non-linear predicted probability of treatment. house=1 if household reported getting a subsidized house at any point in the past. Dependent Variable is the log of total household income from wage earnings *** p<0.01, ** p<0.05, * p<0.1

areas that were never considered for housing projects. Since these areas might be systematically different from areas in which housing was planned.

I trim the sample by dropping different quintiles of the "placebo probability". So I start by dropping the 20% of household least likely to be treated, then 40%, and so on. The remaining variation in the probability of receiving housing under the true predicted probability of treatment (excluded cancelled projects) is then driven by the projects that were built versus those that weren't built. The more of the sample that is trimmed, the more of the remaining variation is due to project cancellations alone (although the sample sizes get considerably smaller).

I find that the results are robust to the trimming the sample in this way. Table 6 shows the impacts on total household earnings for the usual FE (Panel A) and IV (Panel B) estimates. The coefficients are similar to those in Table 4 and significant, and are stable as I gradually drop more of the sample. Only when I have trimmed a whole 80% of the sample (Column 4) are the impacts no longer significant. The estimated coefficients are slightly smaller, but still large and positive. I show that these results hold for the FE estimates for total household salaries in Table 7.[50]

These results provide evidence that the the results in this paper are not driven by non-random

[50]The IV estimates for these measures quickly become imprecise, with many missing outcomes and the trimmed sample reducing the sample size further. The results are not presented here.

Table 6: Impacts on total income with trimming using cancelled projects

	(1)	(2)	(3)	(4)
Sample Trimmed %	**20**	**40**	**60**	**80**
Panel A: OLS with Fixed Effects Impacts on Log Total Income				
house	0.163**	0.203***	0.163**	0.116
	(0.0650)	(0.0750)	(0.0793)	(0.0799)
Observations	2,874	2,219	1,507	743
R-squared	0.272	0.271	0.328	0.486
Households	831	625	417	206
Panel B: Instrumental Variables Impacts on Log Total Income				
house	0.886**	1.313**	1.409**	0.639
	(0.422)	(0.571)	(0.659)	(0.544)
Observations	2,880	2,224	1,510	745
R-squared	0.209	0.123	0.108	0.420
Households	829	623	416	206
Av Group Size	3.474	3.570	3.630	3.617
Weak IV F	53.35	37.73	14.92	15.83

Notes: Clustered standard errors in parentheses. All IV regressions use a non-linear predicted probability of treatment. house=1 if household reported getting a subsidized house at any point in the past. Dependent Variable lginc is the log of total household income from all sources *** $p<0.01$, ** $p<0.05$, * $p<0.1$

project site choices, under the assumption that projects were cancelled were not cancelled for reasons related to the outcomes of households in those areas. So particularly well organized or motivated communities that were able to work hard to get their projects completed, these communities might have also have been able to improve their communities in other ways, and get jobs for those nearby. However, discussions with City officials and urban planners suggested that the reasons for project cancellations were rarely to do with communities living there. They were more likely to be related to changes in budgetary issues, or disputes with developers and contractors.

5.2 Restricting to Treated Areas Only

The fixed effects regression test whether treated households did better than untreated households, while the IV results test whether households living closer to projects did better than those living further away. The trimming with cancelled projects tests whether households living near completed projects did better than households living near cancelled projects. The skeptical reader may still be unconvinced that the project placement or cancellation decisions were endogenous, so that the effects of housing may be due to differences in the growth rates of certain *areas* of the city.

I test whether treated households living in areas that were close to housing projects, and where *most* of the households were treated, and compare the outcomes of households that were

Table 7: Impacts on log total salaries with trimming using cancelled projects

	(1)	(2)	(3)	(4)
	FE	FE	FE	FE
Sample Trimmed %	**20**	**40**	**60**	**80**
house	0.189**	0.210**	0.191**	0.0751
	(0.0741)	(0.0797)	(0.0867)	(0.0910)
Observations	2,167	1,720	1,171	590
R-squared	0.130	0.158	0.199	0.352
Number of personid	719	557	373	184
Av Group Size	3.014	3.088	3.139	3.207

Notes: Clustered standard errors in parentheses.*** $p<0.01$, ** $p<0.05$, * $p<0.1$. house=1 if household reported getting a subsidized house at any point in the past. Dependent var is the log of the sum of all monthly salaries earned by members of the household. All regressions include controls for time varyings household characteristics and household fixed effects.

treated to those that were not. I look at clusters (primary enumeration areas) where more than 20% of households received a government house over the period of 4 years (this is exactly half of all clusters, 45% of clusters had no-one receiving housing). The results are presented in the Appendix, Table 24. I find that the results are very similar to the results presented on total household incomes and salaries. Note that of course I only present the FE estimates here, the aim is to compare households within clusters, and the distance instruments do vary within clusters.[51]

This should rule out a story under which certain high growth neighborhoods and areas saw particularly high growth, and were also targeted for housing project investments at the same time. I return to further robustness checks of this kind in Section 7.

5.3 Linear IV Results

The results presented thus far have used the predicted probabilities of treatment from my maximum likelihood estimator as instruments in a linear 2SLS estimator. This strategy was motivated by the need to more efficiently predict the probability of treatment. In this section I motivate this strategy further by showing the results for a simpler 2SLS estimator. I use multiple project-distance and project-rank measures directly in the first-stage of the IV estimator. I show that the estimate treatment effects are of similar magnitudes to those when the more complicated first stage is used. This is reassuring and fits with the intuition of the (Wooldridge, 2002) that my results are not driven by non-linearities in the functional form of the first stage.

These results are presented in the Appendix, Table 23. For instruments I use distance to the closest 5 projects, the ranking of household among the first 3 closest projects, and whether or not the EA was inside a project as instruments (2SLSa) (Columns 1-3). In the second specification (2SLSb) I use squared project distance instruments as well (Columns 4-6). I show results for some of the main specifications in the paper and find similar coefficient estimates. However, I show that inference is not valid given precision of the first stage estimators. The F-Kleibergen-

[51]The results are also robust to the specification of cluster fixed effects, instead of individual fixed effects.

Paap F statistics for weak instruments are very low: we cannot reject the null of no effect of the instruments on treatment in the first stage. This problem is improved by the inclusion of the distance squared as a regressor, but Stock Yogo maximal bias critical values (Stock and Yogo, 2005) still show that we cannot reject the null of up to 20% bias in IV results. Further, I have estimated the results using limited information maximum likelihood (LIML) estimation techniques in which standard errors are correct in the presence of weak instruments: these estimators inflate the standard errors such that the effects are no longer significant. These results justify the use of the more precise first stage estimator.[52]

6 Mechanisms

In this section I look for mechanisms through which households who received government housing were able to increase their household incomes. I have documented that these increases in household income were due to increases in the total wage earnings of individuals in the household.

In this section, I argue that housing enables household members to leave the home and go out in search of (more) work, because it alleviates the usual burdens of home production associated with living in informal housing. I document three main facts about labour outcomes in treated households. I find that female members of the households are more likely to be employed after receiving government housing, and that the same results do not apply to male household members. I find clear evidence that the household earnings from females *and* males increased significantly after receiving government housing. For young adult members of the household, for whom I have more detailed labour data, I find significant impacts of hours worked.

I then look for mechanisms through which labour supply might be restricted by poor housing conditions. I find that receiving government housing significantly increases feeling of safety. Receiving housing seems to have moved households further away from the city centre and jobs. However, these effects are small, compared to anecdotal accounts of how far houses do move in Cape Town. I also find that improved housing is associated with a number of measures of labour saving technologies in the home. Finally, I discuss other mechanisms through which housing might be driving the results, and some limitations of the data to identify these.[53]

6.1 Labour Supply

I estimate the effect of government housing on the total number and proportion of women in the household working in last 7 days before the survey. The results show a significant increase in female labour supply in households that received government housing. The effects are there in the fixed effects and the IV regressions.

The impact on the proportion of females employed in the household is not significant in the fixed effects regression. I find that this is because the treatment effects do not seem to be present in the early waves of the data- waves 2 and 3. The effects are large and significant however, for the last wave of the survey. This could be because the effects of moving into new housing take a

[52]My non-linear estiamte is considerable more efficient than all other linear first stage estimators, even when the distance measures are aggregated into a single composite measaure, which improves over the efficiency of the basic 2SLS presented here, but not by much.

[53]The data places certain limitations on the mechanisms that can be explored. The CAPS data used in this data focused on the young adult members of the household, and most of the detailed questions about labour supply and employment is recorded only for those individuals. Similarly outcomes related to health and small scale enterprises are not well measured in the data.

Table 8: Impacts on male and female labour supply at the extensive margin

	(1) FE num employed	(2) FE % employed	(3) FD % employed	(4) IV num employed	(5) IV % employed
Panel A: Employment among Female Household Members					
subhere	0.102*	0.0285	0.0842*	0.488**	0.325**
	(0.0532)	(0.0291)	(0.0451)	(0.237)	(0.144)
Observations	2,471	2,471	1,237	2,459	2,458
R-squared	0.032	0.007	0.017	0.087	-0.017
Number of hhs	658	658	654	646	646
Av Group	3.755	3.755	1.891	3.807	3.805
HH Controls	Yes	Yes	Yes	Yes	Yes
Weak IV F				96.03	86.43
Panel B: Employment among Male Household Members					
subhere	0.00256	-0.0200	-0.0497	0.0348	-0.0846
	(0.0502)	(0.0365)	(0.0584)	(0.234)	(0.171)
Observations	2,471	1,906	947	2,459	1,836
R-squared	0.023	0.014	0.013	0.191	0.059
Number of personid	658	609	589	646	539
Av Group	3.755	3.130	1.608	3.807	3.406
HH Controls	Yes	Yes	Yes	Yes	Yes
Weak IV F				96.03	40.89

Notes: Clustered standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1. house=1 if household reported getting a subsidized house at any point in the past. All regressions include controls for time-varying household characteristics. Dependent variable "num employed" is the number of men/women employed in the household (regressions include controls for the number of women/men in the household). % employed is the proportion of women/men of household members who are employed.

while to have an effect for female household members- the adjustment to living in a new home could delay the benefits of that housing. As a result I look at the fixed effects regressions with just the first and forth wave of the data, as a two period first difference estimate.

These results are presented in Table 8, Panel A. The fixed effects results show that households increase both the number of women working in the household (controlling for the number of women living there), and the proportion of women in that household working, by between 8% and 10%. Panel B then looks at the same set of results for men and finds no significant effects.[54] There is no impact of housing labour supply of male members of the household. Men were more likely to be employed than females, by 58% to 44% among adults in the sample.

I look for impacts on employment rates among young adult members of the household. The impacts are small, positive, but not significant in the FE, although significant in the IV results (Appendix, Table 22). However, I have data on hours worked per day by young adults in the sample (this data is not available for all household members). I find that the receiving housing

[54]The coefficients in these regressions are negative, perhaps suggesting some substitution away from labour for men, after women increase their labour supply, but these results are not significant and should not be over-interpreted.

Table 9: Effect of government housing on hours worked per day (young adults)

	(1) FE	(2) FE	(3) IV	(4) IV	(5) IV-Bal
house	0.642**	0.599**	2.289*	2.482*	2.717*
	(0.252)	(0.256)	(1.346)	(1.286)	(1.480)
HH Chars	No	Yes	No	Yes	Yes
Obs	1,630	1,605	1,295	1,293	969
Groups	821	813	502	501	356
Av Group	1.985	1.974	2.580	2.581	2.722
Weak IV F			17.20	21.38	9.232

Notes: Clustered standard errors in parentheses.*** $p<0.01$, ** $p<0.05$, * $p<0.1$. house=1 if household reported getting a subsidized house at any point in the past. All regressions include controls for time-varying household characteristics. Dependent variable is the number of hours worked for young adults in the household, conditional on having done some work. The column IV-Bal is a replication of the results with a fully balanced panel-households that appear in very wave of the data.

increased the number of hours worked in wage labour, in both the OLS fixed effects regressions, and the IV regressions. This suggests that young adults increase their labour supply at the intensive margin since work hours are only observed for those working. The results suggest that young adults worked more than an additional half an hour per day on average, over a mean of about 8 hours a day among those that work.[55]

6.2 Female and Male Earnings

Table 10 presents the main results on the impacts of housing on household male and female incomes. I look at two outcomes: in Columns 1 and 2, the impact on the sum of all male and female salaries earned by household members. In columns 3 and 4 and the average earners *per earner* for males and females. This allows me to capture the full effect of increased earnings in the household, including the effect of increased employment (the extensive margin) but also for increased earnings conditional on being employed.

The results show that receiving government housing seems to have increased earnings among both male and female members of households. The results are consistent with a story where females were more likely to be working, but do not seem to be earning more, conditional on earning (the coefficient is large but not significant).

For incomes earned by male household the fixed effect results suggest that earnings by male members increased, but that the impact is driven solely by increases in the average salaries of male household members, conditional having work (Panel B, Column 3 in Table 10). This finding is consistent with the result that government housing had no significant impact on the probability of employment among male household members. However, the IV results are less clear. The findings of an impact on male earnings are not robust to the use of the instrumental variables. The estimated treatment effects are smaller, and not significant, but still positive in the same direction as the fixed effects results.

[55]These results are robust to using log-hours worked, with an estimated impact of about a 10% increase.

Table 10: Effect of government housing on earnings of female & male household members

	(1)	(2)	(3)	(4)
	FE	IV	FE	IV
	sum earnings	sum earnings	average earnings	average earnings
Panel A: Impacts on Earnings of Female Household Members				
house	0.200**	0.832*	0.148	0.900*
	(0.0899)	(0.500)	(0.0940)	(0.470)
Observations	1,164	1,164	1,139	1,133
R-squared	0.230	0.087	0.115	0.027
Number of hhs	412	412	412	406
Av Group Size	2.825	2.825	2.765	2.791
Weak IV F		36.00		34.05
Panel B: Impacts on Earnings of Male Household Members				
house	0.245***	0.0554	0.255***	0.113
	(0.0888)	(0.308)	(0.0912)	(0.254)
Observations	1,114	1,113	1,098	1,090
R-squared	0.108	0.103	0.096	0.100
Number of hhs	398	398	398	391
Av Group Size	2.799	2.796	2.759	2.788
Weak IV F		35.96		34.21

Notes: Clustered standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1. house=1 if household reported getting a subsidized house at any point in the past. All regressions include controls for time-varying household characteristics. Dependent variable "sum earnings" gives the total earnings brought into the household by females and males, in Panels A and B, respectively. Dependent variable "average earnings" gives the average wage income earned (conditional on earning) by female and male household males of the household, in Panels A and B, respectively.

6.3 Home Production

The theoretical framework in Section 2.2 postulated a link between female supply and the constraints imposed by work at home due to living in informal housing. In this section, I show that receiving government housing lead to significant improvements in housing quality and access to labour saving technology.

Table 11 presents fixed effects estimates of treatment on physical housing conditions. Unsurprisingly, government housing reduces the probability that households are living in a shack. Treated households are significantly more likely to own a stove, a fridge and a microwave. This is possibly because housing provides more space and security to keep such an appliance, or because access to electricity is more readily available. I find no significant impact on the probability that the household has *any* access to electricity. But housing does significant increase the probability of having access to piped water, and in particular piped water in the home. All of these are technologies that could provide significant time savings for women working in the home.

Government housing reduces the probability that households use paraffin- a form a fuel commonly used in informal settlements in South Africa. Many of the devastating fires that occur in townships in South Africa are attributed to the use of paraffin. There is a negative effect on the occurrence of fires in the home (although this coefficient is not significant, it was only measured once in the follow survey rounds) which could be driven by the reduction in paraffin use.

Table 11: Effects of government housing on living conditions in the home

	(1)	(2)	(3)	(4)	(5)
	FE	FE	FE	FE	FE
Panel A: Impact on Household Access to Services					
	Shack	Electricity	Toilet	Piped	Piped In
subhere	-0.579***	0.000640	0.00995	0.102***	0.242***
	(0.0206)	(0.0208)	(0.0210)	(0.0206)	(0.0219)
Observations	3,750	3,789	3,789	3,789	3,789
R-squared	0.480	0.041	0.077	0.028	0.157
Number of personid	1,095	1,095	1,095	1,095	1,095
Panel B: Impact on Ownershipo of Household Appliances and Fuel Use					
	Stove	Fridge	Microwave	Paraffin	Fire
subhere	0.0445*	0.0908***	0.0575**	-0.0439*	-0.0171
	(0.0267)	(0.0259)	(0.0250)	(0.0237)	(0.0193)
Observations	3,749	3,749	3,748	2,942	2,949
R-squared	0.149	0.064	0.158	0.002	0.036
Number of hhs	1,095	1,095	1,095	1,095	1,095

Notes: Clustered standard errors in parentheses.*** $p<0.01$, ** $p<0.05$, * $p<0.1$. house=1 if household reported getting a subsidized house at any point in the past. All regressions include controls for time-varying household characteristics. Dependent variables are dummy variables if the household has: Panel A, Col (1) an informal dwelling, Col (2) access to Electricity, Col (3) access to a flushing toiler, Col (4) access to piped water inside the house or nearby, Col (5) access to piped water in the home. Panel B, Col (1) a stove, col (2) a refrigerator, Col (3) a microwave , Col (4) used a paraffin stove for cooking or heating, Col (5) experienced a fire in the home.

6.4 Safety and Security

One mechanism that could be driving the impacts on increased female labour participation and earnings could be the improved security that good quality housing gives to households. This could allow them to leave the home to take up employment or go in search of work without fear of burglary in their absence. This is related to the hypothesis of Field (2005) who argues households increase their labour supply when security of tenure is improved and they less likely to fear expropriation when they are out of the home.

While expropriation risk may be at play in this setting, de facto tenure security is already thought to be very good in South African informal settlements. On the other hand security threats from burglary and other crime around the home are far more salient threats in this environment. If this is part of the mechanism driving these results, we should see some evidence that adequate housing has positive impacts on feelings of safety around the home. I provide evidence of this in this in Table 12. I find that households that received government housing were far less likely to report that they felt unsafe in their homes at night.

Table 12: Effects of government housing on feelings of being unsafe at home at night

	(1)	(2)	(3)	(4)	(5)
	FE	FE	IV	IV	IVBal
	unsafe	unsafe	unsafe	unsafe	unsafe
house	-0.325***	-0.134**	-1.711**	-1.169*	-0.714*
	(0.0548)	(0.0643)	(0.818)	(0.655)	(0.411)
HH Chars	No	Yes	No	Yes	Yes
Observations	1,408	1,376	1,352	1,344	816
R^2	0.039	0.103	-0.631	-0.210	-0.032
Av Group Size	2	1.955	2	2	2
Weak IV F			8.723	11.47	23.92

Notes: Clustered standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1. house=1 if household reported getting a subsidized house at any point in the past. All regressions include controls for time-varying household characteristics. Dependent variable is a dummy variable = 1 if the household reports feeling unsafe at night in the home.

6.5 Distance and Travel

Government housing in South Africa has been criticised for reinforcing the spatial patterns of segregated living within South Africa cities (Bundy, 2014). Segregation leaves households living far away from jobs and employment opportunities, which is argued to play a causal role in the high rates of urban unemployment for black South Africans (Banerjee et al., 2007; Rospabe and Selod, 2006). In fact government housing is often thought to have moved household *further* away from the original place of living, as new housing projects are built increasingly far away.

Table 13: Effect of government housing on distance from the city center (in kms)

	(1)	(2)	(3)	(4)	(5)	(6)
					Just Movers	
	FE	FE	FE	IV	FE	IV
	citydis	citydis	citydis	citydis	citydis	citydis
house	0.508**	0.540**	0.537**	0.0416	1.441**	0.917
	(0.221)	(0.228)	(0.229)	(0.525)	(0.587)	(2.205)
Obs	3,765	3,725	3,717	3,708	1,243	1,243
R^2	0.008	0.010	0.011	0.005	0.028	0.026
Households	1,077	1,077	1,077	1,068	362	362

Notes: Clustered standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1. house=1 if household reported getting a subsidized house at any point in the past. All regressions include controls for time-varying household characteristics. Dependent variable citydis is the distance of the household from the city center in kms.

I confirm that government housing has moved households slightly further away from the city, on average. Beneficiary households have not had their situation improved with regards to distance from the city. Households that receive the government houses move on average 600m further away from the city during the course of the survey.

Further, this estimate is likely to be an underestimate of the impacts on distance, since for some households I was not able to geocode their new location, or their location was not updated by enumerators in the data files. I restrict the sample to households for which a move is recorded. Here I my point estimates indicate that treated households moved nearly 1.5kms further away from the city than untreated households. Given that the average distance from the city is about 22kms in this sample, this is not a huge difference in relative terms. In this sense the IV results isolate the effects of improved housing without the usual large effect on displacement that comes with large housing projects of this kind. Government housing had a positive impact on household labour supply and earnings, in spite of whatever effect housing did have on distance.

6.5.1 LATE and distance

The results discussed in this paper deserve one important caveat, related to this issue of distance discussed above. The impact of housing estimated by instrumental variables should be interpreted as a local average treatment effect (LATE) (Angrist and Imbens, 1994). The instruments identify the effect of treatment on those household that received housing because of their proximity to housing. These are known as "compliers" in the framework of Angrist and Imbens (1994): in the potential outcomes framework they receive government housing if and only if a housing project is constructed nearby.

The average treatment effect (ATE) is identified by the LATE under the assumption of homogenous treatment effects (Heckman, 1990). If households that were selected to receive housing because of their proximity to housing respond differently to treatment than those who receive housing for different reasons, then this assumption is violated.

This group of compliers is likely to differ from other housing recipients in at least one way. They are less likely to have moved a significant distance from their original place of living when receiving housing, precisely because the housing to which they were assigned was close by to their original place of living. Not all individuals were assigned housing because of their proximity to housing. A significant number of individuals were able to access housing far away from their place of living. Among households with predicted probability of receiving housing $\widehat{G}$ above the median, 47% received government housing. Among those below the median of value of $\widehat{G}$, 24.3% received housing.

In this way, the estimated impact on distance moved may not fully capture the way in which households were relocated further away from the city. If individuals who moved further away from the city centre were likely to suffer worse outcomes because of their distance from jobs and communities, this could lead to the LATE estimator being an over-estimate of the average treatment effect. This might explain some of the difference between the OLS fixed-effects estimates and the IV results presented in this paper.

7 Robustness Checks

In this section I show that the main results on household income, female labour supply and household wage earnings, are robust to a variety of additional robustness checks.

I focus on the validity of the instrumental variables strategy. The validity of the instrumental variables results would be undermined by challenges to the exclusion restriction: that is that the location of households in relation to new housing projects is not correlated with the outcomes of interest except through the channel of government housing.

This assumption could be violated if project placement was driven by community organizing. I have tried to argue that housing location decisions were more commonly made at levels above that of the communities. Secondly, I have used the data on planned and cancelled projects to show that it is the completion of projects, and not just the choice of location, that is driving the results. Trimming had no effect on the results.

Here I check for robustness by checking that the results are not driven by ambitious households moving closer to housing projects in order to gain access to housing. Secondly, to mitigate measurement error in the timing of housing improvements, I estimate a two period first difference model with just the first and last periods. To ensure that the results are not being driven by politicians targeting new housing projects to high growth areas or areas where other urban initiatives were being rolled out, I restrict the analysis to specific areas of the city, and to specific ethnic groups. Finally, I check for impacts of housing on sources of income other than wage earnings.

7.1 Opportunistic Relocation

The exclusion restriction would be violated if households were able to move home in order to access housing. Given that the allocation procedures are well known, highly motivated and mobile households could be able to move closer to projects that were going ahead before allocation took place, in order to get a house. This would mean that more motivated households would also be more likely to be living closer to housing projects.

In practice, this sort of activity is unlikely to have occurred. Most households that receive government housing have either been on the waiting lists in the local areas in which they receive housing for many years before they are given this housing. It is unlikely that households would be able jump the queue after moving to an area so recently. Also all instruments have used distance from housing in the *first* wave of the data. Thus if households moved during the period 2002 to 2009 in order to access new housing projects, this change in proximity would not be reflected in the instruments.

Still, I want to rule out the possibility that the effects were driven by households who had just moved into new areas in 2002 (the first wave). I drop all households from my sample there were already treated by wave 2 (2005). This means that everyone who was treated before 2005 is dropped from the sample. I then replicate the main results presented in the paper, with the instrumental variables and full set of controls, with only households that were treated after 2006. I find that almost all of the results presented above remain robust to this check.[56] These results are presented in Table 14, Panel A shows the fixed effects, Panel B the IV estimates. Thus if the results are driven by successful households relocation decisions, it would have to have been that they moved to their 2002 location in order to pursue a house that they would only get after 2006. While housing projects were often subject to delays, it would be unusual if they took longer than 5 years to build.

[56] Although standard errors are larger since so many households were treated between 2002 and 2005.

Table 14: Replication of the key results without households treated before 2005

	(1) total income	(2) total salary	(3) female salaries	(4) male salaries	(5) female % employed	(6) male % employed
Panel A: Fixed Effects Regressions (Multiple Outcomes)						
house	0.268***	0.321***	0.341***	0.202*	0.0718*	0.0335
	(0.101)	(0.103)	(0.125)	(0.120)	(0.0379)	(0.0627)
Observations	1,900	1,467	966	918	1,844	1,407
R-squared	0.256	0.127	0.172	0.114	0.036	0.045
Number of hhs	498	489	415	385	494	454
Av Group Size	3.815	3	2.328	2.384	3.733	3.099
Panel A: Instrumental Variables Regressions (Multiple Outcomes)						
house	1.401**	1.982**	1.699	-0.154	0.472*	0.298
	(0.698)	(1.004)	(1.127)	(0.978)	(0.260)	(0.310)
Observations	1,900	1,429	858	828	1,835	1,349
R-squared	0.158	-0.120	0.016	0.097	-0.046	0.014
Number of hhs	498	451	307	295	484	396
Av Group Size	3.815	3.169	2.795	2.807	3.791	3.407
Weak IV F	27.57	19.95	13.75	6.726	31.46	23.61

Notes: Clustered standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1. house=1 if household reported getting a subsidized house at any point in the past. All regressions include controls for time-varying household characteristics. Dependent variable in Columns (1)-(4) are the log of total income, salaries of household members, salaries of female household members and salaries of male household members, respectively. Dependent variable "% employed" gives the proportion of female and male household members currently employed, in columns (5) and (6) respectively.

7.2 A two period Diff-in-Diff

One concern for my identification strategy is that the instruments used are good at predicting whether or not a household will be treated in any period, but aren't as accurate at predicting *when* a household will be treated. This is usually because of inaccuracy of data on when housing projects were completed.[57] Thus, using changes in the predicted probability of treatment in the interim periods might be misleading and reduce the efficiency of estimates. To control for this I restrict the sample to just the first and last periods, and compare the changes in key dependent variables using predicted probabilities of ever being treated in any wave. These results are presented in table 15 and consistent with the results presented thus far, if a little larger after these issues of measurement error are dealt with.

7.3 Impacts within subpopulations

In this section I show that the main results are present *within* areas and sub-communities, rather than driven by differences in treatment between these sub-populations. This reinforces the ev-

[57]Sometimes the data indicates the completion date of the project, but not the date of when households in the area were actually able to move in.

Table 15: Replication of the key results with a 2-Period first difference estimator

	(1) total income	(2) total salary	(3) female salaries	(4) male salaries	(5) female % employed	(6) male % employed
Panel A: Fixed Effects OLS Regressions (Multiple Outcomes)						
house	0.285***	0.265**	0.218	0.285	0.0738*	-0.0380
	(0.0919)	(0.133)	(0.187)	(0.182)	(0.0433)	(0.0664)
Observations	1,393	837	404	376	1,360	847
R-squared	0.455	0.266	0.473	0.209	0.050	0.038
Number of hhs	699	419	202	188	680	425
Panel B: Instrumental Variables Regressions (Multiple Outcomes)						
house	0.526	0.981**	1.109*	0.413	0.252*	-0.113
	(0.336)	(0.485)	(0.600)	(0.468)	(0.140)	(0.167)
Observations	1,388	836	404	376	1,360	850
R-squared	0.448	0.194	0.361	0.206	0.023	0.032
Number of hhs	694	418	202	188	680	425
Av Group Size	2	2	2	2	2	2
Weak IV F	89.97	46.66	22.80	30.57	93.86	47.68

Notes: Clustered standard errors in parentheses.*** $p<0.01$, ** $p<0.05$, * $p<0.1$. house=1 if household reported getting a subsidized house at any point in the past. All regressions include controls for time-varying household characteristics. Dependent variable in Columns (1)-(4) are the log of total income, salaries of household members, salaries of female household members and salaries of male household members, respectively. Dependent variable "% employed" gives the proportion of female and male household members currently employed, in columns (5) and (6) respectively.

idence that the effects aren't driven by geographic selection of project sites. It could be that projects were targeted to certain parts of the city- parts of the city that had different trajectories over time. In particular housing could have been targeted towards entire townships, racial groups, or areas a certain distance from the city, that were exhibiting other changes at the time. Indeed it does appear that, on average, housing was built in areas further away from the city, in black areas, and in poorer areas.

Table 16: Average household characteristics by major townships

	Gugulethu N=222	Khayelitsha N=608	Other N=247
Closest Project Distance	0.413	0.144	2.560
Distance to the City	18.57	29.75	21.32
Treated	30%	41%	19.4%
Coloured	7.7%	3.6%	48.8%
Log Income	7.245	7.303	7.598

I divide my sample into three: households living in and around Gugulethu, those living in Khayelitsha, and those living outside of these two townships.[58] Gugulethu and Khayelitsha are the two largest townships, or groupings of townships in the city and are where most of the construction of RDP housing has taken place. Settlements outside of these two townships are relatively neglected. The mean household characteristics for the different areas are presented in Table 16. Clearly other informal settlements have less housing construction, are less likely to get houses, and are more likely to be coloured families. Khayelitsha is further from the center, while Gugulethu is relatively close.

Table 17 shows the results of regressions of log income and employment on treatment, where the sample is restricted to various subgroups. For compactness, I report the treatment effects coefficient for different subgroups in the same columns (both FE and IV). I look in turn at the three different area in turn, just the poorest half of the sample and just the black sample. Sample sizes are small in some of the specifications, but the coefficients remain similar in magnitude to the original results for both FE and IV results for the impacts on household income. This suggests that the results are not driven by difference *across* communities, although I find no evidence of the effect in Gugulethu.[59]

In addition I have a great number of households that were living in areas that *became* housing projects. These areas were being upgraded *in situ* (their entire settlement was replaced with new housing). My use of instruments is less valid for these communities, who may have lobbied for their particular area to be upgraded. In this way it could be that my instrumental variable results are picking up the effect of households that were treated because their area was being upgraded *in situ*. As a result I drop these households from the sample, leaving only households that were outside housing developments, and therefore could not have been ensured access to housing. I find the results are present and just as strong for this group (in panel "Out of Project" in 17).

[58]Refer to the map of Cape Town in the Data section. I define Gugulethu rather more broadly than its strict geographical boundaries; including the townships of Weltevreden Valley, Nyanga, Manenberg and Crossroads Informal Settlement.

[59]This could be cause households in Gugulethu were the household most likely to be moved further away from the city centre, since it is a relatively dense urban area where there was little space for new housing construction.

Table 17: Replication of impacts on Log Income within communities and sub-samples

	(1) FE	(2) IV	(3) FE	(4) IV	(5) FE	(6) IV
	Khayelitsha		Gugulethu		Other	
house	0.262***	1.243*	-0.0150	0.247	0.191***	0.564*
	(0.0844)	(0.733)	(0.0920)	(0.595)	(0.0604)	(0.314)
Obs	1,471	1,470	730	726	3,582	3,561
R^2	0.276	0.168	0.364	0.357	0.290	0.275
	Out of Project		Poor		Black	
house	0.268**	0.988*	0.116*	0.839	0.178***	0.914**
	(0.102)	(0.574)	(0.0696)	(0.562)	(0.0641)	(0.432)
Obs	1,389	1,377	1,733	1,731	3,015	2,998
R^2	0.328	0.294	0.504	0.451	0.264	0.203
HH Chars	Yes	Yes	Yes	Yes	Yes	Yes
Weak IV F		19.61		13.07		95.24

Notes: Clustered standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1. house=1 if household reported getting a subsidized house at any point in the past. All regressions include controls for time-varying household characteristics. Dependent variable is log of total household income.

7.4 Other Channels

Government housing has an impact on household incomes through the channel of increased household earnings from wage employment. The results seem to be driven by increased probability of female labour force participation and earnings, as well as increased earnings for men who work, although this result is not robust to the use of instrumental variables.

I show that sources of income other than labour earnings are not driving the results.[60] In Table 20 I show that receiving government housing was not correlated with getting access to other forms of government grants or welfare. Nor is housing correlated with increases in receipts of remittances or other forms of financial support from other family members or friends.

8 Conclusion

This paper provides new evidence on the relationship between household living conditions and labour supply. I find that government housing has a large and significant impact on household income, and that this effect is driven by increases in earnings from wage labour for household members. This finding is robust to my instrumental variables estimation, which uses proximity from housing projects to predict selection into housing. It is also robust to the use of cancelled projects to control for non-random location choice for housing projects.

[60] We might worry that increased incomes could be driving the results *directly* by increasing reported household incomes, or indirectly if increased welfare facilities job search or increased mobility (Ardington et al., 2009; Franklin, 2015).

I show that the female household members are more likely to be employed in wage paying labour after receiving government housing. I cannot conclusively ascribe the impact on labour to any one particular mechanism. I use evidence from time use surveys and qualitative work on housing conditions to describe the demands on daily life faced by South African households. These include a lack of to water from some distance away, a lack of security from crime, regular fires and floods which cause damage to houses, and the use of inefficient and time consuming cooking and heating technologies.

I provide evidence that government housing alleviates those constraints by improving access to running water, increasing the probability of ownership of labour saving technologies, reducing the use of dangerous fuels for lighting and heating, and improving feelings of security in the home.[61] I also find that housing increases feelings of security in the home which might make it easier for members to leave the household when they otherwise would have stayed to look after the home. I argue that these impacts are driving the impacts on female labour supply.

This finding is consistent with the view that living conditions can have an impact on the ability of females to work (Dinkelman, 2011; Field, 2007). It bolsters the case that informal settlements can act as a poverty trap to those living in them because of this restriction on labour supply (Marx et al., 2013).

Government housing in South Africa has had a transformative effect on South Africa's urban landscape, with over 25% of the total housing stock estimated to have been built by the government in the last 20 years. This perceived success has been a cornerstone of the Government's electoral platform. Governments elsewhere in the developing world seem to be increasingly enthusiastic about large scale housing projects of this kind.

Yet projects on this scale are not easy to evaluate. Randomization of such projects is unlikely to be practically or politically feasible. This paper provides an example of how projects like this can be evaluated ex post with my unique identification strategy of using the combined effect of proximity to urban services as instruments for selection into programmes.

This paper highlights the need to take account the effect of urban and housing policy on the labour outcomes of recipients. Ongoing research is required to fully understand the role of labour, place of living, neighbourhoods, housing design and finance in determining the efficacy of urban policy.

[61] Data limitations prevent me from investigating the health effects of housing, or looking in more detail at the impact of housing on time use patterns.

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A Additional information on first stage estimates

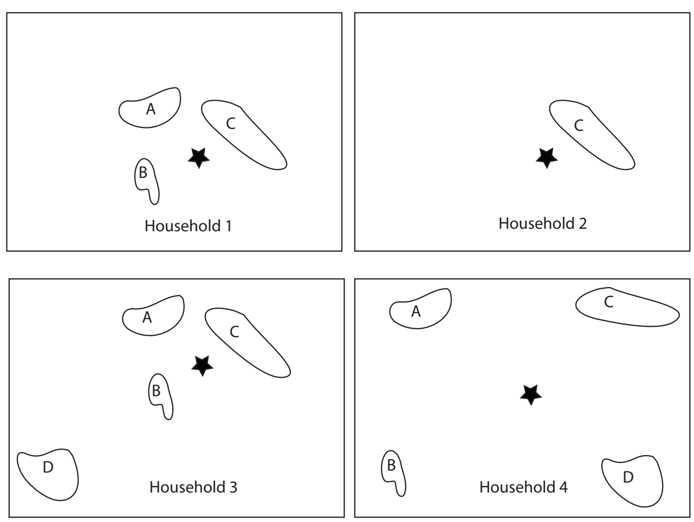
This section covers some additional information related to the estimation of the predicted values for the probability of receiving housing.

I do three things: I explain the rationale for the use of a non-linear predictor of getting housing, as opposed to other linear estimators, or estimators using a single index of housing proximity. Secondly, I explain the time-varying multi-nominal version of the estimator outlined, which explains how I use this framework to predict time varying treatment effects. Thirdly, I present Monte Carlo estimates that show the reliability of the method to recover the true parameters of the data generating process it describes.

A.1 Why a non-linear estimator

I estimate the probability of selection into treatment as the joint probability of being selected by a set of neighboring housing projects. Using only the closest housing project as an instrument would completely miss the effect of living in an area with a great number of projects, relative to a house who just has one, very small project nearby. Additionally, the marginal impact of additional housing projects should diminish as more are built: Consider the possible geographical scenarios depicted in figure 6 below. We want household 1 to be more likely to be treated than 2 of course, but not three times as likely. After all, we might believe the causal effect of a project as close as C to be a 50% chance of treatment for household 2 which would lead to considerable estimation issues for Househould 1 in a linear model. Alternatively, summing distances to all

Figure 6: Hypothetical housing proximity scenarios



projects and estimating a single coefficient would severely penalize household 3 to the benefit of household 1 in the diagram, as 3's extra project D would increase the sum of distances. However, placing a different coefficient on the distance from each project, would also be misleading- a new project's influence should be diminishing in the probability that a household as already been selected. For households like 3, project D is unlikely to influence is probability of treatment at the margin but for households like 4- D could make all the difference.

Trying to incorporate all of these sorts of concerns would involve a great deal of linear restrictions. Instead I estimate a non-linear model that more closely resembles the real world allocation process, and avoids the pitfalls of linear models.

I make two main assumptions in the specification of our function form: firstly, that a household's probability of selection into a individual housing project is a function of a linear combination of a number of proximity instruments, as well as household covariates. I also assume that each project allocates houses independently from the other projects, but that they all do so with the same catchment area; so that parameter on "distance from a project" is constant across households, projects and time.

A.2 Time specific predictions

The problem is complicated further when we consider that we have panel data, and need to estimate a probability of being treated for each time period $P(T_{it} = 1)$ in order to generate a time varying probability of treatment. This problem is analogous to finding the joint probability across programs, except that in this case we find the probability across time. Just like one cannot be treated by two projects at one time, one cannot be treated in more than one time period, and once you are treated you stay treated for all ensuing periods. Thus the probability of not being treated at all at time t is simply the product of the probability of not being treated by any projects built in each time period $A_t \in A$, up to and including t. This can be simply rewritten as the sum of of probabilities across all projects built in the past. In this way, estimation exploits my data on the timing of housing construction to more accurately predict when households were treated.

$$
\begin{aligned}
P(T_{it} = 1) &= 1 - \prod_{j=1}^{t} \prod_{a}^{A_j} P(T_{ia} = 0) \\
&= 1 - \prod_{a}^{A_t, A_{t-1}, \ldots} P(T_{ia} = 0) \\
&= 1 - \prod_{a}^{A_t, A_{t-1}, \ldots} \Lambda(-x_i\beta - dis_{ia}\rho)
\end{aligned}
\tag{13}
$$

In this functional form (13), the probability for a household at each time is an independent event. The probability of having the treatment must be monotonically increasing with time (for each household) as more projects are built. This is the first of two specifications I use to predict treatment.

A more efficient way to estimate the probability of treatment, which takes into account the panel nature of the data, is to construct a single likelihood function for each household, which predicts *when* the household was treated. Each household is assigned a value for TD, which takes on 0 if never treated or $t = 1, 2, \ldots, Y$ if it received the treatment in period t. These are mutually exclusive outcomes. We can then estimate a model of **multinomial form**, in this case for a Y period model. At each time period we continue to use the form (8) in the calculations.

$$
\begin{aligned}
P(TD_i = 0) &= \prod_{t=1}^{4} \prod_{a}^{A_t} P(T_{ia} = 0)) \\
P(TD_i = 1) &= 1 - \prod_{a}^{A_1} P(T_{ia} = 0) \\
P(TD_i = 2) &= (1 - \prod_{a}^{A_2} P(T_{ia} = 0)) \prod_{a}^{A_1} P(T_{ia} = 0) \\
&\dots \\
P(TD_i = Y) &= (1 - \prod_{a}^{A_Y} P(T_{ia} = 0)) \prod_{t=1}^{Y-1} \prod_{a}^{A_t} P(T_{ia} = 0)
\end{aligned} \tag{14}
$$

Notice how probability of treatment is now conditioned on not having been treated earlier. For instance, dummy variable $TD_i = 2$ (got treated in period 2) is the probability of not being selected in period 1 times the probability of being selected in period 2. These dummy variables and their predicted probabilities must, by definition, must sum to 1.

This dummy indicates the the household actually got the treatment in that period. This is different to the outcome of interest, which is the probability of *having the treatment* at a given time period. This can be backed out by simply summing the predicted dummy variables for all time periods up to the present. The probability of treatment at a given time period then simplifies to the same expression given by (13), although the estimation procedure differs. For instance for could calculate the probability of being treated at time 2 using this framework and get the expression give by (13) for $t = 2$:

$$
\begin{aligned}
P(T_{i2} = 1) &= P(TD_i = 1) + P(TD_i = 2) \\
&= (1 - \prod_{a}^{A_1} P(T_{ia} = 0)) + (1 - \prod_{a}^{A_2} P(T_{ia} = 0)) \prod_{a}^{A_1} P(T_{ia} = 0) \\
&= 1 - \prod_{a}^{A_1} P(T_{ia} = 0) \prod_{a}^{A_2} P(T_{ia} = 0) \\
&= 1 - \prod_{t}^{2} \prod_{a}^{A_t} P(T_{ia} = 0)
\end{aligned}
$$

To summarize, I have two non-linear specifications for the probability of being treated at a given time, equation (13) and equation (14). In both models, probability of being treated in a certain time period depends on the projects built up until that point. While we would expect (14) to be the better estimator in the presence of unobserved individual heterogeneity, in the presence of measurement error, we may get less efficient results because it requires the precise time period in which the household was treated. The difference between these two types of estimators and their bias in the presence of unobservables, is explored in the Monte Carlo section below.

A.2.1 Marginal Effects

But first it is useful to have some marginal effects interpretation. This is slightly more complicated than a standard logit framework, but has an intuitive interpretation. We write down the probability of being treated at a particular point using the expression (8) and use the properties of the logistic function, to take the derivative with respect to a particular project b.[62]

$$
\begin{aligned}
P(T_i = 1) &= 1 - \prod_a^A \Lambda(-x_i\beta - dis_{ia}\rho) && (15)\\
&= 1 - \prod_a^A \frac{1}{1 + exp(x_i\beta + dis_{ia}\rho)} && (16)\\
\frac{\partial P(T_i = 1)}{\partial dis_{ib}} &= -\prod_{a \neq b}^A \frac{1}{1 + exp(x_i\beta + dis_{ia}\rho)} \cdot \frac{-exp(x_i\beta + dis_{ib}\rho)}{(1 + exp(x_i\beta + dis_{ib}\rho))^2} \cdot \rho \\
&= \prod_a^A \frac{1}{1 + exp(x_i\beta + dis_{ia}\rho)} \cdot \frac{exp(x_i\beta + dis_{ib}\rho)}{1 + exp(x_i\beta + dis_{ib}\rho)} \cdot \rho \\
&= P(T_i = 0) \cdot P(T_{ib} = 1) \cdot \rho
\end{aligned}
$$

In this framework the marginal effects of distance to particular project depend on a household's current probability of being treated (negatively) and on the probability of being by the treated by the project in question (positively). This is consistent with the idea that a new construction has a relatively bigger effect for a household with few existing projects nearby, and that the probability of being treated drops off faster the further away a particular project gets.

Using the results from the estimation of the first stage in Section 4.3 and the coefficients in Column (4): the coefficient on distance is 0.672, on distance squared it is 0.0079. Imagine a household close to two projects, with characteristics such that the household has a predicted probability of being selected of 10% for both of the projects. Then imagine that one project (b) was originally located 1km away but is relocated slightly further away the household. Then the probability of that household being treated would fall by over 4% for each kilometer that it moved:

$$
\begin{aligned}
\frac{\partial P(T_i = 1)}{\partial dis_{ib}} &= P(T_i = 0) \cdot P(T_{ib} = 1) \cdot (-0.672 + 0.0158 \times dis_{ib}) \\
&= (0.9 \times 0.9) \times 0.1 \times (-0.672 + 0.0158 \cdot 1) = \quad -4.16\%
\end{aligned}
$$

A.2.2 Maximum Likelihood Estimation and Monte Carlo Simulations

In this section I estimate the parameters of the models specified in (13) and (14) using simulated data. Estimation of these models has to be performed using maximum likelihood estimation. I use the Stata **ml** code in order to maximize the log likelihood functions derived from the predicted probability of treatment given by each model, using the Newton-Raphson method.

To perform Monte Carlo tests, I simulate a dataset of $N = 1000$ observations with 3 time periods each. Then for each time household-time observation I generate 5 random project distances (to simulate the construction of houses nearby that household). Each household has a randomly generated household effect x_i that is constant across time and assumed to be unobserved. In addition, each time period has a random effect on the probability of treatment, common to everyone. Then, for each project at each time a latent variable is generated as a function of time

[62] Of course, taking a partial derivative invokes the ceteris paribus assumption. Strictly speaking, this is not plausible in my case. Up until now, I have been discussing a set of distances to projects for each household, these projects will be common to a number of households. So it is hard to imagine the distance from a household to a project changing without it effecting the distance for other households, which in turn would influence the probability of a household being treated

effects, the household fixed effects and, of course, the distance from the household to the project. I use a linearly added logistic error term. If this latent variable is greater than zero we consider a household to be "treated" by that project. A household is treated at that time if it is treated by any *one* of the projects, and it remains treated for the ensuing periods.

$$
\begin{aligned}
y^{\star}_{iat} &= \alpha + \sigma_x x_i + \sigma_\lambda \lambda_t + \rho dis_{iat} + \epsilon_{it} \\
y_{iat} &= \mathbf{1}[y^{\star}_{iat} > 0] \\
y_{it} &= \mathbf{1}[\sum_{a}^{A_t, A_{t-1} \dots} y_{iat} > 0]
\end{aligned}
$$

Where the household characteristics and distance variables are generated in the following way:

$$
\begin{aligned}
&x_i \sim N[0,1], \lambda_t \sim N[0,1] \\
&dis_{iat} \sim U[1,10] \\
&\epsilon_{it} = \frac{exp(\eta_{it})}{1 + exp(\eta_{it})}, \eta_{it} \sim U[0,1]
\end{aligned}
$$

Having generated simulated values, I recover the parameter of interest, which is ρ, using the models specified. I estimate three different specifications. The first (L_{nt}) uses the functional form (13) but without any attempts to control for time trends. The second (L_t) also uses (13), but controls for time by specifying time dummies λ_t in the latent $y^\star$ form. The third (MNL) is the estimation of (14). I then perform Monte Carlo simulations with 1000 repetitions, for each model, while varying the magnitude of variance of the unobserved effects. The results of these simulations, with different "true" values of ρ, are given in table 18. The model performs

Table 18: Results of Monte Carlo Simulations: Estimated value of ρ with different unobserved fixed and time effects

	$\rho = -1$			$\rho = -0.5$		
	L_{nt}	L_t	MNL	L_{nt}	L_t	MNL
$\sigma_x^2 = 0, \sigma_\lambda^2 = 0$	-1.013	-1.013	-1.019	-0.507	-0.507	-0.513
	(0.148)	(0.148)	(0.137)	(0.078)	(0.078)	(0.067)
$\sigma_x^2 = 1, \sigma_\lambda^2 = 0$	-0.914	-0.915	-0.950	-0.433	-0.433	-0.464
	(0.125)	(0.125)	(0.109)	(0.067)	(0.067)	(0.059)
$\sigma_x^2 = 0, \sigma_\lambda^2 = 1$	-0.997	-1.003	-1.007	-0.494	-0.500	-0.503
	(0.144)	(0.143)	(0.143)	(0.076)	(0.075)	(0.076)
$\sigma_x^2 = 3, \sigma_\lambda^2 = 0$	-0.742	-0.744	-0.821	-0.330	-0.332	-0.383
	(0.099)	(0.099)	(0.089)	(0.061)	(0.061)	(0.055)
$\sigma_x^2 = 0, \sigma_\lambda^2 = 3$	-0.836	-0.904	-0.928	-0.416	-0.481	-0.453
	(0.244)	(0.175)	(0.240)	(0.138)	(0.082)	(0.138)

(N=1000, t=3, R=1000)

very well without any fixed effects or time trends, as expected. The introduction of fixed effect biases the estimates towards zero. The bias can be quite considerable when these fixed effects are relatively large, as the example with $\sigma_x^2 = 3$ indicates. The effects are less severe with the introduction of unobserved time effects, but still biased towards zero. The *MNL* estimator performs better when there fixed effects. Importantly, the inclusion of time controls in the *L* model does a very good job of recovering the parameters correctly.

B Additional Figures and Tables

Figure 7: Map of Cape Town and its housing projects in 2009

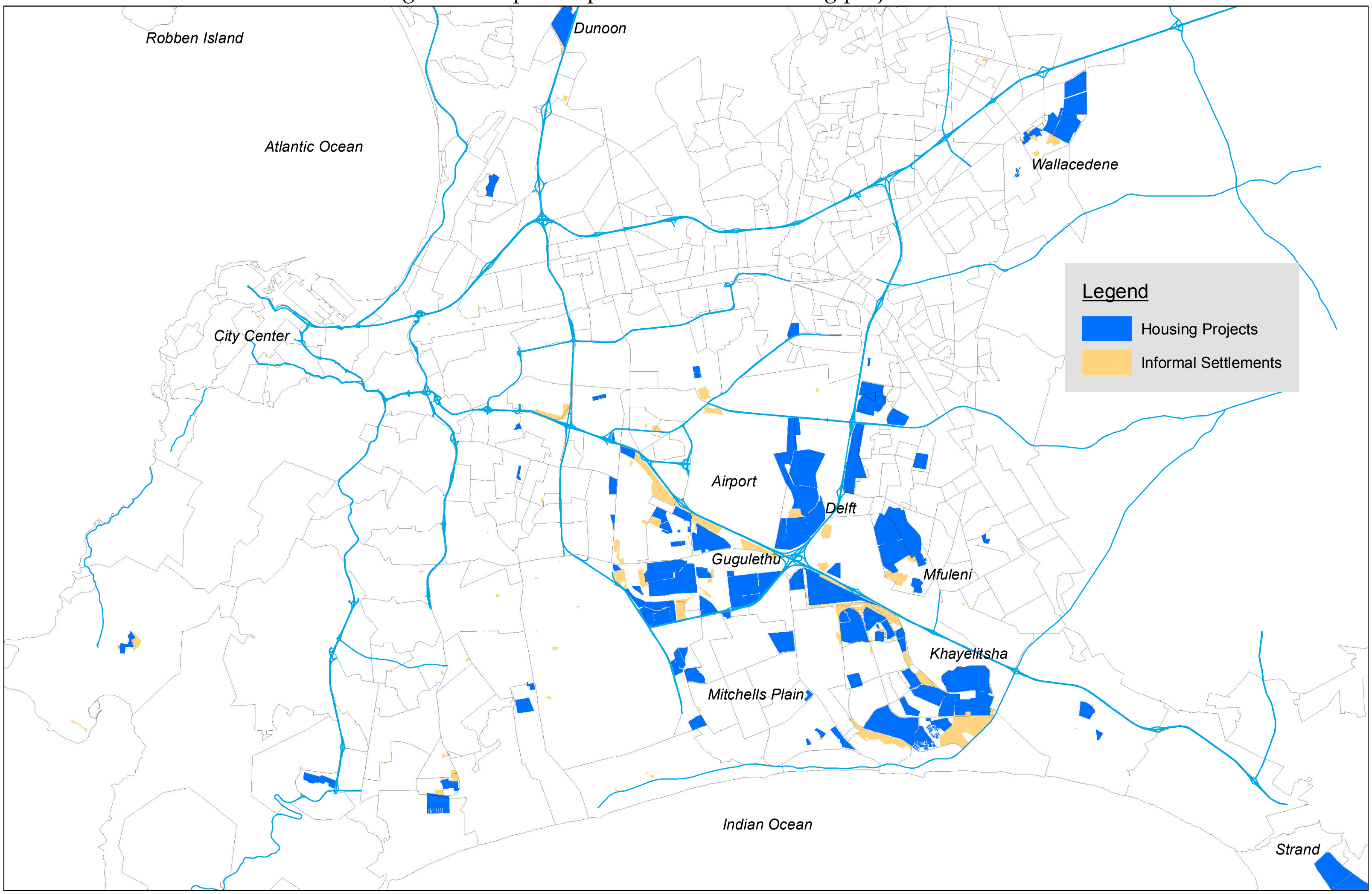


Table 19: Household characteristics in first & last waves, by treatment

	Wave 1 (2002)			**Wave 4 (2009)**		
	Control	Treatment	Diff	Control	Treatment	Diff
From EC	0.712	0.827	0.114***			
			(0.0277)			
Backyard	0.122	0.0742	-0.0478*			
			(0.0198)			
Coloured	0.187	0.0797	-0.107***			
			(0.0228)			
Black	0.790	0.915	0.125***			
			(0.0238)			
Migrant	0.658	0.657	-0.00119			
			(0.0306)			
Shack	1	1	0	0.624	0.188	-0.436***
			(0)			(0.0328)
City distance	22.68	25.12	2.434***	22.51	25.19	2.681***
			(0.394)			(0.436)
Years Ed. head	11.04	11.38	0.341	12.22	12.46	0.238
			(0.214)			(0.239)
Num. Rooms	3.123	3.259	0.136	3.427	3.828	0.401**
			(0.0975)			(0.122)
HH Size	5.189	5.478	0.289	5.549	6.065	0.516*
			(0.149)			(0.221)
Female Head	0.488	0.563	0.0751*	0.533	0.597	0.0649
			(0.0321)			(0.0361)
Age Head	41.66	42.51	0.854	43.95	46.08	2.129*
			(0.758)			(0.947)
Young adult employed	0.112	0.0769	-0.0353	0.465	0.460	-0.00472
			(0.0193)			(0.0360)
% Females Employed	0.442	0.447	0.00439	0.367	0.403	0.0353
			(0.0289)			(0.0277)
Head Employed	0.691	0.632	-0.0596*	0.618	0.562	-0.0554
			(0.0302)			(0.0354)
Health Score	3.858	3.879	0.0214	3.790	4.022	0.233*
			(0.0865)			(0.0903)
Piped Water	0.130	0.107	-0.0233	0.348	0.534	0.185***
			(0.0211)			(0.0351)
Earnings pc	874.9	620.21	-254.75	330.95	400.01	69.06
			(254.74)			(46.177)
Log Income	7.436	7.218	-0.217***	8.194	8.272	0.0784
			(0.0606)			(0.0635)
Obs	713	364		626	344	

Table 20: Impact of treatment on household grants and remittances

	(1)	(2)	(3)	(4)
	FE	FE	FE	FE
	hhgrants	hhgrants	totalsend	totalrec
house	-0.00495	-0.0245	-484.3	-80.95
	(0.0323)	(0.0303)	(629.7)	(197.3)
Observations	3,731	3,723	1,854	1,854
R-squared	0.009	0.140	0.006	0.029
Number of hhs	1,077	1,077	1,062	1,062
HH Chars	No	Yes	Yes	Yes
Av Group	3.464	3.457	1.746	1.746

Notes: Clustered standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1. house=1 if household reported getting a subsidized house at any point in the past. All regressions include controls for time-varying household characteristics. Totalsend and totalrec refer to the total amount of remittances sent and received. hhgrants is dummy for whether households received any one of the household grants such as disability benefits, or the childcare grant.

Table 21: Example of first stage from 2SLS with single fitted instrument

Variable	Coefficient		(Std. Err.)
g-hat	0.885	**	(0.088)
femalehead	-0.070	**	(0.021)
hhsize	-0.009		(0.006)
sexratio	0.097	***	(0.051)
youngratio	0.008		(0.046)
femadultcount	0.002		(0.015)
time2	0.024		(0.020)
time3	0.024		(0.025)
time4	0.037		(0.034)
citydis	0.008	*	(0.004)
maxhhed	0.001		(0.003)
hhmaxage	0.002	**	(0.001)
hhgrants	-0.027	***	(0.016)
Intercept	-0.267	*	(0.122)
N	3711		
R^2	0.318		
$F_{(12,158)}$	28.104		

Notes: These are results from the first stage of the household fixed effects regressions used throughout this paper. These results are basic OLS regression of the dummy variable for having government housing on time varying household characteristics, as well as g-hat, the predicted probability of receiving housing from the maximum likelihood estimator of the probability of getting housing. Clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 22: Impacts on young adult labour supply at the extensive margin

	(1) FE num employed	(2) FE % employed	(3) FD % employed	(4) IV num employed	(5) IV % employed
subhere	0.0338	0.0338	0.0346	0.250**	0.245**
	(0.0311)	(0.0311)	(0.0446)	(0.109)	(0.112)
Observations	2,648	2,648	1,324	2,623	2,622
R-squared	0.151	0.151	0.307	0.135	0.137
Number of personid	662	662	662	662	662
HH Chars	No	No	No	Yes	Yes
Av Group	4	4	2	3.962	3.961
Weak IV F	.	.	.	97.08	87.81

Notes: Clustered standard errors in parentheses.*** $p<0.01$, ** $p<0.05$, * $p<0.1$. house=1 if household reported getting a subsidized house at any point in the past. Dependent variable "num employed" is the number of young adults employed in the household (regressions include controls for the number of young adults in the household). % employed is the proportion of young adults of household members who are employed.

Table 23: Replication of key IV results with basic (FE) 2SLS

	(1) SLSa log income	(2) SLSa head employed	(3) SLSa YA work	(4) SLSb log income	(5) SLSb head employed	(6) SLSb YA work
house	1.099***	0.452**	0.166	1.024***	0.430***	0.206**
	(0.367)	(0.202)	(0.145)	(0.183)	(0.0725)	(0.0867)
Instruments	9	9	9	14	14	14
HH Chars	Yes	Yes	Yes	Yes	Yes	Yes
Obs	3,348	3,487	3,486	3,348	3,487	3,486
R^2	0.198	-0.073	0.157	0.211	-0.065	0.151
GMM2S	Yes	Yes	Yes	Yes	Yes	Yes
Av Group	3.528	3.651	3.650	3.528	3.651	3.650
WeakIVF	4.756	5.282	5.281	23.21	21.23	21.22
Stock Yogo Critical Values						
5% Max. Bias	20.74	20.74	20.74	21.23	21.23	21.23
20% Max. Bias	6.61	6.61	6.61	6.42	6.42	6.42

Notes: Clustered standard errors in parentheses.*** $p<0.01$, ** $p<0.05$, * $p<0.1$. house=1 if household reported getting a subsidized house at any point in the past. Excluding Instruments in all regressions: projdis#1-5, rank3#1-3, inproject.

Table 24: Main FE results only among individuals in clusters where many households were treated

	(1)	(2)	(3)	(4)	(5)	(6)
	FE	FE	FE	FE	FE	FE
	lginc	lginc	lginc	ln_hhtotsal	ln_hhtotsal	ln_hhtotsal
house	0.226***	0.150**	0.144**	0.193***	0.181**	0.176**
	(0.0609)	(0.0588)	(0.0582)	(0.0656)	(0.0722)	(0.0719)
femalehead		-0.297***	-0.239***		-0.221***	-0.203***
		(0.0566)	(0.0596)		(0.0730)	(0.0765)
hhsize		0.133***	0.143***		0.0441***	0.0418***
		(0.0102)	(0.0110)		(0.0124)	(0.0136)
citydis		-0.0118	-0.00929		-0.0216*	-0.0197
		(0.0110)	(0.0108)		(0.0130)	(0.0130)
yamom			-0.0145			0.0753
			(0.0682)			(0.0900)
maxhhed			0.0315***			0.0396***
			(0.00718)			(0.00911)
sexratio			-0.125			-0.184
			(0.142)			(0.184)
youngratio			-0.623***			-0.273*
			(0.112)			(0.143)
Observations	2,388	2,397	2,393	1,895	1,895	1,892
R-squared	0.216	0.301	0.325	0.129	0.147	0.165
Number of hhs	747	758	758	718	718	718
Av Group Size	3.197	3.162	3.157	2.639	2.639	2.635

Notes: Clustered standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1. house=1 if household reported getting a subsidized house at any point in the past. Sample restricted to households in EAs in which more than 20% of hhs received housing by 2009.

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