The Cost of Homeownership: Regular Income Tests and Targeting Efficiency of Public Housing in Hong Kong

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July 2025

Abstract

Subsidized rental housing is known to benefit recipients less than subsidized homeownership, yet regular income tests are crucial for maximizing targeting efficiency. This paper assesses the importance of these tests using a unique housing reform in Hong Kong, which allowed 183,700 public housing tenants to avoid regular income tests by purchasing permanent occupancy rights. Leveraging the reform's incomplete roll-out between 1998 and 2006, I estimate that it increased average household incomes by 23% over 15 years, reduced average household sizes, and persistently altered household demographic composition in treated estates. These impacts are consistent with incumbent families strategically altering co-residence choices to obtain additional public housing units. Consequently, the reform undermined targeting efficiency and increased wait times for low-income households. Back-of-the-envelope calculations suggest it significantly contributed to the proliferation of tiny subdivided private-sector units.

Keywords: means testing, public housing, targeting, subsidized sale
JEL: H44, I38, R31

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*Email: mbwong@hku.hk. I am deeply indebted to Yiran He, Jimmy Ho, and William Lui for assistance with the data. I thank David Autor, Leah Brooks, Remi Jedwab, James Kung, Juan Pantano, Parag Pathak, Steve Ross, Kelvin Wong, Yue Chim Richard Wong, and seminar participants at George Washington University, HKU, and AsRES for helpful comments. I gratefully acknowledge funding from the National Science Foundation and the Kuok Foundation.

1 Introduction

The design of housing assistance programs involves a fundamental trade-off: Governments can either require regular income tests to maximize targeting accuracy or minimize administrative burdens using one-time income tests. This choice has profound implications for the efficiency of housing subsidy programs, yet their consequences remain inadequately understood. For example, 31 percent of Hong Kong's residents live in subsidized rental housing and are subject to recurring income tests—with rents rising to 1.5 or 2 times base levels if income thresholds are exceeded—while another 16 percent occupy subsidized ownership units requiring only initial income testing. Similar choices arise in the design of housing assistance programs globally, from Singapore's large-scale subsidized ownership program to the UK's Right-to-Buy program and analogous social housing policies across Europe and Asia.

Recent studies have highlighted many advantages of subsidized ownership housing, including reduced housing misallocation (Wang 2011), increased credit access and entrepreneurship (Wang 2012), lower crime rates (Disney et al. 2023), increased wealth accumulation (Sodini et al. 2023), and increased labor supply (Jacob and Ludwig 2012; Zhang 2025). However, scholars have paid less attention to how the choice between subsidized ownership and subsidized rental affects targeting efficiency. By inducing continual self-selection of needy populations into the program, rent assistance with regular income tests may better ensure that subsidies are efficiently allocated (Akerlof 1978; Nichols and Zeckhauser 1982). Yet, there is little evidence for this potential advantage of subsidizing rent instead of homeownership.

In this paper, I study the impacts of Hong Kong's Tenants Purchase Scheme (TPS), a large-scale subsidized sale of public rental housing between 1998 and 2006. The policy allowed 183,700 sitting tenant households, who were regularly income-tested, to purchase their rental units at heavily discounted prices. Unusually, however, the switch from subsidized rental to subsidized ownership did not grant buyers the right to lease or resell the purchased units, except to family members. This unique feature of the setting allows me to focus on the impact of removing regular income tests for recipients of housing subsidies, and thereby elucidate the comparative advantages of subsidized homeownership and subsidized rental housing.

The effects of TPS on the treated housing estates are estimated using the staggered and incomplete implementation of the scheme across housing estates in a dynamic difference-in-differences design. As the control group, non-TPS housing estates with similar construction years as TPS estates are used. Estate-level outcomes such as population, household sizes, household incomes, user costs, and commute times are computed from restricted-access 10% and 20% random repeated cross-sections of the Hong Kong Population Census. To ensure that the estimates have a causal interpretation, pre-event differential trends are verified to be absent.

The estimates reveal that TPS sharply reduced the assignment of subsidized units to low-income households in treated estates. Average household income in treated estates rose by 7 percent within a few years, and was a startling 23 percent higher than control 15 years later. Since only 79 percent of units in the treated estates were sold by then, these estimates imply even larger effects of TPS on average incomes in sold units. This increase is driven by large increases in the number of households with incomes above the income thresholds. Over the two decades after the sale, the share of households with incomes above the 1.5 times rent income limits increased by 8.1 percentage points from an initial level of 10.2 percent.

While these estimates are inherently interesting, it is important to clarify what economic mechanisms drive these effects. First, auxiliary results are provided to confirm that the primary effect of TPS was to relax regular income-testing requirements. Specifically, it is documented that exceedingly few purchasing households further gained or exercised the right to transfer units except to eligible family members. Moreover, a collage of suggestive evidence is found to be consistent with the hypothesis that avoidance of income tests was the primary motive for many households who purchased TPS units.

Second, a simple conceptual framework is provided to illustrate how TPS may have affected labor supply and residential sorting. The model suggests that TPS could lead to increases in average incomes through three channels. First, TPS may increase individual labor supply by effectively removing a tax on labor income. Second, TPS may encourage households with higher earning potential to stay. Third, TPS may alter co-residence patterns within extended family networks, by encouraging poorer members of incumbent families to re-enter the public housing queue and receive additional public housing units, while higher-income family members who were no longer subject to income tests became more likely to stay.

The observed increases in income are not purely attributable to labor supply responses. Importantly, average schooling among younger working-age adults in treated estates increased by a year, suggesting that the increases in income are partly explained by changes in demographic structure. Moreover, average housing user costs substantially fell, suggesting that the increases in income are not attributable to increased pressure to meet loan obligations. Average commute times were also unchanged, suggesting that the increases in income do not reflect significant reductions in spatial mismatch between workers and jobs.

The estimates also reveal large changes in population and household composition composition. In treated estates, total population decreased by 5 percent within a few years, and eventually decreased by roughly 7 percent, or roughly 51,000, within two decades. Average household size declined by roughly 5 percent. The share of young men and women who are married fell by 14 p.p. and 11 p.p., respectively. The average number of children residing with young women fell by 0.36. The share of households with extended families substantially fell, while the share of nuclear families rose.

The large increases in average income and declines in household sizes are most consistent with the hypothesis that extended families strategically altered co-residence choices. Consistent with this hypothesis, household head mobility was very low in both treated and control estates. Moreover, household head mobility did not detectably respond to the policy change. The changes were therefore driven primarily by changes in co-residence choices, with lower-income family members leaving to obtain additional public units.

Together, the findings imply significant harm to low-income populations outside the public housing system, whose access to subsidized units was reduced. Back-of-the-envelope calculations suggest that the reduction in available units accounts for more than 16 percent of Hong Kong's subdivided private-sector units in 2016. The removal of regular income tests thus significantly contributed to the suffering of low-income populations unable to obtain public housing, who had little option but to live in cramped and unsafe conditions. These results caution that policymakers must account for the potential erosion of welfare objectives when choosing subsidized ownership over subsidized rental housing.

1.1 Related Literature

This paper contributes to a literature on the targeting of social assistance. The tension between targeting and allocative efficiency was first identified in theoretical work by Akerlof (1978) and Nichols and Zeckhauser (1982). Recent empirical studies have studied this tension in different types of non-housing welfare programs (Deshpande and Li 2019; Finkelstein and Notowidigdo 2019; Lieber and Lockwood 2019). A growing literature also studies how mechanism design for the initial allocation of public housing affects allocative and targeting efficiency (Waldinger 2021; Lee, Kemp and Reina 2022; Naik and Thakral 2022; Thakral and Murra-Anton 2024; Thakral Forthcoming). To date, however, little attention has been devoted to understanding how ownership rights *after* the initial allocation of subsidized housing affect targeting efficiency. This paper advances a better understanding of the trade-offs involved in the design of housing assistance programs by measuring the extent to which regular income tests was important for targeting housing subsidies. My findings highlight that the removal of regular income tests eroded targeting efficiency, due to strategic co-residence decisions within extended family networks—a mechanism that is typically ignored in existing literature, but has particular salience in societies with strong kinship ties, such as Hong Kong.

This paper also contributes to a growing literature that examines the effects of the sale of subsidized housing. Wang (2011, 2012) provides evidence that the subsidized sale of state employee housing in China reduced housing misallocation, raised private-sector prices, relaxed credit constraints, and increased self-employment. Disney et al. (2023) present quasi-experimental evidence that UK's Right-to-Buy housing reform reduced crime due to behavioral changes of the incumbent population. Sodini et al. (2023) show that the subsidized sale of municipal-owned buildings in Sweden caused beneficiaries to experience wealth increases and increased consumption owing to property price appreciation.[1] The Hong Kong setting differs from prior studies, since leasing and resale restrictions prevented the realization of the benefits of homeownership that were emphasized in these studies. The unique setting sheds new light

[1] Disney and Luo (2017) provide theoretical results regarding the welfare effects of UK's Right-to-Buy program, which shares many similarities with Hong Kong's Tenants Purchase Scheme.

on the importance of regular income tests for targeting housing assistance.[2]

This study is among the first to use a quasi-experimental design to document the effects of housing assistance in Hong Kong. Existing studies on Hong Kong's Tenants Purchase Scheme and, more broadly, on Hong Kong's public housing sector generally rely on cross-sectional or time series evidence. Wong and Liu (1988) provide evidence on misallocation using data on rent and income in the Population Census. Lui and Suen (2011) study spatial misallocation using mobility patterns, while Cheung et al. (2021) study turnover rates. Yeung (2001) provides descriptive survey evidence and simulations to study how TPS affected Hong Kong's property prices. Ho and Wong (2006) provide time-series evidence on the effects of TPS on private-sector housing prices, but their estimates are potentially confounded by contemporaneous events such as the Asian Financial Crisis.

The paper proceeds as follows. Section 2 describes institutional background. Section 3 provides a theoretical framework. Section 4 provides descriptive evidence. Section 5 presents the estimated effects of TPS. Section 6 discusses economic mechanisms. Section 7 concludes.

2 Institutional Background

This section explains how TPS changed the nature of occupancy rights granted to purchasing public rental housing (PRH) residents.

2.1 Public Rental Housing in Hong Kong

The purpose of Hong Kong's PRH program is to provide subsidised units for qualifying low-income families. Applicants are funnelled through a waiting-list system, which processes applications mainly on a first-come-first-served basis. To receive offers, applicants must satisfy income and asset requirements.[3] Individual units are then offered to applicants by random computer batching according to each applicant's household size, unit allocation standards, and

[2] A related literature documents the effects of housing assistance on labor supply and child outcomes (e.g., Jacob 2004; Kling, Ludwig and Katz 2005; Jacob and Ludwig 2012; Chyn 2018; Dijk 2019).

[3] Appendix Table A1 shows the PRH income limits.

choice of district. Applicants receive up to three housing offers, which are given out one at a time. If all three offers are rejected, then the applicant must wait one year before reapplying. In 1998, the year before the launch of TPS, 2.3 million Hong Kong residents lived in PRH units, roughly 38 percent of the total population.[4]

The average rent of a PRH unit in 2004 is $1,563 (HKD), which is about half of a similar private-sector unit.[5] Tenants who have lived in PRH units for 10 years or more must declare the income and assets of all household members biennially. Households who report total monthly incomes in excess of household-size-contingent income limits are required to pay either 1.5 times rent or double rent, and households who additionally have large net asset holdings are either pay market rent or asked to move out.[6] To encourage truthful reporting, income and asset declarations are randomly chosen for in-depth verification. Households with all members aged 60 or above are exempted from income checking.[7]

The government allows only the tenant and family members listed on the tenancy agreement to occupy a PRH flat. The tenant must notify the government immediately of any household changes caused by birth and death. Upon death of the tenant, the flat may be transfer to the spouse or to an authorized member residing in the flat who satisfies the income limit. The government also reallocates units if the size of a household significantly falls due to move-out, death, marriage, or emigration of household members. However, these cases are rare. Between 2016 and 2020, the government resolved an average of about 2,200 under-occupation cases each year, roughly 0.3 percent of the total number of PRH households.[8]

[4]See Housing Department (2021) and Legislative Council Secretariat (2020). As of March 2019, public rental housing units accounted for about 29 percent of the stock of permanent housing and housed about 31 percent of total households in Hong Kong (Census and Statistics Department 2020; Transport and Bureau 2019).

[5]Online Appendix Figure A1 plots rent trends for public housing units and similar units in the private sector.

[6]Appendix Figure A2 shows the PRH rent schedule.

[7]The Housing Subsidy Policy (HSP) and the Policy on Safeguarding Rational Allocation of Public Housing Resources (PSRA) were implemented in 1987 and 1996 respectively and are collectively referred to as "Well-off Tenants Policies". Under the PSRA, household income and net asset value are adopted as the two criteria for determining PRH households' eligibility to continue to receive subsidised public housing. Under section 26(1) of the Housing Ordinance, any person who knowingly makes any false statement are liable on conviction to a maximum fine of $50,000 and to imprisonment for six months. Between 2003 and 2006, roughly 6 percent of households were found to have under-reported their incomes, of which 18 percent were prosecuted. See Audit Commission (2007) for more details.

[8]To address under-occupation (UO), tenants are required to declare biennially their occupancy position. These declarations are verified through random unit visits. If the number of household members in a PRH unit is below the

2.2 History of Tenants Purchase Scheme

In 1997, the Hong Kong Housing Authority announced the Tenants Purchase Scheme (TPS), which allowed PRH tenants to buy the units they lived in at a discounted price. The policy announcement was unexpected and its stated goal was to boost Hong Kong's homeownership rate to 70 percent within ten years' time. Between 1998 and 2006, units in 39 PRH estates, totalling 183,700 units and comprising roughly 27 percent of the total stock of PRH units, were made available for sale.

Strong incentives were put in place to encourage rapid sale. Almost all sitting tenants in the selected estates were offered the opportunity to purchase.[9] Tenants who do not wish to purchase can continue to rent and occupy their units as before. The purchase price was set at replacement cost, but given a further discount of 60% on purchase within the first year, which is as low as 12% of market value.[10] To fund the purchase, the government agreed with several banks to provide mortgages of up to 100% of the balance of the purchase price of the unit for up to 25 years. Following the sale, the unit owner became responsible for maintenance and repairs, building management fees, as well as property taxes.

However, in August 2005, the Housing Authority announced that there will be no further sale of PRH units after 2006. In Section 5, I leverage the staggered and incomplete roll-out of TPS across housing estates to identify the impact of the program.[11]

minimum number set by the HA for the unit, the household is asked to move to a suitable unit. Under-occupation is a significant problem. As of March 2021, there were 79,380 UO households, of which 5,320 were considered prioritized UO cases. See Audit Commission (2013) and GovHK (2021).

[9]The exceptions were those living in the following units: 1) Housing for Senior Citizens and Small Household Block; 2) units used for social welfare purposes; and 3) units with common entrance and communal facilities such as bathroom, kitchen and entrance.

[10]New tenants who purchase TPS units enjoy a full credit if they buy within the first year and a halved credit in the second year. After the second year, no credit will be given. Purchasers will need to pay, apart from the price of the unit, the stamp duty, registration fees and legal costs. See Housing Authority (2014) for more details.

[11]In each of the first five phases of TPS launch, around 26,000 to 28,000 PRH units in six selected estates were offered for sale. In the last phase, which comprised phase 6A and phase 6B, around 49,000 PRH units in nine estates were offered for sale (Legislative Council Secretariat 2020).

2.3 Restrictions on Resale and Leasing of TPS Units

TPS granted a peculiar form of occupancy right to purchasing households. TPS unit owners were no longer subject to the regular income tests and under-occupancy unit allocation rules of PRH tenants. Therefore, TPS owners and their registered family members can occupy the purchased unit indefinitely. TPS owners were also permitted to transfer ownership to registered family members in special circumstances, such as old age or illness. At the same time, family members who once resided in TPS units but become unregistered were eligible to apply for PRH units. As such, it became possible for a family member with high earnings potential to gain ownership of the unit without satisfying income tests, while a family member with low earnings potential applied for another subsidized unit.

However, TPS owners were strongly restricted from resale and letting. First, they cannot lease or resale on the open market until a premium equivalent to the current value of the original discount is paid to the government.[12] These transactions were exceedingly rare. Suppose that a unit was purchased at 12 percent of the initial market value, and the household now wishes to sell the unit on the open market and simultaneously purchase another unit of equivalent value on the open market. The premium requirement is then equivalent to an 88 percent transaction levy. This requirement strongly discouraged premium payment. For example, in the district of Tuen Mun, there were 14,383 sold TPS unit as of September 23, 2021, of which only 200 had premiums paid between 2005 and 2020. In other words, the number of premium payments per year was less than 0.1 percent of the stock of sold TPS units.[13]

Second, TPS owners were permitted to sell their unit without payment of a premium only to public housing renters and a small number of eligible purchasers in the Home Ownership

[12] In the first two years after the sale, a TPS unit owner can only sell the unit back to HA at the list price. Within the third to fifth years from the date of first assignment, TPS unit owners can sell back their units to Housing Authority at assessed market value less the original purchase discount. If HA declines to buy back the units, however, TPS unit owners can sell, let or assign their units in the open market. In addition, the Housing Authority may give consent to a request for change of ownership under special circumstances, such as divorce or separation, emigration or long-term working abroad, death, old age, bankruptcy, or terminal illness of owner. TPS owners letting units in breach of the Housing Ordinance are liable on conviction to a maximum fine of $500,000 and to imprisonment for one year. See Housing Authority (2014).

[13] See: https://www.housingauthority.gov.hk/en/home-ownership/information-for-home-owners/premium-payment-arrangement/premium-statistics/index.html

Scheme (HOS) Secondary Market. Such transactions were rare. Most of these eligible purchasers can rent or wait to buy from the government at subsidized rates and therefore had low willingness to pay. Meanwhile, TPS owners were generally unwilling to sell at discounted prices, since they are ineligible to purchase in the secondary market and would not be able to obtain a unit of equivalent value in the open market. For TPS units in the district of Tuen Mun, there were only 702 such transactions between the beginning of 2002 and October 2021. The number of transactions on the HOS Secondary Market per year was therefore less than 0.3 percent of the stock of sold TPS units.[14]

3 Theoretical Framework

In this section, a model is developed to analyze how TPS affects the behavior of public renters. In the baseline model, household choose whether to live in public housing and how much labor to supply. An extended model then considers whether family members co-reside in public housing. It is shown that income-contingent rents reduce the labor supply of those living in public housing, but encourage high-wage individuals to self-select into private-sector housing. By removing regular income tests, TPS increases average income in treated estates both by increasing labor supply and by altering co-residence decisions within extended family networks.

3.1 Baseline Model

Households have utility $u(c,h,l)$ over consumption c, quantity of housing services h, and leisure l. Household income is given by $I = w(T-l)$, where T denotes total hours. If renting in the private sector, the household's utility is:

$$\begin{aligned} u^*(w) = &\max_{c,h,l} u(c,h,l) \\ &\text{s.t. } c + rh \leq w(T-l), \end{aligned}$$

[14]See: https://www.housingauthority.gov.hk/en/home-ownership/hos-secondary-market/transaction-records/index.html

where r denotes the rent per housing service in the private sector.

For public housing, the quantity of housing services is fixed at $\overline{h}$. Public housing rent is given by $R(I)$, which is an income-contingent rent schedule with two notches:

$$R(I \mid \overline{I}) = \begin{cases} \overline{R} & \text{if } I < 2\overline{I} \\ \overline{R} + \tau & \text{if } I \in [2\overline{I}, 3\overline{I}] \\ \overline{R} + 2\tau & \text{if } I > 3\overline{I}. \end{cases}$$

For public rental housing in Hong Kong, $\tau = \frac{1}{2}\overline{R}$. Furthermore, $\overline{R} + 2\tau < r\overline{h}$, so public housing rent is always lower than the private sector. The utility of a household in public housing is:

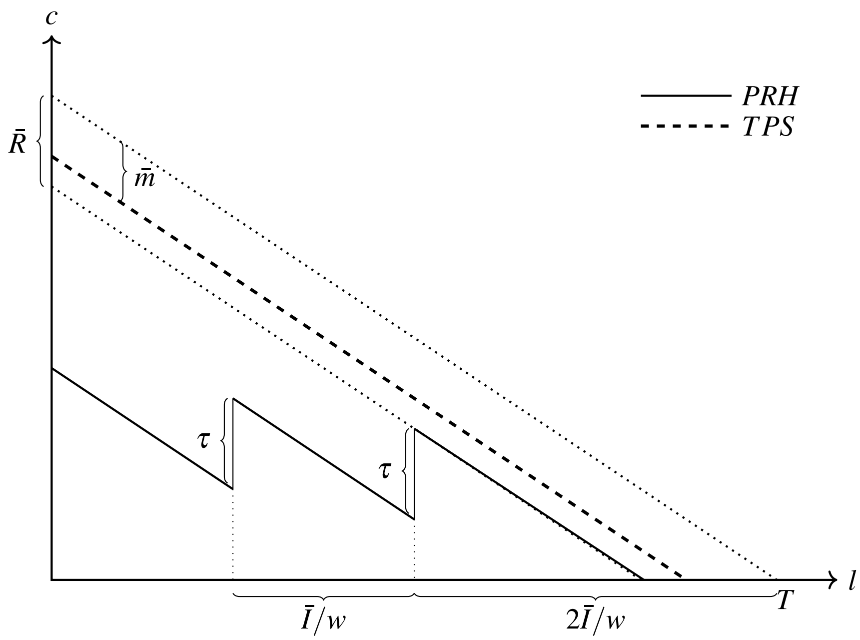
$$\begin{aligned} u_{PRH}(w) = &\max_{c,l} u(c, \overline{h}, l) \\ &\text{s.t. } c + R(w(T-l) \mid \overline{I}) \leq w(T-l). \end{aligned}$$

Since housing services offered to public housing tenants are fixed at quantity $\overline{h}$, households with sufficiently high wages prefer to rent private housing despite the subsidy for public housing. The reason is that public renter households with high wages consume less housing service than they would have in the private sector. Therefore, even if $\tau = 0$, there exists some cutoff w_{PRH} such that $u^*(w) \lesseqqgtr u_{PRH}(w)$ if and only if $w \lesseqqgtr w_{PRH}$.

Rent notches help the government better target housing subsidies toward the needy. By reducing the cost of residing in public housing disproportionately for high-income households, rent notches causes $u_{PRH}(w)$ to fall disproportionately for households with high wages. Therefore, as τ increases, the cutoff w_{PRH} falls.

However, rent notches create disincentives to work. As shown in Figure 1, the budget set of a household who choose public housing closely resembles households who chooses between leisure and consumption in the presence of tax notches. A household who increases their labor earning from below to above the income threshold reduces their consumption if they work more. Therefore, household are strongly disincentivized from working more.

Figure 1: Budget set of PRH and TPS households



3.2 Effects of TPS on Household Sorting and Labor Supply

As explained in Section 2, TPS allows sitting PRH tenants to purchase permanent occupancy rights to their units at heavily subsidized prices but did not grant leasing or resale rights. If they do not move out, the utility of TPS purchasers is given by

$$u_{TPS}(w) = \max_{c,l} u(c, \overline{h}, l)$$
$$\text{s.t. } c + \overline{m} \leq w(T - l).$$

where $\overline{m}$ denotes the cost of residence for TPS owners. TPS alters the household budgets of public housing tenants in two ways. First, it eliminates the rent notches. Second, it potentially alters the baseline housing cost (if $\overline{R} \neq \overline{m}$). Since TPS imposed stringent transfer restrictions, it is assumed that households cannot sell or lease out the unit in order to live elsewhere.

TPS take-up. If $\overline{R} > \overline{m}$, then $u_{TPS}(w) > u_{PRH}(w)$ for all w, so all households buy. If instead $\overline{R} < \overline{m}$, then households who are unconstrained by the rent notches will not purchase, since it strictly reduces their utility. By contrast, households with higher wages and therefore are constrained by the rent notches purchase TPS to remove the rent notches.

Labor supply. Conditional on take-up, TPS has three potential effects. The first potential effect is that TPS alters labor supply. The labor supply effect consists of two components. First, TPS moves some households to a higher utility level, which induces households to reduce labor supply. Second, the removal of the rent notch increases the relative price of leisure, so households substitute away from leisure. The latter substitution effect is likely to be much larger than the former income effect, leading to an overall increase in labor supply.

Household-level residential sorting. The second potential effect is that TPS encourages high-wage households to remain in public housing. Specifically, if $\overline{m} \leq \overline{R}$, it can be shown that $w_{TPS} \geq w_{PRH}$, where w_{TPS} is defined such that $w \lesseqgtr w_{TPS}$ if and only if $u^*(w) \lesseqgtr u_{TPS}(w)$. In a purely static model where w is fixed, this channel is unimportant, since households were initially means tested without any anticipation that means testing will be relaxed, so all treated households would have had $w \leq w_{TPS}$ to begin with. This sorting channel, however, may be more important for households whose earning potential grows over time.

3.3 Effects of TPS on Co-residence within Family Networks

The third potential effect is that TPS affects co-residence patterns within extended family networks. In both PRH units and TPS units with unpaid premiums, government rules allow only family members (such as parent, child, and grandchild) to co-reside. As such, TPS may shift households towards being composed of fewer extended family members and those with higher earning power. The above model treats households as atomistic and unchanging units, so it is extended here to illustrate these possibilities.

For simplicity, we assume that there are only two family subunits, labeled A and B, where A is the higher earner, so $w_A > w_B$. For example, we can think of A as the parent of two adult children who is bound to her high-earning but unmarried adult child, while B is the other adult child who has a nonworking spouse and a grandchild. The two family subunits choose whether to live together as a single household. Co-residence choices within extended family networks are modeled using a Shapley–Shubik–Becker matching framework.

If the two family subunits live separately, the subunit who lives in the PRH unit (call her i) solves

$$v_i^{PRH}(w_i) = \max_{c_i, l_i} u_i(c_i, \bar{h}, l_i)$$
$$\text{s.t. } c_i + R(w_i(T - l_i) \mid \bar{I}_1) \leq w_i(T - l_i).$$

The residence choice of the subunit living elsewhere (call her j) depends on her income: she qualifies for PRH if she has sufficiently low income, but rents in the private sector otherwise. We denote her utility as $v_j(w_j)$.[15] The joint utility from living separately is $V^{PRH}(w_i, w_j) = v_i^{PRH}(w_i) + v_j^*(w_j)$. If living separately, it is always optimal for the higher earning subunit to reside elsewhere in order to avoid the income-based rent notch. Since by assumption $w_A > w_B$, it can be shown that $V^{PRH}(w_A, w_B) \leq V^{PRH}(w_B, w_A)$.

If instead the two family subunits co-reside in the PRH unit, then their joint utility is

$$U^{PRH}(w_i, w_j) = \max_{c_i, c_j} \frac{1}{\delta}[u_i(c_i, \bar{h}, l_i) + u_j(c_j, \bar{h}, l_j)]$$
$$\text{s.t. } c_i + c_j + R(w_i(T - l_i) + w_j(T - l_j) \mid \bar{I}_2) \leq w_i(T - l_i) + w_j(T - l_j),$$

where $\delta > 1$ is a discount factor associated with shared residence. We assume that $\bar{I}_2 > \bar{I}_1$, since by HA rules, a larger household is subject to a higher income limit. The two potential household subunits co-reside if and only if $U^{PRH}(w_A, w_B) > V^{PRH}(w_B, w_A)$.

Now consider a household that has purchased a TPS unit, and hence is no longer subject to income tests. If the two family subunits live separately, the one who resides in the TPS unit

[15] Here $v_j(w_j) = \max\{v_j^{PRH}(w_j), v_j^*(w_j)\}$, where $v_j^{PRH}(w_j) = \max_{c_j, l_j} u_j(c_j, \bar{h}, l_j)$ s.t. $c_j + R(w_j(T - l_j) \mid \bar{I}_1) \leq w_j(T - l_j)$ and $v_j^*(w_j) = \max_{c_j, h_j, l_j} u_j(c_j, h_j, l_j)$ s.t. $c_j + rh_j \leq w_j(T - l_j)$.

solves

$$v_i^{TPS}(w_i) = \max_{c_i, l_i} u_i(c_i, \overline{h}, l_i)$$
$$\text{s.t. } c_i + \overline{m} \leq w_i(T - l_i).$$

The one that lives elsewhere has utility $v_j^*(w_j)$, as before. The joint utility from living separately is $V^{TPS}(w_i, w_j) = v_i^{TPS}(w_i) + v_j^*(w_j)$. If they co-reside, their joint utility is

$$U^{TPS}(w_i, w_j) = \max_{c_i, c_j} \frac{1}{\delta}[u_i(c_i, \overline{h}, l_i) + u_j(c_j, \overline{h}, l_j)]$$
$$\text{s.t. } c_i + c_j + \overline{m} \leq w_i(T - l_i) + w_j(T - l_j).$$

An important implication of removing income tests is that it may now be optimal for the higher earning family subunit to reside in the TPS unit, while the lower earning family subunit leaves to obtain another PRH unit. In other words, it may be the case that $V^{TPS}(w_A, w_B) > V^{TPS}(w_B, w_A)$. As such, the introduction of TPS may cause residential resorting within family networks such that higher earning family members instead live in the subsidized units.

Another implication is that household sizes may shrink. To see this, consider a PRH household with two co-residing subunits. Suppose that member A's labor would have been constrained by the rent notch if only resided in the PRH unit, but the rent notch would not bind if A and B co-resided.[16] For simplicity, assume that $\overline{m} = \overline{R}$. It then follows that $U^{PRH} = U^{TPS}$. Moreover, since the rent notch does not bind under TPS, $V^{PRH} < V^{TPS}$. In other words, TPS makes co-residence less likely.

3.4 Testable implications

Two implications of the above theory are tested below. Section 4.3 tests the prediction that high-wage households were more likely to purchase TPS units. Section 5 tests the prediction that average income increases in treated estates. Section 6 provides additional evidence to

[16] Specifically, suppose that $w_A(T - l_A^*) = 2\overline{I}_1$, where l_A^* denotes A's chosen level of leisure when living alone, and that $w_A(T - \tilde{l}_A^*) = 2\overline{I}_2$, where $\tilde{l}_A^*$ denotes A's chosen level of leisure when co-residing.

distinguish between potential mechanisms. The empirical results will illuminate the extent to which regular income tests are important for inducing self-selection of needy populations into receiving housing subsidies.

4 Data and Summary Statistics

In this section, the data are described and two descriptive facts are shown. First, the vast majority of TPS-eligible households did not become private owners with premiums paid and therefore could not resell or lease their units in the open market. Second, TPS participants were disproportionately larger, younger, and high-income households who were more likely to benefit from a relaxation of income limits and unit allocation rules.

4.1 Hong Kong Population Census

To measure the effects of TPS on estate outcomes, restricted-access data from the Hong Kong Population Census and By-census are used—specifically, the 20% random samples in 2001, 2011 and the 10% random samples in 1996, 2006, 2016. These data provide information about each respondent's age, sex, household composition, employment, and earnings, as well as an indicator for whether the respondent moved in the last five years.[17] Furthermore, these data include identifiers for 136 public rental housing estates, including all 39 estates where residents became eligible to partake in TPS. The identifiers are used to construct a panel of treated and control housing estates for measuring the causal impact of TPS.

4.2 Trends in Ownership and Leasing in TPS Estates

Table 1 shows the trend in ownership and leasing composition of households in TPS estates. There are three findings. First, a large majority of units in TPS estates were sold immediately after the launch of TPS. By 2006, the share of households residing in sold TPS units had risen to 57.4 percent from zero in 1996. By 2016, the share further increased to 71.9 percent.

[17] Real income is deflated using 1996 dollars.

Table 1: Unit ownership of households in TPS estates over time

Year	1996	2001	2006	2011	2016
Share of HHs in unsold TPS units	100.0%	68.9%	42.6%	35.7%	28.1%
Share of HHs in sold TPS units	0.0%	31.1%	57.4%	64.3%	71.9%
TPS premium unpaid, Owner-occupied	0.0%	31.1%	55.6%	62.5%	70.9%
TPS premium unpaid, Rented	0.0%	0.0%	1.8%	1.4%	0.1%
TPS premium paid, Owner occupied	0.0%	0.0%	0.0%	0.3%	0.5%
TPS premium paid, Rented	0.0%	0.0%	0.0%	0.1%	0.4%
Number of households	185962	185641	181876	180022	177413

Notes: Table decomposes ownership status by household in TPS estates. Source: Hong Kong Population Census.

Second, nearly 99 percent of sold TPS units were owner-occupied with their premium unpaid. This implies that only a tiny proportion of sold TPS units were either rented out or resold on the open market, since the premium must be paid before a TPS owner could sell, let, assign, or otherwise alienate the unit on the open market. The number of transactions in HOS Secondary Market was also small, as previously shown in Section 2.3. This suggests that most purchasing households did not move away for many years.

Third, the number of households residing in TPS estates fell from roughly 186,000 in 1996 to 177,000 in 2016. Since the number of units in these estates did not change during this time, this decline anticipates our finding below in Section 5 that the TPS reduced the population and number of households in treated estates.

4.3 Who Became TPS Owners?

Evidence suggests that avoidance of regular income tests motivated households to purchase TPS units. Table 2 shows mean household characteristics in TPS estates in 2006, respectively for residents in sold and unsold TPS units. Larger and higher-income households, for whom these rules were more binding, were more likely to live in sold TPS units.[18] By contrast, households whose members are all over 60 years old and therefore not subject to means testing requirements are less likely to live in sold TPS units. A government study in 2001 similarly reported that "the

[18] See also Online Appendix Figure A4, which plots the distribution of household incomes for sold and unsold units in 2006 for each household size.

Table 2: HH characteristics, sold and unsold units in TPS estates, 2006

	Sold units	Unsold units	Standardized difference
HH size	3.52	2.91	0.45
	(1.3)	(1.36)	
HH income	18668	12853	0.49
	(13157)	(10304)	
Working persons per HH	1.84	1.24	0.54
	(1.16)	(1.09)	
HH with all 60+ y. o.	0.06	0.15	-0.29
	(0.24)	(0.36)	
Single-person	0.06	0.18	-0.36
Nuclear family	0.76	0.71	0.12
Extended family	0.38	0.32	0.17
Non-family	0.08	0.07	0.02
HH size = 1	0.06	0.18	-0.36
HH size = 2	0.16	0.23	-0.16
HH size = 3	0.25	0.25	0.01
HH size = 4	0.32	0.24	0.18
HH size = 5	0.15	0.08	0.23
HH size = 6+	0.06	0.03	0.11
Number of HHs	101112	80764	

Notes: Table shows mean household characteristics in TPS estates in 2006, respectively for TPS buyers and non-buyers.

sale results of TPS flats were better among households who were paying additional rent, of larger size and with non-elderly members" (Housing Authority 2001). Yeung (2001) presents survey evidence that fear of paying extra rent was an important motivator for TPS purchases.

Another piece of evidence comes from the Official Proceedings of Hong Kong's Legislative Council. On October 31, 2012, Council member Wong Kwok-kin made the following remark while lobbying the government to expand TPS:

> Many well-off tenants want to buy their own flats through the TPS so as to avoid the trouble of paying double rent or undergoing random checking. However, many well-off tenants are not sitting tenants in the dozens of TPS estates. Therefore, I

> would like to ask the Secretary: Whether the authorities will study and consider the proposal of giving well-off tenants not living in the existing TPS estates the option to buy PRH flats if they have such a need? (GovHK 2012)

The above evidence confirm that households bought TPS units purely to avoid income tests.

5 Effects of TPS on Estate-level Outcomes

In this section, the effects of TPS are estimated using its staggered and incomplete rollout across housing estates. The estimates reveal that TPS reduced user costs, total population, and average household size in the treated estates, but increased average household income.

5.1 Empirical Strategy

To identify the effects of TPS on estate-level outcomes, I leverage the staggered and incomplete roll-out of the program across estates in a dynamic difference-in-differences design.

The analysis sample includes all 39 treated estates and 43 control estates, chosen as follows. I take all public rental housing estates where residents did not become eligible for TPS. Since the estates chosen for TPS tend to be more recently built, I exclude all estates with any buildings constructed before 1980, to ensure that the control estates had similar building features and resident populations. I also exclude all estates with any buildings constructed after 1996, so that our estimates are not contaminated by influxes of new residents upon the completion of new construction.[19]

I then estimate the following equation:

$$y_{et} = \sum_{\tau \in \mathcal{T}} \beta_\tau \left(T_e \times 1_{t=t_e^*+\tau} \right) + \delta_e + \delta_t + \varepsilon_{et},$$

where e indexes estates, $t \in \{1996, 2001, 2006, 2011, 2016\}$ is the Census year, y_{et} is an estate-

[19] Online Appendix Table A2 and A3 displays the sample restrictions and lists the chosen estates. Building construction years are collated from four sources: (1) data.gov.hk; (2) Wikipedia; (3) website of the Housing Society; and (4) website of the Housing Authority.

level outcome variable, T_e indicates whether estate e was ever treated, t_e^* is the first Census year following treatment for estate e, $\tau \in \mathscr{T} \equiv \{-10, 0, 5, 10, 15\}$ indexes the year relative to t_e^*, and δ_e and δ_t denote estate and year fixed effects. This equation includes year fixed effects and thus controls for confounding city-wide changes in the housing market that contaminates previous estimates of the effects of the TPS program (e.g. Ho and Wong 2006).

Since the timing of TPS introduction was staggered across estates, my main specification uses the interaction-weighted estimator proposed by Sun and Abraham (2020), which computes an average of the cohort-specific average treatment effect on the treated estates, weighted by the shares of each cohort.[20] Standard errors clustered at the estate level are reported.

The β_τ coefficients identify the causal effect of TPS under the assumption that the outcomes of treated estates would have evolved in parallel to those of control estates in the absence of treatment. It is possible to check for pre-treatment trends, since two pre-treatment Census years are available for the later cohort of treated estates. As shown below, the estimates consistently reveal an absence of pre-treatment trends.

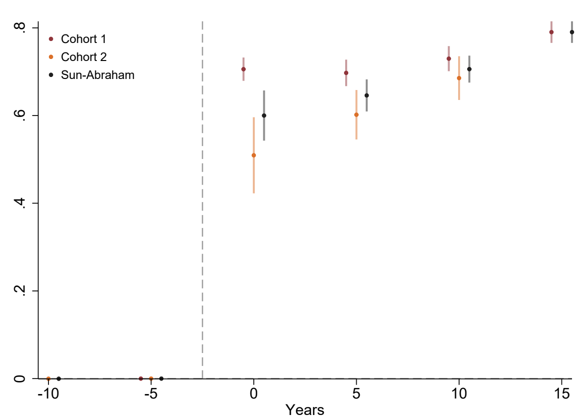
The treatment and control estates are broadly similar in pre-treatment characteristics. Each estate houses roughly 4,500 households, or a population of roughly 18,000. As shown in Online Appendix Figure A3, the treated and control estates are evenly dispersed across Hong Kong. Online Appendix Tables A4-A6 provide detailed comparisons of the pre-treatment characteristics of treated and control estates. Their average household incomes, average number of working people per household, and average rents are highly similar. However, treated estates have larger populations and larger average household sizes than control, suggesting that there remain systematic differences between the treated and control estates. The estates that are treated in the earlier cohort are also very similar to those in the later cohort.

For robustness, I report cohort-specific estimates where observations are reweighted using entropy-balancing (Hainmueller 2012), with two goals in mind. First, reweighting the data so that that treated and control estates have the same pre-treatment average household size and average household income enables us to gauge whether observed pre-treatment differences in

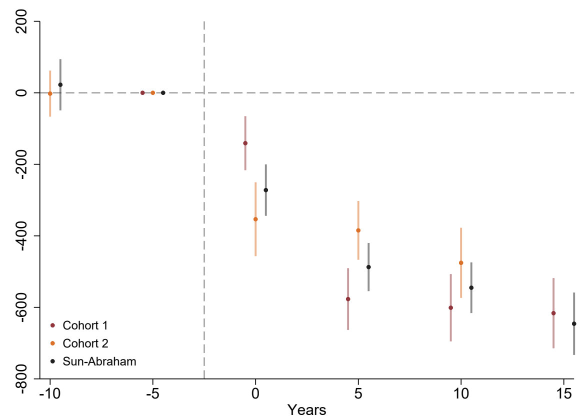
[20]This specification ensures that estimates are not contaminated by treatment effects from other periods when treatment is staggered (Callaway and Sant'Anna 2020; de Chaisemartin and D'Haultfœuille 2020).

Figure 2: TPS take-up and Effects of TPS on user cost

(a) Share of HHs with purchased TPS unit



(b) Average real user cost



Notes: The black series plots coefficients from the interaction-weighted estimator in Sun and Abraham 2020. The maroon and yellow series plots cohort-specific coefficients, estimated with entropy balancing weights (Hainmueller 2012) that are based on estate-level average household size and income in 1996. Sample is all estates where all buildings were built after 1979 and before 1996. Year 0 denotes the first observed Census year following treatment. Confidence intervals at the 95% significance level (clustered at the estate level) are shown. Online Appendix Table A7 and A8 displays coefficients and pre-treatment means.

estate characteristics lead to selection bias. Second, cohort-specific estimates allow us to gauge whether the effects were similar across the cohorts. As reassuringly shown below, cohort-specific estimates using entropy-balancing weights are highly similar to the main estimates.

Figure 2-7 show the effects of TPS on estate-level outcomes. Within each panel, the black series plots coefficients from the Sun-Abraham interaction-weighted estimator. The maroon and yellow series plots cohort-specific estimates using entropy-balancing weights, as described above. Year 0 denotes first observed Census year following treatment.

5.2 Program Uptake and Effects on Housing Costs

Figure 2 Panel (a) shows that the share of households residing in sold TPS units immediately rose by 60 percent once residents became eligible to purchase TPS units in Year 0. This share eventually reached 79 percent higher than control in Year 15.

Panel (b) shows that the average user cost—defined as the sum of monthly rental and mortgage payments—fell dramatically in the TPS estates relative to control. Average user cost fell by \$272 (HKD), or 22 percent relative to 1996 levels, in treated estates by Year 0. The decline deepened and reached \$646, or 51 percent lower by Year 15. These estimates imply that mortgage payments were lower than counterfactual rent payments immediately after the rollout of TPS and further diverged over time.

5.3 Effects on the Distribution of Household Incomes

Figure 3 Panel (a) shows that by Year 0, average real household income in treated estates rose by \$1132 (HKD) per month, or 7 percent relative to the 1996 mean in treated estates. Average real household income continued to diverge between treatment and control estates. By Year 15, average real monthly household income was \$3712 (or 23 percent) higher in treated estates.

Panel (b) shows that the average number of working members per household also rose. By Year 5, the average number of working members per household in treated estates increased by 0.21 (or 13 percent). This positive effect persisted until Year 15, where working members were 0.22 more than treated-estate families. Once again, these estimated effects do not appear to be driven by pre-existing trends or selection of estates into treatment.

The estimated effects on the average incomes are unlikely to be driven by pre-existing trends or selection of estates into treatment. In both of the above panels, we do not detect pre-treatment trends in Year -10. The cohort-specific estimates using entropy-balancing weights are also highly similar to the Sun-Abraham estimates, even though they are less precise.

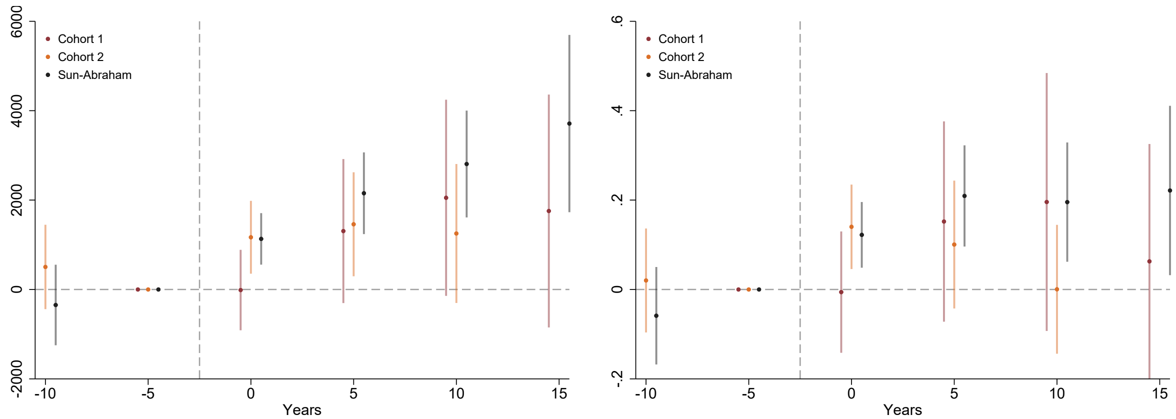
Figure 4 plots the effect of TPS on the share of households within household income bins.[21] The figure reveals that the share of households with incomes much lower than the 2 times PRH income limit dramatically fell in treated estates, while the share of households with incomes both above and slightly below the PRH limit increased.

The lack of a discontinuous response at the cutoff is consistent with the fact that public renter households did not appear to bunch around the income limit even before treatment, as shown in Online Appendix Figure A8. One possible reason is that optimization frictions prevented bunching just below the very large rent notch since it is difficult to coordinate among household members. Another possible reason is measurement error. Consistent with the latter,

[21] This exercise relates to a growing literature on bunching at tax kinks, tax notches, and wage floors (Saez 2010; Kleven and Waseem 2013; Kleven 2016; Cengiz et al. 2019; Blomquist et al. 2021).

Figure 3: Effects of TPS on estate average HH income

(a) Average real HH income (b) Average working members per HH



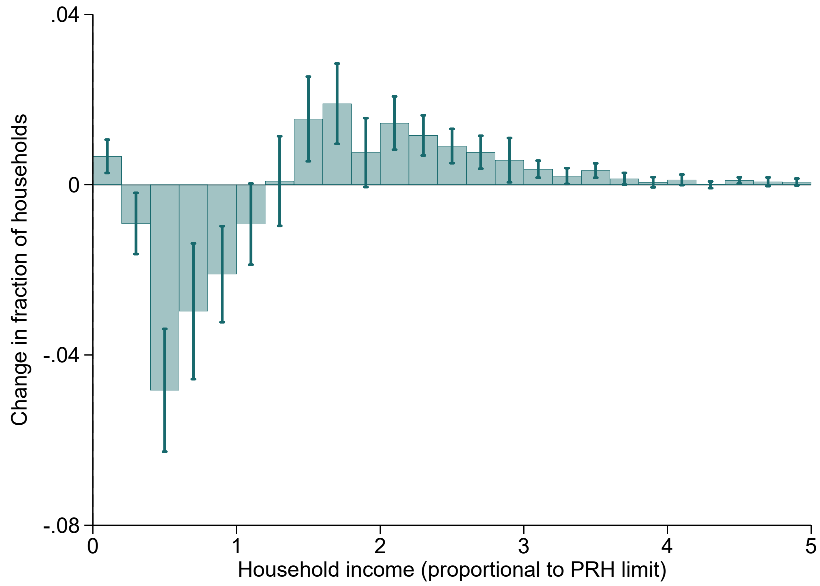
Notes: The black series plots coefficients from the interaction-weighted estimator in Sun and Abraham (2020). The maroon and yellow series plots cohort-specific coefficients, estimated with entropy balancing weights (Hainmueller 2012) that are based on estate-level average household income and working members in 1996. Sample includes all estates where all buildings were built after 1979 and before 1996. Year 0 denotes first observed Census year following treatment. Confidence intervals at the 95% significance level (clustered at the estate level) are shown.

I observe bunching at round numbers in the data, especially for one-person households, which may obscure bunching.

6 Mechanisms and Implications

The previous section showed that TPS increased average household income in treated estates. According to the conceptual framework in Section 3, the changes in average income can be due to at least three channels: (1) changes in labor supply, (2) changes in household-level residential sorting, or (3) changes in co-residence patterns within extended family networks. Although it is not possible to fully disentangle these mechanisms, since individuals cannot be traced across Census waves, this section provides suggestive evidence that changes in co-residence patterns within family networks are the primary driver of the observed effects. It then leverages the estimates to quantify the contribution of TPS to the subsequent emergence of tiny subdivided units in Hong Kong.

Figure 4: Effect of TPS on HH income distribution



Notes: Figure plots the effect of TPS on the share of households within a given household income bin in the second Census following treatment relative to that of the last Census year before treatment, estimated using the interaction-weighted estimator in Sun and Abraham (2020). Income is normalized to be a proportion of the PRH income limit for the relevant household size. Confidence intervals at the 95% significance level (clustered at the estate level) are shown.

6.1 Changes in Labor Market Characteristics

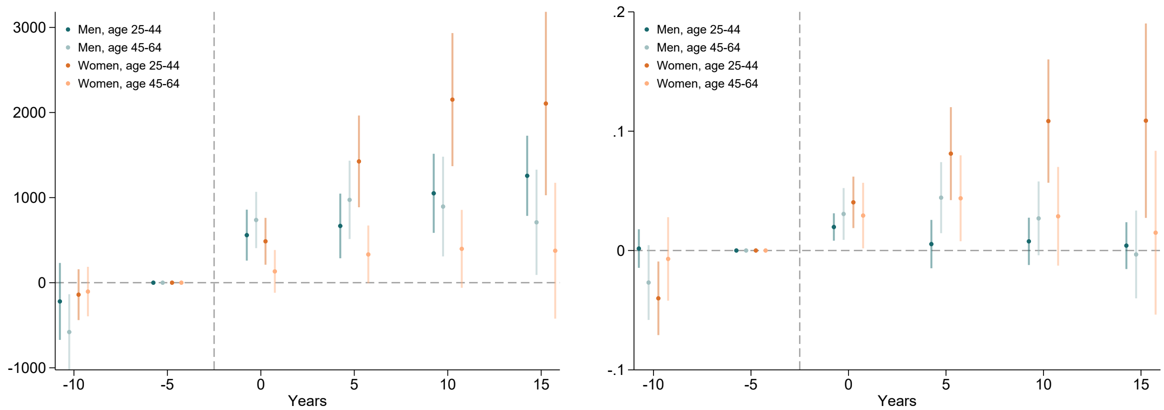
The possibility that the increases in average household incomes reflect increased labor supply is first investigated. Specifically, the effects of TPS on average incomes and employment within four working-age demographic groups are examined. In all demographic groups, average incomes increased. The increases were especially large for younger adults. However, it is found that their average years of schooling also substantially increased. This suggests that the increases in income are not fully attributable to labor supply responses, but instead partly attributable to the fact that TPS induced higher human capital residents to stay.

Figure 5 Panel (a) shows that, by Year 10, the average income of younger women (aged 25-44) rose by $2151 (HKD), or 54 percent of the pre-treatment average, which is the largest increase in all groups. By contrast, the average income of men in the same age group only rose by $1050, or about 11 percent. For older men and women between 45 and 64 years old, average

Figure 5: Effects of TPS on estate-level personal income and employment, by demographic group

(a) Average personal income

(b) Employment rate



Notes: The series plots coefficients from the interaction-weighted estimator in Sun and Abraham (2020) based on estate-level average personal income and employment rate among demographic groups. The orange plots represent women while blue plots represent man. The older is displayed in lighter colors and the young is shown in darker ones. Sample is all estates where all buildings were built after 1979 and before 1996. Year 0 denotes first observed Census year following treatment. Standard errors (clustered at the estate level) are shown in bars. Online Appendix Table A9 and A10 display coefficients and pre-treatment means.

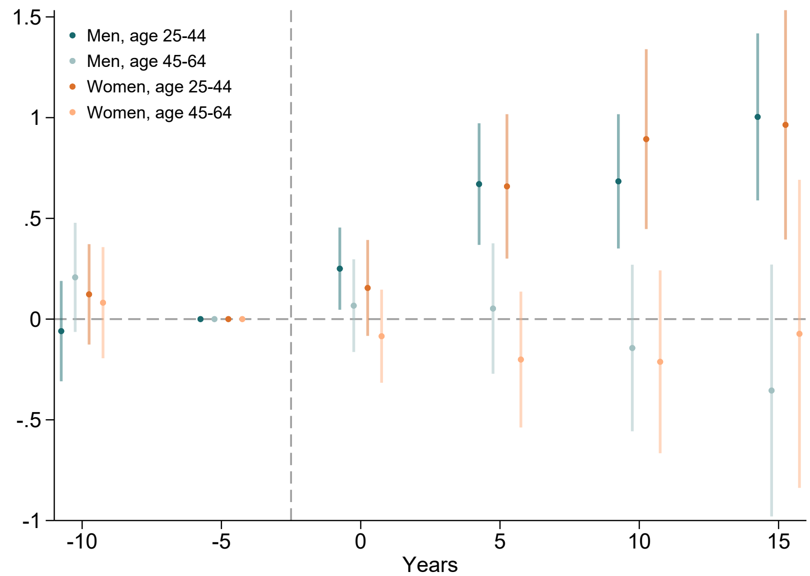
incomes rose by \$895 and \$398, or 13 percent and 20 percent, respectively.

The increases in average income of young adults coincided with increases in employment, as shown in Panel (b). All working-age adults experienced an immediate increase in employment rate. The increase intensified over time for younger women, for whom the employment rate rose by 4 p.p. (or 8 percent) in Year 0, and eventually increased by 11 p.p. (or 23 percent) in Years 10 and 15. For other demographic groups, however, the increase in employment did not persist. Young men and older women experienced statistically significant increases in Years 0 and 5, but the increases are no longer significant after Year 10.

While the observed increases in income and employment are likely to *partly* reflect endogenous changes in labor supply, labor supply responses are very unlikely to account for the *entire* increase, for four reasons. First, the observed effects on income are much larger typical estimates of labor supply effects.[22] Second, as shown previously in Section 5.2, housing user costs sharply fell, so households did not feel pressure to increase labor supply in order to meet

[22]For instance, Jacob (2004) finds that public housing in the USA reduced individual labor supply by 5 percent, while we find that TPS increased household income by 23 percent.

Figure 6: Impact of TPS on estate-level average schooling year, by demographic group



Notes: The series plots coefficients from the interaction-weighted estimator in Sun and Abraham (2020) based on estate-level average individual years of schooling among demographic groups. The orange plots represent women while blue plots represent men. The older is displayed in lighter colors and the young is shown in darker ones. Sample is all estates where all buildings were built after 1979 and before 1996. Year 0 denotes first observed Census year following treatment. Confidence intervals at the 95% significance level (clustered at the estate level) are shown. Online Appendix Table A11 displays coefficients and pre-treatment means.

debt obligations. Third, as shown below, there were significant increases in average schooling among residents, which are likely to account for part of the increase in income. Fourth, as argued below, TPS had little impact on spatial mismatch in the treated estates.

The fact that TPS induced a large increase in the average years of schooling among young adults can be seen in Figure 6. By Year 10, the average schooling for younger women and younger men increased by 1 and 0.7 years, respectively. Specifically, the share of younger adults who are high-school dropout significantly declined, while the share of younger adults with some college experience and advanced degrees increased (see Online Appendix Figure A9). The average years of schooling for older adults remained unchanged.

These increases in average schooling most likely reflect the fact that younger residents with more schooling became more likely to stay in the estates, while younger residents with less schooling became more likely to move out. Since I focus on residents aged 25-44, the vast majority have already past their schooling age. As such, it is unlikely that they are induced to dramatically increase in their human capital investment in response to the policy.

Moreover, TPS did not detectably reduce the average commute times of working people in treated estates, as shown in Online Appendix Figure A10. Previous studies have shown that public housing in Hong Kong, both rental and ownership, features significant spatial misallocation due to rationing, as exhibited by larger commuting distances of its residents relative to private-sector counterparts (Lui and Suen 2011). Consistent with highly limited residential mobility both before and after the subsidized sale, the lack of response in commute times suggests that TPS did not meaningfully alter the degree of spatial misallocation, despite the reconfiguration of household members within family networks.

6.2 Changes in Population and Household Size

Not only did average schooling increase in treated estates, average population and household sizes fell. Figure 7 Panel (a) shows that total population in treated estates immediately declined by 5 percent. This reduction was persistent and reached 7 percent lower than control in Year 15. Since the total population in TPS estates in 1996 was roughly 733,000, these estimates imply that the total population in TPS estates fell by roughly 51,000.

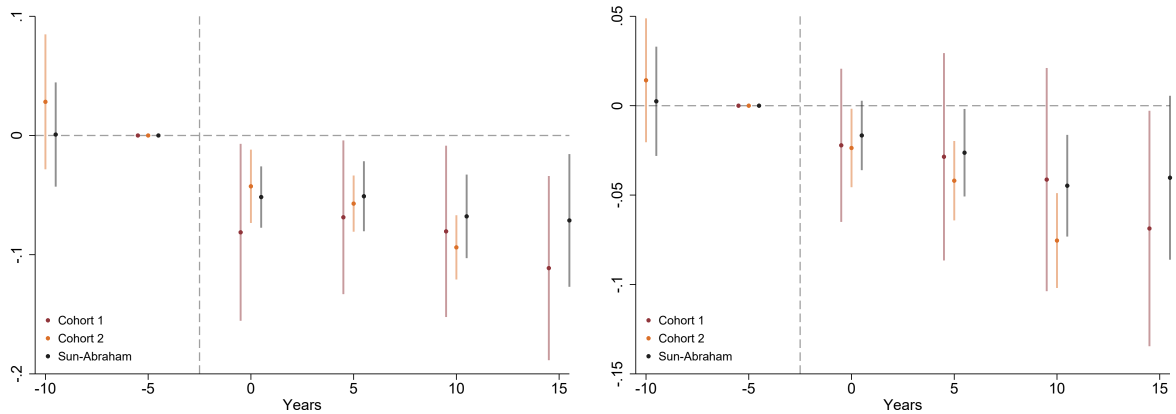
Panel (b) shows that average household size in treated estates immediately declined by 1.7 percent. This decline widened over time, eventually reaching 4.1 percent lower than control, in Year 15. The shares of households with one, two, or three members rose, while the shares of households with four, five, or six members fell.

The number of households in treated estates also immediately and persistently declined by roughly 2-3 percent. This decline in the number of households suggests housing units became underutilized as a consequence of TPS sales. These estimates imply that the total number of households in TPS estates fell by roughly 5,600 by Year 15 (see Online Appendix Table A8).

The reduction in population in the treated estates is concentrated on cohorts that were born after 1960. Cohorts born before 1960 did not experience changes in population (Online Ap-

Figure 7: Effects of TPS on estate composition

(a) Log population (b) Log average HH size



Notes: The black series plots coefficients from the interaction-weighted estimator in Sun and Abraham (2020). The maroon and yellow series plots cohort-specific coefficients, estimated with entropy balancing weights (Hainmueller 2012) that are based on estate-level average log population and household size in 1996. Sample is all estates where all buildings were built after 1979 and before 1996. Year 0 denotes first observed Census year following treatment. Confidence intervals at the 95% significance level (clustered at the estate level) are shown. Online Appendix Table A8 displays coefficients and pre-treatment means.

pendix Figure A7). This finding is consistent with evidence that older households were less likely to purchase TPS units (Section 4.3).

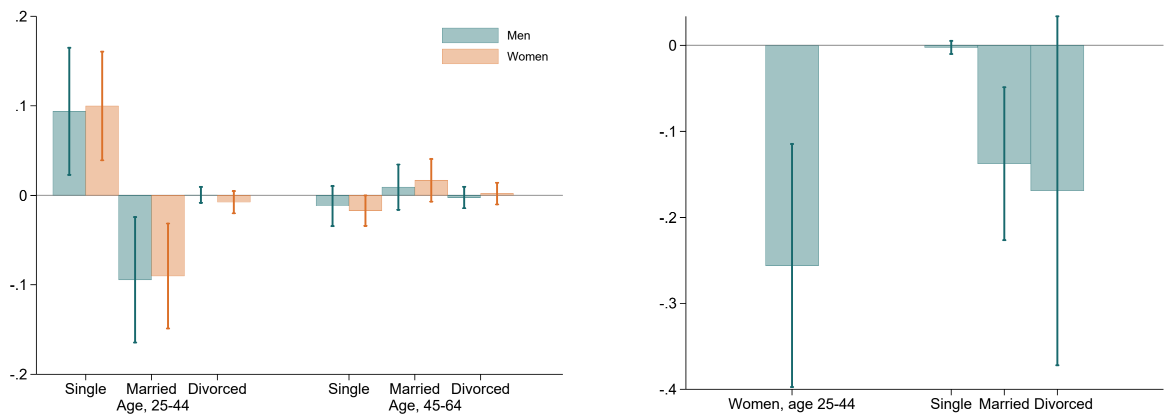
6.3 Changes in Household Structure

This subsection provides further evidence that the observed changes reflect changes in population sorting. Specifically, there was a very large increase in never-married residents. There was also a dramatic drop in the number of children among married and divorced women. These large changes are very unlikely to be driven by changes in marriage and fertility behavior, but rather are likely to reflect changes in population sorting within extended family networks.

Figure 8 Panel (a) shows that the share of younger women who are married sharply fell, while the share of young women who are single correspondingly increased. The share of younger women (aged 25-44) who are single increased by 10 p.p. (or 50 percent) by Year 5, and 14 p.p. (or 70 percent) by Year 15. The share of younger women who are married significantly decreased, dropping by 9 p.p (or 12 percent) by Year 5 and widening to 11 p.p (or 14 percent) by Year 15. At the same time, the share of younger men who are married saw a

Figure 8: Effects of TPS on share of married persons and number of children in treated estates

(a) Marital status (b) Number of children



Notes: Panel A plots the effect of TPS on single rates, marriage rates and divorce rates among different groups in the second Census following treatment relative to that of the last Census year before treatment, estimated using the interaction-weighted estimator in Sun and Abraham (2020). Panel B plots the effect of TPS on number of children among young women groups in different marital status in the second Census following treatment relative to that of the last Census year before treatment, estimated using the interaction-weighted estimator in Sun and Abraham (2020). Confidence intervals at the 95% significance level (clustered at the estate level) are shown. Online Appendix Table A12 and A13 displays coefficients and pre-treatment means.

similar decline of 9 p.p (or 13 percent) by Year 5 and eventually decreased by 14 p.p by Year 15. Among older age groups (45-64 years), the shares of men and women who are single, married, and divorced, respectively remained largely unchanged (see Online Appendix Figures A11).

The average number of children of younger adult residents also declined.[23] Among younger men and women, the average number of children has decreased by 0.27 and 0.26 (or 21 and 16 percent) by Year 5, and further decreased to 0.40 and 0.36 (or 32 and 23 percent), respectively, by Year 15 (Online Appendix Table A13). Figure 8 Panel (b) shows that the number of children among married and divorced young women declined by a similar magnitude. Furthermore, in Year 5, the share of extended-family households fell by 2.9 percentage points, while the share of single and nuclear family households rose by 0.8 and 1.7 percentage points, respectively (see Online Appendix Figure A6).

For several reasons, the effects of the treatment are most likely due to changes in co-residence choices within family networks. First, as previously argued in Sections 2.3 and 4.2,

[23] Here, children is defined as all offspring residing with the parent.

exceedingly few TPS households gained the right to lease or sell units, so residence in almost all PRH and TPS units is restricted to family members. Second, household-level mobility is exceedingly low. The share of household heads who moved in the past five years in the baseline year of 2001 was roughly 14 percent. Third, there was no detectable change in household-level mobility in the treated estates, relative to the control estates (see Online Appendix Table A8 Column (8)). Fourth, as previously shown, there was a large decline in households with extended family members.

6.4 Contribution to Subdivided Unit Crisis

In the decade after the end of the Tenants Purchase Scheme, Hong Kong gained international notoriety for the fact that its low-income populations became increasingly cramped into tiny subdivided units. These units were typically less than 15 square meter in size and have poor hygiene and safety conditions. According to the Population Census in 2016, there were 91,787 such units. The proliferation of these units has been universally condemned and is generally attributed to a shortage of public rental units. Between 2011 and 2019, as subsidized units proliferated, the average wait time for PRH applicants rose from 2.0 to 5.5 years.

The contribution of the Tenants Purchase Scheme to the subdivided unit crisis can be quantified using back-of-the-envelope calculations leveraging my estimates. Specifically, TPS caused two types of units to become unavailable to low-income households who qualify for PRH units: the units that became unoccupied, and the units that became instead allocated to high-income households that do not qualify. A conservative estimate of this number (ignoring labor supply effects) can be formed by multiplying the number of units eligible for TPS purchases by the estimated effect of TPS on the share of households with incomes above two times the PRH income limit. The estimated number is $183700 \times 0.081 = 14880$. In other words, TPS can account for at least $14880/91787 = 16\%$ of subdivided units in 2016.

7 Conclusion

Much of the existing literature on housing assistance emphasizes the economic advantages of subsidized ownership over subsidized rental programs. This study reveals a critical trade-off: the removal of regular income testing may significantly undermine the targeting of housing subsidies to low-income populations. Leveraging Hong Kong's Tenants Purchase Scheme—a natural experiment converting 183,700 subsidized rentals to ownership units—I document that the scheme resulted in substantial population re-sorting. Using a difference-in-differences design, I find that treated estates experienced a 23 percent rise in average household income, a doubling in the share of households exceeding income eligibility thresholds, and a one-year increase in young adults' schooling. Concurrently, household size and population declined by 5–7 percent, alongside a sharp rise in childless and single adults.

These shifts cannot be attributed to changes in labor supply or human capital investment alone. Instead, persistently low household head residential mobility suggests that extended family networks strategically reorganized in response to the scheme: higher-income family members retained access to subsidized units, while lower-income family members exited to receive additional public housing units. The resulting reduction in the availability of subsidized units for low-income populations was a significant contributor to the subsequent subdivided unit crisis. These results underscore that regular income tests are important for efficiently targeting housing subsidies. In choosing whether to offer subsidized ownership or rental housing, policymakers must weigh the purported efficiency gains of ownership programs against the erosion of welfare objectives that the removal of regular income tests may entail.

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A Online Appendix

Table A1: Public rental housing (PRH) income limit in 2006

Household Size	**Income Limit (HKD)**
1 Person	6800
2 Person	10300
3 Person	12100
4 Person	14600
5 Person	16300
6 Person	18100
7 Person	19700
8 Person	21300
9 Person	22400
10 Person and above	23900

Table A2: Sample restrictions

	Treated	Control
All estates observed in Census years 1996-2016	39	97
No construction after 1996	39	72
No construction before 1980	39	43

Notes: Table counts the number of estates identified in the data and after imposing sample restrictions.

Table A3: List of estates

Treated estates, Cohort 1	Treated estates, Cohort 2	Control estates	
Cheung On Estate	Yiu On Estate	Ap Lei Chau Estate	Lower Wong Tai Sin (2) Estate
Choi Ha Estate	Cheung Fat Estate	Butterfly Estate	Lung Hang Estate
Chuk Yuen North Estate	Cheung Wah Estate	Chak On Estate	Mei Lam Estate
Fu Heng Estate	Fu Shin Estate	Cheung Hang Estate	On Ting Estate
Fung Tak Estate	Hing Tin Estate	Choi Fai Estate	On Yam Estate
Fung Wah Estate	King Lam Estate	Choi Yuen Estate	Sam Shing Estate
Heng On Estate	Kwai Hing Estate	Chuk Yuen South Estate	Sha Kok Estate
Hin Keng Estate	Kwong Yuen Estate	Chun Shek Estate	Shek Wai Kok Estate
Kin Sang Estate	Lei Cheng Uk Estate	Hau Tak Estate	Shun Tin Estate
Tai Wo Estate	Lei Tung Estate	Hing Man Estate	Siu Sai Wan Estate
Tak Tin Estate	Leung King Estate	Jat Min Chuen	Sun Chui Estate
Tin King Estate	Long Ping Estate	Ka Fuk Estate	Sun Tin Wai Estate
Tin Ping Estate	Lower Wong Tai Sin (1) Estate	Ka Wai Chuen	Tai Yuen Estate
Tsui Wan Estate	Nam Cheong Estate	Kai Yip Estate	Tin Shui (1) Estate
Wah Kwai Estate	Po Lam Estate	Kwong Fuk Estate	Tin Shui (2) Estate
Wah Ming Estate	Pok Hong Estate	Kwong Tin Estate	Tin Yiu (1) Estate
Wan Tau Tong Estate	Shan King Estate	Kwun Tong Garden Estate	Tin Yiu (2) Estate
Yiu On Estate	Tai Ping Estate	Lai Kok Estate	Tsz Man Estate
	Tsing Yi Estate	Lai On Estate	Wang Tau Hom Estate
	Tsui Lam Estate	Lee On Estate	Wu King Estate
	Tsui Ping North Estate	Lok Wah North Estate	Yiu Tung Estate
	Tung Tau (2) Estate	Lok Wah South Estate	

Notes: Table tabulates all estates included in analysis.

Table A4: Estate characteristics, treated vs control estates, 1996

	Treated estates	Control estates	Normalized difference
Year built	1989	1986	0.57
	(2)	(5)	
Population	18794	15318	0.5
	(7722)	(6232)	
Number of HHs	4768	4167	0.33
	(1965)	(1639)	
Average HH size	4.0	3.7	0.89
	(0.3)	(0.4)	
Working persons per HH	1.6	1.6	-0.04
	(0.3)	(0.2)	
Average HH income	16221	16323	-0.04
	(2782)	(2307)	
Average rent	1255	1297	-0.17
	(180)	(281)	
HH with all 60+ y. o.	0.07	0.09	-0.39
HH above 1.5X rent cutoff	0.10	0.12	-0.33
HH above 2X rent cutoff	0.02	0.03	-0.18
Average commute time (minutes)			
Male, 25-44 year old	18.9	17.5	0.37
Female, 25-44 year old	15.3	15.0	0.11
Male, 45-64 year old	17.8	16.4	0.39
Female, 45-64 year old	14.1	13.2	0.35
Number of estates	39	43	

Notes: Table shows mean estate characteristics in 1996, respectively for TPS and non-TPS estates.

Table A5: Incomes and schooling by demographic groups, treated vs control estates, 1996

	Treated estates	Control estates	Normalized difference
Average individual income			
Male, 25-44 year old	9815	9731	0.14
	(534)	(633)	
Female, 25-44 year old	3985	4566	-0.38
	(1657)	(1419)	
Male, 45-65 year old	6830	6937	-0.11
	(936)	(985)	
Female, 45-65 year old	1959	1994	-0.07
	(512)	(528)	
Years of schooling			
Male, 25-44 year old	8.61	8.93	-0.43
	(0.75)	(0.73)	
Female, 25-44 year old	8.10	8.26	-0.19
	(0.87)	(0.84)	
Male, 45-65 year old	6.64	6.42	0.29
	(0.77)	(0.74)	
Female, 45-65 year old	4.82	4.60	0.24
	(0.94)	(0.88)	
Number of estates	39	43	

Notes: Table shows mean estate characteristics in 1996, respectively for TPS and non-TPS estates.

Table A6: Estate characteristics, treatment vs weighted controls, 1996, by treatment cohort

	Cohort 1			Cohort 2		
	Treated estates	Control estates	Standardized difference	Treated estates	Control estates	Standardized difference
Year built	1989	1989	0	1988	1988	0
	(1)	(5)		(2)	(5)	
Population	18576	15544	0.47	18980	15945	0.44
	(7603)	(5207)		(8005)	(5420)	
Number of HHs	4636	3889	0.46	4882	4072	0.46
	(1876)	(1310)		(2077)	(1369)	
Average HH size	4.0	4.0	0	3.9	3.9	0
	(0.2)	(0.4)		(0.3)	(0.4)	
HH with all 60+ y. o.	0.06	0.04	0.61	0.07	0.05	0.43
	(0.04)	(0.03)		(0.05)	(0.04)	
Working persons per HH	1.63	1.68	-0.18	1.61	1.65	-0.14
	(0.26)	(0.29)		(0.28)	(0.27)	
Average HH income	16360	16355	0	16103	16048	0.02
	(2722)	(2689)		(2894)	(2466)	
Average rent	1278	1328	-0.23	1236	1279	-0.18
	(147)	(279)		(206)	(262)	
HH above 1.5X rent cutoff	0.10	0.10	0.03	0.10	0.10	0.04
	(0.05)	(0.05)		(0.05)	(0.04)	
HH above 2X rent cutoff	0.02	0.02	0.23	0.02	0.02	0.15
	(0.01)	(0.01)		(0.01)	(0.01)	
Number of estates	18	43		21	43	

Notes: Table shows mean estate characteristics in 1996, separately for the two treated cohorts and their respective controls, whose means are computed with entropy balancing weights (Hainmuller 2012) that are based on estate-level average household size and income in 1996.

Table A7: Effect of TPS on average user cost

t = -10	22.56
	(36.57)
t = 0	-272.06**
	(36.74)
t = 5	-487.30**
	(34.38)
t = 10	-545.04**
	(36.21)
t = 15	-645.81**
	(44.54)
Treated mean, 1996	1255
R2	0.95
Num. of estate-years	410
Num. of estates	82

Notes: Table shows coefficients from the interaction-weighted estimator in Sun and Abraham (2020). Sample is all estates where all buildings were built after 1979 and before 1996. Year 0 denotes first observed Census year following treatment. Standard errors (clustered at the estate level) are shown, with $\sim$ = significant at the 10% level, * = significant at the 5% level, and ** = significant at the 1% level.

Table A8: Effect of TPS on estate HH composition

	(1) Share of TPS units sold	(2) Log population	(3) Log num. of HH	(4) Log average HH size	(5) Share of single-person HH	(6) Share of nuclear family HH	(7) Share of extended family HH	(8) Share of HH moved in last 5 years
t = -10	0.00~	0.00	-0.002	0.003	0.002	-0.004	0.001	-0.13~
	(0.00)	(0.02)	(0.018)	(0.016)	(0.009)	(0.009)	(0.008)	(0.07)
t = 0	0.60**	-0.05**	-0.035**	-0.017~	0.001	0.018*	-0.021**	-0.01
	(0.03)	(0.01)	(0.009)	(0.010)	(0.005)	(0.007)	(0.007)	(0.05)
t = 5	0.65**	-0.05**	-0.025*	-0.027*	0.008	0.017~	-0.029**	0.01
	(0.02)	(0.01)	(0.010)	(0.013)	(0.007)	(0.010)	(0.009)	(0.05)
t = 10	0.71**	-0.07**	-0.023*	-0.046**	0.016~	-0.008	-0.012	0.02
	(0.02)	(0.02)	(0.010)	(0.015)	(0.008)	(0.012)	(0.012)	(0.05)
t = 15	0.79**	-0.07*	-0.031~	-0.041~	0.011	0.006	-0.020	0.00
	(0.01)	(0.03)	(0.017)	(0.024)	(0.012)	(0.021)	(0.021)	(0.10)
Treated mean, 1996	0.00	18794	4768	3.96	0.07	0.70	0.22	0.14
R2	0.98	0.99	1.00	0.94	0.87	0.88	0.81	0.50
Num. of estate-years	410	410	410	410	410	410	410	410
Num. of estates	82	82	82	82	82	82	82	82

Notes: Table shows coefficients from the interaction-weighted estimator in Sun and Abraham (2020). Sample is all estates where all buildings were built after 1979 and before 1996. Year 0 denotes first observed Census year following treatment. Treated estate mean in 1996 for population, number of households, and household size are reported without taking logs. Standard errors (clustered at the estate level) are shown, with ∼ = significant at the 10% level, * = significant at the 5% level, and ** = significant at the 1% level.

Table A9: Effect of TPS on estate HH income distribution

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Average real HH income	Share of HH above 1.5X rent cutoff	Share of HH above 2X rent cutoff	Working persons per HH	Average real personal income			
					Men, 25-44 y.o.	Women 25-44 y.o.	Men, 45-64 y.o.	Women, 45-64 y.o.
t = -10	-347	-0.003	0.000	-0.06	-220	-141	-580*	-104
	(460)	(0.010)	(0.003)	(0.06)	(231)	(152)	(227)	(148)
t = 0	1132**	0.032**	0.009**	0.12**	559**	486**	737**	133
	(294)	(0.005)	(0.002)	(0.04)	(153)	(140)	(169)	(128)
t = 5	2153**	0.065**	0.017**	0.21**	667**	1426**	973**	332~
	(466)	(0.008)	(0.003)	(0.06)	(194)	(275)	(235)	(174)
t = 10	2807**	0.077**	0.019**	0.20**	1050**	2151**	895**	398~
	(610)	(0.009)	(0.003)	(0.07)	(237)	(399)	(299)	(234)
t = 15	3712**	0.081**	0.022**	0.22*	1256**	2105**	710*	376
	(1012)	(0.014)	(0.004)	(0.10)	(240)	(550)	(316)	(407)
Treated mean, 1996	16221	0.102	0.023	1.62	9815	3985	6830	1959
R2	0.71	0.70	0.63	0.67	0.71	0.82	0.63	0.66
Num. of estate-years	410	410	410	410	410	410	410	410
Num. of estates	82	82	82	82	82	82	82	82

Notes: Table shows coefficients from the interaction-weighted estimator in Sun and Abraham (2020). Sample is all estates where all buildings were built after 1979 and before 1996. Year 0 denotes first observed Census year following treatment. Standard errors (clustered at the estate level) are shown in bars, with ~ = significant at the 10% level, * = significant at the 5% level, and ** = significant at the 1% level.

Table A10: Effect of TPS on employment rates, by demographic group

	(1) Men, 25-44 y.o.	(2) Women, 25-44 y.o.	(3) Men, 45-64 y.o.	(4) Women, 45-64 y.o.
t = -10	0.002	-0.04*	-0.03~	-0.01
	(0.008)	(0.02)	(0.02)	(0.02)
t = 0	0.020**	0.04**	0.03**	0.03*
	(0.006)	(0.01)	(0.01)	(0.01)
t = 5	0.005	0.08**	0.04**	0.04*
	(0.010)	(0.02)	(0.02)	(0.02)
t = 10	0.008	0.11**	0.03~	0.03
	(0.010)	(0.03)	(0.02)	(0.02)
t = 15	0.004	0.11**	0.00	0.01
	(0.010)	(0.04)	(0.02)	(0.04)
Treated mean, 1996	0.93	0.48	0.77	0.33
R2	0.60	0.79	0.55	0.58
Num. of estate-years	410	410	410	410
Num. of estates	82	82	82	82

Notes: Table shows coefficients from the interaction-weighted estimator in Sun and Abraham (2020). Sample is all estates where all buildings were built after 1979 and before 1996. Year 0 denotes first observed Census year following treatment. Standard errors (clustered at the estate level) are shown, with $\sim$ = significant at the 10% level, * = significant at the 5% level, and ** = significant at the 1% level.

Table A11: Effect of TPS on average years of schooling, by demographic group

	(1)	(2)	(3)	(4)
	Men, 25-44 y.o.	Women 25-44 y.o.	Men, 45-64 y.o.	Women, 45-64 y.o.
t = -10	-0.06	0.12	0.21	0.08
	(0.13)	(0.13)	(0.14)	(0.14)
t = 0	0.25*	0.15	0.07	-0.08
	(0.10)	(0.12)	(0.12)	(0.12)
t = 5	0.67**	0.66**	0.05	-0.20
	(0.15)	(0.18)	(0.17)	(0.17)
t = 10	0.68**	0.89**	-0.14	-0.21
	(0.17)	(0.23)	(0.21)	(0.23)
t = 15	1.00**	0.96**	-0.35	-0.07
	(0.21)	(0.29)	(0.32)	(0.39)
Treated mean, 1996	8.6	8.1	6.6	4.8
R2	0.91	0.91	0.71	0.82
Num. of estate-years	410	410	410	410
Num. of estates	82	82	82	82

Notes: Table shows coefficients from the interaction-weighted estimator in Sun and Abraham (2020). Sample is all estates where all buildings were built after 1979 and before 1996. Year 0 denotes first observed Census year following treatment. Standard errors (clustered at the estate level) are shown, with $\sim$ = significant at the 10% level, * = significant at the 5% level, and ** = significant at the 1% level.

Table A12: Effect of TPS on marital status, by demographic group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Single				Married				Divorced			
	Men, 25-44 y.o.	Women, 25-44 y.o.	Men, 45-64 y.o.	Women, 45-64 y.o.	Men, 25-44 y.o.	Women, 25-44 y.o.	Men, 45-64 y.o.	Women, 45-64 y.o.	Men, 25-44 y.o.	Women, 25-44 y.o.	Men, 45-64 y.o.	Women, 45-64 y.o.
t = -10	-0.02	0.01	-0.004	0.001	0.03~	0.00	0.006	-0.02*	-0.004	-0.003	-0.004	0.012~
	(0.02)	(0.02)	(0.008)	(0.006)	(0.02)	(0.02)	(0.010)	(0.01)	(0.004)	(0.005)	(0.005)	(0.007)
t = 0	0.03	0.02	-0.011*	-0.010~	-0.03	-0.03	0.010	0.02	-0.001	0.001	0.000	-0.002
	(0.02)	(0.02)	(0.005)	(0.006)	(0.02)	(0.02)	(0.007)	(0.01)	(0.003)	(0.006)	(0.004)	(0.005)
t = 5	0.09**	0.10**	-0.012	-0.017*	-0.09**	-0.09**	0.009	0.02	0.001	-0.008	-0.002	0.002
	(0.04)	(0.03)	(0.011)	(0.009)	(0.04)	(0.03)	(0.013)	(0.01)	(0.005)	(0.006)	(0.006)	(0.006)
t = 10	0.13**	0.12**	-0.003	-0.001	-0.13**	-0.10*	0.003	0.03*	0.001	-0.015*	-0.008	-0.015~
	(0.04)	(0.04)	(0.015)	(0.012)	(0.04)	(0.04)	(0.016)	(0.01)	(0.005)	(0.007)	(0.006)	(0.008)
t = 15	0.14*	0.14*	-0.018	-0.019	-0.14*	-0.11~	0.025	0.08**	-0.005	-0.027**	-0.011	-0.036**
	(0.07)	(0.06)	(0.021)	(0.017)	(0.07)	(0.06)	(0.022)	(0.02)	(0.005)	(0.009)	(0.008)	(0.010)
Treated mean, 1996	0.29	0.20	0.04	0.02	0.69	0.77	0.92	0.83	0.01	0.02	0.02	0.04
R2	0.71	0.72	0.74	0.72	0.73	0.75	0.79	0.79	0.32	0.59	0.61	0.79
Num. of estate-years	410	410	410	410	410	410	410	410	410	410	410	410
Num. of estates	82	82	82	82	82	82	82	82	82	82	82	82

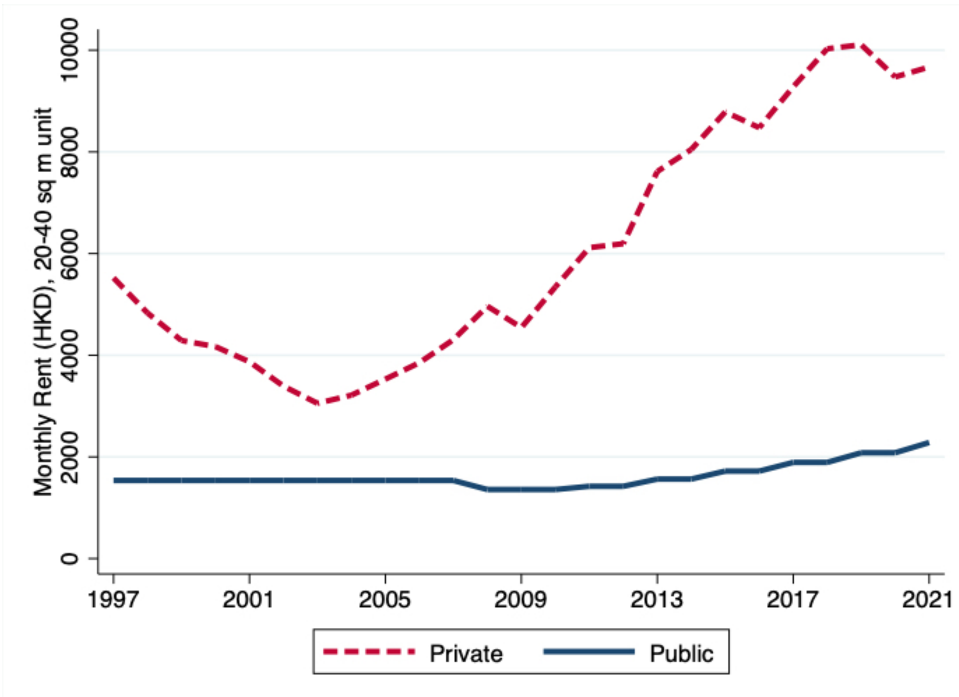
Notes: Table shows coefficients from the interaction-weighted estimator in Sun and Abraham (2020). Sample is all estates where all buildings were built after 1979 and before 1996. Year 0 denotes first observed Census year following treatment. Standard errors (clustered at the estate level) are shown, with ∼ = significant at the 10% level, * = significant at the 5% level, and ** = significant at the 1% level.

Table A13: Effect of TPS on average number of children, by demographic group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Men, 25-44 y.o.	Women, 25-44 y.o.	Men, 45-64 y.o.	Women, 45-64 y.o.	Women 25-44 y.o.		
					Single	Married	Divorced
t = -10	0.08	-0.02	0.01	0.00	-0.001	-0.04	0.00
	(0.06)	(0.06)	(0.07)	(0.06)	(0.003)	(0.05)	(0.14)
t = 0	-0.10*	-0.06	0.06	0.03	-0.004	0.00	-0.05
	(0.05)	(0.04)	(0.04)	(0.05)	(0.004)	(0.03)	(0.09)
t = 5	-0.27**	-0.26**	0.05	0.04	-0.002	-0.14**	-0.17
	(0.08)	(0.07)	(0.06)	(0.06)	(0.004)	(0.05)	(0.10)
t = 10	-0.36**	-0.37**	-0.09	-0.10	-0.005	-0.29**	-0.38**
	(0.10)	(0.09)	(0.07)	(0.07)	(0.004)	(0.05)	(0.11)
t = 15	-0.40*	-0.36**	-0.11	-0.17	-0.003	-0.26**	-0.14
	(0.16)	(0.13)	(0.11)	(0.11)	(0.005)	(0.07)	(0.17)
Treated mean, 1996	1.26	1.58	2.02	2.04	0.00	1.95	1.54
R2	0.77	0.83	0.86	0.84	0.27	0.88	0.53
Num. of estate-years	410	410	410	410	410	410	410
Num. of estates	82	82	82	82	82	82	82

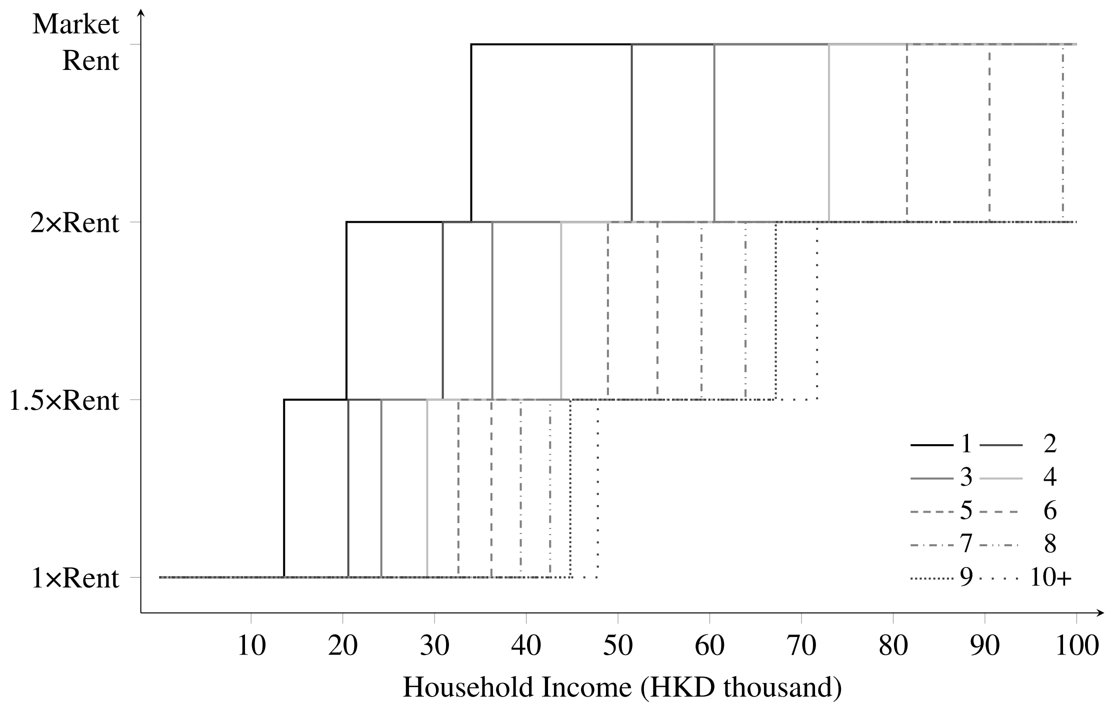
Notes: Table shows coefficients from the interaction-weighted estimator in Sun and Abraham (2020). Sample is all estates where all buildings were built after 1979 and before 1996. Year 0 denotes first observed Census year following treatment. Standard errors (clustered at the estate level) are shown, with $\sim$ = significant at the 10% level, * = significant at the 5% level, and ** = significant at the 1% level.

Figure A1: Public and Private Rent for Similar Units



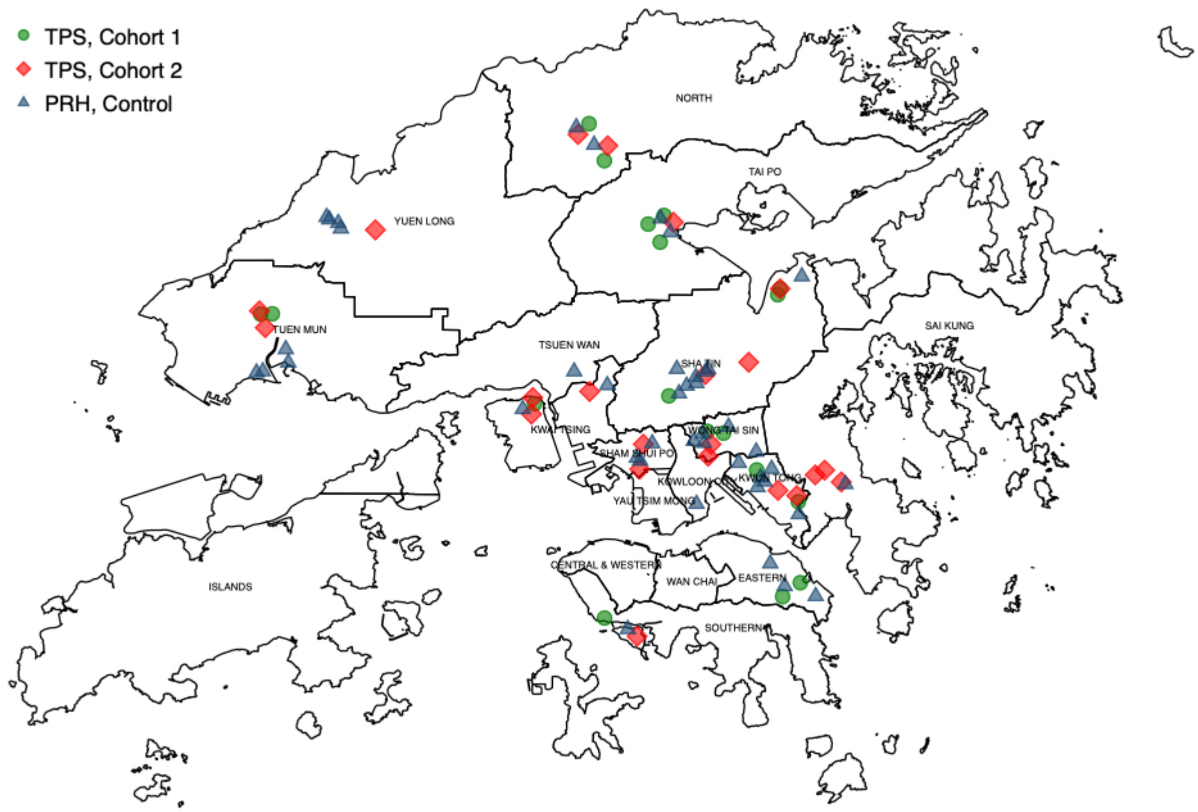
Notes: Figure plots rent indices for PRH and comparable private homes. The PRH rent index is constructed as follows. I first construct a PRH rent index with 2016 normalized to one using government announcements about the percentage changes in PRH rent. I then multiply the rent index by the average rent of households residing in 20-40 square meter PRH units in the 5% sample of the 2016 Hong Kong Population census. Note that 20-40 square meter units accounts for 67.2 percent of PRH housing stock in 2016. The rent index for comparable private sector homes is constructed as follows. First, I compute the average rent of comparable private homes in 2016. We take the average rent by district of renters in 20-40 square meter private-sector units in the 5% sample of the 2016 Hong Kong Population Census. I average across districts, with the number of 20-40 sq m PRH units in each district in 2016 as weights. Next, I obtain private-sector rent indices for Class A (i.e., <40 square meters) units by region (Hong Kong Island, Kowloon and New Territories) from the Rating and Valuation Department (RVD). I take the average across the RVD indices, weighted by the number of 20-40 square meter PRH units in each region. I then normalize 2016 to be the average rent of comparable private homes in 2016, as calculated from the Census data.

Figure A2: Rent schedule by household income and size



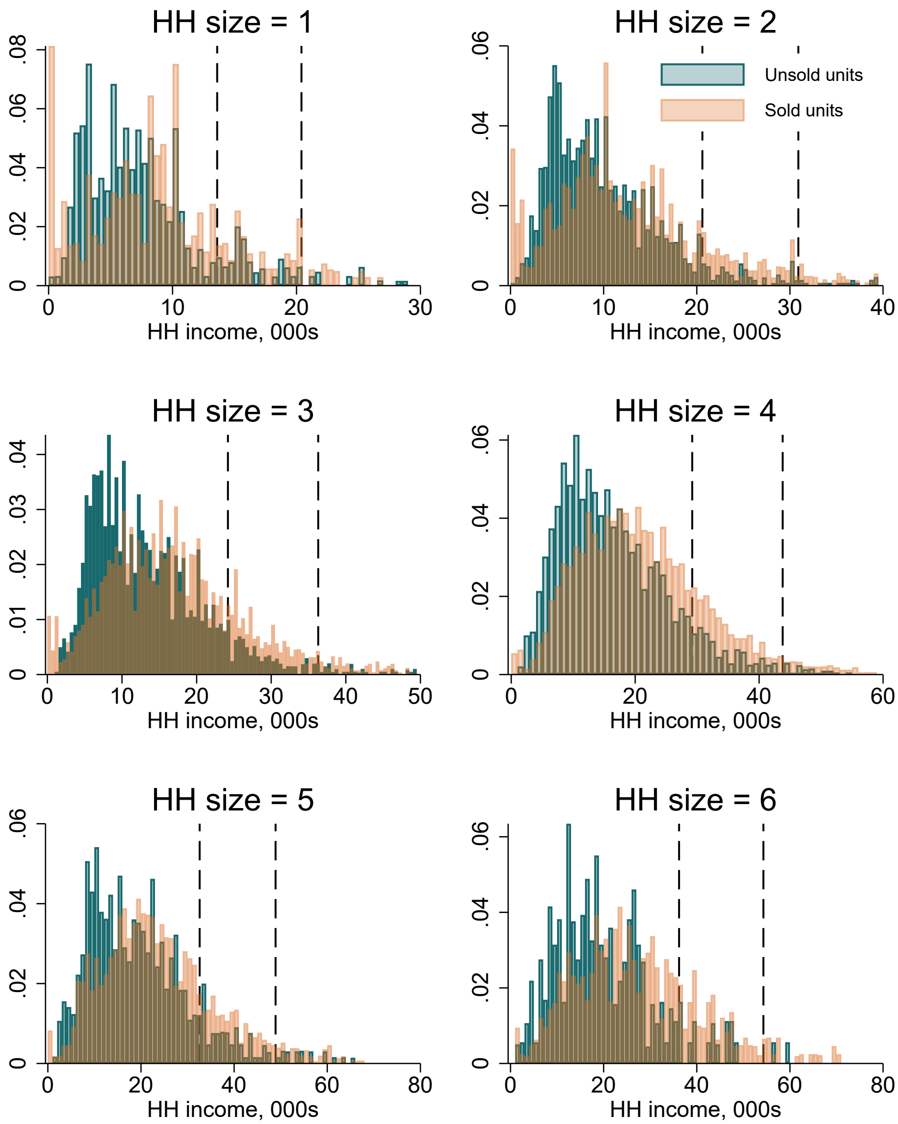
Notes: Under the "Well-off Tenants Policies", households with an income exceeding two times and not more than three times the prevailing PRH income limits have to pay 1.5 times net rent plus rates. Those with household income exceeding three times and not more than five times the prevailing PRH income limits are required to pay double net rent plus rates. PRH households with total household income or net assets value exceeding the prescribed limits (i.e. five times and 100 times of the PRH income limits respectively), as well as those who have private domestic property ownership in Hong Kong are required to vacate their PRH flats. These households have to secure accommodation on the private rental market at prevailing market rents. This figure illustrates the rent levels paid by households under the 2006 Public Rental Housing (PRH) Income Limits (see Appendix Table A1), as a function of household income and household size.

Figure A3: Map of treated and control estates



Notes: Figures plots each treated and control estate included in the analysis sample.

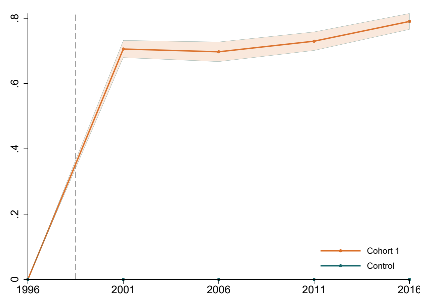
Figure A4: HH income distribution by household size, sold vs unsold units, 2006



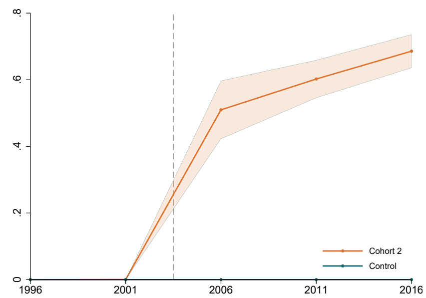
Notes: Figure plots the distribution of household income in TPS estates in 2006 by household size, respectively for sold and unsold units. The 1.5X and 2X rent income limits are plotted in dashed vertical lines. Households with all members above age 60 are excluded.

Figure A5: Trends in housing estate outcomes, treated vs weighted control estates

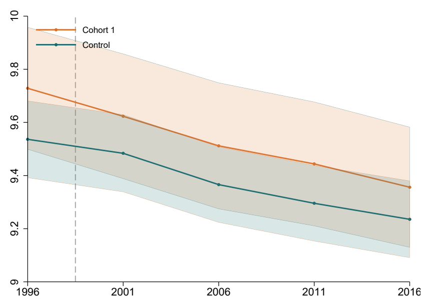
(a) Share of HHs in TPS - Cohort 1



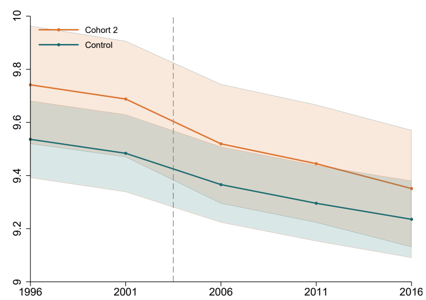
(b) Share of HHs in TPS - Cohort 2



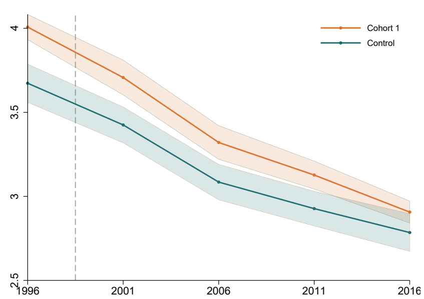
(c) Log population - Cohort 1



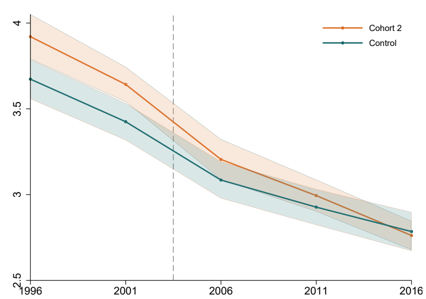
(d) Log population - Cohort 2



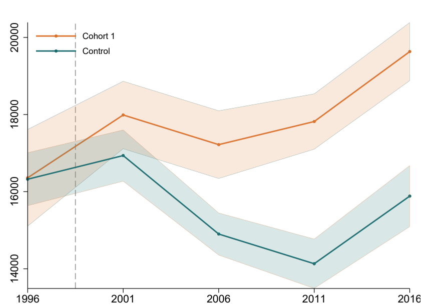
(e) Average HH size - Cohort 1



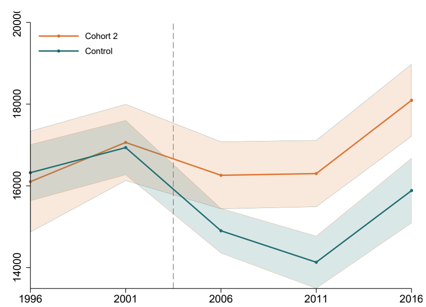
(f) Average HH size - Cohort 2



(g) Average HH income - Cohort 1



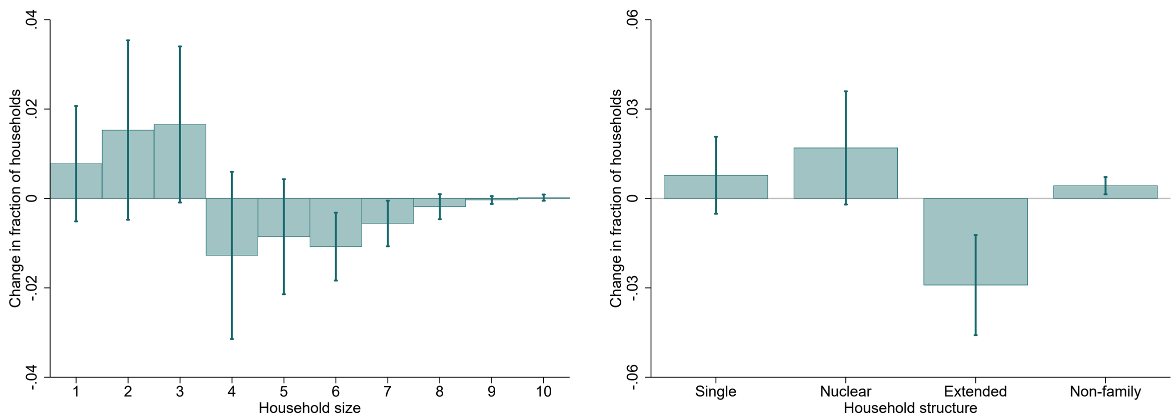
(h) Average HH income - Cohort 2



Notes: Each panel shows the trend in mean estate characteristics, separately for the two treated cohorts and their respective controls, whose means are computed with entropy balancing weights (Hainmuller 2012) that are based on estate-level average household size and income in 1996. Sample includes all estates where all buildings were built after 1979 and before 1996.

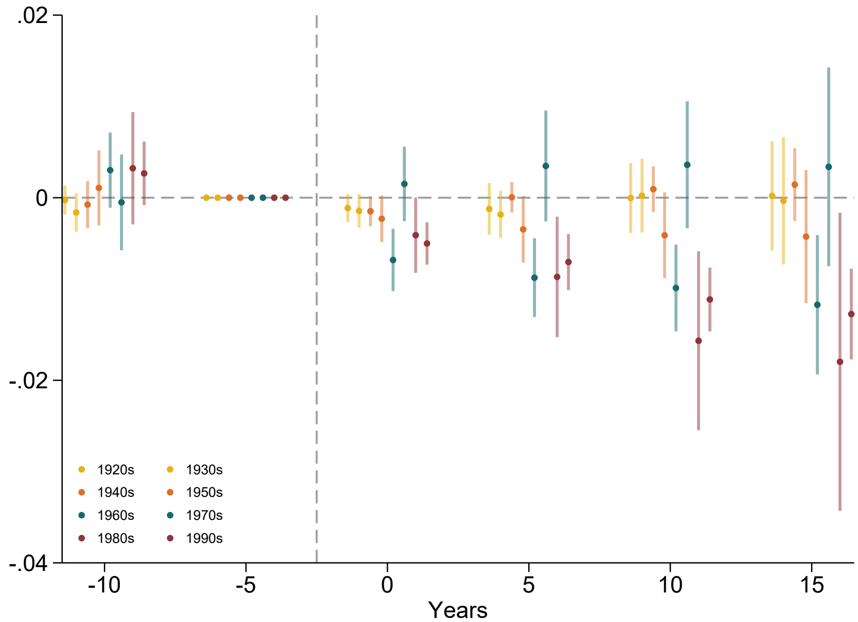
Figure A6: Effect of TPS on distribution of household types

(a) HH size (b) HH type



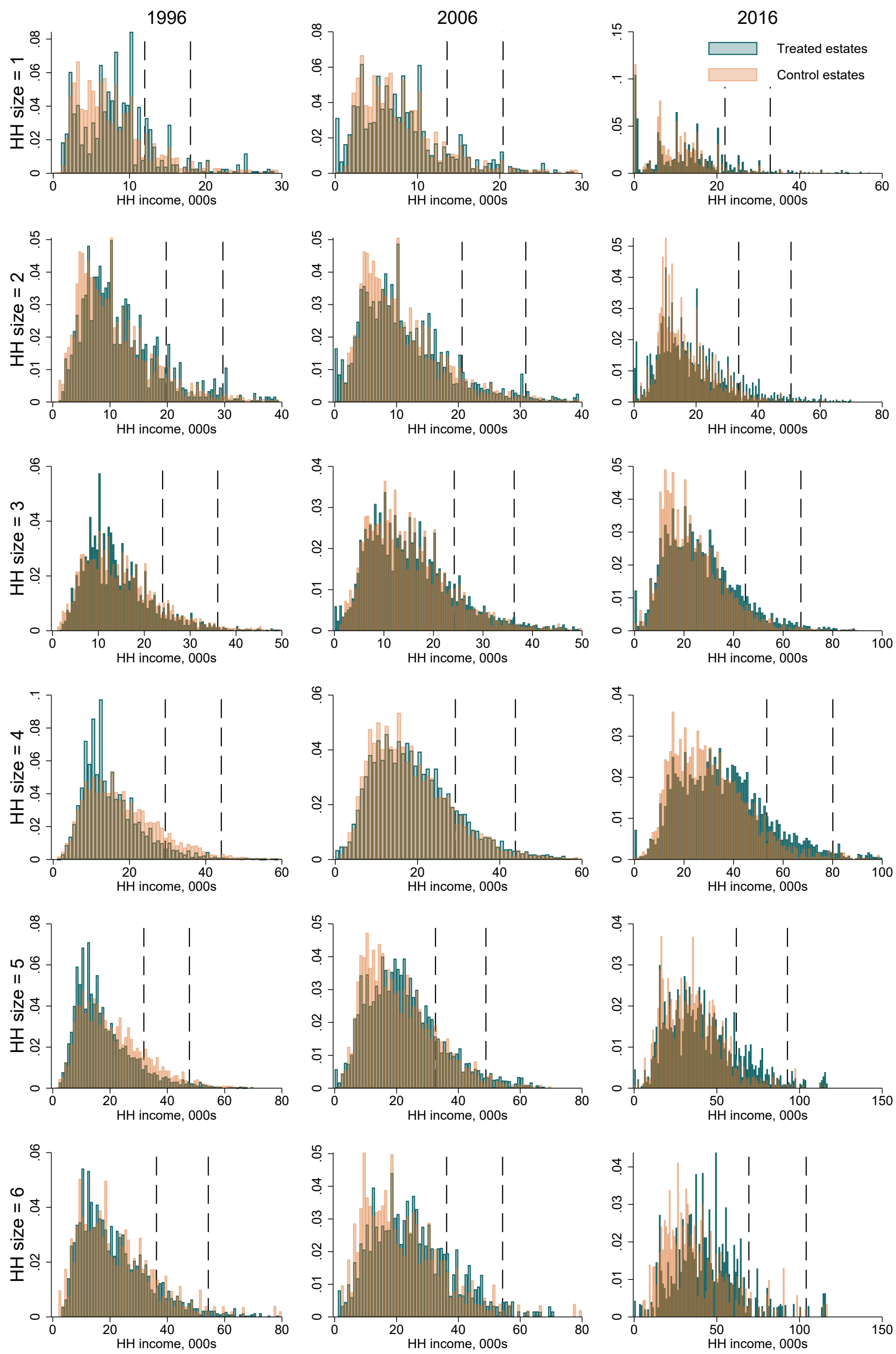
Notes: Figure plots the effect of TPS on the share of households with a given household type in the second Census year following treatment relative to that of the last Census year before treatment, estimated using the interaction-weighted estimator in Sun and Abraham (2020). Confidence intervals at the 95% significance level (clustered at the estate level) are shown. Single households include only one person. Nuclear households include a couple and any of their children. Extended-family households include a nuclear family and additional relatives, e.g. at least one parent of the couple. Online Appendix Table A8 displays coefficients and pre-treatment means.

Figure A7: Effect of TPS on population by birth cohort



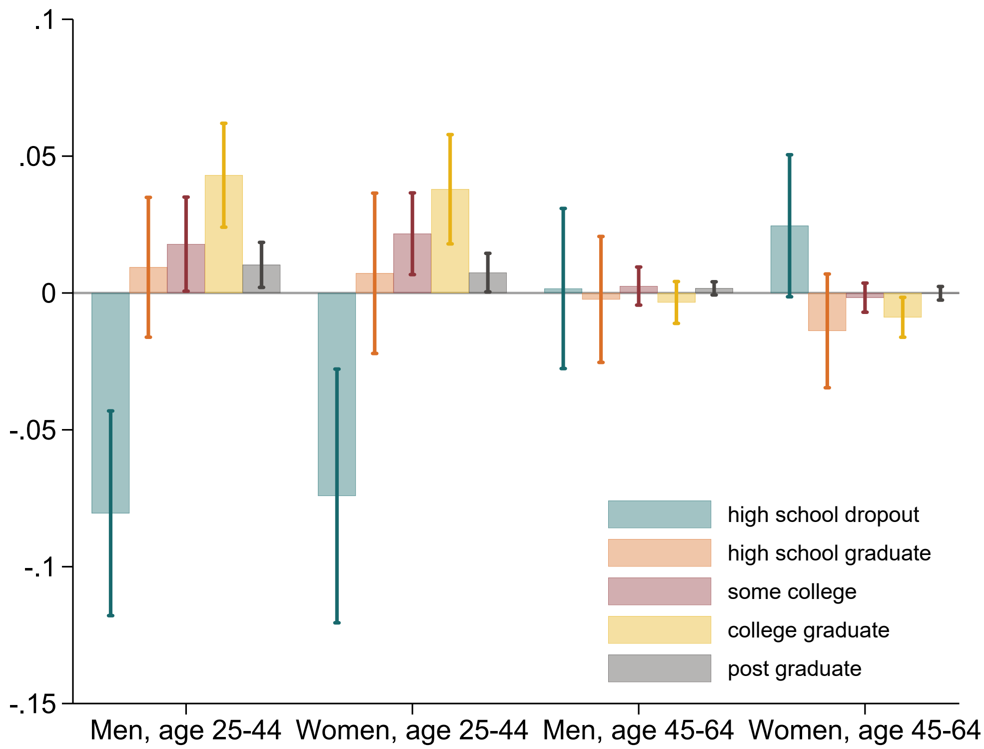
Notes: The series plots coefficients from the interaction-weighted estimator in Sun and Abraham (2020) based on estate-level cohort size in 1996. The cohort plots are shown in sequential order. Sample is all estates where all buildings were built after 1979 and before 1996. Year 0 denotes first observed Census year following treatment. Confidence intervals at the 95% significance level (clustered at the estate level) are shown.

Figure A8: HH income distribution by household size, treated vs control estates



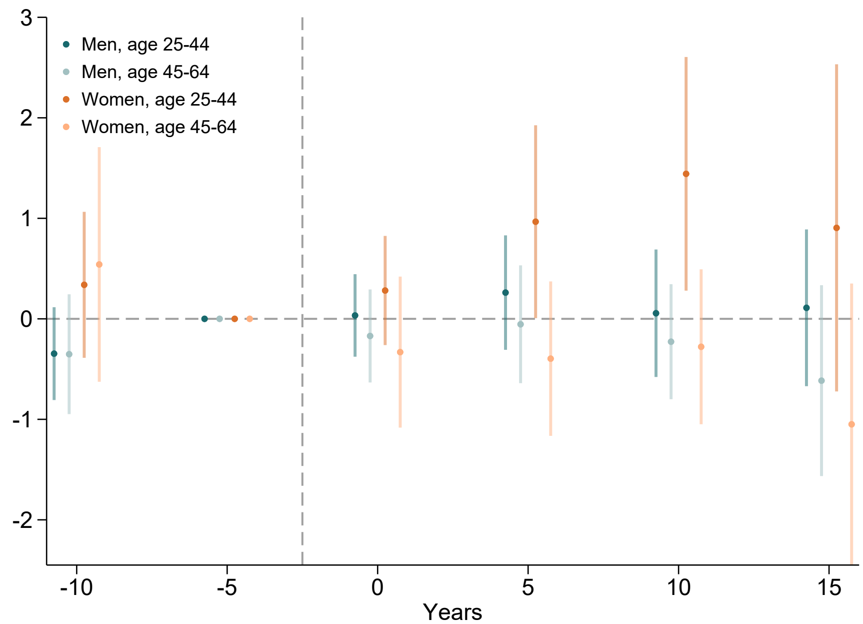
Notes: Figure plots the distribution of household income in treated and control estates, respectively in 1996, 2006, and 2016. The 1.5X and 2X rent income limits are plotted in dashed vertical lines. Households with all members above age 60 are excluded.

Figure A9: Effect of TPS on education attainment by demographic group



Notes: Figure plots the effect of TPS on education attainment among different groups in the second Census following treatment relative to that of the last Census year before treatment, estimated using the interaction-weighted estimator in Sun and Abraham (2020). The education attainment are classified in 5 categories: high school dropout, high school graduate, some college, college graduate and post graduate. Confidence intervals at the 95% significance level (clustered at the estate level) are shown.

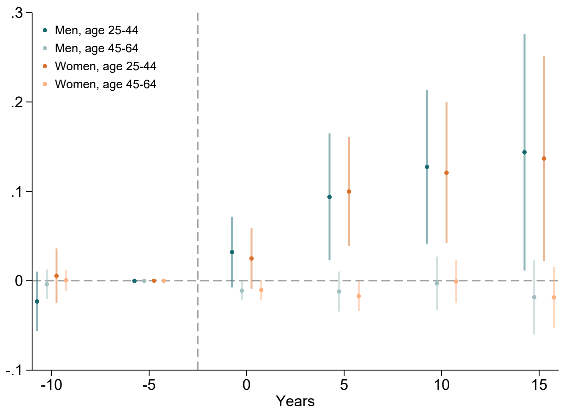
Figure A10: Effect of TPS on estate average commute minutes



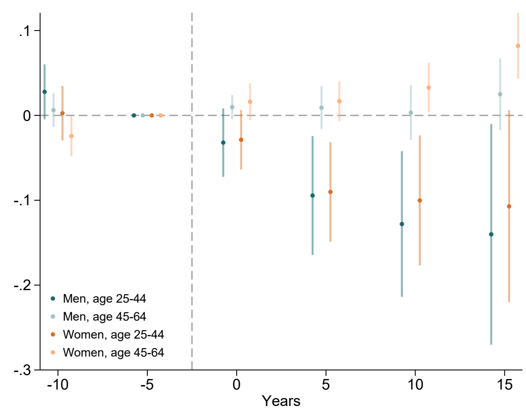
Notes: Commute time is defined as the time for a person to drive from the estates to their workplace, as defined by Google's Distance Matrix API with time set at 8:00am August 25, 2020. The series plots coefficients from the interaction-weighted estimator in Sun and Abraham (2020) based on estate-level average commute minutes among demographic groups. The orange plots represent women while blue plots represent man. The older is displayed in lighter colors and the young is shown in darker ones. Sample is all estates where all buildings were built after 1979 and before 1996. Year 0 denotes first observed Census year following treatment. Confidence intervals at the 95% significance level (clustered at the estate level) are shown.

Figure A11: Impact of TPS on marital status among demographic group

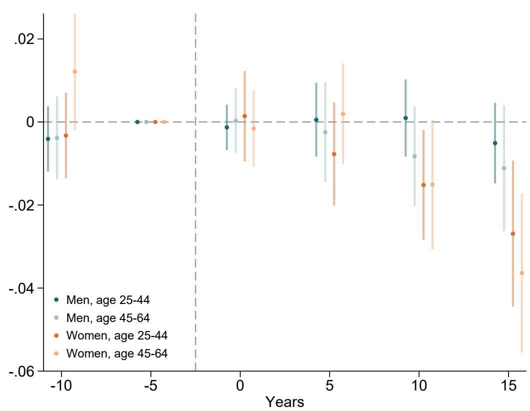
(a) Single



(b) Married



(c) Divorced



Notes: The series plots coefficients from the interaction-weighted estimator in Sun and Abraham (2020) based on estate-level share of the single, married and divorced among demographic groups in 1996. The orange plots represent women while blue plots represent man. The older is displayed in lighter colors and the young is shown in darker ones. Sample is all estates where all buildings were built after 1979 and before 1996. Year 0 denotes first observed Census year following treatment. Confidence intervals at the 95% significance level (clustered at the estate level) are shown. Online Appendix Table A12 displays coefficients and pre-treatment means.