Targeting Housing Assistance with Rent Notches: Evidence from a Quasi-experiment

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Abstract

This paper studies the effects of rent notches on the household composition and average incomes of Hong Kong's public housing residents. I leverage the staggered roll-out of the Tenants Purchase Scheme between 1998 and 2006, which allowed 183,700 tenants to buy permanent occupancy rights at discounted prices and thereby removed rent notches. Difference-in-difference estimates reveal that household sizes declined by 4-5 percent in the treated estates, while average households income rose by 23 percent. The average schooling of younger adults increased by one year. These results suggest that the removal of rent notches worsened the targeting of housing assistance towards low-income populations, partly due to endogenous changes in household formation.

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1 Introduction

Many governments use income-contingent rent schedules to target housing assistance to households in need. For example, 31 percent of Hong Kong's population resides in public rental housing, which requires renter households to pay 1.5 times or double rent if their incomes surpass certain thresholds. Another 16 percent of Hong Kong's population instead live in subsidized ownership units, where they are initially mean tested, but the cost of their continued residence is not conditional on observed income. In theory, rents that discontinuously increase with income — i.e., rent notches — may induce households who reduced need for assistance to eventually self-select out (Akerlof 1978; Nichols and Zeckhauser 1982). However, rent notches also disincentive work (Jacob and Ludwig 2012; Zhang Forthcoming). Given this downside, it is important to know the extent to which rent notches improve targeting efficiency.

In this paper, I measure the impact of rent notches on the residential sorting, co-residence choices, and average incomes of Hong Kong's public rental housing tenant population. I leverage the staggered implementation of the Tenants Purchase Scheme (TPS), which enabled 183,700 households to avoid rent notches by converting their public rental housing units into non-transferable ownership units.[1] I find that the removal of rent notches did not detectably alter household-level residential mobility. Instead, it led to large changes in household composition in treated estates. The share of households residing with extended family and the number of married persons fell. Average schooling and the incomes of public renter households dramatically increased. These findings suggest that removing rent notches substantially altered co-residence choices, which in turn reduced the targeting efficiency of housing assistance.

My analysis begins with a close examination of institutional detail and descriptive statistics. I document that the vast majority of TPS buyers gained permanent occupancy rights but not leasing and resale rights. As of 2016, almost twenty years after the program's launch, about

[1] In a similar policy, the Right-to-Buy program in the UK transferred ownership of over 2.8 million council houses to tenants between 1980 and the mid-2000s (Disney and Luo 2017). Ireland's sale of council houses boosted its homeownership rate from 70.8 percent in 1971 to 79.3 percent in 1991. Social housing tenants in Austria can acquire a right-to-buy option by paying a capital contribution at the start of their tenancy. In Sweden, the conversion scheme allows tenants in public rental housing to establish a cooperative. See Legislative Council Secretariat (2020). Hong Kong's Tenants Purchase Scheme is unusual in requiring a hefty land premium to be repaid before resale and leasing after purchase, which effectively rendered the sold unit non-transferable.

99 percent of households in sold TPS units were owner-occupiers who had not paid the land premium to the government. As such, they were legally prohibited from leasing out or reselling their units on the open market. Transfers of units with unpaid premium to eligible buyers in a restricted secondary market were also very rare. There was no detectable increase in residential mobility following program roll-out. Therefore, the scheme essentially did not change the housing endowment of the treated households. Instead, households appear to have purchased TPS units purely to avoid regular means tests. Consistent with this hypothesis, non-elderly households with high incomes, for whom the rent notches were more binding, were more likely to purchase TPS units.

Having argued that the main effect of TPS was to remove notches in the rent schedule, I use a simple household optimization model to illustrate how this change affects the behavior of income-tested public housing residents. I show that the rent notches are effectively lump-sum taxes that kick in when household labor income exceeds certain thresholds. In the model, rent notches encourage households with high earning ability to self-select out of subsidized housing. They also distort the labor supply of those who choose to live in public housing. Furthermore, by taxing labor income in a manner that may depend on household size, the notches affect co-residence choices. These endogenous co-residence choices can improve the targeting efficiency of housing subsidies toward low-income populations.

I estimate the effect of the subsidized sale on treated housing estates using the scheme's staggered and incomplete roll-out across housing estates between 1998 and 2006 in a dynamic difference-in-differences design. As the control group, I use non-TPS housing estates with similar construction years as TPS estates. My main specification then uses the interaction-weighted estimator proposed by Sun and Abraham (2020), which computes the mean of the cohort-specific average treatment effects on the treated estates, weighted by the shares of each treatment cohort. Estate-level outcomes such as population, household sizes, household incomes, user costs, and commute times are computed from restricted-access 10% and 20% random samples of the Hong Kong Population Census.

The estimates reveal that the subsidized sale induced large changes in household composition. Total population in treated estates decreased by 5 percent within a few years, and eventu-

ally decreased by roughly 7 percent, or roughly 51,000, within two decades. Average household size declined by roughly 5 percent. The share of households with extended families also fell.

The subsidized sale also dramatically increased average household incomes in treated estates. The increases were not only large, but also grew substantially with time. Average household income 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.

These income effects are too large be plausibly explained by changes in labor supply responses alone. They also are not attributable to increased pressures to meet loan obligations, since the average user cost of households in the treated estates, defined as the sum of monthly rental and mortgage payments, significantly fell. They instead reflect the fact that higher-income households and household members were less likely to move out of these estates, while lower-income household members were more likely to move out. Consistent with this hypothesis, the average schooling of young households increased by a year. Based on the above, I conclude that TPS significantly reduced the targeting efficiency of public housing subsidies towards low-income populations.

Related literature. Although a large literature has developed to study the targeting of social assistance, there are relatively few studies that focus on housing assistance. There is also a growing literature on the trade-off between allocative and targeting efficiency in the mechanism design for the initial allocation of public housing (Thakral 2016, Forthcoming; Waldinger 2021; Lee, Kemp and Reina 2022; Naik and Thakral 2022). This novel contribution of this paper is to examine how rent notches affect the targeting of housing assistance. My findings highlight how rent notches can improve targeting efficiency through their effect on co-residence choices. A much larger 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).

This paper relates to a growing literature that examines the effects of public housing privatization. Most notably, Wang (2011, 2012) provides evidence that the privatization of state employee housing in China reduced housing misallocation, raised private-sector prices, relaxed credit constraints, and increased self-employment. Disney et al. (2021) 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. (Forthcoming) show that the privatization of municipal-owned buildings in Sweden caused beneficiaries to experience wealth increases and increased consumption owing to property price appreciation.[2] The Hong Kong setting differs from these settings, since leasing and resale restrictions prevented the realization of the benefits of privatization that were emphasized in these studies.

Finally, this study is the first to use a natural experiment 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 instead 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) provides time-series evidence on the effects of TPS on private-sector housing prices, but their estimates are likely confounded by contemporaneous events such as the Asian Financial Crisis.

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

[2] 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.

2 Institutional Background

In this section, I explain 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. Individual units are then offered to applicants by random computer batching according to each applicant's household size, unit allocation standards, and 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. The average wait time for housed applicants was 2.0 years in 2011, but had risen to 5.5 years by 2019. 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.[3] The average rent of a PRH unit in 2016 is $1,563, which is on average about 18.4 percent of a similar private-sector unit.[4]

Well-off Tenants Policy. To improve the targeting of public housing provision to low-income families, the "Well-off Tenants Policy" was created to reallocate PRH units from households whose incomes have significantly risen to families that are more in need. This policy requires tenants who have lived in PRH units for 10 years or more to 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 asked to move out. To

[3] 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).

[4] As shown in Online Appendix Figure A1, there was a large increase in private-sector rents between 2004 and 2020. The rent of 20-40 square meter units in the private sector nearly tripled, from $3,214 in 2004 to $9,474 in 2020. The average monthly rent of public housing units of similar quality increased from $1,536 in 2004 only to $2,082 in 2020.

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 the policy.[5]

Under-occupation. To ensure equitable utilization of PRH units, the government reallocates units if the size of a household significantly falls due to move-out, death, marriage, or emigration of some household members. 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.[6]

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.[7] Tenants who do not wish to purchase can continue to rent and occupy their units as before. The purchase price was set at

[5]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.

[6]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 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).

[7]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.

replacement cost, but given a further discount of 60% on purchase within the first year, which is as low as 12% of market value.[8] 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.

In response to the collapse of private-sector property prices during the Asian Financial Crisis, the government dropped the target for increasing homeownership in 2002. In August 2005, the Housing Authority announced that there will be no further sale of PRH units after 2006. In Section 6, I leverage the staggered and incomplete roll-out of TPS across housing estates to identify the impact of the program.[9]

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 Well-off Tenant Policy and under-occupancy unit allocation rules of PRH tenants, so they can occupy the purchased unit unconditionally. However, they were largely restricted from resale and letting.

Premium payment requirement. TPS household cannot lease or resale on the open market until a premium equivalent to current value of the original discount is paid to the government.[10] For example, suppose that a unit was purchased at 12 percent of the initial market value, and the

[8] 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.

[9] 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).

[10] 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).

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. Because of this requirement, extremely few TPS owners paid the premium. 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.[11]

Restricted secondary market. TPS households were permitted to sell their unit without payment of a premium only to public housing renters and other eligible purchasers in the Home Ownership Scheme (HOS) Secondary Market. Most of these eligible purchasers can wait to buy from the government at a discounted price and therefore had low willingness to pay. However, 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. Transactions in the HOS Secondary Market were thus very rare. For TPS units in the district of Tuen Mun, there were only 702 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.[12]

3 Theoretical Framework

In this section, I use a model to illustrate how TPS affected the behavior of public renters. My baseline model focuses on the trade-off between targeting and labor supply distortions in the presence of rent notches. The extension considers coresidence choice.

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

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

3.1 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:

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

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

The government provides public housing, where 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:

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

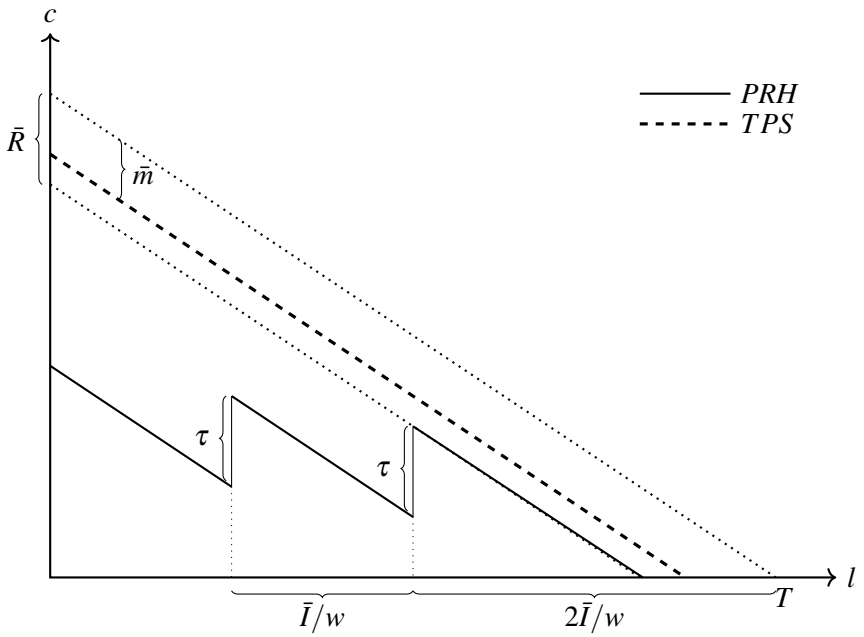
Note that for PRH in Hong Kong, $\tau = \frac{1}{2}\overline{R}$. Furthermore, $\overline{R} + 2\tau < r\overline{h}$, so public housing rent is always lower than the private sector. The utility of a household in public housing is:

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

Since the 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 fewer housing services than they would have chosen in the private sector. Therefore, even if $\tau = 0$, there exists some cutoff w_{PRH} such that $u^*(w) \lesseqgtr u_{PRH}(w)$ if and only if $w \lesseqgtr w_{PRH}$.

Rent notches help the government better target housing subsidies toward the needy. The reason is that rent notches reduce the cost of residing in public housing disproportionately for

Figure 1: Budget set of PRH and TPS households



high-income households. This in turn 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.

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 without gaining leasing or resale rights at heavily subsidized prices. Assuming that 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.

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 $\overline{R} > \overline{m}$).

If $\overline{R} \geq \overline{m}$, then $u_{TPS}(w) \geq u_{PRH}(w)$ 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 notchees purchase TPS to remove the rent notches.

TPS purchase alters labor supply in two ways. 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 given the size of rent notches, leading to an overall increase in labor supply.

TPS also encourages high-wage households to remain in public housing. One might expect this sorting effect to be small, since the treated population was initially means tested without any anticipation that means testing will be relaxed. However, this effect could be large if household incomes grow over time, and if the public-rent differential is small, so many households would prefer to move out if their income grew.[13] In particular, the sorting effect is likely larger for younger households whose earning potential grow over time. For these households, the removal of rent notches is likely to significantly discourage them from moving out.

Moreover, TPS may affect household composition. Since this margin of response is assumed away in the above model, here I provide an informal discussion of two possible mechanisms.

First, rent notches in Hong Kong are contingent on household size. Consider two people who may or may not coreside. Suppose one of them is discouraged from work by the rent notch, while the other does not participate in the labor market. Forming a joint household increases their income threshold, removes the disincentive to work, and may increase their joint utility above the sum of their individual utilities. In other words, rent notches encourage co-residence. By removing the rent notch, TPS reduces household sizes.

Second, rent notches function like taxes on labor earnings, so they reduce the value of

[13] Appendix Figure A1 plots the trends in rent differential during this period.

coresidence with a high earner relative to the value of coresidence with someone with low earner. A switch to TPS, which eliminates the notch, increases coresidence with higher-wage persons reduce coresidence with lower-wage persons.

4 Data and Summary Statistics

In this section, I describe the data and provide two descriptive facts. 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, I use restricted-access data from the Hong Kong Population Census and By-census, 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.[14] Furthermore, these data include identifiers for 136 public rental housing estates, including all 39 estates where residents became eligible to partake in TPS. This allows me to construct a panel of estates for analysis in Section 6.

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.

[14] 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. Since the premium must be paid before a TPS owner could sell, let, assign, or otherwise alienate the unit on the open market, this implies that only a tiny proportion of sold TPS units were either rented out or resold on the open market. The number of transactions in HOS Secondary Market was also small, as later 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 6 that the TPS reduced the population and number of households in treated estates.

4.3 Who Became TPS Owners?

There is strong evidence that avoidance of household-size-contingent unit allocation rules and means testing requirements 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.[15] By contrast, households whose members are all over 60 years old and therefore not subject to means testing requirements are

[15]See also Online Appendix Figure A3, 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.

less likely to live in sold TPS units. A government study in 2001 similarly reported that "the 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 suggests that transfer restrictions were stringent and households bought TPS units purely to avoid household-size-contingent unit allocation rules and mean testing requirements. Given this, the framework in Section 3 predicts that average household incomes in TPS estates would rise, since well-off households purchasing TPS units would become less likely to move out. The next section confirms this prediction.

5 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 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.[16]

I then estimate the following equation:

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

where e indexes estates, $t \in \{1996, 2001, 2006, 2011, 2016\}$ is the Census year, y_{et} is an estate-level outcome variable, T_e indicates whether estate e was ever treated, t_e^* is the first Census year

[16] Online Appendix Table A1 and A2 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.

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.[17] 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 A2, the treated and control estates are evenly dispersed across Hong Kong. Their average household incomes 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. Online Appendix Tables A3-A6 provide detailed comparisons of the pre-treatment characteristics of treated and control estates.

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 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.

[17] 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).

6 Results

In this section, I estimate the effects of TPS using its staggered and incomplete rollout across housing estates. The estimates reveal that TPS reduced total population and average household size in the treated estates, increased average household income, substantially reduced user costs, and did not alter commute times.

6.1 Effects on Household Composition

Figure 2 visualizes the effects of TPS on estate composition. 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.

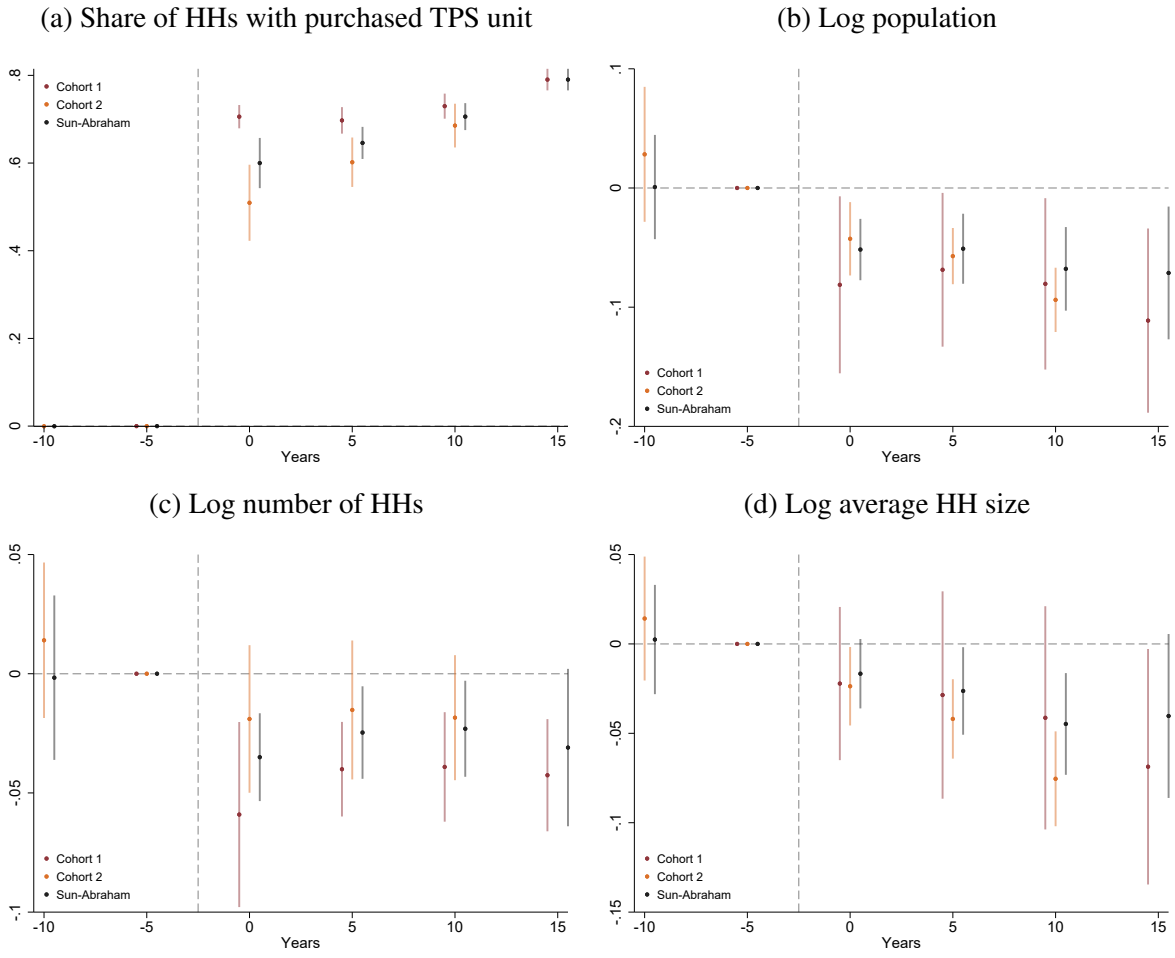
The share of households residing in sold TPS units immediately rose by 60 percent once residents became eligible to purchase TPS units in Year 0. As shown in Panel (a), this share eventually reached 79 percent higher than control in Year 15.

The average user cost, defined as the sum of monthly rental and mortgage payments, in the TPS estates fell dramatically relative to control. As shown in Online Appendix Table A10, average user costs fell by $272, or roughly 22 percent of the average rent in treated estates in 1996 by Year 0. The decline deepened and reached $646 by Year 15, or roughly 51 percent. In other words, mortgage payments were lower than counterfactual rent payments immediately after the rollout of TPS and further diverged over time.

Total population in treated estates immediately declined by 5 log points, as shown in Panel (b). This effect was persistent and reached 7 log points 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.

The number of households in treated estates immediately and persistently declined by roughly 2-3 log points, as shown in Panel (c). 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 4,000.

Figure 2: Effects of TPS on estate composition



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 first observed Census year following treatment. Standard errors (clustered at the estate level) are shown in bars. Online Appendix Table A7 displays coefficients and pre-treatment means.

Average household size in treated estates immediately declined by 0.08, relative to a mean of 4.0 in the pre-treatment year of 1996, as show in Panel (d). This decline widened over time, eventually reaching 0.21, or roughly 5 percent lower than control, in Year 15.

These estimated effects are unlikely to be driven by pre-existing trends or selection of estates into treatment. In all of the above panels, we do not detect pre-treatment trends in Year -10. Furthermore, the cohort-specific estimates using entropy-balancing weights are highly similar

to the Sun-Abraham estimates, even though they are less precise.[18]

The shares of households with one, two, or three members, while the shares of households with four, five, or six members fell. Furthermore, 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 A5).

The reduction in population in the treated estates is concentrated on birth cohorts that were less than 65 years old around the time of program launch. Consistent with evidence that elderly households were less likely to purchase TPS units, birth cohorts that were above 60 years old at the time of program launch did not experience changes in population (see Online Appendix Figure A6).

6.2 Effects on Household Income

While TPS reduced household sizes, average household incomes rose in treated estates. As shown in Figure 3, by Year 0, average real household income in treated estates rose by 1132 dollars 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 dollars (or 23 percent) higher in treated estates.

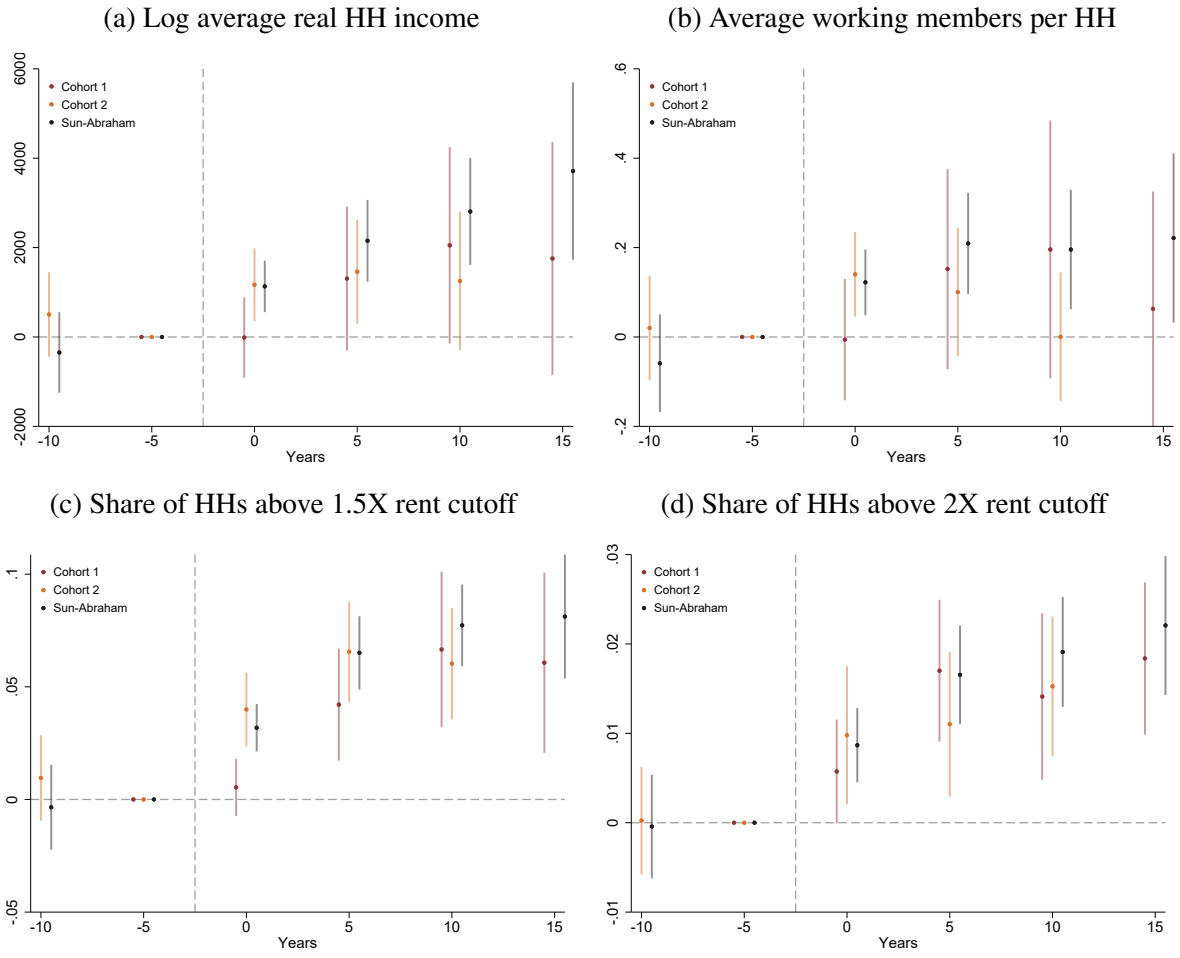
The average number of working members per household also rose. By Year 5, the average number of working members per household in treated estates increased by 0.2 (or 12 percent). This positive effect persisted until Year 15. Once again, these estimated effects do not appear to be driven by pre-existing trends or selection of estates into treatment.

The share of households above the 1.5X rent income limit rose sharply. By Year 0, the share of households above the 1.5X rent income limit increased by 3.2 percentage points, or 31 percent relative the 1996 mean in treated estates of 10.2 percent. This divergence further widened thereafter. By Year 15, the share of households above the 1.5X rent income limit was 8.1 percentage points (or 80 percent) higher than control.

A similar pattern exists for the share of households above the 2X rent cutoff. By Year 0,

[18] Online Appendix Table A7 tabulates the Sun-Abraham estimates. Online Appendix Figure A4 plots the raw trends in average outcomes in treated and control estates.

Figure 3: Effects of TPS on estate average HH income



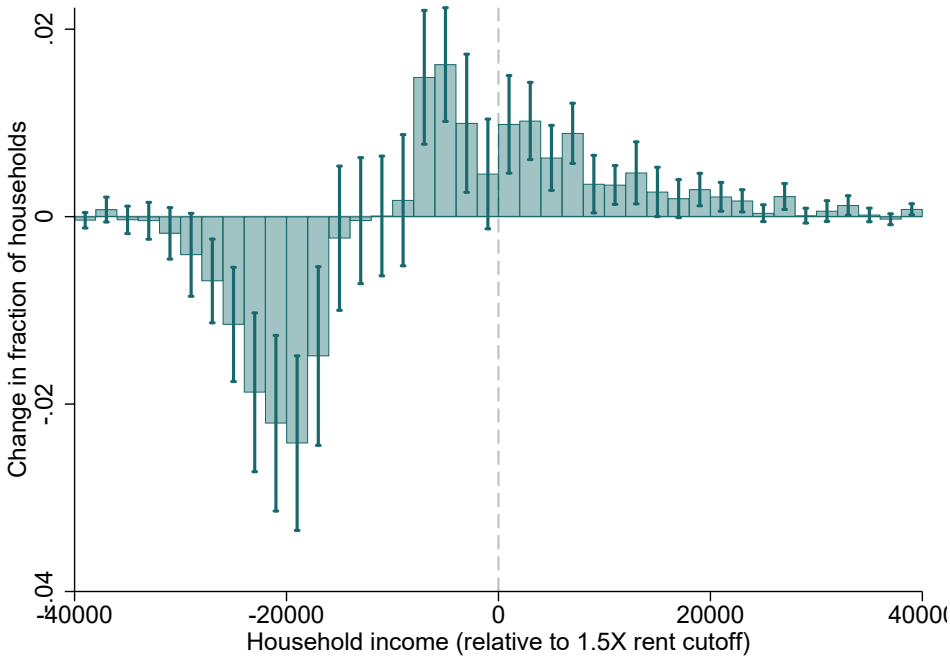
Notes: The black series plots coefficients from the interaction-weighted estimator in Sun and Abraham (2020). The maroon and yellow series plots cohort-specific coefficients, estimated with entropy balancing weights (Hainmueller 2012) that are based on estate-level average household size and income in 1996. Sample includes 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 A8 displays coefficients and pre-treatment means.

the share of households above the 2X rent increased by 0.9 percentage points (or 40 percent) in treated estates. By Year 15, the share of households above the 2X rent increased by 2.2 percentage points, roughly double the 1996 mean in treated estates.

Figure 4 plots the effect of TPS on the share of households within household income bins.[19] The figure reveals that the share of households with incomes much lower than the 1.5X rent

[19]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 4: Effect of TPS on HH income distribution relative to 1.5X rent cutoff



Notes: Figure plots the effect of TPS on the share of households within a given household income bin relative to the 1.5X rent income limit 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). Standard errors (clustered at the estate level) are shown in bars.

income limit dramatically fell in treated estates, while the share of households with incomes both above and slightly below the income 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 A7. 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, I observe bunching at round numbers in the data, especially for one-person households, which may obscure bunching.

The increase in household incomes is driven by increases in average incomes in all working age demographic groups. The increase for younger women is the largest. By Year 10, the

average income of women between ages 25-44 rose by 54 percent in treated estates (see Online Appendix Table A8). By contrast, the average income of men in the same age group only rose by 11 percent. TPS also caused the average schooling of adults between ages 25-44 in treated estates to increase, while the average schooling of adults between between ages 45-64 stayed the same (see Online Appendix Table A9). Since the data do not allow me to follow individuals or households over time, I cannot rule out the possibility that the increases in income and schooling are partly driven by changes in labor supply and human capital investment. The fact that average income increased much more for women than men is consistent with meta-analyses that show that female labor supply to be much more elastic. However, the observed effects appear much larger than implied by typical estimates for the elasticity of labor supply (Evers, De Mooij and Van Vuuren 2008).

Another possible explanation for increased incomes is that TPS reduced spatial match and improved the labor market opportunities of residents in the treated estates. Previous studies have shown that public housing in Hong Kong, both rental and ownership, features significant misallocation due to rationing, as exhibited by larger commuting distances of their residents relative to private-sector counterparts (Lui and Suen 2011). However, as shown in Online Appendix Table A11, TPS did not meaningfully reduce the average commute times of working persons in the treated households, in any of four demographic groups. This suggests that TPS did not reduce spatial mismatch. This lack of housing reallocation is consistent with stringent restrictions against resale and leasing.

7 Conclusion

This paper studies how public housing rent schedules affect the targeting of housing assistance in Hong Kong. I leverage the staggered roll-out of the Tenants Purchase Scheme, which allowed 183,700 tenants to buying permanent occupancy rights at discounted prices. Since resale and leasing of the sold units was severely restricted, I first argue that the primary effect of the scheme was to eliminate rent notches for sitting tenants households. Using a difference-in-difference research design, I then find that the removal of notches in the public housing rent schedule had

large positive effects of average incomes and negative effects on household size in the treated estates.

I conclude that the Tenants Purchase Scheme substantially worsened the targeting efficiency of housing assistance in Hong Kong. Although only about 80 percent of estate resident households purchased TPS units, the scheme increased average household income in the treated estates by 23%. The shares of households above the 1.5X and 2X rent income cutoff both doubled. The average schooling of young adults increased by one year. Household sizes and population fell by 5-7%. These combined effects cannot be explained by changes in labor supply responses or human capital investment alone. They instead reflect the fact that higher-income households and household members were less likely to moving out of these estates, while lower-income household members became more likely to move out. As a result, TPS reduced the probability that housing subsidies reached low-income households.

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Online Appendix

Table A1: 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 A2: 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 A3: 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 A4: Estate HH composition, treated vs control estates, 1996

	Treated estates	Control estates	Normalized difference
Single-person HH	0.07	0.09	-0.32
	(0.05)	(0.05)	
Nuclear family HH	0.70	0.68	0.12
	(0.11)	(0.09)	
Extended family HH	0.22	0.22	0.09
	(0.09)	(0.07)	
Non-family HH	0.005	0.007	-0.37
	(0.005)	(0.007)	
HH size = 1	0.07	0.09	-0.32
	(0.05)	(0.05)	
HH size = 2	0.09	0.14	-1.11
	(0.04)	(0.06)	
HH size = 3	0.18	0.20	-0.62
	(0.03)	(0.04)	
HH size = 4	0.33	0.30	0.51
	(0.08)	(0.05)	
HH size = 5	0.20	0.18	0.58
	(0.04)	(0.05)	
HH size = 6	0.09	0.07	0.64
	(0.03)	(0.03)	
HH size = 7	0.03	0.02	0.51
	(0.02)	(0.02)	
HH size = 8	0.01	0.01	0.47
	(0.01)	(0.01)	
HH size = 9	0.003	0.002	0.43
	(0.003)	(0.002)	
HH size = 10	0.001	0.001	0.06
	(0.002)	(0.002)	
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 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	0.03	0.002	-0.004	0.001	-0.13~
	(0.00)	(0.02)	(0.018)	(0.06)	(0.009)	(0.009)	(0.008)	(0.07)
t = 0	0.60**	-0.05**	-0.035**	-0.08*	0.001	0.018*	-0.021**	-0.01
	(0.03)	(0.01)	(0.009)	(0.03)	(0.005)	(0.007)	(0.007)	(0.05)
t = 5	0.65**	-0.05**	-0.025*	-0.13**	0.008	0.017~	-0.029**	0.01
	(0.02)	(0.01)	(0.010)	(0.04)	(0.007)	(0.010)	(0.009)	(0.05)
t = 10	0.71**	-0.07**	-0.023*	-0.19**	0.016~	-0.008	-0.012	0.02
	(0.02)	(0.02)	(0.010)	(0.04)	(0.008)	(0.012)	(0.012)	(0.05)
t = 15	0.79**	-0.07*	-0.031~	-0.21**	0.011	0.006	-0.020	0.00
	(0.01)	(0.03)	(0.017)	(0.07)	(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. 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 A8: 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 A9: Effect of TPS on estate-level average schooling

	(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	-0.06	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 in bars, with $\sim$ = significant at the 10% level, * = significant at the 5% level, and ** = significant at the 1% level.

Table A10: 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

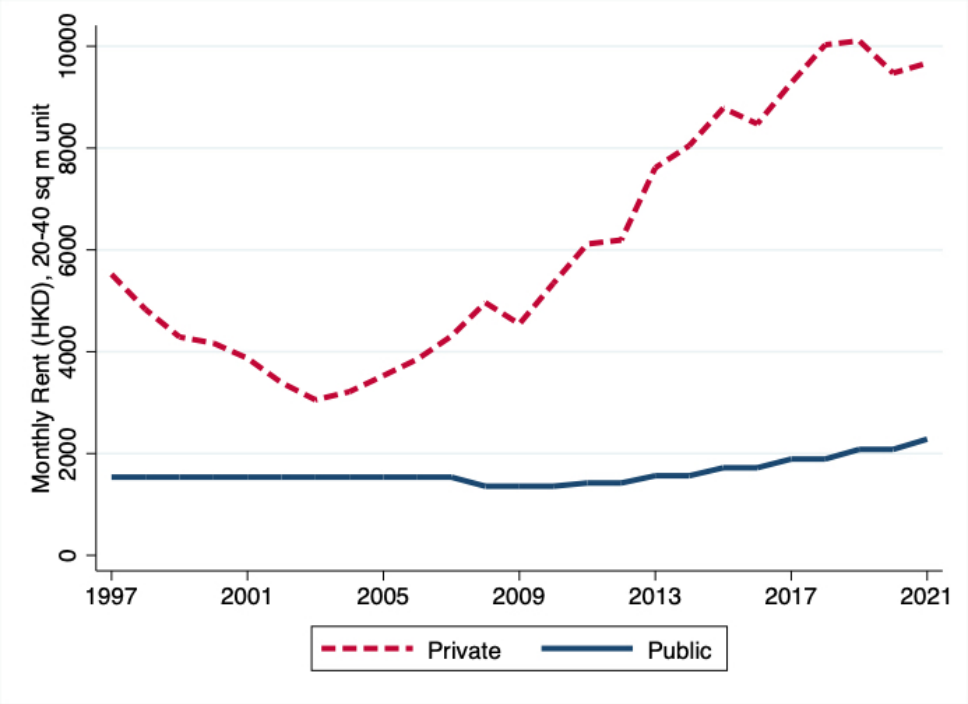
Notes: Table shows coefficients from the interaction-weighted estimator in Sun and Abraham (2020). Sample is all estates where all buildings were built after 1979 and before 1996. Year 0 denotes first observed Census year following treatment. Standard errors (clustered at the estate level) are shown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

Table A11: Effect of TPS on commute times

	(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	-0.38	0.43	-0.29	0.42
	(0.36)	(0.41)	(0.44)	(0.62)
t = 0	-0.11	0.33	-0.21	-0.32
	(0.25)	(0.30)	(0.28)	(0.40)
t = 5	0.08	1.16*	-0.03	-0.32
	(0.33)	(0.59)	(0.35)	(0.42)
t = 10	0.10	1.63**	-0.20	-0.35
	(0.43)	(0.63)	(0.32)	(0.42)
t = 15	-0.12	0.91	-0.78	-1.05
	(0.46)	(0.88)	(0.56)	(0.73)
Treated mean, 1996	18.9	15.3	17.8	14.1
R2	0.93	0.82	0.89	0.73
Num. of estate-years	410	410	410	410
Num. of estates	82	82	82	82

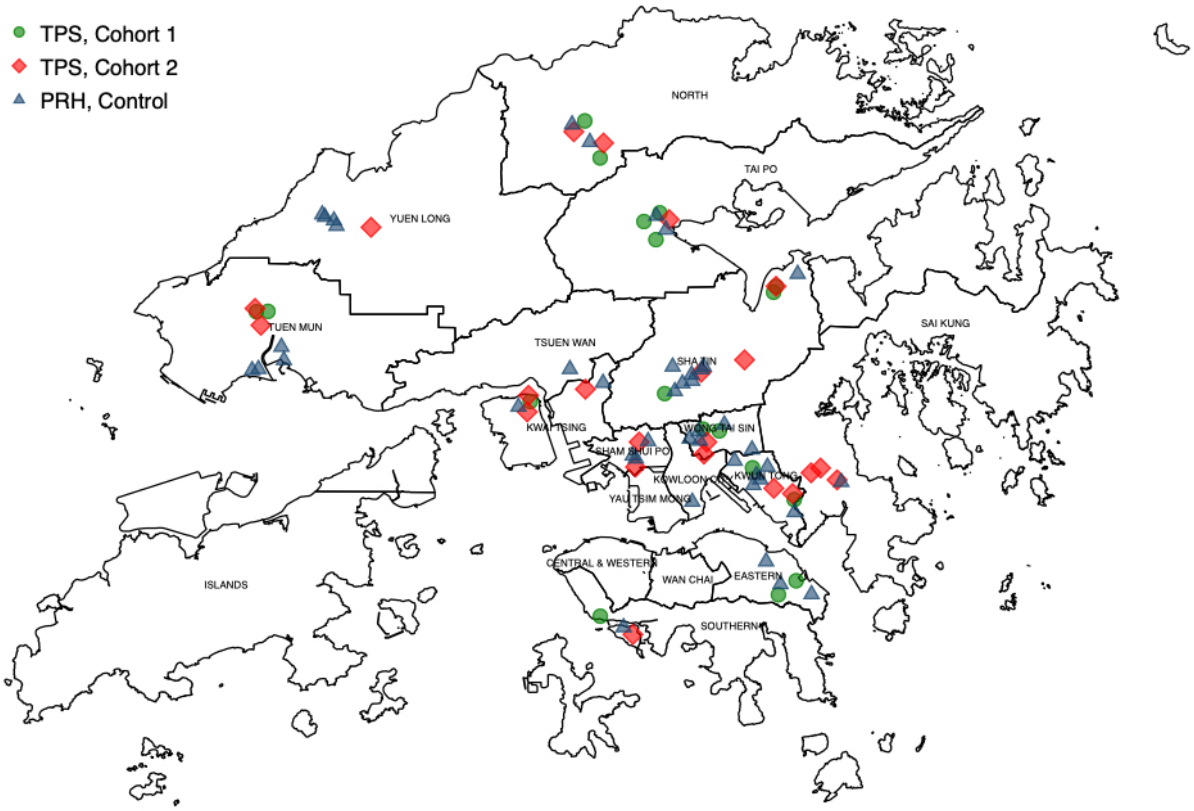
Notes: Table shows coefficients from the interaction-weighted estimator in Sun and Abraham (2020). Sample is all estates where all buildings were built after 1979 and before 1996. Year 0 denotes first observed Census year following treatment. Standard errors (clustered at the estate level) are shown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



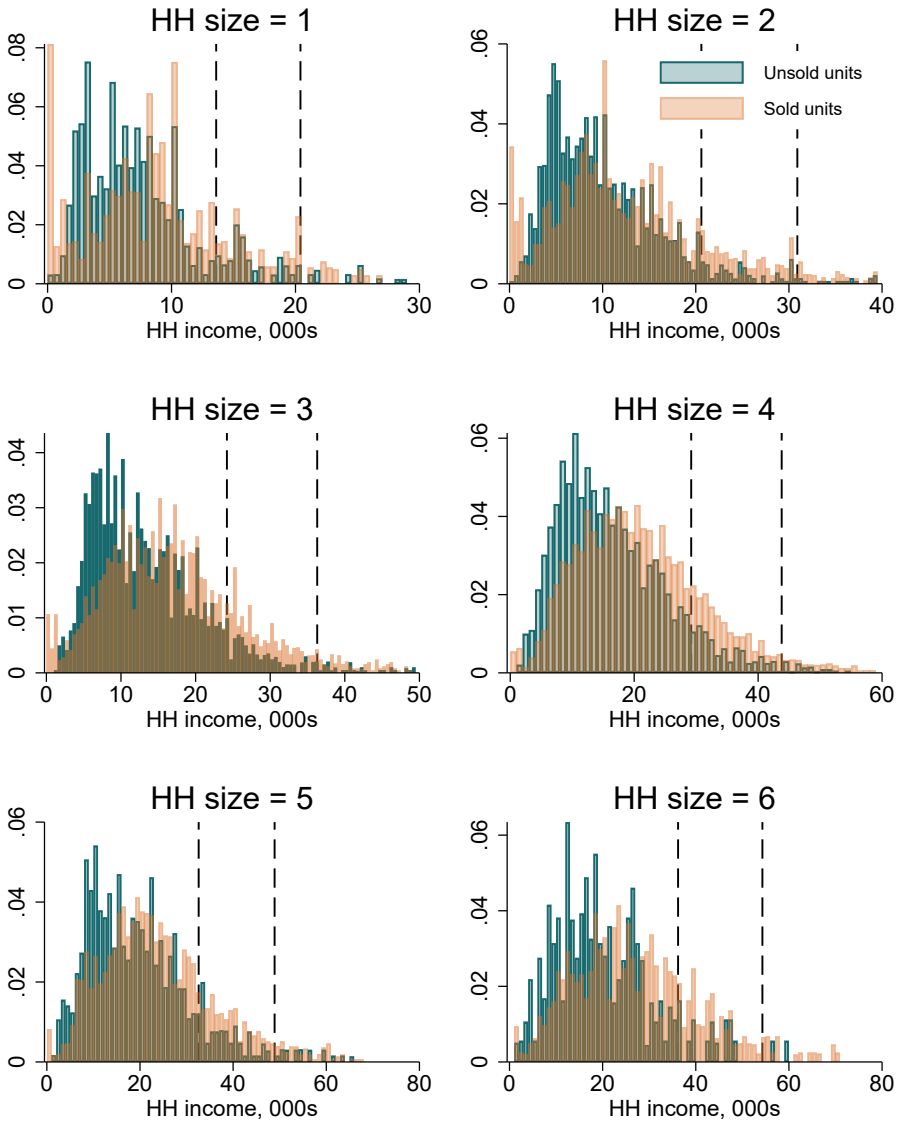
Notes: Figure plots rent indices for PRH and comparable private homes. The PRH rent index is constructed as follows. I first construct a PRH rent index with 2016 normalized to one using government announcements about the percentage changes in PRH rent. I then multiply the rent index by the average rent 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: Map of treated and control estates



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

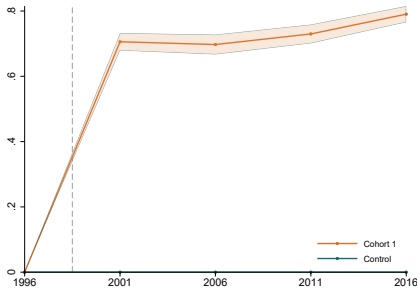
Figure A3: HH income distribution by household size, sold vs unsold units, 2006



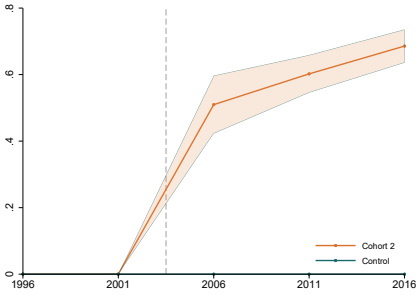
Notes: Figure plots the distribution of household income in TPS estates in 2006 by household size, respectively for sold and unsold units. The 1.5X and 2X rent income limits are plotted in dashed vertical lines. Households with all members above age 60 are excluded.

Figure A4: Trends in housing estate outcomes, treated vs weighted control estates

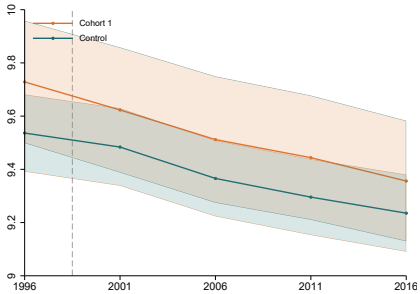
(a) Share of HHs in TPS - Cohort 1



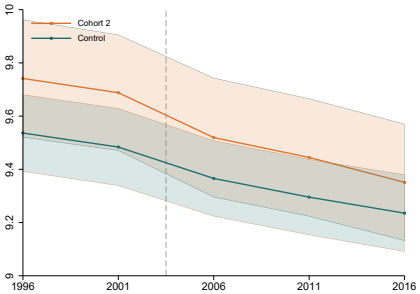
(b) Share of HHs in TPS - Cohort 2



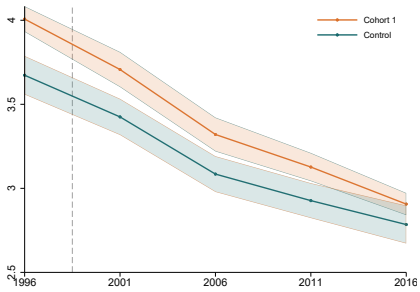
(c) Log population - Cohort 1



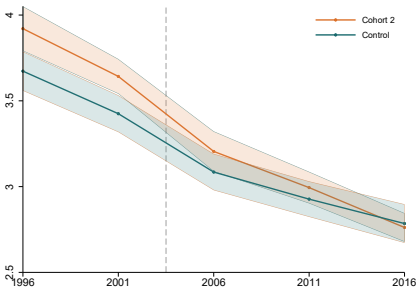
(d) Log population - Cohort 2



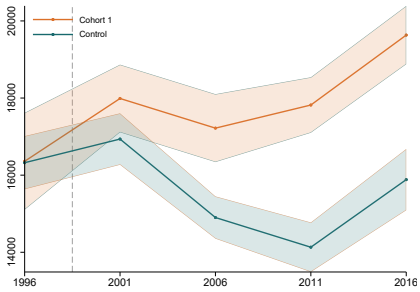
(e) Average HH size - Cohort 1



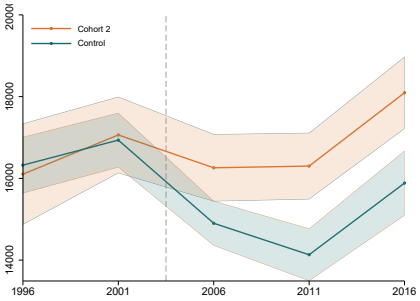
(f) Average HH size - Cohort 2



(g) Average HH income - Cohort 1

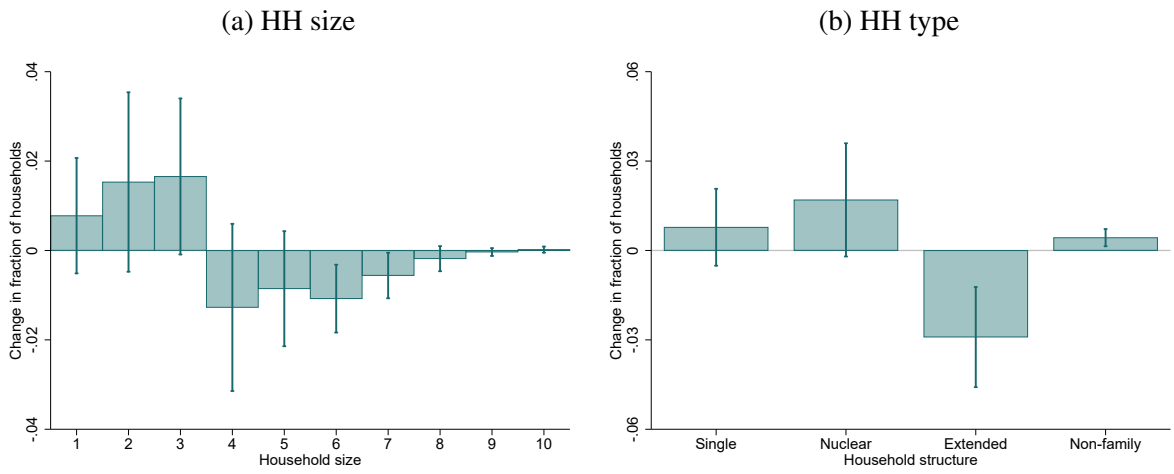


(h) Average HH income - Cohort 2



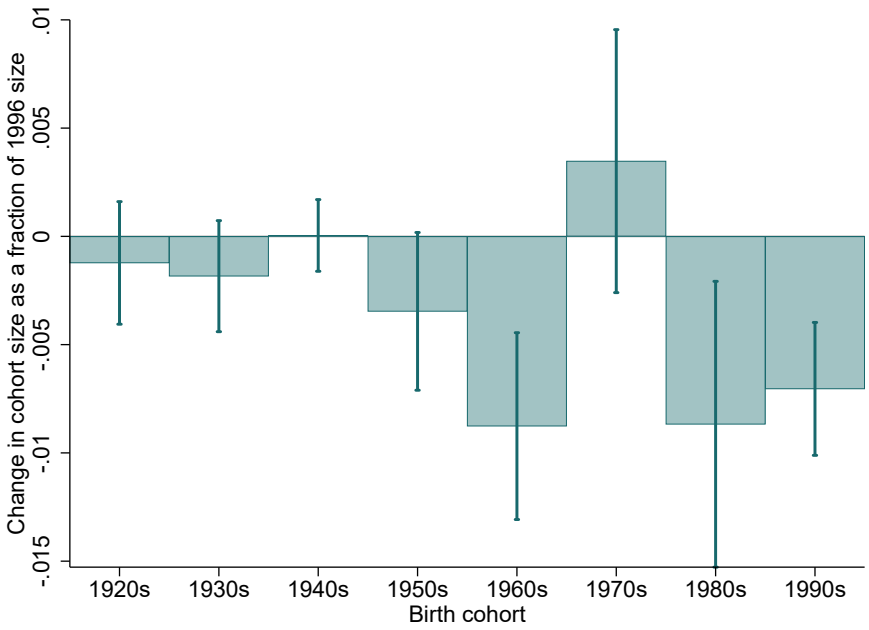
Notes: Each panel shows the trend in mean estate characteristics, separately for the two treated cohorts and their respective controls, whose means are computed with entropy balancing weights (Hainmuller 2012) that are based on estate-level average household size and income in 1996. Sample includes all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

Figure A5: Effect of TPS on distribution of household types



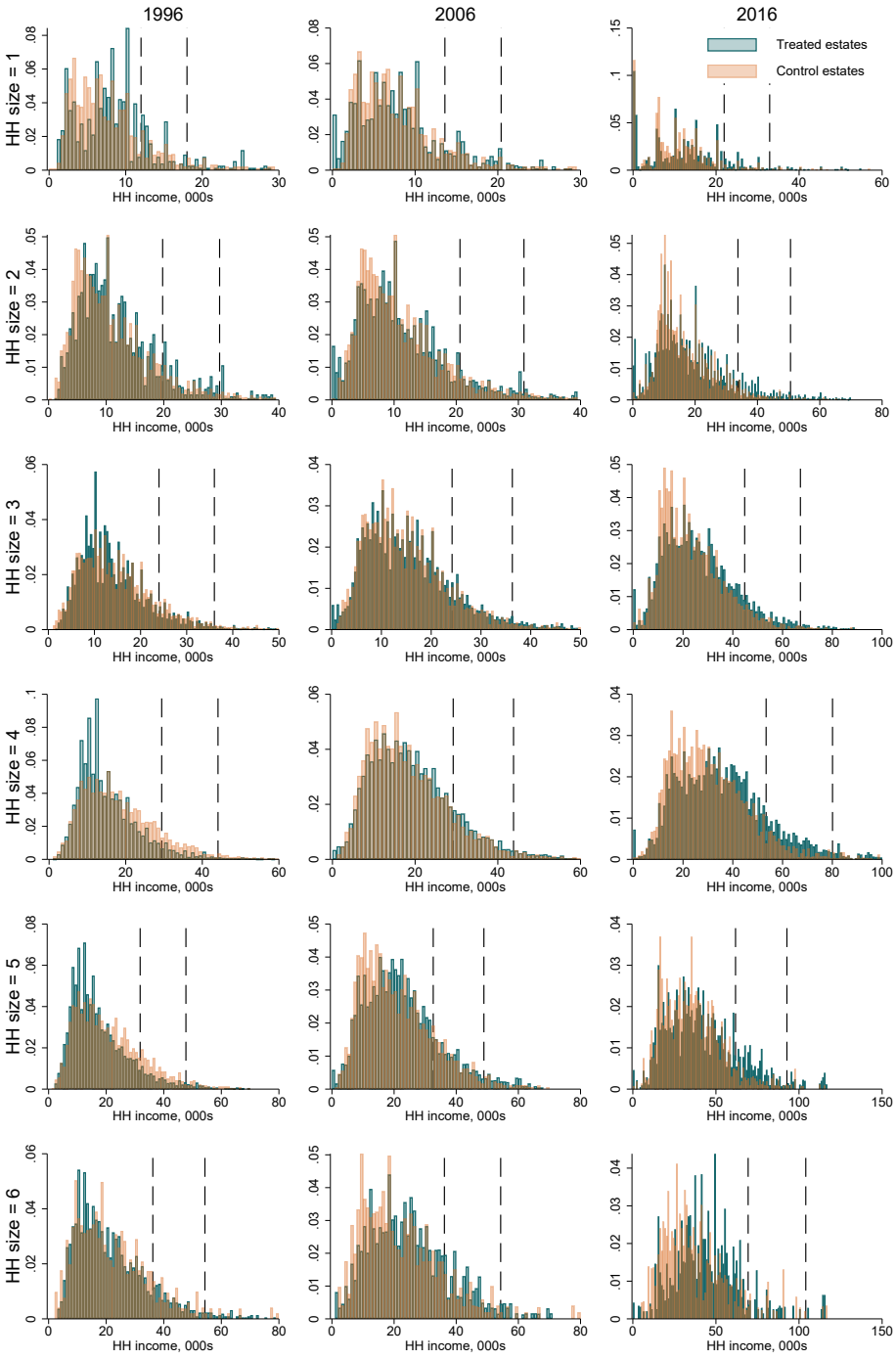
Notes: Figure plots the effect of TPS on the share of households with a given household type in the second Census year following treatment relative to that of the last Census year before treatment, estimated using the interaction-weighted estimator in Sun and Abraham (2020). Standard errors (clustered at the estate level) are shown in bars. 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.

Figure A6: Effect of TPS on population by birth cohort



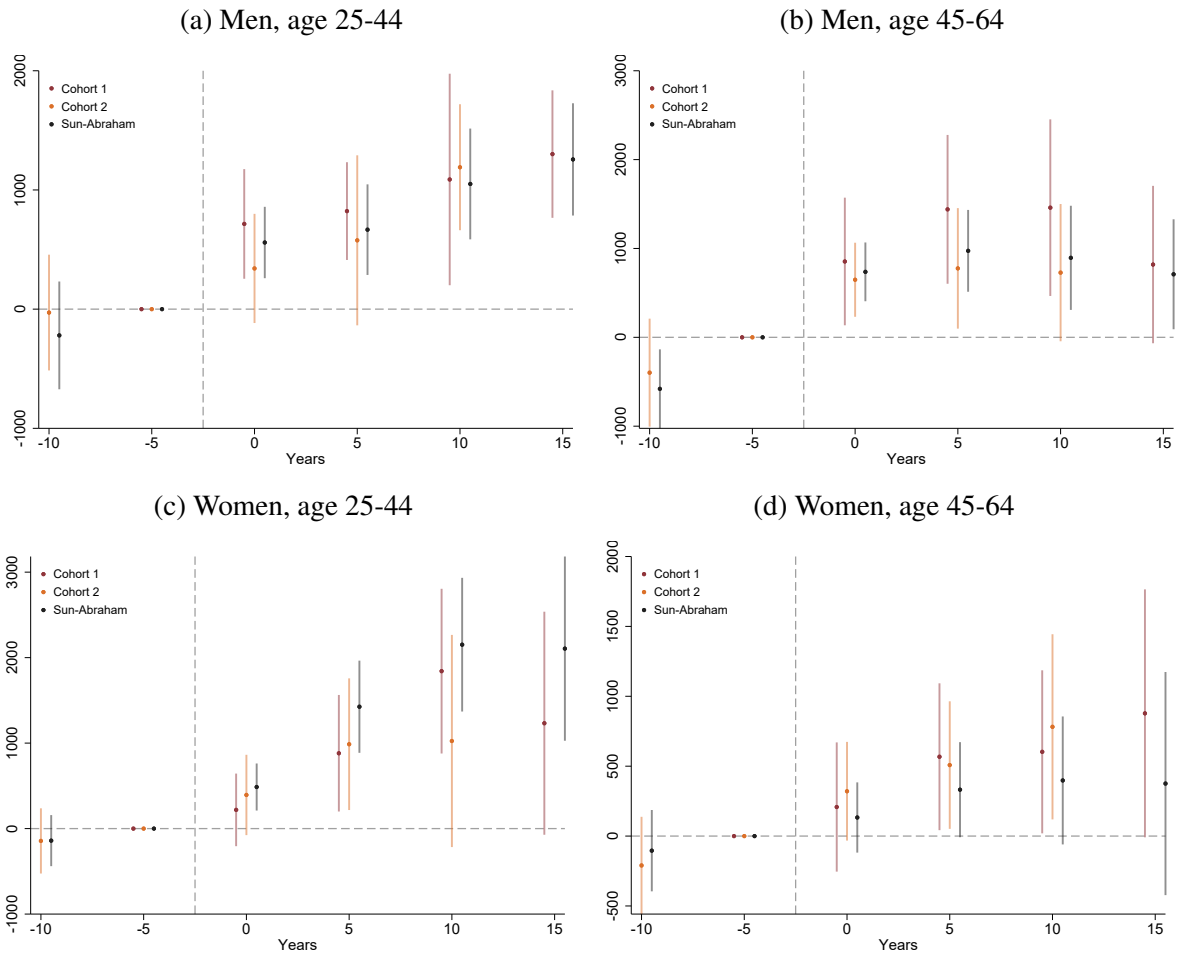
Notes: Figure plots the effect of TPS on the cohort size in treated estates as a fraction of 1996 cohort size in the second Census year following treatment relative to that of the last Census year before treatment, estimated using the interaction-weighted estimator in Sun and Abraham (2020). Standard errors (clustered at the estate level) are shown in bars.

Figure A7: 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 A8: Effect of TPS on estate average income by demographic group



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 first observed Census year following treatment. Standard errors (clustered at the estate level) are shown in bars.