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Quantifying Misallocation of Public Housing

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Abstract — We empirically investigate how rent control combined with the use of a non-market allocation mechanism – centralised waiting lists – affects the efficiency of housing allocation among public-housing tenants. We demonstrate that the consumption of Dutch public housing is misallocated by around €14 thousand on average, which represents 7.5% of the average value of a public housing unit. This entails particularly large transfers in housing consumption from younger households to older households. The resulting annual welfare loss is modest, estimated at around €64 per household residing in the public sector.

Keywords — Misallocation, public housing, rent control, non-market allocation.

JEL codes — R21, R30

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1 Introduction

In this paper we investigate how the use of rent control combined with a non-market allocation mechanism – centralised waiting lists – affects the efficiency of public-housing allocation. To this end, we build and improve on the methodology developed by Glaeser and Luttmer (2003). We quantify the extent to which housing is misallocated among public-housing tenants using administrative microdata from the Netherlands. Misallocation of public housing is arguably a particularly relevant welfare dimension to explore in European countries with large rent-controlled public-housing sectors, such as Denmark, the Netherlands and Austria. 28% of Dutch households reside in the public-housing sector.

Public-housing markets are typically characterized by strict rental regulations. Rent control is often used as a textbook example of how the regulation of prices affects welfare through reductions in the quantity and quality of supply. It is therefore not surprising that there is a substantial body of literature dedicated to the investigation of these supply effects (Olsen, 1972; Fallis and Smith, 1984; Gyourko and Linneman, 1989; Sims, 2007; Diamond et al., 2019).[1] Arguably, this focus on supply is rather narrow, as it ignores the effects that rent control has on the efficiency of housing allocation among tenants. Below-market rents induce excess demand for housing, generating a rationing problem (Bulow and Klemperer, 2012; Geyer and Sieg, 2013). Because market prices cannot be used to allocate housing to the household with the highest willingness to pay, public-housing authorities rely on alternative non-market allocation mechanisms to deal with excess demand.

According to theory, this rationing problem may introduce substantial welfare losses in the form of inefficient matches of households with housing, meaning that housing is not allocated to the household who values it the most. Glaeser and Luttmer (2003) theoretically demonstrate that the welfare losses from this form of housing misallocation may outweigh the welfare costs of rent-control-induced undersupply. Bulow and Klemperer (2012) demonstrate that the consumer surplus lost due to misallocation may even offset any consumer surplus gained from reduced rental prices. Both studies

[1] There is also a growing economic literature focusing on public housing, and in particular exploring various positive and negative externalities of public housing (see *e.g.*, Autor et al., 2014; Palmer et al., 2017; Diamond and McQuade, 2019).

apply theoretical models where housing is rationed using random allocation.

In practice, we observe a large range of alternative ways in which rent-controlled housing may be rationed, such as waiting lists, lotteries and various combinations of the two. A substantial body of theoretical literature investigates the welfare effects of such alternative non-market allocation mechanisms.[2] This literature demonstrates that the chosen method of allocation can, at least in theory, have a substantial impact on welfare. For instance, mechanisms that allow households to be more selective in their residential choices tend to give more efficient matches (Arnosti and Shi, 2020; Bloch and Cantala, 2017; Thakral, 2019). Still, a key message is that all these mechanisms induce inefficient allocation compared to the use of market prices.

An inefficient (non-market) allocation mechanism is not the only potential source of misallocation. Rental regulations often enforce particularly low rents for existing tenants by restricting annual rent increases. This introduces implicit transaction costs for households planning to move, as they have to forego the advantageous rent of their current dwelling. Consequently, rent-controlled housing can also become misallocated through reduced household mobility (see *e.g.*, Abdulkadiroğlu and Sönmez, 1999; Sönmez and Ünver, 2011; Gyourko and Linneman, 1989; Munch and Svarer, 2002; Mense et al., 2023).[3] In the Dutch public-housing system this source of misallocation is potentially amplified by income-based eligibility criteria which are only verified upon entry to a new home, meaning that public-housing tenants no longer meeting the income criteria would have to exit the sector entirely (and pay market prices) if they decide to move.

It is ultimately an empirical question whether the cost from misallocation in a given regulated housing market is sizeable. We focus on the extensive public-housing market in the Netherlands. Dutch public housing is subject to strict rental regulations, with both absolute rental caps as well as restrictions on annual rental increases. Most public-housing units are allocated through regionally centralised waiting lists, where

[2]See *e.g.*, Abdulkadiroğlu and Loertscher (2007); Abdulkadiroğlu and Sönmez (1999); Andersson et al. (2015); Andersson and Svensson (2014); Arnosti and Shi (2020); Bloch and Cantala (2017); Chen and Sönmez (2004); Ehlers and Klaus (2007); Kaplan (1986, 1987); Sönmez and Ünver (2011, 2010); Thakral (2019); Waldinger (2021). This housing literature also has strong links to a broader literature on non-market allocation mechanisms, including the market for organ donation (*e.g.*, Su and Zenios, 2004) and day care (*e.g.*, Kennes et al., 2014).

[3]Stamp-duty taxes similarly reduces residential mobility in the owner-occupied market by introducing transaction costs (Hilber and Lyytikainen, 2012).

households are allowed to defer their choice of residence indefinitely, and reject offered housing without losing their place in the queue. Recent theoretical insights indicate that this allocation method, referred to as a *centralised waiting list with choice*, is one of the most efficient mechanisms among a large set of possible non-price mechanisms studied (Arnosti and Shi, 2020). Another distinguishing feature is that regulated and unregulated housing markets coexist in the same geographical space, which may lead to different results than in geographically segmented markets (Chapelle et al., 2021).

Although there is a growing theoretical literature on allocation mechanisms, there are very few prior studies quantifying allocative welfare outcomes in practice. Davis and Kilian (2011) find that price ceilings on natural gas in the US have introduced substantial misallocation costs (which they refer to as 'allocative costs'). Li (2018) demonstrates the misallocation losses of using lotteries to obtain an automobile licence in China. Glaeser and Luttmer (2003) study housing allocation under rent control in New York city, and provide evidence of significant misallocation, concluding that households have a 21% chance of consuming a different number of rooms than they would under efficient allocation.[4] However, they do not explicitly calculate the associated welfare loss. To our knowledge, our study is the first to calculate the welfare costs from misallocation of rent-controlled housing, applying a measure of misallocation inspired by Glaeser and Luttmer (2003). We are also the first to investigate how the gains and losses of misallocation are distributed across demographic groups.

The main innovation by the study of Glaeser and Luttmer (2003) is that it introduces a methodology to measure the amount of misallocation generated by rental regulations. A key feature is that it allows one to distinguish misallocation from differences in consumption that arise from different distributions of housing supply and household characteristics in regulated and unregulated markets. Thereby it allows for differences in the choice sets of households in different markets.[5] Their methodology is based on

[4]We are aware of a range of studies that demonstrate differences in housing consumption between households in regulated and rent-controlled housing as well as evidence of reduced household mobility in rent-controlled housing (Mense et al., 2023). However, these studies are silent on the cost of not allocating housing to the household with the highest willingness to pay.

[5]Li (2018) investigates the extensive margin of having access to the regulated automobile market in China through winning the lottery for licenses. He assumes that conditional on access, the choice set of automobiles is identical in regulated and non-regulated markets. This assumption cannot be applied to housing markets, because the supply of housing in unregulated and regulated segments (and the resulting choice sets) are likely to differ from one another.

an *ordinal* measure of housing consumption, capturing how much housing is consumed by a household with a given set of characteristics (*e.g.*, age, household income and household size) relative to other households in the same market. For example, let us suppose households with children are observed to consume *more* housing than households without children in an unregulated housing market. In this case, public housing is found to be misallocated if households with children consume *less* housing than households without children in the public-housing market, indicating that there is a mismatch between households and housing.

Expanding on this paper we make a number of contributions. First, we estimate the welfare cost of misallocation. To this end, we exploit nationwide register data from Statistics Netherlands, which allows us to employ the market value of a home as a comprehensive measure of housing consumption for both public and private housing. This aids the economic interpretation of our results, as it provides us with a monetary measure of misallocation, which we subsequently apply to calculate the associated welfare cost.

Second, we improve on the existing methodology by constructing a weighted measure of misallocation which does not depend on the number of categories defined for housing consumption. Glaeser and Luttmer's original, unweighted, measure of misallocation is construed to be an increasing function of the number of categories used. This property is particularly unattractive when considering continuous housing consumption characteristics, where the number of categories is in principle infinite (*e.g.*, house size or market value). We demonstrate that by employing a weighted misallocation measure, the magnitude of the measured level of misallocation no longer mechanically increases with the number of categories used.

Third, as highlighted by Glaeser and Luttmer (2003), a drawback of their methodology is that they find a non-negligible level of misallocation when performing placebo tests comparing unregulated markets with each other. Under the key assumptions of their methodology, a household type (as determined by its observed household characteristics) consuming more housing than another household type in an unregulated market, should consume more housing than the latter household type in any other unregulated market. A failure of the placebo test indicates that either market-specific conditions are affecting the ordering of housing preferences among household types, or that unobserved household characteristics are causing omitted variable bias.

We also find non-negligible placebo effects when we apply Glaeser and Luttmer's original method, but demonstrate that we can substantially reduce the placebo effects by excluding income and wealth from our measure of misallocation, treating these variables as controls rather than applying them to predict housing demand.[6] This is a non-trivial finding, as it implies that differences in income and wealth may bias our measure of misallocation unless appropriately controlled for. This could be the case if market-specific conditions (such as access to amenities, expected tenure duration, or the investment value of a home) influence the housing demand of affluent households differently than that of less affluent households. Alternatively, potential sources of omitted variable bias (such as parental wealth) may correlate with income and wealth. Some placebo effects remain statistically significant even after controlling for income and wealth, however they are of such a negligible size that they do not invalidate our methodology.

An additional benefit of omitting income and wealth from our measure of misallocation is that intentional redistribution of housing consumption from high-income to low-income households is not included in our measure of misallocation. Given inequality-averse social preferences, redistribution of housing from high-income to low-income households may generate societal welfare benefits, and this is often viewed as an underlying aim of public-housing systems (see *e.g*, Lui, 2007). Still, allocation mechanisms targeting housing to specific income groups tend to produce less efficient overall housing matches than less targeted alternatives, meaning there is often a trade-off between efficiency and equity (Arnosti and Shi, 2020). The size of this trade-off depends on the social preference for equality, household preferences for housing and the (unobserved) distribution of outside options, *i.e.* where people end up living if they are not allocated into public housing (Waldinger, 2021). Holding income and wealth constant allows us to take a step back from this debate, measuring misallocation entirely based on household characteristics such as household size, composition, birthplace and age.

[6]Using income and wealth as *predictors* entails treating changes in how housing is allocated across households with different income and wealth levels as misallocation. Using them as control variables entails holding these characteristics fixed, meaning we omit any changes in allocation across these characteristics from our analyses.

Our paper delivers the following empirical findings. First, we measure an average market value of misallocation of around €14 thousand per household, which represents around 7.5% of the value of an average public housing unit. This means that, on average, public-housing tenants are allocated a home which is worth €14 thousand more, or less, than what they would be consuming under efficient allocation. This generates a welfare loss because overconsuming households have a lower willingness to pay for their excess housing consumption than underconsuming households. The estimated annual welfare loss is modest, at around €64 per household and about €134 million in the aggregate.[7]

Second, we examine how the misallocation of public housing is distributed across demographic subgroups within the public-housing sector. We find that young and single-person households substantially underconsume housing relative to older and larger households. We also find that households with a breadwinner or a partner born in the region overconsume public housing relative to households from outside the region. This indicates that the allocation mechanism applied in the Netherlands – regionally centralised waiting lists with choice – benefits older households, larger households, and households born in the region.

The allocation mechanism appears to be particularly disadvantageous for households with a breadwinner aged between 25-35, who underconsume housing by around €25 thousand on average (almost twice as much as the average level of misallocation). This is a particularly important result for the theoretical economic literature on the efficiency of different non-market allocation mechanisms, which often overlooks the important role played by age.[8] Although part of the welfare costs for young households will be compensated over their lifetime (as they become older), some generational inequalities could persist, particularly because in most countries (including the Netherlands) the share of regulated housing is decreasing over time (Angel, 2020). This could potentially constrain the access of young households to the public-housing market even as they grow older.

[7] We arrive at the total welfare loss of around €134 million by aggregating the average annual welfare loss of €64 per household across the 2.1 million households living in the public-housing sector.

[8] Theoretical models tend to assume that housing preferences do not change while households accumulate waiting time. This is clearly a constraining assumption in markets such as the Netherlands, where average regional waiting times often exceed 10 years.

In order to contextualise our study, we provide a brief overview of how the Dutch public-housing market is organised in Section 2. Sections 3 and 4 describe the data and methods applied to construct the measures of misallocation used in our study. Section 5 contains our main results and discussion, with robustness checks discussed in Section 6. Section 7 concludes.

2 Institutional context

2.1 Public housing

Around 32% of the housing stock in the Netherlands is regulated, rent-controlled, rental housing, of which the vast majority is public housing (*i.e.* owned by non-profit public-housing corporations). In this study we focus on public housing, representing around 28% of the total housing stock.

Housing corporations are responsible for the upkeep and management of the public-housing stock.[9] They are also responsible for the allocation of vacant public-housing units to eligible households, with aggregate targets regulated at the national level: 90% of housing has to be allocated to households earning less than an income threshold, which in 2020 was set at around €45 thousand per year depending on the size of the household (for comparison, the median household income was €52 thousand). Rents are strictly regulated, with an intricate quality assessment system determining the maximum rental price, resulting in a substantial discrepancy between regulated and unregulated prices.[10] The average rent in the public sector is around €590 versus around €850 in the private sector.[11] Annual rental increases for sitting tenants are also regulated, and in recent years depend on inflation and household income.

Housing corporations use centralised waiting lists organised at the regional level to allocate housing. Households accumulate waiting time by subscribing to a regional waiting list and paying a small annual registry fee (typically less than €25 per year).

[9]We note that these corporations have been committed to building and maintaining homes of a high standard.

[10]Maximum rents partly depend on the market value of a home, but also on more specific criteria such as the size and quality of the kitchen and bathroom. There is also an overall maximum monthly rent set at around €760, although corporations may let up to 15% of their housing stock above this cap. The subset of public-housing units above the price cap is less strictly regulated, but still subject to some price controls.

[11]The average net rent paid for public-housing units is even less, as about two-thirds of public-housing tenants receive rental subsidies.

Once a household moves into a public-housing unit, their waiting time is reset to zero. Typically they choose to remain registered on the waiting list (and in some regions this happens automatically) as many low-income individuals reside a large part of their life within the public-housing sector, moving from one public-housing unit to another, meaning that the waiting time of tenants within the public-housing sector typically accumulates from the time of their last residential move.

Available housing is publicly advertised through a central database, and any eligible household on the regional waiting list may apply. The market is thick, meaning that there tend to be many households applying for each available house. Housing is first offered to the household that has waited the longest. As described above, the system is essentially a *waiting-list with choice*, as described in Arnosti and Shi (2020): households may apply to as many houses as they please, may choose to reject an offer, and do not lose their place in the queue if an offer is rejected, allowing them to continue waiting until they find a preferable home. Eligibility criteria for income are only tested upon entry to a new home. Consequently, households residing in public-housing units who are no longer eligible due to a higher income cannot move within the public-housing sector, but are not required to leave their current dwelling.

Subject to these general practices, there is some variation in the applied allocation rules between regions. Some corporations assign extra queuing points, boosting the registered waiting time of targeted households. For example, extra points may be assigned to people actively applying for homes within recent months, or to younger households to give them a better chance of gaining a home. Conversely, the waiting time of tenants already residing in the public-housing sector may be discounted relative to the waiting time of starters. Occasionally, corporations exclusively offer specific types of housing to specific groups on the waiting list, such as accessible housing to the disabled or elderly. Larger homes are often exclusively offered to families with children. Most corporations are also familiar with the use of lotteries, particularly in the allocation of urgent or short-term rentals such as student accommodation. However, the vast majority of public housing is still allocated through the regional waiting list.[12]

Municipalities may also enforce additional allocation requirements, although their right

[12]Based on a survey of six Dutch housing regions, Kromhout and Wittkämper (2019) report that 70-90% of public housing has been allocated through a waiting list.

to do so is restricted by national legislation. They are entitled to demand that as much as 50% of their public-housing stock is allocated to households already living or working in the region, which is a common restriction in the highly urbanised Randstad area. Municipalities are also entitled to ascribe urgency status to particular households (such as single parents or refugees). Households with urgency status take precedence over the centralised waiting lists, but are often assigned housing according to a principle of take-it-or-leave-it, rather than allowing them to choose exactly where they wish to live. The share of vacant housing reserved for households with urgency status is subject to negotiations between the municipality and the housing corporations, which generates large discrepancies in how urgency status is applied. There are records of municipalities reserving as much as 30% of their vacant public-housing units for households with urgency status in a given year, whereas around half of the municipalities do not have any urgency provisions (Kromhout et al., 2020).

2.2 Unregulated housing

To analyse whether public housing is misallocated, we wish to know how it would have been allocated if it were organised as an efficient (unregulated) market. To this end, we use information on how unregulated housing is allocated. We distinguish between two unregulated market segments: the owner-occupied market and the private-rental market.

Private-rental housing is relatively rare in the Netherlands and is an unusual type of tenure to hold for an extended amount of time. Only 12% of the housing stock is rented privately. Although private-rental housing is less regulated than public housing, it is estimated that around 40% of private-rental homes are still subject to (sometimes mild forms of) rent control. The administrative dataset we use does not provide an overview over which private-rental homes are subject to controls. However, we observe housing benefits which are exclusively granted to tenants of rent-controlled homes, allowing us to disregard these households from our sample of 'unregulated' rental housing. Still, some tenants in rent-controlled housing without subsidies remain in our private-rental sample (we estimate around 20% of the sample). Therefore we also utilise the owner-occupied market as an alternative 'unregulated' comparison market.

The owner-occupied market is the largest market segment in the Netherlands, representing 60% of the housing stock. We treat it as an 'unregulated' comparison market since there are no price regulations in this market segment, and as such the price mechanism should efficiently allocate housing to the household with the highest willingness to pay. Still, we note that this market segment is subject to tax distortions such as mortgage interest deductibility and transfer taxes. Transfer taxes in particular may lead to some misallocation through reduced household mobility (Hilber and Lyytikainen, 2012). However, transfer taxes are low, currently set at a mere 2% for owner-occupiers, with an exemption for first-time buyers under the age of 35.

3 Data

We utilise administrative microdata from Statistics Netherlands for the year 2020, which contains a complete register of all households, including a comprehensive set of household characteristics.[13] For each house, we know the household occupying it, the type of owner (*i.e.,* whether the home is owner-occupied, or let by a housing corporation or a private landlord) and its geographical location. We distinguish between three broad housing markets: public housing, owner-occupied housing and private-rental housing. As indicated above, we will treat the latter two types of housing as 'unregulated'.[14] We also distinguish between 36 different housing regions, characterised by a common centralised waiting list (regions are listed in Appendix C).

To capture misallocation, we examine the difference in the allocation of housing consumption across observable household characteristics between the treatment group (public housing) and the control groups (owner-occupied and private-rental housing). Household characteristics include household size and composition (number of adults and children), income, net wealth (excluding net housing wealth), the age of the household head and the birthplace of the household head and their partner (if they

[13]2020 was the most recent year for which data was available when we commenced our analysis. Housing conditions are measured on January 1st, meaning we are not concerned about the COVID pandemic affecting the housing distribution. Besides, the distribution of the housing stock across households does not change much from one year to the next (as most households will not change their living situation in a given year). However there may be temporary effects on household income, which is measured across 2020 in the administrative data. This partly motivates the inclusion of household wealth as a more stable measure of accumulated income over time.

[14]We remove tenants of private-rental housing from our sample if they receive housing benefits, as these houses are subject to rental regulations, whilst not being subject to the general allocation mechanism of the public-housing market. Institutional housing (*e.g.,* jails and nursing homes), vacant housing, and homes with multiple households are also excluded from our analysis.

have one).[15] When analysing the data, we split observed household characteristics into categories, applying five income and wealth quantiles (*i.e.*, quintiles), seven age categories, eight household composition categories, and a dummy variable indicating whether the birthplace of a household head and/or their partner is within the region they are currently living in (as displayed in Table 1). Our final dataset comprises 5.35 million households, which is around 67% of the total Dutch population.

Housing consumption is captured by the assessed market value of a house, which is used to levy national and municipal property taxes. The market value is imputed by tax authorities based on sales prices from recent transactions of nearby properties with similair physical attributes. This provides us with a comprehensive, continuous, measure of housing consumption for all market segments.[16]

The descriptive statistics (Tables 1 and 2) show that there is a large amount of overlap in observable household and housing characteristics between our treatment and control groups. Still, as expected, incomes are lower in the public-housing market than in the other housing segments. We also see a particular concentration of household wealth among owner-occupiers. In terms of the number of children and age of the household head, the samples for public housing and owner-occupied housing are similar, whereas private-rental properties appear concentrated amongst younger and single-person households. In the private-rental segment the majority of households are born outside the region, whereas the opposite is true for owner-occupied housing. Interestingly, almost half of the tenants of public housing are born outside the region they live in, suggesting that this group of tenants is more mobile than owner-occupiers and less mobile that private-rental tenants.

Unsurprisingly, public-housing units are smaller and less expensive on average than housing in the unregulated market segments. However, the average market value of housing *per square metre* is remarkably uniform across markets, particularly between public and owner-occupied housing. In other words, the lower assessed market value

[15]We exclude households earning less than the minimum wage, the top and bottom percentiles in terms of household wealth, the top percentile in terms of household size (households with more than 7 members), and households where the main income earner is under the age of 25. We have information on education for a subset of households but do not use this information in our baseline analysis, as the sample is biased towards younger and higher-educated households.

[16]A potential drawback of using assessed market values is that there may be inaccuracies in the assessment. For a discussion of such assessment inaccuracies we refer to studies such as Avenancio-León and Howard (2022)

Table 1: Distribution of household characteristics

	Public housing	Owner-occupied	Private-rental
Panel A: Household income			
1st quintile	0.492	0.089	0.156
2nd quintile	0.272	0.228	0.286
3rd quintile	0.155	0.31	0.316
4th quintile	0.080	0.372	0.241
Panel B: Net household wealth (excl. housing)			
1st quintile	0.362	0.120	0.289
2nd quintile	0.312	0.174	0.209
3rd quintile	0.178	0.229	0.177
4th quintile	0.099	0.241	0.165
5th quintile	0.049	0.236	0.160
Panel C: Age of breadwinner			
25-35	0.158	0.134	0.366
36-45	0.143	0.150	0.141
46-55	0.173	0.172	0.119
56-66	0.203	0.208	0.122
67-75	0.156	0.197	0.113
76 +	0.168	0.138	0.139
Panel D: Household composition			
1 adult, no child	0.481	0.306	0.509
1 adult, 1 child	0.085	0.040	0.042
1 adult, 2+ children	0.063	0.020	0.023
2 adults, no child	0.231	0.387	0.310
2 adults, 1 child	0.056	0.099	0.061
2 adults, 2 children	0.044	0.111	0.038
2 adults, 3+ children	0.032	0.032	0.012
3+ adults	0.007	0.006	0.005
Panel E: Birthplace			
Outside region	0.485	0.407	0.566
In region	0.515	0.593	0.434

Notes: All quantiles (*e.g.*, income quintiles) are determined at the national level. The reported shares sum up to one for each column in each panel. The number of observations are: 1,528,061 (public housing), 2,348,155 (owner-occupied) and 370,455 (private-rental)

of public housing seems to be primarily driven by smaller housing units as opposed to a lower (assessed) quality of housing.

As we will demonstrate, appropriately controlling for income is crucial in our analysis. We therefore flexibly control for income by including income splines, interacting each income quintile with continuous (logged) housing income. This presents some computational difficulties for the fifth income quintile in particular as it so rarely occurs in the public-housing market (only 3% of households in the public-housing market belong to

Table 2: Average household and housing characteristics

	Public housing	Owner-occupied	Private-rental
Market value (€)	187,531	284,562	244,750
	(73,234)	(127,904)	(139,286)
Housing size (m^2)	85.15	124.96	91.39
	(28.16)	(131.49)	(85.29)
Market value per m^2 (€)	2,324	2,399	2,984
	(1,181)	(1,003)	(1,702)
Household income (€)	40,553	66,836	58,520
	(20,536)	(24,668)	(23,831)
Net household wealth (€)	16,126	83,320	52,383
	(56,464)	(172,948)	(147,906)
Age of breadwinner (years)	57	57	50
	(18)	(17)	(20)
Household size	1.88	2.15	1.70
	(1.14)	(1.11)	(0.91)
Number of children	0.50	0.51	0.27
	(0.93)	(0.87)	(0.66)
Birthplace in region (dummy)	0.51	0.59	0.43
	(0.50)	(0.50)	(0.49)
Observations	1,528,061	2,348,155	370,455

Note: Mean values, with standard deviations in parentheses. Net household wealth excludes all housing wealth.

the top income quintile). Therefore this household group is disregarded in our main analyses.

In an alternative specification (see Appendix B.4), we use matching to minimize the differences in the sample distribution of household characteristics between the three markets. This also allows us to include the top income quintile in our analysis. The measured level of misallocation remains in line with our main results, indicating that our analysis is robust to the observed differences in household characteristics between markets (see Section 6).

The outcome of interest to our analysis is the (regional) *ordering* of housing consumption for each housing segment. In our main analysis, we therefore determine the level of housing consumption (captured by market value) using deciles, forming ten housing categories. The deciles are calculated separately for each market segment and each region.[17]

[17] We test the sensitivity of our results to the division of housing consumption into more quantiles, and to the use of national (versus regional) quantiles.

4 Methods: measuring misallocation

4.1 Assumptions

4.1.1 Standard assumptions

We proceed by assuming that we observe a continuous measure of housing consumption in market m, denoted by H_m. It is possible to distinguish between markets across several different dimensions, such as types of tenure and geographical regions. In our application, we use housing tenure to distinguish between different housing markets, m. The public-housing market is treated as regulated, whereas the owner-occupied and private-rental market are defined as two separate unregulated markets. In doing so, we apply the same key assumptions as in Glaeser and Luttmer (2003):

Assumption 1 (No reversals): *If household A consumes more of housing attribute, H, than household B in the unregulated market, m, then household A will consume weakly more of that attribute than household B would in any other unregulated market.*

This assumption implies that households can be ordered by their demand for a housing attribute. Household ordering is assumed to be captured by an unobserved demand index, $\theta \in R$, which determines the household's chosen consumption level. Actual housing consumption varies between markets, also when they are unregulated, because of differences in the composition of both household and housing characteristics (*e.g.*, in some markets, houses may be larger or more varied in terms of amenities). In contrast, efficient *ordering* of housing consumption is assumed to be identical between unregulated markets, and as such is assumed to be independent of the market-specific distribution of household and housing characteristics.

Given Assumption 1, it is in principle possible to test for the consequences of regulation by comparing the ordering of the housing consumption of households A and B in the regulated market with the ordering in an unregulated market. However, this introduces the difficulty that we observe a household in a certain market, but we do not observe the counterfactual, *i.e.* the same household in another market. For that reason, rather than focusing on individual households, we focus on household types, for which it is possible to define the counterfactual.

We assume that each household belongs to a demographic group, i, defined by its observable household characteristics (referred to as a *household type*). Households who belong to the same group and market may have different observed levels of housing consumption due to unobserved variation in demand. Hence, we define for each household type a probability density function (PDF) of demand, denoted by $\phi_m(\theta|i)$, and a corresponding cumulative distribution function of demand (CDF), denoted by $\Phi_m(\theta|i)$.

In order to apply Assumption 1, we follow Glaeser and Luttmer (2003) by assuming that the distribution of the latent demand of a household type is constant over markets, except for a market-specific shift parameter, λ_m. More formally:

Assumption 2 (No differential selection on unobservables): *for all unregulated markets m, and for all groups i, the CDF of demand $\Phi_m(\theta|i) = \Phi(\theta + \lambda_m|i)$.*

This assumption means that the shape of the latent demand distribution for each household type in each market is identical except for a market-specific shift in the distribution. This assumption implies that the ordering of housing consumption between *different household types* should be the same for different unregulated markets.

Together, these assumptions imply that a household type which consumes more of a housing characteristic than another household type in one unregulated housing market will consume more of that characteristic than the latter type in any other unregulated market. Given these assumptions, a difference in the ordering of housing consumption of a particular household type between the regulated market and the counterfactual (unregulated market) is interpreted as misallocation induced by rental regulations.

Misallocation is only measured between household types in the same market. As such, potential misallocation between households categorised as the same type (due to unobserved household differences) is not measured in this framework, neither is misallocation between different markets. This results in a conservative estimate of misallocation.

4.1.2 Relaxing the standard assumptions

We test the joint prediction of the assumptions outlined above using a range of placebo tests comparing different unregulated market segments with each other. Initially, we find non-negligible placebo effects, which would imply that our assumptions do not hold (see Section 5.2). However, once we hold income and wealth constant across households, the placebo effects reduce to a negligible size (see Table 3). This motivates us to adjust the above assumptions, by making them conditional on household income and wealth.

Assumption 1′ (No reversals, conditional on income and wealth): *If household A consumes more of a housing attribute, H, than household B in the unregulated market, m, then household A will consume weakly more of that attribute than household B would in any other unregulated market, conditional on the two households having the same income and wealth.*

We make similar adjustments to the second assumption. Let us denote income and wealth of group i by W_i. We then define the (arbitrary) function $f_m(W)$, which may differ between different markets m. We then assume the following:

Assumption 2′ (No differential selection on unobservables, conditional on income and wealth): *for all unregulated markets m, and for all groups i, the CDF of demand θ, $\Phi_m(\theta|i) = \Phi(\theta + \lambda_m + f_m(W)|i)$.*

Note that the vector W_i may be extended to include any characteristic which one may wish to hold constant across markets in order to remove its influence on the measured level of misallocation. We also control for region fixed effects in the vector W_i, thereby allowing for demand to differ between regions.

4.2 Empirical approach

To quantify the amount of misallocation in the housing market, we estimate a generalised ordered probit model, where latent housing demand is distributed across categorical values of housing consumption as in Glaeser and Luttmer (2003). This approach allows us to flexibly capture the effects of observable household characteristics, X_i, while controlling for income, wealth and the region a household lives in, which are captured in the vector W_i.

In our application, we distinguish between K categories of housing consumption of equal size, *i.e.*, housing consumption quantiles. Housing consumption is measured using the market value of a house, and consumption quantiles are separately determined for each region and market. In our main specification we work with deciles (*i.e.*, $K = 10$), so that category 1 refers to the bottom 10% of housing within a given market and region (in terms of market value), and category 10 refers to the top 10%.

For each household type with characteristics, X_i, W_i, residing in market m, we define a PDF of latent demand, denoted by $\phi_m(\theta|X_i, W_i)$ and a corresponding CDF, denoted by $\Phi_m(\theta|X_i, W_i)$. The latent demand for these households is assumed to be normally distributed. We allow for heterogeneous variation in housing consumption choices across household characteristics such as income and household composition. The distribution of latent demand is thus given by:

$$\phi_m(\theta|X_i, W_i) = N(X_i'\beta + W_i'\gamma_m - \lambda_m, e^{X_i'\sigma + W_i'\eta_m,}). \tag{1}$$

The consumed category of housing H, of an individual household i, with latent demand θ, in market m, with K consumption categories is then given by:

$$H(\theta|X_i, W_i, \mu_n^m) = \begin{cases} 1 & \text{if } \theta \leq \mu_1^m \\ 2 & \text{if } \mu_1^m < \theta \leq \mu_2^m \\ 3 & \text{if } \mu_2^m < \theta \leq \mu_3^m \\ \vdots & \\ K & \text{if } \theta > \mu_K^m, \end{cases} \tag{2}$$

where μ_n^m denotes the market specific cut-offs. These cutoffs determine the relationship between the continuous latent demand function and categorical housing consumption outcomes. Note that the μ_n^m are determined by the distribution of household characteristics and housing supply within the market. For example, two households with identical latent demand functions residing in different markets, may still consume a different amount of housing if the standard of the housing stock differs between the markets. Similarly, a household consuming the highest-valued house in a market with few competing tenants may not consume the highest-valued house in an identical market with many competitors.

We then take several steps. First, we estimate the heterogeneous ordered probit model for the unregulated market. This provides us with estimated coefficients of the latent demand distribution ($\widehat{\beta}, \widehat{\gamma}_m, \widehat{\sigma}$ and $\widehat{\eta}_m$).[18] The basic idea is that housing consumption in the unregulated market reveals the relationship between a household's characteristics

[18] λ_m is not separately identified from the market specific cut-offs.

and its marginal willingness to pay for housing. Second, we employ the observed household characteristics (X_i) in the public-housing market and the adhering coefficients ($\widehat{\beta}$ and $\widehat{\sigma}$) estimated in the unregulated market to generate a counterfactual latent demand function for public housing. Crucially, in this step, we exclude the effect of the control variables (W_i). In other words we estimate a counterfactual latent demand distribution that is unaffected by differences in income, wealth and regional fixed effects.

Second, we proceed to estimate the counterfactual market-specific cutoffs ($\widehat{\mu}_n^{ph}$), as specified in Appendix A.1. We set the counterfactual cutoffs such that the predicted amount of public housing consumed of each housing category is equal to the observed amount of public housing supplied, eliminating the influence of any variation in the distribution of housing characteristics and housing supply between markets. The possibility of setting the cutoffs to fit the (normally distributed) estimated latent demand function to the actual distribution of supply is an important benefit of working within the framework of an ordered probit model. Applying these counterfactual cutoffs, and the counterfactual latent demand curve, we estimate the counterfactual consumption of public housing: $\widehat{H}(\widehat{\theta}|X_i, \widehat{\mu}_n^{ph})$.

We aim to compare this counterfactual allocation of public housing to the actual allocation of public housing. In order to avoid differences between actual and counterfactual housing allocation arising from the underlying assumptions of the ordered probit model, we compare the estimated counterfactual allocation to an *estimated* actual allocation. To obtain the latter, we estimate the same probit model for the public-housing market, repeating the steps outlined above to arrive at an estimate of the actual public-housing consumption, $\widetilde{H}(\widetilde{\theta}|X_i, \widetilde{\mu}_n^{ph})$.

Finally, we determine the level of housing misallocation for each household type by measuring the difference in the predicted housing consumption between the actual and the counterfactual estimation. In Glaeser and Luttmer (2003), the level of misallocation M_i for household type i is measured as the share of the normalised PDF that falls in one category in the regulated market, but in another in the counterfactual situation:[19]

$$M_i = \sum_{n=1}^{K} \sum_{k=1}^{K} 1 \times \int_{-\infty}^{\infty} I_{\widetilde{H}_i = k,\, \widehat{H}_i = n}(\theta)\, \phi_{\mathrm{norm}}(\theta | X_i)\, d\theta, \quad \forall\, k \neq n\,. \tag{3}$$

Here, $\widetilde{H}$ denotes the estimated actual housing consumption, $\widehat{H}$ the estimated counterfactual housing consumption, K the number of categories used in the analysis, and I is an indicator function equal to one when the estimated (actual) housing category differs from the counterfactual estimate (see Appendix A.2). This captures the probability that the allocation of public housing would be different if it were unregulated.

An important drawback of the measure M_i, is that the amount of misallocation measured positively depends on the number of categories chosen (see Appendix B.2). With more categories, the likelihood of ending up in the same category in two different markets diminishes, increasing the probability of being misallocated. This is a particularly unattractive property when dealing with a continuous measure of housing consumption, such as the market value of housing, because one could essentially have an infinite amount of categories, reducing the probability of being in the exact same category to zero.

To produce a measure of misallocation that does not mechanically increase as we add more categories, we calculate a weighted monetary version of equation (3), where the probability of a mismatch between two categories is weighted by the absolute difference in their average market value:

$$M_i^w = \sum_{n=1}^{K} \sum_{k=1}^{K} |p_k - p_n| \times \int_{-\infty}^{\infty} I_{\widetilde{H}_i = k,\, \widehat{H}_i = n}(\theta)\, \phi_{\mathrm{norm}}(\theta | X_i)\, d\theta, \quad \forall\, k \neq n\,, \tag{4}$$

where p_n denotes the average market value of housing in category n. By adding weights, we essentially include information about the magnitude of the observed misallocation. As we divide housing consumption into more quantiles (K), the likelihood of ending up in a different quantile will increase, but the distance between quantiles decreases.

[19]Different markets have different distributions of latent demand. Therefore the PDF of latent demand is normalised (by subtracting by the market's population mean and dividing by the market's population variance), to be able to compare the difference in the probability of a household consuming a given housing category between markets.

Consequently M_i^w does not mechanically increases in the number of quantiles.[20] Rather, the measure will become more precise as we add categories. Ultimately our choice in the number of quantiles involves a trade-off between precision and computational efficiency, as the estimation procedure becomes more computationally demanding for more categories. In our baseline analysis, we opt for deciles ($K = 10$), but we check the sensitivity of our results to the addition of more quantiles (see Appendix B.2). An additional benefit of M_i^w, is that it provides us with a monetary measure of misallocation, which is helpful in gauging the economic significance of our results.

The average level of misallocation is then the sum of the misallocation for each houshold type, i, multiplied with the share of that household type in the public-housing market, S_i^{ph}:

$$M^w = \sum_i S_i^{ph} \, M_i^w \, . \tag{5}$$

Equation (4) uses absolute weights $|p_k - p_n|$ resulting in a positive measure of misallocation, regardless of whether a household type consumes *more* housing or *less* housing in the counterfactual (unregulated) market than in the actual (regulated) market. We also explore which households consume more housing and which households consume less by introducing a non-absolute weighted measure of misallocation, which we will refer to as a measure of over- and underconsumption:

$$O_i^w = \sum_{n=1}^{K} \sum_{k=1}^{K} (p_k - p_n) \times \int_{-\infty}^{\infty} I_{\widetilde{H}_i = k, \, \widehat{H}_i = n}(\theta) \, \phi_{\text{norm}}(\theta | X_i) \, d\theta, \quad \forall \, k \neq n \, . \tag{6}$$

A negative value of O_i^w implies that a household is allocated less housing in the regulated market than they would in an unregulated market (*i.e.*, they are underconsuming), whereas the opposite is true when O_i^w is positive. Again, it is important to note that our notion of misallocation compares estimates of the actual allocation of public housing to the counterfactual unregulated allocation of *the same* stock of housing among *the same* population of tenants. This entails holding the public-housing stock and tenant population fixed, such that an increase in the housing consumption of one household requires an equivalent decrease in housing consumption of other households. Thus, we are not analyzing whether housing is over- or underconsumed relative to the consumption of

[20]Whether it gets larger or smaller depends on the distribution of demand within categories.

other goods, but rather whether it is over- or underconsumed relative to the housing consumption of other households. This means that the average level of O_i^w across all households is zero by construction.

To obtain standard errors, we apply a cluster-bootstrapping procedure, where we cluster standard errors by region. A more detailed description of the method can be found in Appendix A.3.

4.3 Welfare implications

The above method tells us how much the consumption of housing has been misallocated. However, this does not directly provide us with information about the resulting welfare loss. Since misallocation entails a transfer of housing consumption, the overall welfare effect depends on the difference between the marginal benefits gained by overconsuming households, and benefits lost by underconsuming households.

It is important to stress that by isolating the welfare costs of misallocation, we only provide a partial analysis of the welfare effects of public housing. As we have already highlighted, we keep the number of public-housing units and tenants fixed in our counterfactual analysis. Consequently, welfare effects associated with changes to the supply of housing or to the aggregate demand for housing are disregarded.[21] Furthermore, we disregard welfare transfers between consumer and producer surplus generated by regulated rents, as we remain agnostic about the marginal cost of supply.[22] We also abstract from any welfare effects arising from social preferences for redistribution, since income and wealth are held constant in our analysis (meaning that we essentially capture welfare effects of misallocation between households with equivalent income levels).

If we assume that the marginal benefit of consumption is decreasing in the level of consumption (implying downward-sloping demand curves), the marginal loss generated by the household that is underconsuming always exceeds the benefit gained by the household that is overconsuming. Hence, the aggregate welfare effects of misallocation are negative.

[21] For instance, regulated prices may increase aggregate housing demand generating spillover effects in the prices of unregulated housing, as discussed in Mense et al. (2023)

[22] For a broader overview of how access to affordable housing can affect consumer surplus, we refer to studies such as Bulow and Klemperer (2012), Sieg and Yoon (2020) and Favilukis et al. (2023)

To illustrate this welfare loss, let us consider a market with two households. The total stock of housing, S, is given such that $H_1 + H_2 = S$. Both households have a downward-sloping inverse demand function $P_1(H_1)$ and $P_2(H_2)$ respectively. Given these assumptions it is straightforward to show that total welfare is maximised when both households pay the same marginal price for housing:

$$
\begin{aligned}
& \max_{H1,H2} \int_0^{H1} P_1(H)\, dH + \int_0^{H2} P_2(H)\, dH \quad \text{s.t. } S = H_1 + H_2, \\
\rightarrow \quad & \max_{H1} \int_0^{H1} P_1(H)\, dH + \int_0^{S-H1} P_2(H)\, dH, \\
\rightarrow \quad & P_1(H_1) = P_2(S - H_1).
\end{aligned}
$$

Misallocation generates a deadweight loss through the deviation from this optimum. In Figure 1, we illustrate the equilibrium, where the optimal price has been standardised to one.[23] The shaded areas of the figure represent the deadweight loss. This loss is generated because the consumer surplus gained through overconsumption cannot compensate the losses generated by the underconsuming household.[24]

Moving away from our two-household example, we now calculate the welfare loss generated by each household type in public housing. For convenience, we assume that all households have an identical log-linear demand function, given by:

$$\log H_i = c_m + \gamma \log P_i(H_i)\,. \tag{7}$$

Consequently, a key parameter for the overall deadweight loss is the price elasticity of housing demand (*i.e.*, the slope of the demand function), $\gamma < 0$. In our main analysis we apply an elasticity of -0.5, but we also check the sensitivity of our results to a range of elasticities suggested in the literature between -0.35 and -0.8 (see *e.g.*, Zabel, 2004; Van Ommeren and Van der Vlist, 2016).

[23] Here the x-axes (horizontal axes) should be read from left to right for household 1 and from right to left for household 2. As such, the consumption of household 1 is decreasing as you move from right to left, whereas the consumption of household 2 is increasing.

[24] Whether the losses accrue to households or producers depends on the marginal costs of supply. If the marginal cost of supply equals the market value of a home in the unregulated market, the darkly shaded triangle of Figure 1 represents lost consumer surplus of household 1, whereas the lighter triangle represents lost producer surplus. In the event that public-housing costs lie well below the market price, the entire welfare loss accrues to household 1.

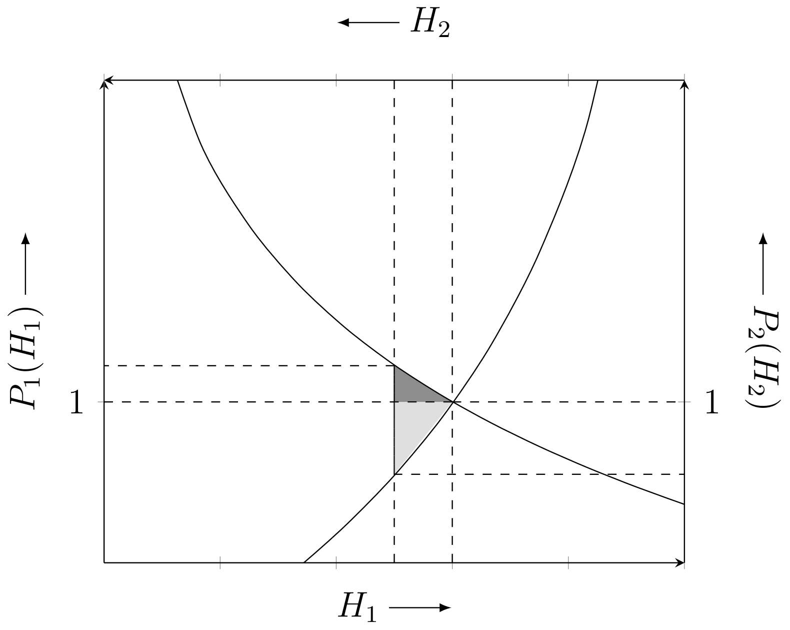

Figure 1: Deadweight loss with two-households

In our estimate of misallocation, we have already calculated the probability of a household type consuming a given household quantile. This allows us to calculate the expected housing consumption of a household for the purpose of estimating the welfare effect: $\mathbb{E}[H_i] = \sum_{n=1}^{K} \Pr(H_i = n) \times p_n$. To simplify our notation, we denote the expected housing consumption in the public and counterfactual market by $\widetilde{H_i}$ and $\widehat{H_i}$ respectively. Note that the difference in expected housing consumption between the treated and counterfactual market, is equivalent to our definition of over- and underconsumption (see Appendix A.4), such that: $O_i^w = \widetilde{H_i} - \widehat{H_i}$.

We standardise the price in the counterfactual market to one, so that $P_i(\widehat{H_i}) = 1$. In doing so, we assume that the market value of a public-housing unit accurately reflects the marginal willingness to pay for this housing unit in a counterfactual market with efficient allocation. This assumption seems fairly intuitive, but requires us to disregard any general equilibrium effects of regulation on the market value of public housing. Returning to equation (7), the marginal willingness to pay of a household can now be expressed as: $P_i(\widetilde{H_i}) = \left(\widetilde{H_i}/\widehat{H_i}\right)^{1/\gamma}$.

Given these assumptions, it can be shown (see Appendix A.5) that the welfare loss generated by the misallocation of household type *i* equals:

$$\Delta W_i = \widetilde{H}_i - \widehat{H}_i - \frac{\gamma}{(1+\gamma)} \left(\widetilde{H}_i \left(\frac{\widetilde{H}_i}{\widehat{H}_i} \right)^{\frac{1}{\gamma}} - \widehat{H}_i \right) . \tag{8}$$

The average welfare loss per household follows immediately:

$$\Delta W = \sum_i S_i^{ph} \, \Delta W_i \, . \tag{9}$$

The market value of housing reflects the lifetime value of a house, as opposed to its annual value. Therefore, as a final step, we translate the welfare cost of misallocation to an *annual* cost, w^a, by presuming that housing can be treated as a perpetuity, with a discount rate ρ:

$$\Delta w^a = \rho \Delta W \, . \tag{10}$$

In our application we will use $\rho = 3.7\%$. This is the rate recommended for discounting real estate by the Dutch advisory committee (De Vries et al., 2022). It also roughly corresponds with the parameter estimated for the English housing market by Koster and Pinchbeck (2022).

4.4 Placebo tests

The standard assumptions underlying our methodology imply that the different markets defined in this study – public, owner-occupied and private-rental housing – are not affected by unobserved differences in demand that change the ordering of household preferences (other than those introduced by price regulations and non-market allocation). This may be a restrictive assumption. For example the role of housing as an investment good may affect the demand for owner-occupied housing differently than the demand for rental housing. Different expected tenure durations may also play a role, since public housing and owner-occupied housing are both relatively stable forms of tenure in the Netherlands, whereas private-rental housing is often a more transitory form of accommodation. Consequently, private-rental tenants may be less oriented towards future needs in their choice of a home than they would be in markets with more permanency.[25] Variation in the price of housing between markets might also have income effects that affect the demand of some households more than others. If these potential shifts in demand interact with observed household characteristics (*e.g.*, if the role of housing as an investment good affects wealthy households differently than less wealthy households), they could potentially bias our results.

When comparing household types it is also crucial that unobserved predictors of housing consumption are not correlated with observed household characteristics, as this could introduce omitted-variable bias. For example, if low-income households in the owner-occupied market are more likely to have (unobserved) financial aid from their parents than the group of low-income households in the public-housing market, this may lead us to interpret a lower housing consumption of the latter group as misallocation, whereas it is actually the result of differences in demand arising from the parental income. Similarly, unobserved differences in amenities between markets (such as access to good schools, cafes and public parks) may be more likely to affect high-income households, who tend to value these amenities more than low-income households (Brueckner et al., 1999; Kim, 2006; Lee and Lin, 2018; Almagro and Domínguez-Iino, 2022).

If our assumptions hold, there should be no misallocation between unregulated markets. Therefore, we include placebo tests comparing unregulated market segments with each-other. Firstly, we compare owner-occupiers with households in the private-rental market as a placebo test. However, because these two markets may be affected by small differences in regulation, we also compare tenants in one region (*e.g.*, owner-occupiers in the highly urbanised Randstad region) with tenants of the same market in other regions (*e.g.*, owner-occupiers in the rest of the Netherlands).

5 Results

5.1 Baseline results

As outlined above, we estimate heterogeneous ordered probit models of housing consumption for each housing market separately. Results from this intermediate stage

[25] De Regt et al. (2022) find that private renters move two to three times more often than owner-occupiers, whereas renters in public housing move only 40% more often than owner-occupiers.

of our analysis are reported in Appendix B.1. Reported coefficients demonstrate how household characteristics impact latent demand. These results are hard to interpret on their own, as household characteristics have non-linear marginal effects on latent demand (due to heterogeneous standard deviations and different cut-off values for different markets). However, we note that the two unregulated markets (owner-occupied and private rental housing) have more similar coefficients than public housing.

Our main estimates of misallocation (as defined in equation (5)) and the resulting welfare loss (as defined in equation (10)) are summarised in Table 3, along with the results of our placebo tests. We observe an average level of misallocation of €$13,692$ when comparing public housing with the owner-occupied market, and €$14,353$, when comparing with the private-rental market. Hence, our results seem more or less invariant to the choice of unregulated counterfactual market. This level of misallocation represents around 7.5% of the average market value of a public-housing unit. This means that, on average, households consume 7.5% more, or less, housing in the public-housing market than they would in a counterfactual unregulated market.

This generates a welfare loss because overconsuming households have a lower willingness to pay than underconsuming households. Using equation (8) and $\gamma = -0.5$, we calculate a total welfare loss (in perpetuity) from this misallocation of €$1,465$ and €$1,586$. Discounted at an annual rate of 3.7%, this results in a modest annual welfare loss per household in public housing of €64 and €71 respectively. This represents less than 1% of the average annual rent paid in the public-housing sector, and less than 0.3% of the minimum annual wage. For the Dutch public-housing sector as a whole this translates to an aggregate welfare loss of around €134 million per year, which is around 0.01% of GDP. In other words, the allocative cost generated by regulation of Dutch public housing is limited. This could in part be due to the limited scope of our analysis. Recall that our results do not shed light on the *overall* effects of the public-housing system on housing consumption and resulting welfare effects, as this may also be affected through other channels (such as the supply channel). However, given the scope of our analysis, the limited welfare losses are consistent with literature concluding that centralized waiting lists are a relatively efficient non-market allocation mechanism (Arnosti and Shi, 2020).

We find a very small (albeit statistically significant) level of misallocation of around

Table 3: Misallocation costs

	Misallocation perpetuity (€, per hh)	Welfare cost annual (€, per hh)	Treated households (N)	Counterfact. households (N)
Panel A: Treatment analyses				
Public *vs.* own-occ	13,692***	64.42***	1,528,061	2,348,155
	(1,067)	(10.92)		
Public *vs.* priv-rent	14,353***	71.26***	1,528,061	370,455
	(1,069)	(11.58)		
Panel B: Placebo analyses				
Priv-rent *vs.* own-occ	2,383***	1.48	370,455	2,348,155
	(782)	(1.15)		
Randstad *vs.* rest, own-occ	2,482**	1.45	1,092,981	1,255,174
	(997)	(1.18)		
Randstad *vs.* rest, priv-rent	766	0.11	227,505	142,950
	(783)	(0.63)		

Note: Regionally clustered bootstrapped standard error in parenthesis, *** $p<0.01$, ** $p<0.05$, * $p<0.1$

€2 thousand in the placebo test when comparing private-rental housing with owner-occupied housing, which does not translate into a statistically significant loss of welfare.[26] This negligible level of misallocation may be due to some rent-control prevalent in the private-rental market or due to transfer taxes in the owner-occupied market. In supplementary placebo tests we compare the allocation of housing in the urbanised (and relatively expensive) Randstad region with other regions in the Netherlands within the same unregulated market segment. We find no evidence of misallocation in the private-rental market, whereas there is some evidence of misallocation in the owner-occupied market. This implies that there are some uncaptured regional differences in housing demand between owner-occupiers with the same observed characteristics. Still, the comparatively small order of magnitude of the measured placebo effects, indicate that our main results are robust to these potential sources of bias.

In Table 4 we look at the average effect a given household characteristic has on the predicted over- or underconsumption of public housing. Underconsumption is clearly most prevalent amongst the young, particularly the group between 23-35 years of age, who on average consume €$24,537$ less public housing than they would under an efficient allocation. Conversely, older households tend to overconsume housing, particularly the group between 67-75 years of age. This implies that the applied

[26] Statistically significant effects on misallocation may translate to statistically *insignificant* effects on welfare because a given fluctuation in the measured level of misallocation has a strongly non-linear effect on household welfare (as displayed in Figure 1).

allocation mechanism favours older households. This makes sense, as older households have had more opportunity to accumulate waiting time than younger households. Consider, for instance, an older household that has been living in a public-housing unit for 15 years (and has remained registered on the waiting list) desiring a change of location following their retirement. They would have many options available right away, as the average waiting time for a housing unit tends to be a lot lower than 15 years. Younger households, on the other hand, have to wait until they are at least 33 years old to accumulate the same waiting time (as Dutch waiting lists usually have a minimum age of 18 years to register). Thus it should not be surprising that the group between 23-35 years of age is mostly found in less attractive public-housing units.

Table 4: Overconsumption across household categories

	Marginal effect at mean (in €)	
	Public *vs.* own-occ	Public *vs.* priv-rent
Age breadwinner		
25-35	- 24,513***	- 26,197***
	(2,098)	(2,060)
36-45	- 8,934***	- 11,655***
	(1,296)	(1,165)
46-55	- 1,057	- 3,166***
	(1,260)	(903)
56-66	7,101***	6,787***
	(1,507)	(1,350)
67-75	15,155***	17,408***
	(1,611)	(1,480)
76 +	9,043***	13,394***
	(2,716)	(2,102)
Household composition		
1 adult, 0 child	- 11,304***	- 10,956***
	(884)	(821)
1 adult, w/child	13,818***	12,947***
	(1,244)	(1,135)
2 adults, 0 child	4,497***	4,612***
	(605)	(573)
2(+) adults, w/child	16,811***	16,320***
	(1,775)	(1,580)
Birth-place		
Outside region	- 3,072***	- 2,750***
	(920)	(886)
In region	2,875***	2,564***
	(839)	(808)
Control for income, wealth and region	YES	YES

Note: Regionally clustered bootstrapped standard errors in parenthesis.

This age advantage has not gone unnoticed in the Dutch policy debate, and several

Dutch housing corporations are already incorporating measures to try to boost the chances of young households, such as assigning extra points for starters or conversely discounting the waiting times of households already in public housing. However, our results demonstrate that, despite these efforts, age clearly still provides tenants with an advantage.

Across household types, we see that housing is underconsumed by single-person households and overconsumed by larger households. A potential explanaition could be that additional selection criteria applied by housing corporations and urgency status from municipalities generate particular benefits for larger households. For instance, large housing units are often exclusively offered to families with children. We are also aware of some schemes allowing (previously) single households to combine their waiting time if they wish to move in with their partner, which clearly gives couples an advantage. Even without such schemes, newly formed households receive the waiting time of the member who has waited the longest, giving a potential advantage. An alternative explanation is that reduced household mobility is more likely to result in overconsumption by households with more than one member. For instance, implicit moving costs may be a larger deterrent for a couple of empty-nesters looking to downsize than for a recently divorced person.

Another potential explanation is that larger households have a stronger reaction to higher price levels than smaller households, due to a higher overall cost of living (even when we control for income). This may generate a lower relative demand for housing for this particular group in the private market, which would imply that our key assumption of ordering of household preferences does not hold between two unregulated markets with different price levels: larger households would consume more of the available housing in the cheaper market. Given that price levels vary significantly between the unregulated markets compared in our placebo analyses, the near-zero results of our placebo tests alleviate this concern.

Finally, we find that households born outside the region they currently inhabit (as defined by the birthplace of the breadwinner and their partner), are more likely to underconsume public housing. Conversely, households born in the region are more likely to overconsume public housing. In the Netherlands, waiting time cannot be transferred between regions. Registering for several waiting list is costly both in terms of time and money, as each waiting list requires it's own (small) registry fee, and each system has different rules for application and registry to keep track of. As discussed in Section 2, some municipalities also explicitly require that up to 50% of their properties are assigned to households already living or working in the region. This clearly generates barriers between regions, which is a likely factor in the underconsumption of public housing by households born outside the region. If anything, we are surprised that the difference between households from within and outside the region is not larger, but this could in part be due to household members moving (or deciding to move) to the region when they are young, enabling them to register for their preferred waiting list from an early age.

Concerns among Dutch policy-makers about inter-regional barriers has prompted several investigations into the potential of a national waiting list, but as of yet no concrete steps have been taken to implement such a system (Kromhout et al., 2020). Our result imply that such a system could somewhat improve the efficiency of Dutch housing allocation.

5.2 Investigating the role of income and wealth using placebo tests

Table 5 displays the results of our placebo tests when income and wealth are included along with the other household characteristics in vector X_i of equation (1), as opposed to being placed with the control variables in vector W_i.

The misallocation and associated welfare costs measured in these placebo tests are considerably larger than those in the main analysis (see Table 3), where income and wealth are used as control variables. These non-negligible placebo results indicate that the key assumptions outlined in Section 4.1 do not hold between households with different levels of income or wealth. This, in turn, implies that income and wealth affect housing consumption differently in different unregulated markets. As we have reflected on in Section 4, this could be due to interactions between income and wealth and unobserved, market-specific, preference shifters. For example, the role of owner-occupied housing as an investment good may have a different effect on households with a higher level of wealth than it does on less wealthy households. There may also be omitted variable bias, for example if low-income owner-occupiers are more likely to receive financial aid from parents than low-income renters.

Table 5: Misallocation including income and wealth

	Misallocation perpetuity (€, per hh)	Welfare cost annual (€, per hh)	Treated hh	Countrfct. hh
Private-rental *vs.* owner-occupied	8,717***	25.53***	370,455	2,348,155
	(1,402)	(6.55)		
Randstad *vs.* rest, owner-occupied	3,362***	3.26**	1,092,981	1,255,174
	(672)	(1.41)		
Randstad *vs.* rest, private-rental	5,138**	7.81*	227,505	142,950
	(2,491)	(5.55)		

Note: Regionally clustered bootstrapped standard error in parenthesis, *** p<0.01, ** p<0.05, * p<0.1

Besides these potential market-specific biases, we also find positive placebo effects between different regions within the same unregulated market when we compare the highly urbanized Randstad area to the rest of the Netherlands. This may be explained by different price levels between regions interacting with housing income and wealth. For example, low-income households may be more sensitive to tightening budget constraints in more expensive markets. Unobserved regional characteristics, such as the presence of amenities more intensively used by high-income individuals, may also influence the housing consumption of high-income households relative to low-income households.

5.3 Channels of misallocation

In the introduction to this paper we have discussed two potential channels of misallocation: an inefficient allocation mechanism to assign housing and reduced household mobility. We attempt to isolate the degree of misallocation generated by the allocation mechanism itself, by measuring misallocation among a group of 'recent movers' who have been in their home for two years or less. In the event that the allocation mechanism itself is efficient, and reduced household mobility is the main cause of misallocation, we would expect to see little misallocation among this group. Note that this measure disregards any misallocation occurring *between* recent movers and households with a longer tenure duration.

We observe a level of misallocation among recent movers which is similar to the full sample (see Table 6). This indicates that the allocation mechanism for vacant housing itself introduces inefficiencies.

Table 6: Misallocation for recent movers

	Misallocation perpetuity (€, per hh)	Welfare cost annual (€, per hh)	Treated households	Counterfact. households
Public *vs.* own-occ	11,504***	42.5***	234,450	314,353
	(1,792)	(12.47)		
Public *vs.* priv-rent	12,600***	51.73***	234,450	135,150
	(1,527)	(10.80)		

Note: Bootstrapped standard error in parenthesis, *** p<0.01, ** p<0.05, * p<0.1

Table 7: Misallocation of housing size, 10 categories

	Misallocation perpetuity	Treated households	Counterfactual households
Panel A: Treatment analyses, housing size (m^2, per hh)			
Public *vs.* own-occ	8.28***	1,536,494	2,354,985
	(0.71)		
Public *vs.* priv-rent	9.18***	1,536,494	375,163
	(0.67)		
Panel B: Placebo analyses, housing size (m^2, per hh)			
Priv-rent *vs.* own-occ	1.57***	375,163	2,354,985
	(0.32)		
Randstad *vs.* rest, own-occ	0.55	1,097,375	1,257,610
	(0.53)		
Randstad *vs.* rest, priv-rent	0.28	230,855	144,308
	(0.22)		

Note: Regionally clustered bootstrapped standard error in parenthesis, *** p<0.01, ** p<0.05, * p<0.1

5.4 Measuring misallocation of housing size

As we have highlighted, a benefit of applying the market value of housing as a measure of housing consumption is that it captures all aspects of housing in one measure. However, a potential drawback is that we do not observe the underlying distribution of specific housing characteristics. In the policy debate, the efficient use of housing space (*i.e.*, efficient allocation of size) often receives particular attention. Thus we perform separate analyses using the surface area of a home (m^2), to see whether the insights of our main analysis also apply to this particular housing characteristic.[27]

Using size as the dependent variable (Table 7), we see that the average level of misallocation amounts to around 8-9m^2, which is around 10% of the size of an average public-housing unit. The market value measure of misallocation is slightly smaller by

[27] Another potential benefit of using housing size as a dependent variable is that it does not depend on imputed values (whereas the assessed market values do), and as such it should be less prone to inaccuracies generated by faulty imputations.

Table 8: Misallocation of housing size, 10 categories

	Housing size (m^2)	
	Pub *vs.* own-occ	Pub *vs.* priv-rent
Age breadwinner		
25-35	- 11.07***	- 12.80***
	(1.26)	(1.39)
36-45	- 2.80***	- 4.50***
	(0.82)	(0.69)
46-55	- 0.64*	- 0.53*
	(0.44)	(0.30)
56-66	3.61***	3.75***
	(0.28)	(0.36)
67-75	5.95***	7.89***
	(0.90)	(0.96)
76 +	1.93***	4.44***
	(1.23)	(1.22)
Household composition		
1 adult, 0 child	- 7.82***	- 8.73***
	(0.64)	(0.55)
1 adult, w/child	9.92***	10.61***
	(0.76)	(0.62)
2 adults, 0 child	2.31***	2.97***
	(0.27)	(0.15)
2(+) adults, w/child	12.22***	13.76***
	(1.00)	(0.84)
Birthplace		
Outside region	-0.78***	-0.58***
	(0.15)	(0.13)
In region	0.62***	0.50***
	(0.12)	(0.13)
Control for income, wealth and region	YES	YES

Note: The table displays marginal effects at means. Regionally clustered bootstrapped standard errors in parenthesis.

comparison, amounting to around 7,5% of the average housing value. However, we emphasize that a measure of housing consumption based on size is less comprehensive than the market value measure, as it does not capture potential substitution through other housing attributes. For example, households that are underconsuming housing size might be making up for this by living in a more attractive location.

When investigating how housing size is misallocated across tenants (Table 8) , similar patterns emerge as those observed for market value: young and single-person households underconsume housing, whereas older and larger households overconsume housing. Also households from outside the region undercondume housing relative to households with a breadwinner or partner born in the region. However, if we compare across demographic groups, having children appears to play a particular important role in the distribution of housing size. This could be explained by public-housing corporations actively targeting larger homes to families with children.

This is an interesting result to view in context of the debate on the potential role of public housing in combating, or generating, overcrowding (see *e.g.*, Currie and Yelowitz, 2000). Since larger households are overconsuming housing space relative to the private market, this indicates that the non-market allocation mechanism *reduces* the risk of overcrowding in public-housing units.[28]

6 Sensitivity checks

We perform a number of sensitivity checks in order to evaluate the robustness of our results. First, we check the sensitivity of our results to the number of housing categories (*i.e.*, the market value quantiles) applied (See Appendix B.2 for results). Although we observe a slight increase in the measured level of misallocation as we add more quantiles, the magnitude of the misallocation remains essentially unchanged.

Second, we examine the effect of using national housing deciles, instead of regional deciles. This increases both our measure of misallocation, and our placebo analyses by around €1-€3 thousand (Appendix B.3), indicating that our regional housing deciles result in a more precise measure of misallocation.

Third, to check whether our results are affected by differences in the composition of household characteristics between markets, we perform a version of our analysis where we first match the counterfactual households with the treated households before estimating the ordered probit model (Appendix B.4). The main benefit of using matched samples is that it reduces concerns that there exist (uncaptured) interactions between observed household characteristics and latent housing demand. This would imply that the matched sample improves the internal validity of our model.[29] A potential drawback of matching is that it influences our results through a selection effect, because the level of misallocation could be different for the selection of matched households. This would imply that the matching procedure reduces the external validity of our results.

[28]That being said, it is important to keep in mind that, on average, public-housing units are smaller than private-housing units, meaning that public housing could still contribute to overcrowding through the supply channel.

[29]The matching procedure also enables us to retain the top income quintile in the data, without generating convergence issues.

The results of the analyses on the matches sample are remarkably similar to our main results, which implies that observable differences in household characteristics between markets do not appear to substantially influence our analyses.

Fourth, we check the effects of adding education level as an additional household characteristic (Appendix B.5). We exclude education in our main analysis because administrative data on education is largely incomplete for older households in particular. If we rerun our main analysis on the subset of households for which education is observed, the measured level of misallocation increases by around €1-€3 thousand indicating some selection bias (potentially because misallocation tends to be higher among the young). Once we add education, this result remains virtually unchanged, indicating that education isn't an important determinant of misallocation.

Fifth, we check the sensitivity of our welfare analysis to a range of different demand elasticities (Appendix B.6). The welfare effects remain modest with a range of elasticities, γ, between -0.35 and -0.8. The estimated annual welfare loss ranges between €44-€104 million in these analyses. In other words, the welfare cost of misallocation remains modest, even if we assume that demand is highly inelastic.

Finally, we investigate how much misallocation is generated by a synthetic allocation mechanism where public-housing tenants are randomly assigned homes (Appendix B.7). We do this to evaluate whether the non-market allocation mechanism outperforms the random allocation modelled by Glaeser and Luttmer (2003) and Bulow and Klemperer (2012). Perhaps surprisingly, the synthetic randomised allocation is found to be relatively efficient. This implies that the average influence of age, birthplace and housing composition on the unregulated market allocation is closer to random (*i.e.*, zero effect) than it is to the public-housing allocation. The patterns of overconsumption for different age-groups and household compositions are exactly opposite in the random market compared to the public-housing market (whereas effects of birthplace are negligible). Thus, being older and having a larger household increases the expected housing consumption in the unregulated markets (as is the case with public housing). However, these patterns are much stronger in the public-housing market than they are in the unregulated markets. The main take-away is that strong sorting of housing across any of the household characteristics considered in this study (other than income and wealth) is likely to result in a deviation from the unregulated market outcome.

7 Conclusion

In this paper, we empirically investigate how rent control combined with the use of a non-market allocation mechanism – centralised waiting lists with choice – affects the efficiency of housing allocation. To this end, we build and improve on the methodology originally developed by Glaeser and Luttmer (2003) and quantify the extent to which housing in the Netherlands is misallocated among public-housing tenants. Rather surprisingly, despite the attention the study by Glaeser and Luttmer (2003) received in the economics literature, we are not aware of any follow-up study quantifying misallocation in the same way, which may be due to some of the methodological issues we address in this paper.

Our study delivers the following findings. First, we measure an average level of misallocation of around €14 thousand per public-housing unit, which represents around 7.5% of the value of an average home. This results in a modest annual welfare loss, estimated at around €64 per household in the public-housing sector. This finding is consistent with theories suggesting that the allocation method applied in the Netherlands is reasonably efficient.

Second, we find that public housing induces a transfer of housing from younger households, single-person households and households from outside the region to older households, larger households and households from the region. This finding is particularly relevant for the theoretical economic literature on the efficiency of different non-market allocation mechanisms (*e.g.*, Arnosti and Shi, 2020; Thakral, 2019; Waldinger, 2021). In this literature, it is concluded that the use of waiting lists with choice is a relatively efficient method because households with the highest willingness to pay for public housing are also willing to wait the longest. However, for tractability, these analyses implicitly assume that household preferences for housing do not change over time. They also often disregard regional dynamics. This is relevant, because, in line with our empirical results, allocation based on regionally centralised waiting lists means that only older households from the region have the long waiting times necessary to get the most attractive houses. In other words, the most attractive public-housing units in the Netherlands are not allocated to households who value it the most, but rather to the households who have been residing for a longer time in the right region. An interesting question for future research is whether waiting systems can be improved to address these issues.

Third, methodologically, we show that a failure to address the fundamental issue that key assumptions do not hold for income and wealth results in non-negligible placebo results, meaning that substantial levels of misallocation are found when comparing unregulated markets with each other. Addressing this concern by appropriately controlling for income and wealth improves the credibility of the empirical methodology for potential application in future investigations into the efficiency of alternative allocation mechanisms. More fundamentally, we hope that methods are developed which allow one to investigate misallocation in the regulated housing market while including differences in household preferences related to income and wealth, rather than controlling for them. Thus, a challenge for future research is to produce an unbiased measure of misallocation across household income and wealth. Ideally, a subsequent welfare analysis would also take social preferences into account, noting that a deviation from the market outcome may improve welfare by reducing housing inequalities between income groups (Waldinger, 2021).

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A Technical Appendix

A.1 Fitting cutoffs

We fit counterfactual cutoff-points $\widehat{\mu}^m$ by matching housing consumption and supply in each housing category. We do so by imposing the constraint in equation (A.1). The right-hand side of (A.1) is the share of the total housing stock in category 1 in market m ($S^m_{H=1}$), and the left-hand side is the aggregate probability of households in market m consuming housing-category 1 given the distribution of latent demand $\phi_m(\theta|X_i)$, the market-specific cutoff $\widehat{\mu}^m_1$, and the share of households in market m of type i (S^m_i):

$$\begin{aligned} & \sum_i \left[S^m_i \times \Pr(H_i \leq 1|\widehat{\theta}) \right] = S^m_{H=1}. \qquad \text{(A.1)} \\ \rightarrow \quad & \sum_i \left[S^m_i \times \Phi\left(\frac{\widehat{\mu}^c_1 - X_i'\widehat{\beta}}{exp(X_i'\widehat{\sigma})} \right) \right] = S^m_{H=1}. \end{aligned}$$

We solve the equation for $\widehat{\mu}^m_1$ by noting that the predictions of the latent demand curve are normal (per household type) by construction. This in turn allows us to recursively solve for subsequent cutoff points $\widehat{\mu}^m_k$, where $k > 1$:

$$\sum_i \left[S^m_i \times \Phi\left(\frac{\widehat{\mu}^m_k - X_i'\widehat{\beta}}{exp(X_i'\widehat{\sigma})} \right) \right] = S^m_{H=k} + \sum_i \left[S^m_i \times \Phi\left(\frac{\widehat{\mu}^m_{k-1} - X_i'\widehat{\beta}}{exp(X_i'\widehat{\sigma})} \right) \right]. \qquad \text{(A.2)}$$

A.2 The Indicator Function

The indicator function, I, selects those parts of the normalised demand distribution where the categorical housing consumption differs between markets. It can be defined through combinations of the Heaviside step function Θ, depending on the normalised cut-offs that define the respective categories. This function evaluates to zero (one) if their argument is negative (positive). For most combinations of categories, I takes the following form:

$$I_{\widetilde{H}_i=k,\, \widehat{H}_i=n}(\theta) = \Theta(\theta - \widetilde{\mu}_{k-1})\, \Theta(\widetilde{\mu}_k - \theta)\, \Theta(\theta - \widehat{\mu}_{n-1})\, \Theta(\widehat{\mu}_n - \theta)\,. \qquad \text{(A.3)}$$

For cases involving the bottom or top category of either the actual or counterfactual housing consumption, we define in addition:

$$I_{\widetilde{H}_i=k,\,\widehat{H}_i=n}(\theta) = \begin{cases} \Theta(\widetilde{\mu}_k - \theta)\,\Theta(\theta - \widehat{\mu}_{n-1})\,\Theta(\widehat{\mu}_n - \theta) & \text{for} \quad 1 < n < K \text{ and } k = 1 \\ \Theta(\theta - \widetilde{\mu}_{k-1})\,\Theta(\theta - \widehat{\mu}_{n-1})\,\Theta(\widehat{\mu}_n - \theta) & \text{for} \quad 1 < n < K \text{ and } k = K \\ \Theta(\theta - \widetilde{\mu}_{k-1})\,\Theta(\widetilde{\mu}_k - \theta)\,\Theta(\widehat{\mu}_n - \theta) & \text{for} \quad n = 1 \text{ and } 1 < k < K \\ \Theta(\theta - \widetilde{\mu}_{k-1})\,\Theta(\widetilde{\mu}_k - \theta)\,\Theta(\theta - \widehat{\mu}_{n-1}) & \text{for} \quad n = K \text{ and } 1 < k < K. \end{cases}$$

A.3 Standard and expected error

We calculate bootstrapped standard errors clustered on the regional level. This bootstrap procedure provides 100 random draws (with replacement) of the regions in our original dataset and proceeds to repeat our estimation for each new sample.[30] Sampling with replacement means that the new dataset has the same number of regions as the original dataset, but that regions can appear multiple times (or not appear at all) in the new sample. This way the composition of our sample changes between every draw. The error is calculated as the average difference between these analyses and our original analysis across the 100 draws.

Glaeser and Luttmer (2003) perform an additional calculation to correct their misallocation measure for expected errors arising from different group compositions in the treatment and control groups. For each random draw, they create a synthetic housing consumption for the treatment group by generating a random value for the latent housing demand, applying the estimated distribution of demand from the ordered probit analysis on the control group. Each household is then efficiently assigned a synthetic housing consumption based on their latent housing demand. They proceed to perform a heterogeneous ordered probit analysis on this synthetic housing consumption. The synthetic probit estimates may not reproduce the counterfactual results due to the different sample compositions. The misallocation arising from this comparison can thus be interpreted as a composition error. The expected error is then measured as the average value of this synthetic composition error across 100 random draws. We

[30]In some of the supplementary analyses in the appendices, we have reduced the number bootstraps to decrease the computational time. We always include a note in the table if less than 100 bootstraps have been applied.

find negligble expected errors across all specifications used in this paper.[31] This is likely due to the fact that we apply a large, comprehensive, administrative dataset of all households in the Netherlands. We therefore omit this step from our final analysis.

A.4 Measuring overconsumption

In this paper, we build on the measure for misallocation by defining a measure of overconsumption (see equation (6)). This measure is almost identical to our weighted measure of misallocation, except it applies the difference in prices between categories as weights, as opposed to the *absolute* difference in prices. This generates a measure which is positive when a household is overconsuming housing, and negative when they are underconsuming housing.

Alternatively, it is possible to measure overconsumption by comparing the expected values of housing consumption in the treatment and counterfactual market directly. This approach is applied in our calculation of the welfare effect in Section 4.3, which requires point estimates of housing consumption. In this appendix we show formally that these two definitions of overconsumption are equivalent.

We start from the difference between the expected values of housing consumption in the treatment market and counterfactual market:

$$\begin{aligned} O_i^w &= \mathrm{E}(\widetilde{H}) - \mathrm{E}(\widehat{H}) \\ &= \sum_{n=1}^{K} \Pr(\widetilde{H} = n) \times p_n - \sum_{k=1}^{K} \Pr(\widehat{H} = k) \times p_k \,. \end{aligned} \tag{A.4}$$

Subsequently, we manipulate each term by introducing a sum of conditional probabilities (since we sum over all possibilities, the conditional probabilities sum to one).

$$\begin{aligned} O_i^w = \sum_{n=1}^{K} \sum_{k=1}^{K} \Big(&\Pr(\widetilde{H} = n) \Pr(\widehat{H} = k \,|\, \widetilde{H} = n) \times p_n \\ &- \Pr(\widehat{H} = k) \Pr(\widetilde{H} = n \,|\, \widehat{H} = k) \times p_k \Big) \,. \end{aligned} \tag{A.5}$$

Bayes' Theorem states that both terms describe the same, joint probability $\Pr(\widetilde{H} =$

[31] Results are available upon request.

$n, \widehat{H} = k)$. We thus obtain:

$$O_i^w = \sum_{n=1}^{K} \sum_{k=1}^{K} \Pr(\widetilde{H} = n, \widehat{H} = k) \times (p_n - p_k)\,. \tag{A.6}$$

This expression coincides with equation (6), where the integral over the product of the filter function I and the normalised PDF gives precisely the joint probability in the above expression.[32]

A.5 Welfare equation

Starting from the inverse demand function formulated in equation (7) we are able to back out each household's WTP for the housing consumed in the treated market, $P(\widetilde{H})$. We do so by considering the difference between (logged) housing consumption:

$$\log P_i(\widetilde{H_i}) - \log P_i(\widehat{H_i}) = \left(\log \widetilde{H_i} - \log \widehat{H_i}\right) \times \frac{1}{\gamma}\,, \tag{A.7}$$

Here, $\widetilde{H_i}$ and $\widehat{H_i}$ denote the consumption of housing in the treated and counterfactual market respectively, while $P_i(\widetilde{H_i})$ is the price the household is willing to pay for the consumed level of housing in the treated market.

We subsequently standardise the price per unit of housing consumption in the counterfactual market to one. We obtain:

$$P_i(\widetilde{H_i}) = \left(\frac{\widetilde{H_i}}{\widehat{H_i}}\right)^{\frac{1}{\gamma}}\,. \tag{A.8}$$

We then derive the welfare loss due to misallocation of a household with characteristics

[32]The condition $n \neq k$ in equation (6) states that there is no overconsumption or misallocation in case the same housing category is consumed in the treatment and counterfactual market. The $n = k$ contribution to overconsumption found from our alternative definition in equation (A.4) is indeed zero, as the difference in market value in (A.6) vanishes.

X_i as follows:

$$\begin{aligned}
\Delta W_i &= \int_{\widehat{H_i}}^{\widetilde{H_i}} \left[1 - P_i(H_i)\right] \, dH_i \\
&= \int_{\widehat{H_i}}^{\widetilde{H_i}} \left[1 - \left(\frac{H_i}{\widehat{H_i}}\right)^{\frac{1}{\gamma}}\right] \, dH_i \\
&= \left(\widetilde{H_i} - \widehat{H_i}\right) - \frac{\gamma}{1+\gamma} \left[\widetilde{H_i} \, \left(\frac{\widetilde{H_i}}{\widehat{H_i}}\right)^{\frac{1}{\gamma}} - \widehat{H_i}\right] .
\end{aligned} \tag{A.9}$$

B Supplementary Tables

B.1 Result probit analysis

Table B.1: Probit coefficients of latent demand

	public housing		owner-occupied		private-rental	
	β	σ	β	σ	β	σ
1st income spline	0.368***	0.065**	0.246***	-0.103***	0.104***	-0.121***
	(0.127)	(0.021)	(0.035)	(0.010)	(0.016)	(0.018)
2nd income spline	0.374***	0.064**	0.240***	-0.105***	0.100***	-0.122***
	(0.128)	(0.021)	(0.034)	(0.010)	(0.016)	(0.018)
3d income spline	0.382***	0.065**	0.239***	-0.107***	0.101***	-0.121***
	(0.129)	(0.020)	(0.034)	(0.010)	(0.016)	(0.017)
4th income spline	0.390***	0.065**	0.240***	-0.106***	0.103***	-0.120***
	(0.130)	(0.020)	(0.034)	(0.010)	(0.017)	(0.017)
2nd wealth quintile	0.064***	-0.016**	0.011***	-0.098***	0.011**	-0.030***
	(0.030)	(0.003)	(0.005)	(0.003)	(0.004)	(0.005)
3d wealth quintile	0.135***	-0.018*	0.061***	-0.107***	0.038***	-0.032***
	(0.051)	(0.003)	(0.011)	(0.003)	(0.010)	(0.007)
4th wealth quintile	0.208***	0.000	0.119***	-0.078***	0.068***	-0.012
	(0.061)	(0.005)	(0.018)	(0.004)	(0.015)	(0.008)
5th wealth quintile	0.359***	0.063***	0.259***	0.057***	0.136***	0.117***
	(0.082)	(0.009)	(0.033)	(0.006)	(0.029)	(0.014)
Age 36-45	0.413***	-0.066***	0.025***	0.065***	0.060***	0.004
	(0.098)	(0.008)	(0.003)	(0.008)	(0.009)	(0.013)
Age 46-55	0.638***	-0.101***	0.063***	0.119***	0.083***	0.028**
	(0.144)	(0.012)	(0.006)	(0.010)	(0.013)	(0.012)
Age 56-66	0.891***	-0.132***	0.152***	0.133***	0.122***	0.010
	(0.208)	(0.014)	(0.018)	(0.015)	(0.020)	(0.016)
Age 67-75	1.138***	-0.134***	0.293***	0.086***	0.191***	-0.049**
	(0.291)	(0.012)	(0.038)	(0.015)	(0.033)	(0.019)
Age 76+	1.051***	-0.127***	0.338***	0.094***	0.170***	-0.021
	(0.303)	(0.012)	(0.044)	(0.02)	(0.033)	(0.032)
1 adult, 1 child	0.491***	-0.127***	0.045***	0.010	0.091***	-0.073***
	(0.126)	(0.013)	(0.007)	(0.007)	(0.012)	(0.015)
1 adult, more children	0.856***	-0.197***	0.136***	-0.004	0.175***	-0.104***
	(0.244)	(0.020)	(0.016)	(0.010)	(0.027)	(0.025)
2 adults, no children	0.392***	-0.093***	0.051***	-0.046***	0.059***	-0.072***
	(0.109)	(0.008)	(0.009)	(0.006)	(0.009)	(0.008)
2 adults, 1 child	0.597***	-0.143***	0.073***	-0.024**	0.102***	-0.073***
	(0.156)	(0.019)	(0.012)	(0.012)	(0.016)	(0.015)
2 adults, 2 children	0.780***	-0.160***	0.125***	-0.070***	0.152***	-0.082***
	(0.217)	(0.025)	(0.017)	(0.013)	(0.023)	(0.016)
2 adults, more children	0.860***	-0.181***	0.153***	-0.018	0.185***	-0.032**
	(0.226)	(0.029)	(0.022)	(0.018)	(0.028)	(0.024)
More adults	0.578***	-0.161***	0.062***	0.116***	0.103***	0.032*
	(0.133)	(0.028)	(0.014)	(0.021)	(0.022)	(0.024)
Born in region (dummy)	0.138**	-0.049***	-0.013***	-0.049***	0.006	-0.032*
	(0.058)	(0.009)	(0.004)	(0.007)	(0.008)	(0.024)
Region dummies	Yes		Yes		Yes	
Observations	1,033,055		2,328,518		472,319	

Note: Regional bootstrapped standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

B.2 Sensitivity to the number of categories

Table B.2 shows that adding housing categories (*i.e.*, adding more housig quantiles) causes a strong increase in the unweighted measure of misallocation, M, as can be expected on theoretical grounds. With a higher number of categories, there is a higher likelihood of consuming a different housing category in the treatment than in the counterfactual. The *weighted* misallocation measure, M^w, as specified in equation (4), is however relatively stable across categories. As more categories are added the measure becomes more precise, as opposed to growing mechanically (as is demonstrated by the decrease in the weighted measure of misallocation between 25 an 50 quantiles). Our measured level of misallocation appears reasonably robust to the addition of additional housing quantiles, remaining around €14 thousand.

B.3 Using national housing categories

In our main analysis we have used regional housing categories since the waiting lists are centralised at the regional level. This omits any misallocation occurring between regions, but allows us to flexibly control for differences in the distribution of supplied housing between regions. In an alternative analysis we check the effects of measuring housing categories at the national level. This analysis increases all our measures by €1-€3 thousand, including our placebo analyses.

B.4 Matching

We perform propensity score matching, using the nearest neighbour approach (applying a calipher of 0.3, as suggested by the literature). We only allow one, unique, match per household, limiting the treatment to households for which matches can be found using the aforementioned criteria. This leaves us with a dataset of 895 thousand households in the analysis using owner-occupied housing as a counterfactual, and 378 thousand using private-rental housing as a counterfactual.

Table B.4a, shows the result of a propensity score test on all observable household characteristics. The match between public- and owner-occupied housing works reasonably well: although most of the demographic characteristics still have a bias above 5%, none of them exceed 20%. The match with the private-rental market works less well, with a particularly poor match in average income, age and birthplace. This is probably due to

Table B.2: Misallocation of public vs owner-occupied housing, 5-50 categories

	K=5	K=8	K=10	K=15	K=25	K=50
M^w (€)	12,354	13,637	13,692	13,898	14,113	14,078
	(1,093)	(1,082)	(1,067)	(1,029)	(984)	(785)
M (%)	35	51	57	68	78	88
	(0.05)	(0.04)	(0.03)	(0.02)	(0.03)	(0.01)

Note: Regionally clustered bootstrapped standard error (based on 25 bootstraps for $K > 10$) in parenthesis. M^w represents monetary measures as specified in equation (4), whereas M represents percentages, as specified in equation (3). K refers to the number of categories

Table B.3: National misallocation

	Misallocation perpetuity (€, per hh)	Welfare cost annual (€, per hh)	Treated hh	Countrfct. hh
Panel A: Treatment analyses				
Public *vs.* own-occ	15,078***	74.63***	1,528,061	2,348,155
	(2,527)	(24.53)		
Public *vs.* priv-rent	17,763***	106.23***	1,528,061	370,455
	(3,202)	(35.71)		
Panel B: Placebo analyses				
Priv-rent *vs.* own-occ	5,878***	9.25**	370,455	2,348,155
	(1,353)	(4.63)		
Randstad *vs.* rest, own-occ	4,868**	5.99	1,092,981	1,255,174
	(1,738)	(5.06)		
Randstad *vs.* rest, priv-rent	1,233	1.33	227,505	142,950
	(986)	(0.93)		

Note: Regionally clustered bootstrapped standard errors (based on 25 bootstraps) in parenthesis, *** p<0.01, ** p<0.05, * p<0.1

the limited overlap in observed household characteristics between the private-rental market and public-housing market.

Table B.4b displays the misallocation costs for the matcehd samples. On the whole, the analysis on the matched sample provides similar results to those presented in the paper (the level of measured misallocation varies by ± 2 thousand). This is true of both the treatment analyses and of the placebo analyses. In other words, differences in the sample composition between markets do not appear to be driving our results.

Table B.4a: Propensity score testing, matched samples

	Match 1			Match 2		
	Public	Own-occ	Bias	Public	Priv-rent	Bias
Panel A: Household income						
Average income (log)	10.819	10.813	1.3***	11.107	10.939	37.5***
2^{nd} quintile	0.338	0.341	-0.7***	0.266	0.280	-3.3***
3^{rd} quintile	0.261	0.252	2.1***	0.368	0.295	15.6***
4^{th} quintile	0.137	0.138	-0.2	0.240	0.186	13.1***
5^{th} quintile	0.052	0.054	-0.9***	0.107	0.086	7.2***
Panel B: Net household wealth (excl. housing)						
2^{nd} quintile	0.250	0.261	-2.6***	0.228	0.212	3.9***
3^{rd} quintile	0.251	0.243	2.0***	0.216	0.172	11.0***
4^{th} quintile	0.178	0.166	3.1***	0.190	0.155	9.3***
5^{th} quintile	0.093	0.1	-2.6***	0.140	0.153	-3.7***
Panel C: Age of breadwinner (25+)						
36-45	0.128	0.158	-8.5***	0.156	0.148	2.3***
46-55	0.184	0.156	8.0***	0.211	0.129	22.1***
56-66	0.221	0.161	15.3***	0.266	0.127	35.5***
67-75	0.154	0.205	-13.4***	0.062	0.111	-17.4***
76 +	0.175	0.171	1.0***	0.029	0.137	-40.0***
Panel D: Household composition						
1 adult, 1 child	0.072	0.043	12.4***	0.063	0.043	9.3***
1 adult, 2+ children	0.042	0.026	8.9***	0.018	0.024	-4.7***
2 adults, no child	0.330	0.294	7.7***	0.322	0.310	2.6***
2 adults, 1 child	0.085	0.078	2.7***	0.109	0.074	12.3***
2 adults, 2 children	0.065	0.103	-14.0***	0.047	0.053	-2.7***
2 adults, 3+ children	0.038	0.040	-1.3***	0.009	0.019	-8.7***
3+ adults	0.013	0.005	9.0***	0.013	0.007	5.8***
Panel E: Birthplace						
In region	0.560	0.591	-6.4***	0.579	0.434	29.3***

Note: Table contains results from the propensity score test on the matches samples. The bias measures the difference between the treatment (public housing) and the counterfactual markets. *** p<0.01, ** p<0.05, * p<0.1

Table B.4b: Misallocation, matched samples

	Misallocation perpetuity (€, per hh)	Welfare cost annual (€, per hh)	Treated households	Counterfact. households
Panel A: Treatment analyses				
Public *vs.* own-occ	12,415***	43.55***	895,049	895,049
	(907)	(6.81)		
Public *vs.* priv-rent	16,623***	75.04***	377,829	377,829
	(739)	(7.10)		
Panel B: Placebo analyses				
Priv-rent *vs.* own-occ	2,932***	2.29	434,443	434,443
	(706)	(1.30)		
Randstad *vs.* rest, own-occ	3,511**	2.8	1,645,210	1,645,210
	(1,159)	(1.1)		
Randstad *vs.* rest, priv-rent	1,630	0.62	159,520	159,520
	(1,586)	(0.48)		

Note: Regionally clustered bootstrapped standard error in parenthesis, *** p<0.01, ** p<0.05, * p<0.1

B.5 Adding education

We check the sensitivity of our main analysis to changes in the household characteristics considered in our analysis by adding education as an additional household characteristic. This has two effects, first it reduces the data for which we are able to measure misallocation (as information on education is lacking for around 30% of Dutch households) and second it adds a dimension across which misallocation is measured. We evaluate both effects separately (in Table B.5), by first repeating our main analysis on the reduced dataset (panel A), and then including education as a household characteristics in a second analysis (panel B).

B.6 Sensitivity to demand elasticity

The elasticity of demand captures how demand responds to a change in housing prices. If the elasticity of demand increases in absolute terms, this means that the amount of housing consumed is *more* sensitive to prices. Seen the other way around, it also means that willingness to pay is *less* sensitive to the quantity of housing consumed. Thus, the difference in willingness to pay between overconsuming and underconsuming households (and the associated welfare loss) gets smaller as the absolute value of the elasticity term increases. In Table B.6 we check the sensitivity of our results to a range of alternative elasticities suggested by the literature, ranging from $\gamma = -0.3$ to $\gamma = -0.8$. We see that welfare losses remain modest within this range of elasticities.

Table B.5: Misallocation with education

	Misallocation perpetuity (€, per hh)	Welfare cost annual (€, per hh)	Treated hh	Countrfct. hh
Panel A: Education selection				
Public *vs.* own-occ	€15,016***	€77.14***	916,943	1,200,123
	(1,565)	(13.02)		
Public *vs.* priv-rent	€17,212***	€100.64***	916,943	211,966
	(1,755)	(16.24)		
Priv-rent *vs.* own-occ	€2,918***	€2.81	211,966	1,200,123
	(962)	(2.03)		
Randstad vs. rest, own-occ	€3,986***	€4.51	555,497	644,626
	(1,379)	(3.10)		
Randstad vs. rest, priv-rent	€632	€0.4	128,181	83,785
	(431)	(0.29)		
Panel B: Education included				
Public *vs.* own-occ	€14,971***	€76.48***	916,943	1,200,123
	(1,536)	(12.43)		
Public *vs.* priv-rent	€17,387***	€100.86***	916,943	211,966
	(1,620)	(14.88)		
Priv-rent *vs.* own-occ	€3,821***	€3.85	211,966	1,200,123
	(1,257)	(3.22)		
Randstad vs. rest, own-occ	€5,040***	€7.32	555,497	644,626
	(1,905)	(5.69)		
Randstad vs. rest, priv-rent	€972	€0.61	128,181	83,785
	(800)	(0.53)		

Note: Regionally clustered bootstrapped standard errors (based on 25 bootstraps) in parenthesis, *** p<0.01, ** p<0.05, * p<0.1

Table B.6: Welfare with different demand elasticities

	$\gamma = -0.35$	$\gamma = -0.5$	$\gamma = -0.8$
Panel A: Public *vs.* owner-occupied			
Annual welfare loss (€/h.h.)	94.09***	64.42***	39.59***
	(19.05)	(10.92)	(7.32)
Aggregate welfare loss (million €)	198***	135***	83***
	(40)	(23)	(15)
Panel A: Public *vs.* private-rental			
Annual welfare loss (€/h.h.)	104.34***	71.26***	43.73***
	(18.38)	(11.58)	(7.66)
Aggregate welfare loss (million €)	219***	150***	92***
	(39)	(24)	(16)

Note: Regionally clustered bootstrapped standard error in parenthesis, *** p<0.01, ** p<0.05, * p<0.1

B.7 Random allocation

As a supplementary analysis, we also evaluate what the level of misallocation would be under (synthetic) random matching, where housing is allocated randomly across all observable housing characteristics. In this analysis, we randomly allocate public housing among public-housing tenants in the sample.

The results for this exercise are presented in Table B.7a. Random allocation appears relatively efficient. In other words, randomly allocating housing such that there is no systematic relationship between demographic household characteristics (age and household composition) and consumption, almost mimics the allocation in the unregulated market segments.

If we look at how this misallocation is divided between demographic groups (Table B.7b), we see the opposite pattern than in our main analysis. Young and single-person households overconsume housing related to other household types. This indicates (perhaps unsurprisingly) that older and larger households tend to consume more housing than younger and smaller households, also in the unregulated market. Interestingly, the random allocation produces no misallocation over households based on their birthplace, which is equivalent to saying that birth place plays no role in the allocation of unregulated rental housing. This intuitively makes sense, as there is no obvious reason why being born inside or outside the region should affect your demand for housing. There *is* a statistically significant effect of birthplace in the owner-occupied market, but this is of a negligible size.

Table B.7a: Misallocation with random assignment

	Misallocation perpetuity (€, per hh)	Welfare cost annual (€, per hh)	Treated households	Counterfact. households
Panel A: Random matching, no income and wealth				
Public *vs.* own-occ	4,892***	5.88**	1,528,662	2,348,155
	(936)	(2.89)		
Public *vs.* priv-rent	2,777***	1.92**	1,528,668	370,455
	(461)	(0.89)		

Note: Regionally clustered bootstrapped standard error in parenthesis, *** p<0.01, ** p<0.05, * p<0.1

Table B.7b: Overconsumption with random assignment

	Marginal effect at mean (in €)	
	Public *vs.* own-occ	Public *vs.* priv-rent
Age breadwinner		
25-35	6,684***	4,403***
	(1321)	(1,003)
36-45	4,767***	1,313***
	(1,157)	(427)
46-55	3,396**	772***
	(838)	(265)
56-66	-377**	-666***
	(159)	(205)
67-75	-6,066***	-3,077***
	(1,361)	(797)
76 +	-8,355***	-2,596***
	(1,828)	(696)
Household composition		
1 adult, 0 child	1,468***	1,929***
	(327)	(420)
1 adult, w/child	-1,631***	-2,570***
	(367)	(594)
2 adults, 0 child	-858***	-588***
	(210)	(162)
2(+) adults, w/child	-2,597***	-3,188***
	(588)	(811)
Birthplace		
Outside region	-550***	8
	(163)	(169)
In region	334***	-70
	(147)	(150)
Control for income, wealth and region	YES	YES

Note: Regionally clustered bootstrapped standard errors in parenthesis.

C Additional Information Data

Table C1: List of Regions

	Housing region	Randstad
1	Groningen	No
2	Regio Utrecht	Partly
3	Regio Eemvallei	Partly
4	Baarn	Yes
5	Oost-Utrecht en West-Gelderland	Partly
6	Gooi & Vechtstreek	Yes
7	Almere	Yes
8	Regio Amsterdam	Yes
9	IJmond/Zuid-Kennemerland	Yes
10	Noord-Kennemerland	Yes
11	Kop van Noord Holland	Yes
12	Arnhem-Nijmegen	No
13	West-Friesland	Yes
14	Walcheren	No
15	West-Brabant	No
16	Oss	No
17	's-Hertogenbosch	No
18	Meijerijstad	No
19	Tilburg	No
20	Eindhoven	No
21	Limburg	No
22	Achterhoek	No
23	Zuid-Gelderland, Vijfheerenlanden, Altena & Molenlanden	Partly
24	Drechtsteden	Yes
25	Rijnmond	Yes
26	Haaglanden	Yes
27	Holland Rijnland	Yes
28	Regio Midden-Holland	Yes
29	Friesland	No
30	Kampen, Steenwijkerland, Zwartewaterland & Zwolle	No
31	Noord-Veluwe	No
32	Stedendriehoek	No
33	Hengelo Borne	No
34	Almelo	No
35	Lelystad	No
36	Olst-Wijhe-Raalte	No

Note: Some regions are only partly within the Randstad. In these regions, households are allocated to the Randstad on the municipal level.