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The Incidence of Rent Subsidies: Evidence on Rents, Housing Choices and Supply

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Abstract

This paper asks whether, and under which circumstances, rent subsidies to low-income households increase rents. We utilize a reform in Finland that caused large quasi-random variation in housing allowances. We find that large increases in allowances for affected housing units had little or no effect on their rents relative to other units. Thus, the incidence of the reform was on allowance recipients. The reform led to only small changes in recipients' housing choices, and at most modest changes in rental supply. Rent subsidies can be effective even in supply-constrained contexts, if housing choices are unresponsive to the subsidy.

JEL-Codes: H220, R280.

Keywords: housing allowance, rent subsidy, incidence, housing demand, housing supply.

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1 Introduction

Housing affordability is a pressing issue in urbanized societies. Therefore, many governments support low-income households through large targeted rent subsidy programs, such as housing allowances or housing vouchers.¹ These transfers can shield households from financial distress, evictions and homelessness (Fetzer et al., 2023). Yet, whether they work as intended crucially depends on the extent to which they increase rents and end up benefiting landlords instead of subsidy recipients. The existing estimates of the rent effects from various countries range from a pass-through of zero to a pass-through of more than 50% (see, for example, Gibbons and Manning 2006; Collinson and Ganong 2018; Brewer et al. 2019; Eerola and Lyytikäinen 2021). It is not well understood why these estimates vary so much depending on the context. Theoretically, the pass-through of subsidies to rents depends on the induced changes in rental demand and supply, but so far, there is very little empirical evidence on the effects of housing subsidies on quantities supplied and consumed. Without understanding these mechanisms, it is difficult for policy-makers to know what pass-through effects they can expect when considering policy reforms.

In this paper, we combine rich population-wide register data with a compelling quasi-experimental research design to shed light on whether and in which contexts rent subsidies have the adverse effects of increasing rents. Our research design is based on a major reform of the Finnish housing allowance (HA) system in 2015. The reform substantially increased housing allowances for some types of housing units, while changes in other unit types were small. These changes ranged from 0 to up to more than 150 euros per month depending on unit and household type (mean HA payment in the sample period was around 300 euros per month). We leverage this variation to identify the rent effects of HA changes using a differences-in-differences design. After comparing rents paid by HA recipients in these different types of units, we then analyze the demand- and supply-side responses using register data to understand the drivers of the rent effects.

Throughout our analysis, we do not find evidence of the increased HA passing through to landlords in the form of higher rents. In our preferred specification, we measure treatment exposure by calculating the predicted HA change for a given

¹In 2020, the annual rental subsidy spending was 0.9% of GDP in Finland, 1.3% in the UK, 0.73% in Germany and 0.69% in France. The average of the OECD-25 countries was 0.3%. For more details, see the OECD Affordable Housing Database: <https://www.oecd.org/housing/data/affordable-housing-database/>.

housing unit caused by the reform, holding constant the pre-reform recipient and rental contract characteristics. An additional euro of predicted HA change translates to 0.9 euros of actual HA change (standard error 0.034 and F-statistic >700 , indicating a very strong first-stage effect). The point estimate for the pass-through, the effect of a one euro increase in HA on rent, is roughly 0.03 euros. The standard error is approximately 0.018, suggesting that we can rule out even moderate effects of the reform on rents with a high degree of confidence. These regressions control for unobserved housing quality through housing unit fixed effects and are estimated using new rental contracts only, ruling out rent stickiness as a potential reason for the small rent effects. We also verify that our results are robust to alternative ways to compute treatment exposure.

To understand why the rent effects are so small, we then zoom in on the behavioral responses in the rental market. To clarify the mechanisms through which HA payments may lead to higher rents, we write down a stylized conceptual framework where only a fraction of the renters are eligible for HA and where the supply of rental housing can be affected not only by construction but also by conversions of units from owner-occupied housing to rental housing. Housing allowances can increase rents if rental demand by recipients is relatively elastic and if rental supply is relatively inelastic, so that the increased demand is reflected in higher rents instead of higher quantities. Thus, our observation of the low pass-through of HA increases to rents could be either due to large changes in supply of treated units or due to small changes in the demand for treated units. Which is the case is of first-order importance for understanding how rent subsidies work also in other contexts.

To analyze the demand-side responses, we use register data on households' moving patterns and housing choices. We ask (i) are HA recipients more likely to stay in units that received a large increase in HA, and (ii) do they start choosing units with large HA increases when they move after the reform. We do not observe changes in the propensity of recipients to move out of units that received a large HA increase after the reform. However, conditional on moving, households start choosing units with slightly larger HA increases. The effect kicks in sharply after the reform and is precisely estimated but economically small: after the reform, households choose units with approximately 4 euros, or less than 0.2 standard deviations, larger HA changes.

To analyze the supply-side responses, we use register data on the supply of units to the rental sector. We observe more construction of units with larger HA increases relative to units with smaller HA increases after the reform. However, these changes are small and become detectable only some years after the reform took place. This

is important for the interpretation of our rent results since the changes in recipient household choices kick in immediately after the reform. Thus, any demand shift from the reform materializes very quickly, while changes in construction show up in the data some years after the reform. We interpret this as evidence that changes in construction can have at most a limited role in explaining the rent effects. The supply of rental housing could potentially increase very quickly also through the conversion of owner-occupied units to rental units. However, we do not observe changes in the types of owner-occupied units that are converted to rentals. We verify that the changes in construction and conversion contribute very little to the overall composition of the rental housing stock in the medium-run. We also study the heterogeneity of our rent effects by city size (reflecting housing supply elasticity), and do not find any evidence of higher pass-through in city groups with lower supply elasticity.

We conclude from this evidence that a large supply response is *not* the main driver of our observed rent effects. Rather, our interpretation is that for the majority of recipients, housing choices were relatively inelastic with respect to the incentives generated by the reform. If household willingness-to-pay for different types of units is not much affected by HA changes, the incidence of HA increases is largely on the recipients, whether supply is very elastic or not. There are many potential explanations for this finding. An important reason might be that less than half of the HA spells that started during our analysis period lasted more than a year. Thus, most HA recipients can expect to at some point bear the full rental burden of their unit when moving to a new housing unit. This suggests that other reasons besides rent subsidies are the most important drivers of housing choices.

Related literature. The effectiveness of rent subsidies depends on their pass-through to rents, but there are only a handful of papers studying this. Early studies from Europe find that more than 50% of the rent subsidies accrued to landlords through higher rent (Gibbons and Manning, 2006; Fack, 2006; Kangasharju, 2010; Viren, 2013). More recent evidence from Europe, Israel and New Zealand finds more moderate, but still economically significant rent effects (Hyslop and Rea, 2019; Sayag and Zussman, 2020) or very small or even zero rent effects (Brewer et al., 2019; Eerola and Lyytikäinen, 2021). Findings on the rent effects of various changes in the US housing voucher program are also heterogeneous.² Susin (2002) finds that program

²The US housing voucher program differs from the other programs in two important ways. First, only a small share of eligible households receive a voucher, making the program size small relative to the rental market. Second, recipients must live in eligible housing units, and many move at the time of receiving a voucher (Eriksen and Ross 2013 and Chyn et al. 2019).

expansions led to higher rents in affected metropolitan areas, whereas [Eriksen and Ross \(2015\)](#) do not. [Collinson and Ganong \(2018\)](#) find that metro-area rent ceiling increases did pass through to rents paid by recipients.

The large heterogeneity in the existing pass-through estimates, even across different studies in the same country and institutional context, can arise from a number of reasons. First, there are many differences in subsidy program design and housing market conditions across contexts. Second, the heterogeneity can reflect differences in the quality of the research design or statistical uncertainty. In particular, the earlier studies use relatively small datasets either from survey sources or samples of register data. While the more recent studies use register data on recipient rents and better research designs, they lever only relatively small variation in rent subsidies or programs that concern only a small fraction of the rental market, leading to relatively imprecise estimates.

Our first contribution comes from leveraging high-quality data and large-scale reform to estimate the rent effects of rent subsidies. Together they result in pass-through estimates that are very precise compared to prior literature, and allow us to provide compelling evidence on the validity of our research design. Furthermore, our data allows us to address the problem of unobservable housing quality. The existing pass-through estimates allow for two alternative (not mutually exclusive) interpretations. The point estimates may reflect either higher quality-adjusted rents and/or better housing quality which is then reflected in higher rents. Often these two effects cannot be reliably disentangled. As we can observe same housing units before and after the reform, we are able to control for unobservable time-invariant housing quality.

Our second contribution is to use register data on HA recipients and the overall rental market to provide some of the first evidence on the key demand- and supply-side mechanisms through which changes in HA can affect rents. Causal evidence on the responsiveness of subsidy recipients to the incentives created by different subsidy schemes in terms of housing consumption is almost non-existent.³ We contribute to this strand of the literature by analyzing the effects of a general large-scale subsidy program on recipient housing choices and by comparing changes in recipient choices

³[Collinson and Ganong \(2018\)](#) find evidence suggesting that voucher design had effects on the neighborhood quality of voucher holders in the Dallas area. Other exceptions are [Öst \(2014\)](#) and [Gibbons et al. \(2020\)](#) who both find that the benefit cut induced households to downsize conditional on moving. However, these studies use variation or data that concerns only a subset of recipients (single parents or public housing tenants).

to the changes in rental supply.

Our findings on household choices also contribute to the broader comparisons of the effectiveness of cash transfers relative to other policies that subsidize low-income households' housing consumption. Under many alternative policies, like socially provided rental housing or rent controls implemented on privately-owned housing, the tenant receives an in-kind subsidy in form of rent discounts compared to market rents (e.g. [Chen et al. 2023](#)). In such programs, the tenant loses the present value of the rent discounts if she moves to an unregulated unit. Consistent with this, [Diamond et al. \(2019\)](#) and [Mense et al. \(2023\)](#) find that rent discounts significantly reduce household mobility. Our mobility results are in contrast with these findings. In our case, even though the reform induced variation in housing allowances at the housing unit level, the housing allowance is a tenant-based subsidy. The tenant does not lose the subsidy if she moves to another unit. Moreover, as the tenant's income increases, the rent subsidy gradually phases out. In a typical rent control policy, a tenant is able to occupy the rent-controlled unit indefinitely once she has obtained the housing unit even if her financial situation changes. Our results suggest that, compared to unit-specific rent discounts under various rent control policies, tenant-based rent subsidies result in significantly smaller lock-in effects and distortions in the housing market.

Our findings also add to our understanding on how effective assistance program design can be in directing recipient housing choices more broadly. A growing literature is interested in the effects of various moving to opportunity initiatives ([Aliprantis et al. 2024](#)). Evidence from randomized controlled trials suggests that simply receiving a housing voucher may not trigger moves to higher quality neighborhoods (e.g. [Jacob et al. 2015](#)), unless the program is designed to incentivize such moves and that the recipients receive information and counseling concerning their housing search (e.g. [Chetty et al. 2016](#) and [Bergman et al. 2024](#)). Our results are similar as we find only weak responses to monetary incentives to stay in or move to particular types of housing units. Moreover, although RCTs can provide compelling evidence on recipient choices under the *status quo*, they cannot take into account confounding general equilibrium changes that may follow from scaling up the programs, such as rent increases ([Davis et al. 2021](#), [Chyn and Daruich 2022](#) and [Fogli et al. 2024](#)). Our results come from a large-scale policy reform where general equilibrium effects are baked in.

In addition to housing choices, we also shed light on the supply-side responses in the rental market. Rent subsidies are often suspected to have larger rent effects

in locations with inelastic housing supply (e.g. [Susin 2002](#); [Eriksen and Ross 2015](#)). We are the first to provide direct evidence on the effects of a rent subsidy program on housing supply using register data on the total housing stock. We also study separately residential construction and conversions from the owner-occupied sector to the rental sector. Our interpretation is that these changes are too small for supply responses to be important drivers of our rent results, especially in the short-run. This challenges some of the prior interpretations of rent effects: Housing allowances are not necessarily passed directly through to landlords even when housing supply is not very elastic.

The rest of the paper proceeds as follows. In the next section, we present our institutional context, data and research design. In Section 3, we present our results on rent effects. In Section 4, we present and discuss our results on household mobility and rental housing supply. Section 5 concludes.

2 Institutional context and research design

2.1 Institutional context and data

Approximately one third of Finnish households live in rental housing. The rental housing market consists of an unregulated segment (70%) and a regulated social housing segment (30%).⁴ In our analysis, we focus on new rental contracts in the unregulated rental market. While rent increases of existing rental contracts are typically tied to some publicly available index, new rental contracts in the unregulated rental market are not subject to any constraints on rent setting. New rental contracts can therefore be expected to respond to changes in housing allowances and to provide a credible benchmark for studying the rent effects of the reform.

Housing allowance is an important part of the Finnish social security system with a stated aim of reducing the housing costs of low-income households. We focus on the general HA intended for working-age households. In 2020, total outlays amounted to 1.57 billion euros (0.66% of GDP) and roughly 400,000 households (15% of all households) received general HA. In 2015, when the reform was implemented, roughly 30% of non-student households in the unregulated rental market received general HA

⁴The regulated social housing sector is subsidized by government and municipalities. Tenant selection is based on income, wealth and housing need. Because rents are regulated and independent of tenant characteristics, they should not be affected by changes in housing allowances or other demand-side factors.

(see Appendix Table A1 and Appendix Figure B1).

Our first data source is the register of housing allowances from Social Insurance Institute of Finland (Kela). The data cover the universe of monthly recipient-level HA payments and include information on the characteristics of recipient households and their housing units. We do not directly observe new contracts in our data, but we define a contract as new if the recipient received HA in another address at most 4 months ago. The construction of the analysis samples is described in detail in Appendix A.⁵

Table 1 provides summary statistics on our sample focusing on apartments with floor area between 15m² and 100m². The first column contains all HA recipients and the second the subset of new rental contracts. Most HA recipient households are single-member, their disposable monthly income before HA is around 900 euros and the average rent around 600 euros. As we will discuss below, the HA system covers rent only up to a rent ceiling. For an overwhelming majority of recipients (82% and 85%), this ceiling is binding. For these households, the marginal unit of additional housing consumption is not subsidized.

⁵Students were covered by a separate student housing supplement up until 2017 and became eligible for general HA in 2017. Pensioners have a separate, but similar housing allowance system. We exclude pensioners and students from our HA register estimation sample throughout the whole sample period.

Table 1: Summary statistics, HA register data.

	All payments	New contracts
	mean	mean
Household size	1.6	1.8
Share single-member households	0.68	0.57
Apartment surface	48.1	51.6
Household income	901.6	931.1
Rent	578.5	621.7
Housing allowance received	305.7	327.3
Share rent ceiling binding	0.82	0.85
Observations	11188052	219204

Notes: HA register 1/2010-12/2019, all monetary values in 2020 euros. Throughout, the sample is restricted to unregulated rental market residents in units of floor area between 15 and 100 m² and excludes student households. The first column summarizes all month-by-household payments to HA recipients. The second column summarizes the subset of recipients with new rental contracts. The contract is defined as new if the recipient received HA in another address at most 4 months ago. Household income refers to income excluding HA and social assistance.

Our second data source is the population-wide household register data provided by Statistics Finland (see Appendix A). In addition to rich demographic and socio-economic information on Finnish households, the data include information on the characteristics of their housing units. For each housing unit, we observe size of the unit (m^2), construction year and current tenure status (renter in regulated or unregulated sector or owner-occupied). This information allows us to analyze changes in rental supply in relation to the HA reform and to separate between construction of new units and conversions of owner-occupied units into rental units.

2.2 Housing allowance system and the 2015 reform

The legislation governing HA payments was renewed in late 2014, with the new legislation taking effect in 2015. The stated objective of the legislative changes was to simplify the system. The most important change concerned the calculation of the rent ceiling, which sets an upper limit to the approved housing costs that can be covered via HA. Prior to the reform, the rent ceiling depended in a complicated way on a number of housing unit and household characteristics. The reform substantially simplified the determination of the rent ceiling. At the same time, the reform also

made the HA system slightly more generous on average.

Before the 2015 reform, HA was determined according to the following formula:

$$HA = 0.8[\min(Rent/m^2, MaxRent_m^2) \cdot \min(FloorArea, Max_m^2) - d_1], \quad (1)$$

where $Rent/m^2$ denotes the actual monthly rent per square meter of the unit and $MaxRent_m^2$ the ceiling on the monthly rent per square meter. The ceiling varied depending on construction year, floor area, and heating system of the building and affordability group of the municipality.⁶ This ceiling was binding for a large majority of recipient households before the reform (see Eerola and Lyytikäinen, 2021). $FloorArea$ denotes the actual size of the unit, and Max_m^2 denotes a ceiling on the size of the unit. This ceiling varied by household size.⁷ Finally, d_1 denotes a deductible which was increasing in household income and governed the phase-out of the allowance. The HA covered up to 80% of the rent of the unit.

The reform replaced the ceiling on rent per square meter and the ceiling on unit size with a single ceiling on total rent. Since January 2015, the HA is determined as

$$HA = 0.8[\min(Rent, MaxRent) - d_2], \quad (2)$$

where $Rent$ denotes the actual rent and $MaxRent$ the ceiling on the total monthly rent. $MaxRent$ depends on household size and the affordability group of the municipality, but not on other housing unit characteristics. The deductible term changed little apart from declining slightly for families with children, making the HA slightly more generous for them regardless of their choice of apartment.

The reform treated apartments of different sizes and rents differently depending on whether the ceiling on rent per square meter, the ceiling on floor area, or both were binding before the reform. As an example, Panel (a) of Figure 1 illustrates the maximum HA before and after the reform for a low-income (zero deductible) single-member household in Helsinki. Before the reform, maximum HA increases with unit size up to a limit after which maximum HA is constant (for units smaller than 37m², only the ceiling on rent per square meter is binding, but for units larger than 37m², also the ceiling on floor area is binding). After the reform, the maximum HA is

⁶Municipalities are divided into four affordability groups depending on the local rent level. The city of Helsinki constitutes one affordability group with the highest rent ceiling. Other groups in descending order of the rent ceiling are the rest of the Helsinki Metropolitan Area (3 municipalities), roughly 30 large and mid-sized cities, and all other municipalities consisting of small towns and rural municipalities.

⁷For example, the ceiling on floor area was 37m² for singles, 57m² for two-person households and 77m² for three-person households.

independent of unit size. Thus, the maximum HA increased significantly in small units, but only a little in larger units.

To illustrate how the reform affected actual HA amounts, we calculate predicted changes in allowances, implied by the differences between equations (1) and (2). To do so, we take our main estimation sample in the years prior to the reform (2010-2014), and for each unique observation of a new rental contract (a unit-household combination), we compute the HA that the household would have received using 2014 policy parameters and 2015 policy parameters. We call the difference between these two (hypothetical) HA levels the *predicted HA change*. These predicted HA changes would accurately describe the changes in HA if there were no changes other than the policy change taking place. We use these predicted HA changes as a continuous treatment variable in a DID framework, as we explain in more detail later.

Panel (b) of Figure 1 shows the predicted HA change averaged over the floor area groups. As the figure shows, there is a systematic pattern in the variation generated by the reform that follows the change in the maximum HA of the left panel of the figure. The predicted HA change was large in small units, close to zero in mid-sized units and positive again for larger units.

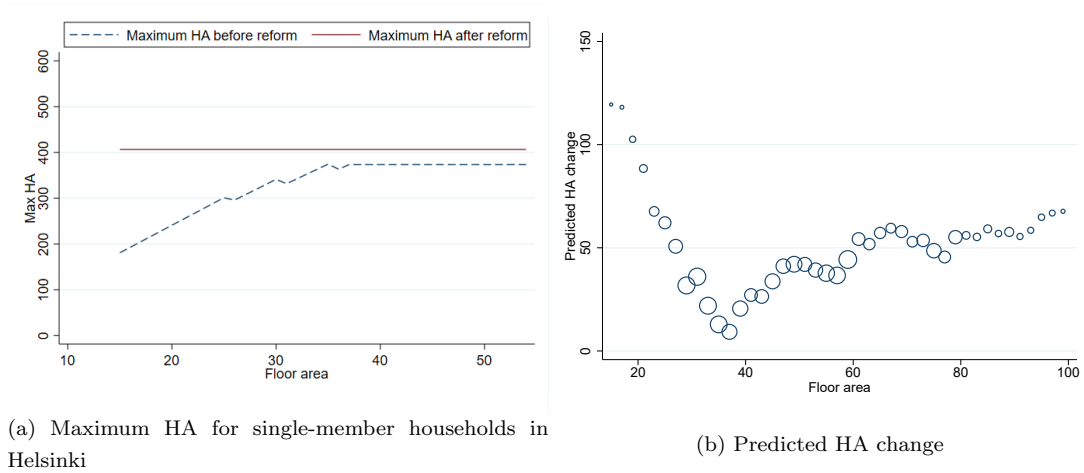


Figure 1: Maximum HA before and after the reform in Helsinki (left) and average predicted HA change in HA register data (right), both by floor area.

Notes: Panel (a) illustrates the maximum HA before and after the reform in Helsinki for a single-member household with a zero deductible renting a housing unit built before 1986. Panel (b) illustrates average predicted HA changes by unit floor area based on HA register data. The size of the circle is proportional to the number of units in each bin.

Eligibility for HA also changed with the reform. For example, prior to the reform,

a single-member household with a high deductible (high income relative to other recipients) might have been eligible for HA in a 35m² unit but not in a 25m² unit (as the cap on rent per square meter may have been binding in the smaller unit). After the reform, the eligibility no longer depended on the characteristics of the unit other than the total rent. Therefore, in the units where HA became more generous, the pool of eligible households also increased, which will strengthen our identification strategy although we will not use those changes directly for estimation. Furthermore, in September 2015, an earnings deduction of 300 euros/month was introduced. This made the HA system more generous to existing recipients with labor earnings and enlarged the pool of eligible households. We do not exploit this variation in our analysis. Changes in eligibility are described in more detail in Appendix B.1.

2.3 Research design

Graphical analysis. The reform changed the HA in different ways for different types of rental units. This variation is the starting point for our empirical analysis. We start our analysis by dividing housing units into discrete floor area groups based on Panel (a) in Figure 1 and describing the evolution of HAs and rents in these groups over time. This graphical analysis allows us to transparently examine and assess the magnitude of HA changes caused by the reform and possible coinciding changes in rents. We show separately the pool of all contracts and the subset of new rental contracts.

Continuous treatment DID. In our econometric analysis, we use a continuous treatment DID strategy, which exploits the full variation in the HA changes induced by the reform. For this analysis, we use a repeat observation sub-sample where housing units are observed with new tenants at least once before and once after the reform. For each unit, we compute a continuous *treatment exposure*: We use the pre-reform unit and tenant characteristics and compute predicted HA changes implied by the changes in the allowance formula, as described in Equation (1) and Equation (2). In other words, the treatment exposure of apartment j measures the change in HA that would have occurred due to the reform, if there had not been any changes in the characteristics of the unit, the tenant or the rental contract.

We use the treatment exposure in event study regressions as well as DID and DID-IV regressions. We start with the following event study style regressions:

$$y_{it} = \sum_{\substack{s=2010q1 \\ s \neq 2014q4}}^{2019q4} \theta_s \Delta pred_HA_j + \gamma_t + \omega_j + u_{it}, \quad (3)$$

where the outcome y_{it} is either HA or rent, i indexes the rental contract, t time and j the housing unit, and $\Delta pred_HA_j$ is the predicted HA change of the unit. The regression includes fixed effects for the time period (quarter, γ_t) and for the housing unit (ω_j).

The corresponding continuous treatment DID specification is

$$y_{it} = \beta \times \Delta pred_HA_j \times post_t + \gamma_t + \omega_j + u_{it}, \quad (4)$$

where the outcome y is either HA or rent, i indexes the rental contract, t time and j the housing unit. The regression includes fixed effects for the time period (γ_t) and for the housing unit (ω_j). $\Delta pred_HA_j$ is the predicted HA change of the unit. This regression compares the change in the HA or rent of a given housing unit from pre-treatment to post-treatment time period as a function of the predicted change in HA induced by the reform. Since all units are treated at the same time, there are no issues with a staggered treatment and we estimate the regression using two-way fixed effects.

Following Callaway et al. (2024), we can interpret the coefficient of interest in the continuous treatment DID regression as an average causal response to treatment on the treated under a *strong parallel trends assumption*, which demands that low-exposure units provide a good counterfactual for what would have happened to outcomes in high-exposure units had they received a small exposure.⁸ The assumption would fail if, for example, small units received on average larger treatment exposure and the effect of HA on rents was different for small units and for other units. We thus have to assume some degree of homogeneity in the effects of HA on rents across units with different exposures.

We also interpret our DID estimates through a DID-IV regression, which relates the size of the rent increase to the size of the HA increase by estimating the effect of a one euro change in HA on rents. The DID-IV estimates are informative about the incidence of changes in HA between tenants and their landlords and also facilitates

⁸If the strong parallel trends assumption holds, then β estimated using a two-way fixed effects regression is a weighted average of the average causal response on the treated of a given exposure for units who get that exposure, with all weights positive (although the weights do not correspond to the population distribution of the exposure).

comparison with previous studies. Here, our regression of interest writes

$$Rent_{it} = \beta HA_{it} + \gamma_t + \omega_j + \epsilon_{it}, \quad (5)$$

where the outcome variable is monthly *Rent* in rental contract *i* in time *t* and the parameter of interest is β . Similar to the event study specification, we include fixed effects for the time period (γ_t) and housing unit (ω_j). The endogeneity concerns in Equation (5) are addressed by instrumenting HA with the treatment exposure interacted with a post-reform indicator ($\Delta pred_HA_j \times post_t$). The coefficient β in this regression will simply be the DID-estimate for rents divided by the DID-estimate for allowances (similar to how the typical IV estimator amounts to scaling the reduced-form parameter by the first-stage parameter).

Regarding heterogeneous treatment effects in the context of a DID-IV specification, as summarized by [De Chaisemartin \(2010\)](#), we can interpret our estimates for β as local average treatment effects even if the conventional IV assumption of instrument exogeneity is not satisfied as long as the instrument is uncorrelated with potential outcomes, accompanied with two parallel trends assumptions: one related to the first-stage and another related to the second-stage outcome. These are the same parallel trends assumptions we make in our DID estimation. We inspect the credibility of these parallel trends assumptions by comparing the pre-reform trends in units receiving varying treatment intensity as a part of our event study analysis.

Finally, both the DID and DID-IV specifications rely on the stable unit treatment value assumption (SUTVA), which requires that the increase of HA in some housing units does not affect rents in other housing units. This assumption would be violated if, for example, housing allowances had increased for small units enough to have decreased the demand for medium-sized units, which is possible at least in principle. However, the failure of the SUTVA assumption in our case would most likely lead to an upward bias in our estimate on the effects of HA on rents (if there was an important shift in demand away from the units with small predicted HA changes, we would expect a rent decrease in this group). Given that we find point estimates which are close to zero, the concern of an upward bias is not too worrying in our context.

3 Rent effects

3.1 Main results

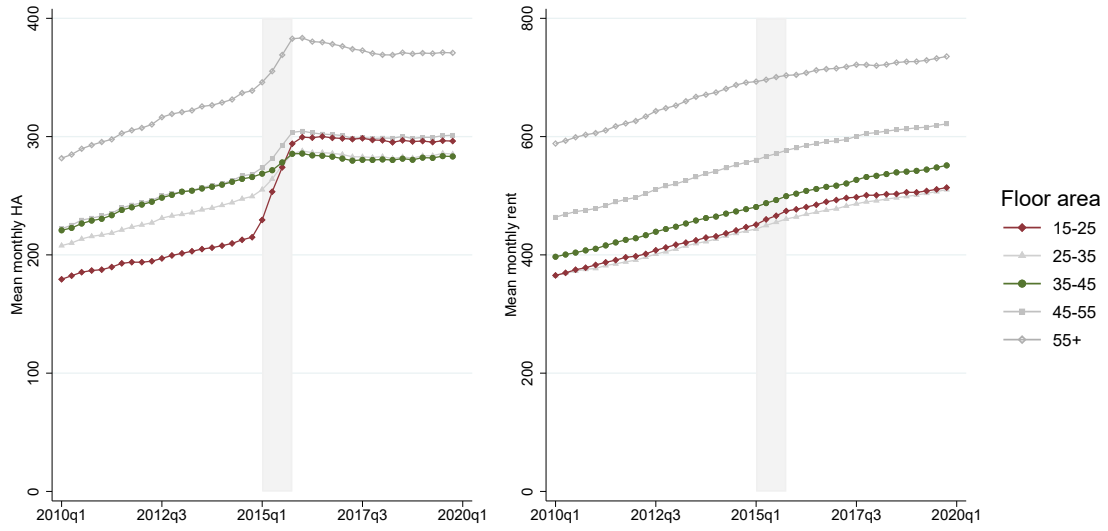
Descriptive evidence. We first report a set of descriptive graphs in which we divide housing units into groups by their floor area. Panel (a) of Figure 2 shows the evolution of mean monthly HA payments to recipient households (left) and their rents (right) in the different floor area groups. As expected based on Panel (b) in Figure 1, housing allowances in units of 15–25m² (highlighted in red) increased dramatically in 2015, while increases in other floor area groups were moderate. Especially in medium-sized units (35–45m², highlighted in green), HA deviates in 2015 only slightly from its pre-reform trend. The development of mean rents in the different floor area groups in turn is stable around the reform period, and there are no visible differences between the groups.

Panel (b) in Figure 2 shows the evolution of monthly HA payments and mean rents in new rental contracts only (for the remainder of our analysis on rent effects, we focus exclusively on new rental contracts). Rents in existing contracts can be rigid, but in new rental contracts there are no legal or other reasons not to expect immediate rent effects if HA increases are indeed passed through to rents. The bottom panels are very similar to the top panels of Figure 2, and indicate no changes in the relative rents between groups.

Descriptive evidence about the size of the reform relative to the rental market. While Figure 2 documents that the reform caused substantial variation in housing allowances paid to tenants in different floor area groups, it does not show how large these changes were relative to the size of the rental market. For example, if only 1% of households were HA recipients, then a 100-euro HA increase for recipients would translate to only small changes for all renters on average. This matters especially if landlords cannot charge different rents from recipients and non-recipients: if they cannot price-discriminate, then what matters for the pass-through is the average subsidy increase at the level of the rental market. In this case, identifying any rent effects of HA requires that the treatment is substantial enough to impact the entire rental market, taking into account non-recipients as well.

We verify in Appendix B.1 that this is indeed the case in our setting. Appendix Figure B1 shows that a large share of households, between 15% and 25% across different floor area groups, are HA recipients prior to the reform. Moreover, after the reform, recipient shares *increased* in more treated units. The increased HA and

higher recipient shares in small units relative to medium-sized units jointly translate to large variation in the average HA payments to all renters (including non-recipients) across floor area groups, as described in Appendix Figure B2. This suggests that the reform provides strong identifying variation to study the rent effects of HA changes at the market level.



(a) All payments.



(b) New contracts

Figure 2: Mean HAs and rents by floor area group.

Notes: The figure shows mean monthly HA paid to recipient households and mean monthly rents paid by recipients in our estimation sample, at quarterly level. The light gray shaded area refers to year 2015. For existing HA spells, the reform was rolled out during 2015, but for new rental contracts, it became effective immediately in the beginning of 2015. "All payments" refers to all monthly HA payments to recipients, and "New contracts" refers to the first payment made to a recipient who has changed addresses. For details on sample selection and identifying new rental contracts, see Appendix A.

Differences-in-differences estimates. Next, we turn to the continuous-treatment DID analysis using our repeat observation sub-sample with only housing units that we observe at least once before and after the reform. This analysis compares changes in rents across housing units which received different-sized treatment exposures, where the treatment exposure is defined as a predicted HA change assuming that only the parameters of the HA system changed, but other characteristics of the unit and the recipient remained the same (see Section 2.3 for details). First, we report event study type evidence in Figure 3. The left panel shows that a one-euro increase in the predicted HA change (treatment exposure) led to approximately one-euro increase in the actual HA payment. This means that the reform induced significant exogenous variation in HAs, although we cannot perfectly predict the new HA received by the new tenant (since the tenant characteristics also changed, which we do not control for, as did possibly the rent). The right panel shows that prior to the reform rents developed similarly in units that received different-sized treatment exposures lending support to our parallel trends assumption. The right panel also shows that increases in treatment exposure did not lead to increases in rents paid by HA recipients.

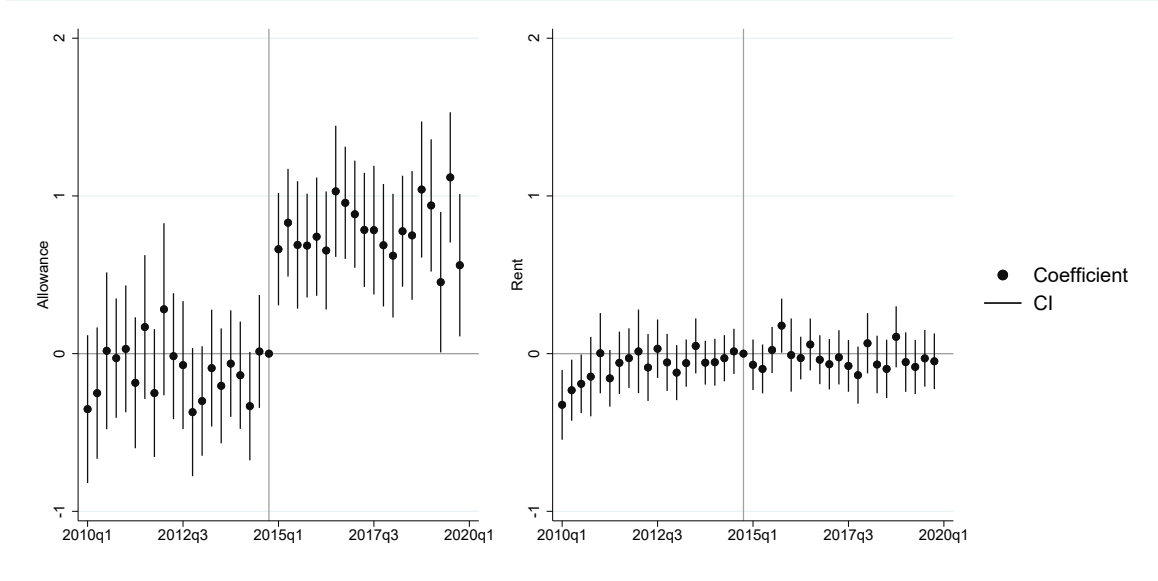


Figure 3: Comparison of monthly HAs and rents of units that received different-sized treatment exposures, with housing unit fixed effects.

Notes: The figure plots coefficients from an event study regression in our fixed effects sample, where the outcome (HA or rent) is regressed on quarter fixed effects, housing unit fixed effects and treatment exposure \times quarter fixed effects, omitting the last quarter before the reform. Dots and whiskers illustrate the point estimate and the 95% confidence intervals of the treatment \times quarter coefficients. Standard errors are clustered at the housing unit level. $N = 22,346$.

The corresponding continuous treatment DID regression results are reported in Table 2. First, in column 1, we regress actual HA on our measure of predicted HA change. The point estimate suggests that a one-euro increase in the predicted HA change is associated with a 0.9 euro increase in the actual HA with a standard error below 0.04. Thus, our predicted HA change is highly correlated with changes in actual HA. Column 2 reports estimates from a similar regression where the outcome is the rent. The point estimate suggests that the average rent increase following a one-euro increase in the predicted HA was very modest, roughly 2.5 cents. For completeness, in column 3, we present the DID-IV results in which HA is instrumented for by our measure of predicted HA change. The estimated effect of HAs on rents, roughly 3 cents per an additional euro of HA, is small and not statistically significantly different from zero. The standard error is 0.0177 and implies that we can rule out even moderate rents effects with a high degree of confidence.

Table 2: Continuous-treatment DID-IV estimates, with housing unit fixed effects.

	DID		IV
	(1)	(2)	(3)
	Allowance	Rent	Rent
Predicted HA change	0.899 (0.0339)	0.0243 (0.0161)	
Allowance			0.0270 (0.0177)
Month \times year FEs	✓	✓	✓
Unit FEs	✓	✓	✓
Outcome mean	314.1	577.3	577.3
N	22346	22346	22346
SE clustered by	Unit	Unit	Unit
First-stage F			705.1

Notes: The table reports results from DID and IV regressions where the treatment or instrument is defined to be a predicted HA change as described in Section 2.3. Columns 1 and 2 report coefficients from a regression of the outcome on our measure of predicted HA change \times post indicator. Column 3 reports the second-stage of an IV regression, where HAs are instrumented for by a predicted HA change \times post indicator. The first stage of this regression corresponds to column 1. Standard errors are clustered at the housing unit level.

3.2 Robustness

Taken together, our results so far imply that increases in HA due to the 2015 reform did not increase rents. Next, we provide additional analyses and robustness checks.

Household characteristics. Our main analysis holds constant the pre-reform characteristics of units and tenants. A factor that could complicate the interpretation of our results is the potential effect of the reform on household sorting into different types of housing units. We study this potential issue in Appendix B.2. We show that there were no large changes in household characteristics across different types of units after the reform. This is consistent with the high predictive power of the first-stage regression in Table 2.

Rent results with alternative treatment definitions. We analyze the robustness of our main results to alternative treatment definitions in Appendix B.3.

We begin by testing whether our results hold when we do not compute a variable describing treatment exposure at all, but simply split the data into discrete treatment and control groups. We do this by comparing the development of rents and allowances in different floor area groups. We use a two-group specification where the 15–25m² units act as the treatment group and the 35–45m² units as the control group. This specification is based on Figure 2 showing that the average HA change is much larger in housing units with floor area 15–25m² than in housing units with floor area 35–45m². This specification yields very similar results to those obtained by using the predicted HA change. Event study results in Appendix Figure B5 show that rents in the treatment and control groups developed similarly prior to the reform and that the rents did not increase in the treatment group relative to the control group after the reform despite large differences in HA increases.

Second, we consider alternative ways to compute the continuous treatment exposure. The treatment exposure definition in our main results reported in Figure 3 and Table 2 holds constant pre-reform observed characteristics of the unit and the tenant. As described above, a potential concern is instrument weakness in the case where tenant characteristics in given types of units change after the reform. We are not particularly concerned by this given the strong predictive power of the first-stage regression. Also, as discussed above, household characteristics do not change in different floor area groups after the reform. Nonetheless, we address this concern by considering two alternative methods for computing the predicted HA changes.

We first approximate the predicted HA changes by the average predicted changes for similar households in similar housing units. For all new contracts before the reform (2010–2014), we compute the predicted HA change given the pre-reform characteristics of the unit and household living in the unit. We then average this measure by household and unit characteristics by splitting the data into groups by floor area (2m² brackets), city size (3 groups⁹), and household size (1, 2 or 3 members¹⁰). We compute the *average* predicted HA change in these cells and label this variable as $\overline{\Delta pred_HA_1}$. This is the average predicted HA change for a household of a given size,

⁹These are Helsinki Metropolitan Area (Helsinki, Espoo and Vantaa), six other largest cities (cities which have a population of at least 100 000, excluding Helsinki MA), and the rest of Finland.

¹⁰We exclude households with 4 or more members, because there are relatively few such households in our main estimation sample and because these households likely consider also units with floor areas larger than 100m², and are thus excluded from our mobility analysis.

living in a unit of given size and given city size.

Using this alternative exposure definition, we can let the exposure of a given housing unit depend on *actual* household characteristics at the time of observation instead of holding constant the pre-reform household characteristics. We will use this treatment definition also in subsequent analysis when we analyze the rent effects by city size and when we explore households' moving behavior in Section 4.1. Results in Appendix Figure B7 show that the effects of the reform on HAs and rents are very similar to our main results. Again, we observe a sharp increase in actual HA payments as a response to predicted HA change, and we observe no differential changes in rents of units with more or less predicted HA change.

Moreover, to make the instrument completely blind to household characteristics, we also compute the average predicted HA changes without household characteristics. To measure changes in HA as a function of the characteristics of housing units only, we proceed by calculating averages of predicted HA changes at the same floor area (2m² brackets) and city size (3 groups) cells as before. We label this variable $\overline{\Delta pred_HA}_2$: This is the *average* predicted HA change for a unit of given size and given city size.¹¹ We will use this variable later on when analyzing rental housing supply in Section 4.2. Rent effects using this alternative treatment definition are reported in Appendix Figure B8 and are again very similar to our main results.

With these alternative treatment definitions the estimation sample of new rental contracts consists of more than 200,000 observations. Therefore, they allow us to use a substantially larger sample than in the specification with housing unit fixed effects. Although we are not able to control for unit fixed effects in these specifications, our main results carry through, and none of the specifications indicate growth of rents in units with higher predicted HA changes.

Rent effects by city size. Our main specification in Table 2 compares units with bigger treatments to units with smaller treatments. Because the treatment was on average larger in more urban areas, some of the identifying variation comes from comparing changes in more urban areas to changes in less urban areas. To test whether the results are robust to comparing different units only within similar types of municipalities, we repeat the same analysis but include city size times quarter fixed effects in Appendix Table B1. The first stage remains strong, and the point estimates are again precisely estimated zeros.

We also analyze changes in HAs and rents separately in different-sized cities,

¹¹Again, we exclude households with 4 or more members.

ranking city sizes from very large to small. In this analysis, we use the alternative treatment definition ($\overline{\Delta pred HA_1}$) to allow for a sufficiently large estimation sample in each group. Appendix Figure B9 and Figure B10 report our descriptive and event-study graphs separately for the three city size groups. Across the board, the estimates for the rent effects of HA are negligible in these location-specific regressions. This is true even for the Helsinki metropolitan area where HA increases were clearly the largest and housing supply is presumably most inelastic (Oikarinen et al., 2015).

Social assistance. In Finland, housing costs of low-income households are covered not only through the HA program but also through social assistance, which is the last-resort form of financial assistance in the Finnish social security system. Social assistance is applied for separately. It is typically granted only for a period of 1–2 months at a time, after which a new application is required. For social assistance recipients, changes in HA may be somewhat mitigated by changes in social assistance. Therefore, for households who are eligible for social assistance, the net benefit change induced by the HA reform is therefore likely smaller than what would be implied by the changes in the HA formula alone. We address this issue in Appendix B.5, where we describe the social assistance program and go over different robustness exercises.

Our HA register data do not include information on social assistance. Therefore, to describe the importance of social assistance for the overall subsidy changes, we use total population register data from Statistics Finland. As discussed in Section 3.1, the HA reform was large enough to cause substantial variation in HA changes not only for the recipients but also for the overall rental market. When we look at the overall rental market, including non-recipients as zeros but excluding students, the average HA payment per renter increased by 416 euros more per year in small (15–25m²) relative to medium-sized (35–45m²) units from before to after the reform. If we take into account changes in social assistance and look at the *sum* of the two subsidies, we see that the total benefit (HA + social assistance) increased by 271 euros more in small relative to medium-sized units. This suggests that although some of the HA increases were indeed offset by larger social assistance payments, there remained large and clear changes in net subsidy payments across different floor area groups.

To study whether our estimated rent effects depend on social assistance eligibility, we divide the sample into two groups based on income, as households with sufficiently high incomes are very unlikely to be eligible for social assistance. Appendix Figure B12 shows the evolution of mean monthly HA payments and mean rents in different floor area groups and Appendix Figure B13 the event-study graphs separately for

the two income groups. The rent effects for the two groups are very similar to one another (and overall similar to our main results). Therefore, it does not seem likely that our main results are mitigated by the countervailing incentives generated by the social assistance program.

4 Why do we observe small rent responses?

The observed effects of HA on rents are an equilibrium outcome that stems from rental housing demand and supply. Our estimates of the rent effects of the HA changes are small in economic magnitudes and also small relative to some earlier findings in the literature. To understand why we observe such small rent effects, we next zoom in on the different margins of adjustment. We start by setting up a conceptual framework to illustrate the different margins of adjustment. We then analyze empirically the changes along these different margins.

Conceptual Framework. In Appendix C, we present a stylized conceptual framework to discuss how the rent effects of small housing allowance increases depend on demand and supply-side responses. We consider a competitive rental housing market where some households receive HA and others do not and where housing is elastically supplied by landlords.

The framework serves to highlight two important observations. First, when HA recipients and non-recipient households co-exist in a competitive rental market, the rent effects of HA changes depend not only on demand responses of HA recipients but also on the share of HA recipients. In fact, the average rent effects are strictly bounded from above by the share of HA recipients. In other words, if the share of HA recipients is, for example, 30%, then a one euro increase in HA *cannot* increase rents by more than 30 cents even in the extreme case where supply is completely inelastic. This highlights that on a relatively competitive rental market, where rents paid by recipients and non-recipients are the same, it is well possible that rent effects of housing allowances are relatively small. Moreover, this also implies that if the rental market is competitive, then identifying any rent effects of HA requires variation that is strong enough to be detected at the level of the overall rental market, not only for recipients. As discussed above, we have verified that this is the case using the population register data with information on non-recipients.

Second, the conceptual framework highlights that the adjustment of rental housing supply may happen through new construction and (net) conversion of owner-occupied

housing to rental housing. These effects are potentially different in the short and long run. If one would consider only new construction, then the housing stock would be effectively almost inelastically supplied in the very short run. However, while new construction has a limited role for supply in the short run, the conversion of units from owner-occupied units to rentals by investor-landlords could potentially be more important also in the relatively short run.

4.1 Housing choices of HA recipients

To understand whether the reform affected recipients' demand for different types of units, we next analyze their housing choices. The reform increased recipient households' financial incentives to occupy units that received larger treatment doses, since allowances in those units increased, but rents did not. We analyze recipient households' responses along two dimensions: household mobility (the propensity to move out of units with smaller or larger exposure to treatment), and housing choices conditional on moving (whether households who move choose units with larger treatment exposure).

Defining treatment exposure. To be able to assess whether households' choices were affected by the reform, we need to be able measure HA changes induced by the reform also for housing units that we do not necessarily observe both before and after the reform. To do so, we use treatment variable $\overline{\Delta pred_HA}_{1c}$: the average predicted HA change of a given household size in a given unit size in a given city size (subscript c indexes cells by household size - unit size - city size). For details, see Section 3.2.

Moving out: mobility rates. To measure whether households are more likely to move out of units where the predicted HA change was smaller, or less likely to move out of units where the predicted HA change was larger, we run the following event study regression in the sample of all payments:

$$y_{it} = \theta \overline{\Delta pred_HA}_{1c} + \sum_{\substack{s=2010q1 \\ s \neq 2014q4}}^{2019q4} \theta_s \overline{\Delta pred_HA}_{1c} + \delta_q + \gamma_m + \epsilon_{it}, \quad (6)$$

where the outcome is $y_{it} = 1$ if during the quarter, recipient household i moves from the unit and 0 otherwise.¹² $\overline{\Delta pred_HA}_{1c}$ refers to the average treatment exposure

¹²We aggregate the data at quarterly level so the outcome indicator variable takes value 1 if household moves in any month of the quarter. We define moving households as households who will

of the *current* unit (before the move). The regression includes quarter fixed effects (δ_q) and municipality fixed effects (γ_m) for the current location. If, after the reform, households are less likely to move out of units with larger treatment exposure (or more likely to move out of units with smaller treatment exposure), we would expect the coefficients θ_s to be negative after the reform.

Moving in: characteristics of new units, conditional on moving. Next, we consider if, conditional on moving, households choose units with larger treatment exposure after the reform. To measure whether household choices shift toward apartments where HA increased on average, we estimate the following regression in the sample of households who *do* move:

$$\overline{\Delta pred_HA}_{1c} = \sum_{\substack{s=2010q1 \\ s \neq 2014q4}}^{2019q4} \theta_s + \omega_m + \epsilon_i. \quad (7)$$

Now, the *outcome of the regression* is the average predicted change ($\overline{\Delta pred_HA}_{1c}$) in the new unit of the household.¹³ We explain this outcome variable simply by the time fixed effects describing the timing of the move, omitting the last period before the reform. The analysis includes fixed effects at the municipality level by the arrival municipality (ω_m). Thus, the regression aims at showing if, conditional on moving and given the choice of municipality, households choose different types of units after the reform than before. If, after the reform, households start choosing units with larger treatment exposure, we would expect to see positive post-reform time effects in this regression. Note that this regression should be interpreted as a simple "differences" estimation (changes in the characteristics of units chosen by households over time) as opposed to a differences-in-differences estimation (differential changes over time across more and less treated units).

Results. Figure 4 describes the results from the two regressions analyzing housing choices. The left panel illustrates the propensity to move out of units with different-sized treatment exposure, as summarized in Equation (6). There are no differences in moving out patterns before and after the reform between units with different treatment exposure. If anything, in the first year after the reform households seem slightly

receive HA in a different address in their next payment, at most 4 months later. We look only at moves from units in the unregulated rental market and the household size at most 3.

¹³We only look at moves where households move to a unit in the unregulated rental market and the household size is at most 3.

more likely than before to move out of units with larger treatment exposure. In other words, the evidence does not suggest that households would leave units with smaller exposure more frequently or leave units with larger exposure less frequently after the reform than before the reform.

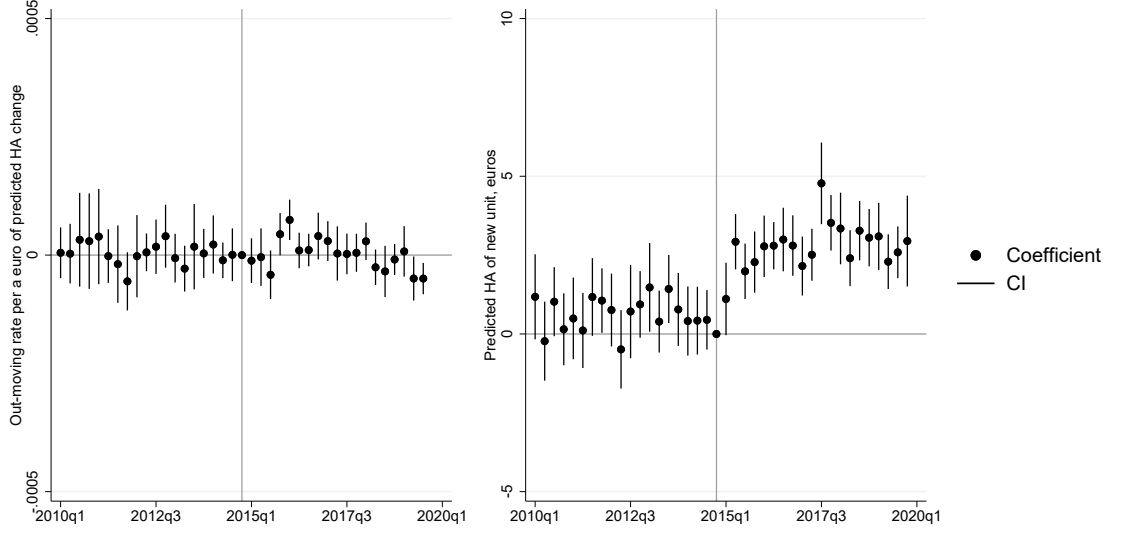


Figure 4: Household choices.

Notes: The left-hand graph depicts the propensity to move out of units with different treatment doses by plotting the event study estimates from Equation (6). The baseline mean quarterly mobility rate (the share of observations where a recipient household changes address from one quartile to the next) is 3.46% in the pre-reform period. The right-hand graph depicts, conditional on moving, the size of the treatment dose of the new unit of the household by plotting the event study estimates from Equation (7). In both panels, we cluster the standard errors at the municipality level. $N = 3,713,762$ in the left figure and $N = 201,293$ in the right figure.

Even if the changes in HA do not affect the decision to move, they might influence the choice of the new unit for those who do move. The right panel of Figure 4 describes the types of units that households choose *conditional on moving*, as measured by the time effects of Equation (7). The graph reveals that there is indeed a small but statistically significant response by recipient households to the reform. The point estimates from 2010 until 2014 are all similar to the end-of-2014 level (reference category). Thus, in the pre-reform period, there are no systematic changes in the types of units chosen by households. However, immediately after the reform, there is an increase in the point estimates. This indicates that after the reform, households who do move start choosing units with larger average treatment exposure (that is, larger predicted HA change). The effect size, however, remains small, stabilizing

at approximately 3-4 euros per unit. The effect stabilizes almost immediately after the reform, suggesting that there was no transition period during which households would, for example, learn about the reform.

Overall, we interpret this as evidence that even if the reform did not change moving frequencies at the extensive margin, the reform did induce a small change in the types of units chosen by households conditional on moving. However, the effect size, although statistically significant, is relatively small (the pre-reform standard deviation of $\overline{\Delta pred_HA}_{1c}$ in the sample is 29, suggesting that households start choosing units with less than 0.2 standard deviations higher predicted HA changes). Moreover, we find that the change is not uniform throughout the distribution: the observed average change (\approx 3-4 euros) is driven mainly by changes at the top of the distribution. When comparing moves before and after the reform, the percentiles at the bottom of the distribution move very little compared to the top of the distribution (the 5th percentile does not change, the median increases by approximately 2 euros, and the 95th percentile increases by more than 12 euros).

Our results suggest that the HA paid to a given housing unit is not a major factor affecting the choice of the bundle of housing and non-housing consumption of the recipients households. One potential reason that could explain why many recipients do not take HA changes into account is the expected duration of allowance spells relative to expected tenure spells in the new housing unit. For example, according to our register data, less than half of the HA spells that started during our analysis period lasted more than a year. Thus, when moving to a new housing unit most HA recipients likely expect to bear the full rental burden of the unit at some point during their tenure. Another explanation for the results relates to the availability of different types of units in the unregulated rental market.

4.2 Developer and landlord choices

Next, we describe changes in the supply side of the rental market.

Defining treatment exposure. To assess whether more units with larger treatment exposure were supplied on the rental market after the reform, we need to measure HA changes induced by the reform for units that may not have existed or may not have been available to rent before the reform. Moreover, to be conservative, we assume that landlords and developers cannot anticipate what type of a household will move into the unit. To this end, we use treatment variable $\overline{\Delta pred_HA}_{2c}$, the average

HA change for a given sized unit in given city size (c indexes cells defined by floor area brackets and city size group). For details, see Section 3.2.

Construction. To measure changes in the composition of construction of new housing units in unregulated rental market, we use the population-wide data on occupied housing units from Statistics Finland, as described in Section 2.1. The data is annual and each observation summarizes the end-of-year situation (as opposed to our monthly HA register data). We define new rental units as unregulated housing units which are built in year t and where someone is living in at the end of year t as a tenant. Our estimation equation is similar to Equation (7), since we are interested in an intensive margin response to the reform. Using the sample of newly constructed rental units we run the following regression:

$$\overline{\Delta pred_HA}_{2c} = \sum_{\substack{s=2010 \\ s \neq 2014}}^{2019} \theta_s + \omega_m + \epsilon_{it}, \quad (8)$$

where the outcome ($\overline{\Delta pred_HA}_{2c}$) is the average treatment exposure in cell (c) defined by floor area and city size of the rented unit. The explanatory variable of interest is the time effects, and we control for municipality fixed effects. Positive time effects in this regression after the reform would indicate that new rental construction shifted toward units with higher average treatment doses.

Conversion of units to rentals. To see whether there have been changes in the types of units converted from the owner-occupied housing to unregulated rental market, we again run a specification which is identical to Equation (8). This time we use the pool of units where the unit was occupied by the owner in $t - 1$ and is occupied by tenant in year t in the unregulated rental market. If investors start converting units with larger treatment exposure into rentals, we would expect to see positive time effects in this regression after the reform.

Composition of the rental stock. Finally, we examine how the overall stock of housing units in the unregulated rental market has evolved. To measure changes over time in the types of units, we take as given the aggregate number of units rented in the unregulated rental market and assess whether the composition of units has shifted toward units with larger treatment exposure. We run a regression which is identical to Equation (8), but this time using the overall unregulated rental housing stock. Again, positive time effects in this regression after the reform, would indicate that

the overall rental stock shifting toward units with larger average treatment exposure. Note that rental stock growth not affecting the distribution of units with different treatment exposure, would not result in a change in the time effects of this estimation equation after the reform.

Results. Figure 5 summarizes the analysis of the changes on the supply-side of the market. Starting with construction, Panel (a) shows that in the pre-reform period, there were no systematic trends in the types of units constructed with respect to average treatment exposure. After the reform, there is a change in the composition toward units with larger treatment exposure, likely reflecting a relative increase in the construction of small units. However, this change is statistically significant only five years after the reform and the size of the estimate is small at roughly 2.5 euros per unit.

Panel (b) shows the results on the composition of owner-occupied units converted into private rental units. In this case, there is a small compositional shift toward larger treatment exposure units two years prior to the reform, but the composition remains largely constant during and after the reform. Given the very small point estimates, we conclude that throughout the time period there have been no economically significant changes in the types of units that are converted into rentals.

Even if we do observe a small shift in the types of units that are built over time, as illustrated in Panel (a) of Figure 5, this does not seem to translate into significant changes in the overall rental stock, since only a small share of rental units are recently built. Panel (c) shows the evolution of the total rental stock. This aggregates over all possible margins through which the unregulated rental housing stock can change, including also conversions of units from rentals to owner-occupied units and units becoming unoccupied (depreciation). Prior to the reform, the total stock had not been changing toward units with larger treatment exposure. After the reform, there was a small statistically significant change toward units with smaller treatment exposure, but this change was tiny in magnitude. Given that the point estimates are very close to zero throughout, we interpret that the unregulated rental housing stock did not start shifting toward units with larger treatment exposure.

For completeness, Panel (d) shows how the full housing stock evolved before and after the reform by floor area group. Especially the stock of smaller and medium-sized housing units grew during the period we analyze, but there are no sharp changes coinciding with the reform. Overall, based on the evidence summarized in Figure 5, supply responses are unlikely to be important drivers of our rent results.

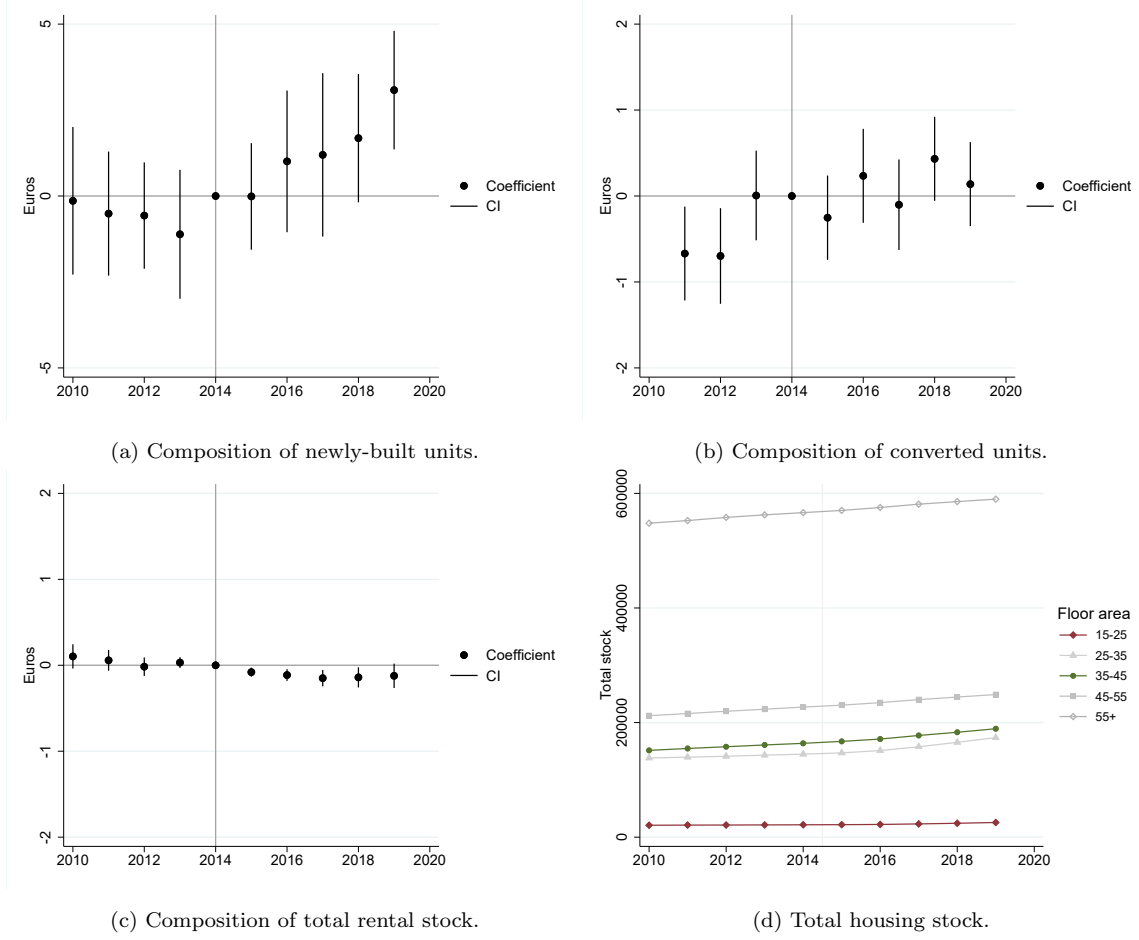


Figure 5: Rental housing supply and housing stock.

Notes: Panel (a) describes the composition of newly built and privately rented units annually (units such that someone lives in the unit at the end of year t and unit is built during t). Panel (b) describes the composition of the units converted from owner-occupied units to privately rented units during the year. Panel (c) illustrates the composition of the total unregulated rental stock. Panel (d) illustrates the full apartment stock (not only the unregulated rental market) in levels by floor area group. Panels (a)-(c) exclude units that are held by households where at least one member was receiving student allowance. In panels (a)-(c), regressions include fixed effects at the municipality level. Standard errors are clustered at the zipcode level. The whiskers describe the 95% confidence intervals. For details on the samples used for each graph, see Appendix A. $N = 41,793$ in Panel (a), $N = 70,907$ in Panel (b) and $N = 2,823,964$ in Panel (c).

5 Conclusions

This paper addresses the question of how rent subsidies for low-income households affect rents by exploiting exogenous variation generated by a large housing allowance reform in Finland and by using rich register data on allowance recipients and the full population. The reform led to differential increases in allowance payments depending on unit characteristics. We find that despite large differences in housing allowance increases by unit types, the reform did not have economically significant effects on the relative rents between different types of units. This implies that the changes in housing allowances in affected units mainly benefited the recipients rather than their landlords.

The mechanisms via which rent subsidies affect rents, and how the rent effects depend on the characteristics of the housing market and of the subsidy program, have received little attention in the literature on the incidence of rent subsidies. We study not only the rent effects of the allowance reform but also the housing consumption choices of recipient households and the quantities of rental housing supplied. Despite large changes in financial incentives to choose different types of units, we observe only modest changes in recipient households' housing choices. Recipients do not stay longer in units with large increases in allowances after the reform. However, conditional on moving, they start choosing units with slightly larger allowance increases than before the reform. We argue that one explanation for small demand responses is that allowance spells are often short.

While the construction of small units increased after the reform, this led to very slow changes in the stock of private-market rental units, given that only a small share of the housing stock is newly built. Taken together, these observations about recipient's housing choices and quantities supplied in the rental market suggest that the small rent effects are more likely due to relatively small demand responses to the reform rather than a strong supply response.

Is our pass-through estimate externally valid? We argue that there is no single structural parameter that describes the pass-through of rent subsidies to rents. This pass-through is always context-specific and depends on the details of the program and housing market conditions. We address this point by carefully documenting demand- and supply-side changes in the housing market that are likely contributing to the small rent effects we uncover, thereby improving the external validity of our study relative to the existing literature. Future research on rent subsidies should aim not only at estimating pass-through but also at characterizing the demand- and

supply-side mechanisms driving the results.

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Appendix

A Data and sample selection

Kela HA register data and sample selection. The HA register of Social Insurance Institution of Finland (Kela) covers years 2008-2019 on a monthly basis, and each regular monthly HA payment is a separate observation. The data contain an ID for the individual to whom the payment was made and the ID of their spouse (if there is one), since the housing allowance is determined at household-level.

We make the following restrictions in selecting our main estimation sample. First, we only include regular monthly payments (excluding for example overpayment recoveries). Second, we exclude the following observations: 1) observations from Åland Islands, as it is a very specific region both in terms of geography and demographics, 2) observations for which either address or zipcode is missing, and 3) observations that are clearly outliers in terms of their rent per m^2 (below 3 euros/ m^2 or above 80 euros/ m^2). We also exclude all housing units with floor area either below 15m^2 or above 100m^2 . Third, we leave out years 2008 and 2009 to avoid any confounders stemming from the financial crisis.

Furthermore, as we want to focus on regular rental contracts, we exclude certain types of observations. First, we exclude recipient households who are owner-occupiers. Second, we exclude apartments from publicly subsidised right-to-occupy apartments (*'asumisoikeusasunnot'* & *'osaomistusoikeusasunnot'*). Third, we exclude social rental housing where rents are regulated and determined based on maintenance and capital costs (identifying these units in the data is based on the information that they benefit from the government-subsidised debt program). Finally, we exclude shared housing units (either if the unit is defined as shared by Kela or if there are more than 12 monthly payments per year for the same unit in our data). This is because the housing units that were occupied by more than one household were subject to special rules in the pre-reform HA system.

We also exclude students from our sample throughout the time period because the housing benefits of students change over time. Before 2017, most students were covered by separate student housing subsidy program (*asumislisä*). In 2017, students became eligible for general HA. To exclude students we use a separate Kela register on student allowance payments (*opintoraha*). We assume that the student status mostly changes end of term in December and June and we classify all individuals who receive student allowance at least once during the half-calendar year (January-June or

July-December) as students for the six month period in question. Thus, for example, someone who received student allowance in February 2018, will be excluded from our estimation sample throughout the first half of 2018.¹⁴

Pensioners have a separate HA program. General HA is not granted to a couple (married or co-habiting) if one of them is entitled to the pensioner's HA or to an individual who is entitled to the pensioner's HA. Before the 2015 reform, families with children entitled to both pensioner's HA and general HA were allowed to choose their program. After the 2015 reform these families have been allocated to the general HA system. The government proposal estimated that the change concerns roughly 2,500 families with children.

We do not directly observe new rental contracts in the data. To determine new rental contracts we proceed as follows: If the individual received HA in another address at most 4 months before, we classify the first observation in the new address as a "new rental contract" (this is done before other sample restrictions, so, for example, if someone moves from social rental housing to private rental housing, the observation in private rental housing is registered as a new rental contract). Misclassification can occur in two ways: First, some contracts can be labeled as new even if in reality they are not. For example, an individual who receives HA may move to a unit in which someone was already living in and therefore the rental contract may benefit from terms that had been set prior to the move. Secondly, some rental contracts that are new will not be identified as such. For example, when an individual appears in the HA register for the first time, we will not classify the observation as a new contract although the individual could well have moved at the same time.

For the housing unit fixed effects analysis, we identify repeat observations of units using the exact street address including the unit number. Apartment floor area is self-reported and there are some repeat observations where the reported floor area group varies, for example if someone living in a shared unit misreports the floor area. We exclude these observations.

Our main outcome variables are the allowances paid to the household and the rent paid by the household (rent excludes other costs such as the water charge). These and other sample characteristics are summarized in Table 1 in the main text. In reporting summary statistics, we report all monetary values in 2020 euros, where the deflator is obtained from the Statistics Finland CPI.¹⁵ To describe household incomes, we use

¹⁴A small fraction of the observations in our main estimation sample are individuals whose spouse is a student (approximately 0.7% before 2017 and 2% after 2017.)

¹⁵Official Statistics of Finland (OSF): Consumer price index [e-publication]. ISSN=1799-0254.

variable "Household income". This variable is used in the summary statistics table as well as in Appendices B.2 and B.5. To compute the predicted HA change used in Table 2, we use variable "Household income net of deductions" (income net of earnings deductible).

Treatment exposure. We compute the treatment exposure for all new rental contracts observed in 2010–2014. We exclude households with more than eight members as they are unlikely to live in units with at most 100m². For all units, we use the information on pre-reform unit and tenant characteristic. We first deflate all pre-reform housing costs from different years to 2014 euros (we use the HA register variable *housing cost used to determine HA* which includes the rent and other necessary costs such as water). We then use the housing cost, floor area of the unit, municipality group and construction year of the building (assuming that all buildings have central heating, which is very likely to be the case), together with the observed pre-reform household size and income (deflated to 2014 euros), to predict the level of HA that the household should have using the 2014 HA policy parameters. Next, for the same observations, we predict the HA that the household should have had in the same unit with the same housing cost and income deflated to 2015 euros with the 2015 HA policy parameters. This *predicted change in HA* for each unit is our measure of treatment exposure.

Statistics Finland register data. We use population-wide register data from Statistics Finland ready-made research data modules (*Folk Basic* and *Income*). In addition, we have obtained data on HAs from the register-based total statistics on income distribution (*Tulonjaon kokonaistilasto*). These data include the annual amount of general HA, pensioners' HA and students' housing supplement. The ready-made data and the tailored data are combined with secured individual identifiers. We also use Statistics Finland's housing unit and building register data. This data covers the universe of buildings and housing units in Finland, with unique identifiers. The data includes information on the building, such as construction year, as well as data on each unit, such as the floor area. The unique unit identifiers can be linked to individuals.

Using the information on units and individuals, we construct a dataset at the level of households (individuals who share the same unit). Throughout, we focus

Helsinki: Statistics Finland [referred: 7.6.2022]. Access method: http://www.stat.fi/til/khi/index_en.html

only observations where the building type is a multi-unit building (referring to blocks of flats in residential use). We focus on units with floor area between 15 and 100 m^2 and exclude units where the floor area is missing. For most of the analysis, we exclude student households based on whether at least some member of the household has been receiving student allowance during the year. For most of the analysis, we only focus on units on the unregulated rental sector.

Table A1 summarizes the Statistics Finland data, both household and unit characteristics, at the household level, for the sample of households living in blocks of flats in the private rental sector, excluding student households. Household income refers to the total disposable income at the household level (after taxes and transfers) and HA refers to general HA (not to students' or pensioners' HA). Households receiving at least 100 euros of HA during the year are classified as HA recipients.

Table A1: Household characteristics, unregulated rental market.

	Non-recipients	Recipients	Both
	mean	mean	mean
Household size	1.4	1.5	1.4
Floor area	49.0	46.2	48.2
Brand-new unit	0.015	0.013	0.015
Income (excl. HA)	29,209	15,998	25,501
HA		3036	845.7
Share HA recipients			0.28
<i>N</i>	2,035,606	794,329	2,829,935

Notes: Statistics Finland register data 2010-2019. Table summarizes the tenants in the unregulated rental market in apartments of floor area between 15 and 100 m^2 , excluding students. Households receiving at least 100 euros of HA during the year are classified as HA recipients.

Our main use of the Statistics Finland register data is to describe changes in the supply of rental units. Panel (d) in Figure 5 gives an overview of the growth of the stock of all units. This includes all units in permanent use in multi-unit buildings at the end of each year, also containing units that are not on the private rental market or that are held by students. For the purposes of Panels (a)-(c) in Figure 5, we look at households living in multi-unit buildings in the private rental sector, excluding student households. Panel (a) only looks at new construction of private-market rental units. We define new construction of private-market rentals as units

which are built in year t and where a private-market tenant is residing at the end of year t . This means that we underestimate the level of new construction slightly, as some units which are completed during year t might not be held by tenants by the end of year t . However, we only focus on the composition changes in new construction instead of levels. To the extent that the units which are occupied at the end of the year are not different from the units which are unoccupied, this is not an issue for the analysis of the composition of new construction. Furthermore, Panel (b) only looks at units which are converted from units held by owner-occupiers in $t-1$ to units held by private rental market tenants in year t .

B Additional Results

In this appendix, we provide additional results supporting our main analyses. First, we describe the size of the HA program and the HA reform relative to the size of the overall rental market. Second, we explore whether the evolution of household characteristics is balanced across different types of units. Third, we define two alternative treatment definitions and show that our main results are robust to alternative treatment exposure definitions. These alternative treatment definitions both provide a robustness check for our main results and also will be used for the analysis of different mechanisms. Fourth, we analyze HA changes and rent separately in different-sized cities. Finally, we analyze the role of the social assistance system.

B.1 Size of the reform relative to the rental market

To understand the importance of the HA reform relative to the size of the rental market, we examine recipient shares and changes in HA payments among renters in the Statistics Finland population register data.

Figure B1 illustrates the share of HA recipients in floor area groups in the unregulated rental market in the population register. As the register data is annual, each observation depicts the end-of-year unit characteristics and any monetary quantities using the register data reflect year totals.¹⁶ Changes induced by the HA reform are fully reflected in the register data only at the end of 2016, as the program was gradually rolled out in the recipient population during 2015 (see Panel (a) of Figure 2). As the figure indicates, a larger share of renters in smaller housing units received HA prior to the reform. Furthermore, larger HA increases in smaller units expanded eligibility, leading to a higher share of HA recipients in these smaller, treated units after the reform. Both factors contribute to a strong treatment across the overall rental market, not just within the HA recipient population.

Figure B2 shows the average annual HA payments to households living in different sized housing units. These numbers include non-recipient households as zeros and therefore allow us to examine the size of the HA reform relative to the overall rental market. We see a clear effect of the reform on HA payments from 2014 to 2016. In the smallest floor area group, annual average HA payment almost doubled. This aggregates over two underlying changes: First, payments to existing HA recipients

¹⁶Some measurement error is therefore possible if, for example, an individual moved during the year and received HA in their former unit but not in the new unit.

increased, as indicated in Figure 2. Second, more households also became eligible in these units, as indicated in Figure B1. We conclude that the reform caused substantial changes in HA across unit floor area groups that are visible among overall renter population, not only HA recipients. This suggests that the reform provides strong identifying variation to study the rent effects of HA changes at the market level, including in particular the supply mechanisms, around the time of the reform.

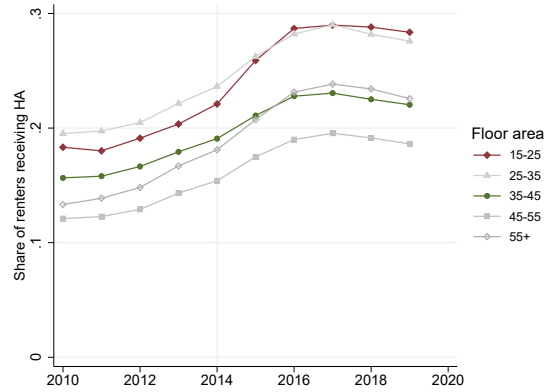


Figure B1: Recipient shares by floor area groups.

Notes: Share of households living in units in the unregulated rental market in 15-100 m^2 units at the end of the year that received some HA during the year. Receiving some HA refers to households receiving at least 100 euros or HA per year. Students are included as receiving zero HA.

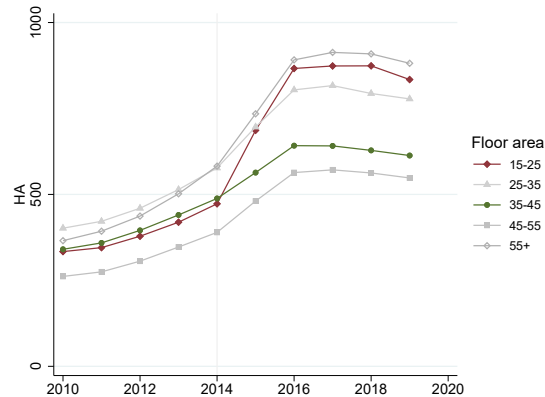


Figure B2: HA changes among the population of renters (not only recipients).

Notes: Average annual HA payment to households living in units in the unregulated rental market in 15-100 m^2 units. Non-recipients are included as receiving zero HA. Students are included as receiving zero HA.

B.2 Household characteristics

In this appendix section, we inspect the evolution of household characteristics that are used to determine HA levels. The aim is to assess whether there were changes in household sorting to treated and non-treated apartments after the 2015 reform.

First, Figure B3 describes the average household size in different floor area groups. The figure indicates that in our two-group DID estimation with 15–25m² and 35–45m² apartments majority of the recipients were single-member households, and the reform did not have any effect on this.

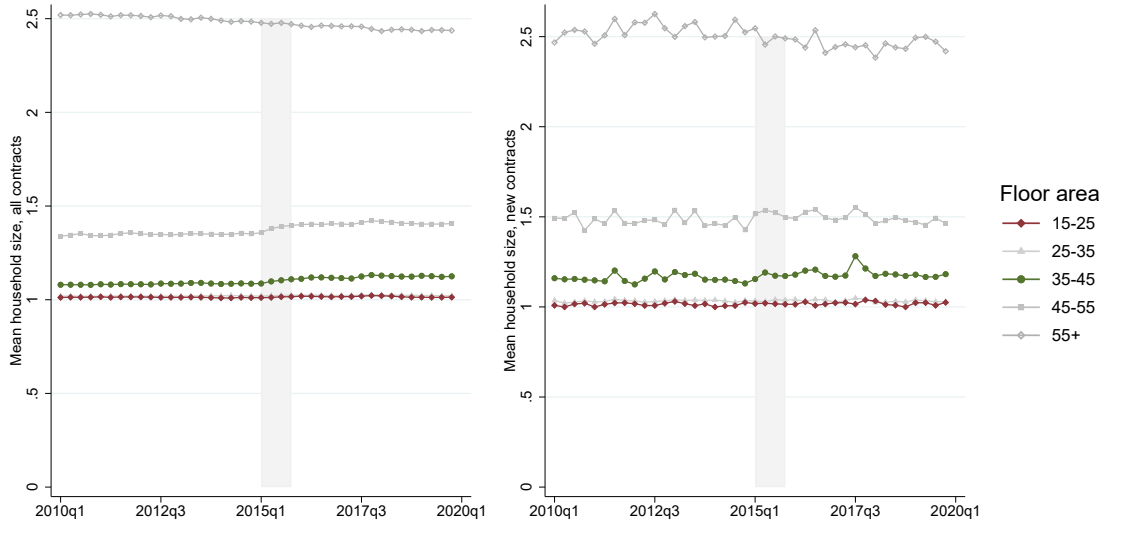


Figure B3: Mean household size in different floor area groups. All payments (left) and new rental contracts (right).

Notes: Mean household size for all payments in our estimation sample (left) and new rental contracts (right), aggregated to quarterly level. The light gray shaded area refers to year 2015. For details on identifying new rental contracts, see Appendix A.

Figure B4 describes the evolution of household incomes in different floor area groups. Our measure of household income excludes any HA payments or social assistance payments (for details on social assistance, see Appendix B.5). While average incomes increase over time, there are no significant changes in the time trends after the reform.



Figure B4: Mean household income in different floor area groups. All payments (left) and new rental contracts (right).

Notes: Mean monthly household income in all payments in our estimation sample (left) and new rental contracts (right), aggregated to quarterly level. The light gray shaded area refers to year 2015. For details on identifying new rental contracts, see Appendix A.

Together, Figure B3 and Figure B4 suggest that household composition in units of different sizes (household sorting into units) did not change significantly after the reform.

B.3 Rent effects using alternative treatment definitions

This appendix section verifies that our main HA and rent results are robust to different definitions of the treatment. In particular, the results hold also when we use the alternative treatment definitions that are used to analyze the demand and supply responses in Section 4.

Discrete treatment. We begin by verifying that our results hold when we do not compute a variable describing treatment exposure at all. We can do this by comparing HAs and rents in different floor area groups (as motivated by Figure 2). In this approach, we compare HAs and rents in very small housing units (15–25m²) to those in medium-sized units (35–45m²). We run the following event study style

regressions using the sample of new rental contracts:

$$y_{it} = \sum_{\substack{s=2010q1 \\ s \neq 2014q4}}^{2019q4} T_i \theta_s + \delta_q + \omega_z + T_i + u_{it} \quad (9)$$

where the outcome variable y_{it} is either HA or rent in rental contract i at time t . T_i denotes an indicator variable that takes value 1 for treated units (15–25m²) and value 0 for control units (35–45m²). The coefficients of interest in Equation (9) are treatment group times quarter fixed effects $T_i \theta_s$. The last quarter before the reform is the omitted category implying that the other coefficients measure the differences in the group difference relative to the pre-reform value. We include fixed effects at the quarter level (δ_q) and either zipcode or housing unit level (ω_z).

In Figure B5, we show the event study estimates from the discrete treatment design, comparing floor area groups 15–25m² (treated units) to 35–45m² (control units). Panel (a) indicates that HAs and rents developed in parallel before the reform. After the reform, there was a sizable increase in HA in the treatment group relative to the control group, amounting to approximately 70 euros of additional HA per month. Despite this increase, there were no changes in the relative rents between the two groups. Panel (b) shows the event study estimates with unit fixed effects. Confidence bands are now wider, but the patterns of average HAs and rents are very similar. After the reform, HAs increased substantially in 15–25m² (treated units) relative to 35–45m² (control units), but rents were unaffected.

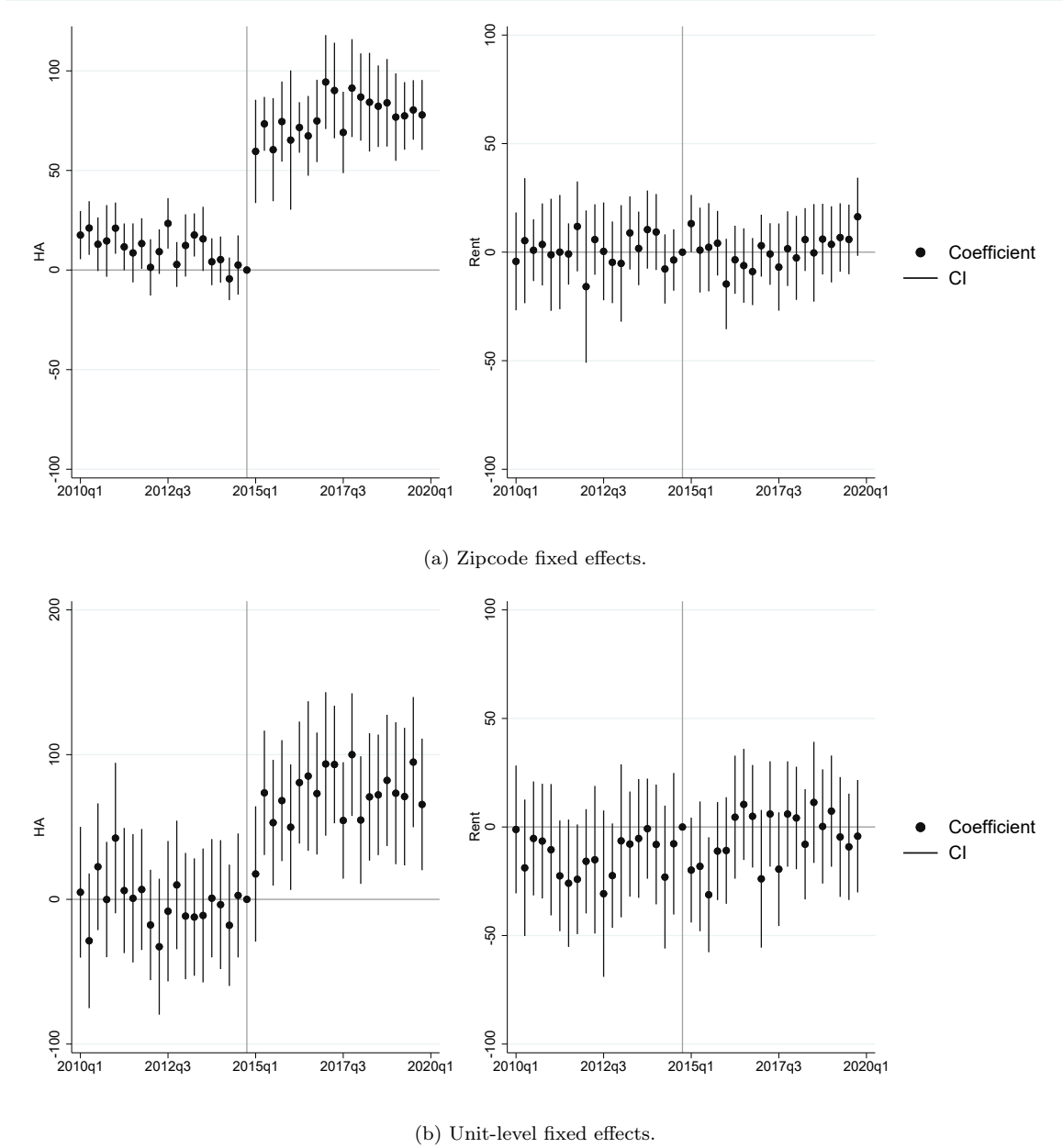


Figure B5: Comparison of monthly HAs and rents between discrete treatment (15–25m²) and control groups (35–45m²), new rental contracts.

Notes: The figure plots coefficients from an event study-regression, where the outcome (HA or rent) is regressed on a treatment group indicator, quarter fixed effects, zipcode *or* unit fixed effects and treatment \times quarter fixed effects, omitting the last quarter before the reform. Dots and whiskers illustrate the point estimate and the 95% confidence intervals of the treatment \times quarter coefficients. Standard errors are clustered at the municipality level in Panel (a) and at the unit level in Panel (b). N = 45,416 in Panel (a) and N = 11,045 in Panel (b).

Average treatment exposure by household size and unit characteristics.

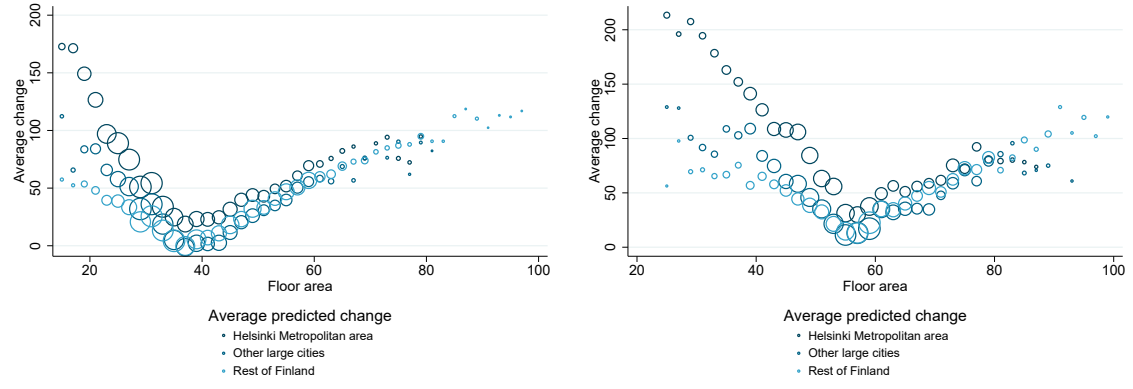
Next, we consider two alternative treatment definitions. First, we report results from a regression which uses as the treatment variable the average predicted HA changes by household and unit characteristics ($\overline{\Delta pred_HA_1}$). We use this treatment definition also in our mobility analysis in Section 4.1 and in Appendix B.4 where study the rent effects by city size. Second, we exclude household characteristics and only use unit characteristics in the assignment of treatment exposure to housing units ($\overline{\Delta pred_HA_2}$). This treatment definition is also used to analyze the evolution of the housing stock in Section 4.2.

In our unit fixed-effects estimation shown in Table 2, we computed for each observation the predicted HA change as described in Appendix A. We next use these predicted HA changes to calculate two alternative treatment definitions.

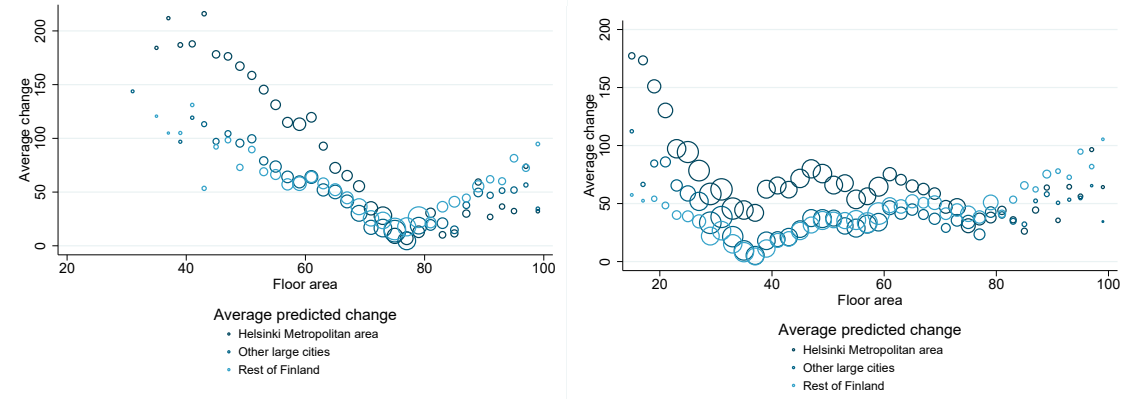
For treatment definition $\overline{\Delta pred_HA_1}$, we average the treatment exposure by household type and unit type. Household type refers to household size (1, 2, or 3 members) and unit type refers to groups by floor area (2m² brackets) and city size (3 groups). The groups are: 1) Helsinki Metropolitan Area (Helsinki, Espoo and Vantaa), 2) six other large cities (Tampere, Turku, Oulu, Jyväskylä, Kuopio and Lahti), and 3) rest of Finland which pools together the remaining municipalities. This gives us the predicted average HA change for example for a 2-member household living in a 40-m² unit in Helsinki MA. We will use this average predicted HA change as the treatment variable for any 2-member household with a new rental contract in a 40-m² unit in Helsinki MA.

For treatment definition $\overline{\Delta pred_HA_2}$, we average the predicted HA changes only across unit types: floor area (2m² brackets) and city size (3 groups). This gives us the predicted average HA change for example for a 40-m² housing unit in the Helsinki MA to be used as the treatment variable for a new rental contract in a 40-m² housing unit in Helsinki MA. In calculating the averages, we only use households with at most 3 members.

The resulting average predicted HA changes in different types of units are illustrated in Figure B6 where Panels (a)–(c) relate to treatment definition $\overline{\Delta pred_HA_1}$ for different-sized households and Panel (d) to definition $\overline{\Delta pred_HA_2}$.



(a) Average predicted HA change ($\overline{\Delta pred_HA_1}$) for single-member households. (b) Average predicted HA change ($\overline{\Delta pred_HA_1}$) for 2-member households.



(c) Average predicted HA change ($\overline{\Delta pred_HA_1}$) for 3-member households. (d) Average predicted HA change ($\overline{\Delta pred_HA_2}$).

Figure B6: Different measures of treatment exposure.

Notes: Panels (a)-(c) summarize $\overline{\Delta pred_HA_1}$ and Panel (d) summarizes $\overline{\Delta pred_HA_2}$. Different treatment exposures correspond to predicted HA change in the sample of new contracts observed in 2010–2014, averaged for different types of units and households. $\overline{\Delta pred_HA_1}$ averages over household size, unit floor area and city size group. $\overline{\Delta pred_HA_2}$ averages over unit floor area and city size group. Cells with fewer than 6 observations are excluded.

We then run the following event study regression in the sample of all new rental contracts 2010–2019 using average predicted changes based on either of the two treatment measures ($\overline{\Delta pred_HA_1}$ or $\overline{\Delta pred_HA_2}$):

$$y_{it} = \sum_{\substack{s=2010q1 \\ s \neq 2014q4}}^{2019q4} \theta_s \overline{\Delta pred_HA}_{kc} + \omega_c + \gamma_z + \delta_{tr} + \epsilon_{it}, \quad (10)$$

where the outcome is either HA or rent of the unit-household pair i . Subscript k in the treatment variable refers to the treatment definition (alternative treatment 1 or

alternative treatment 2) and c to the cell. ω_c refers to a cell-level fixed effect at the level of the treatment definition (household and unit type if using treatment 1 and unit type if treatment 2), γ_z to a zip-code fixed effect and δ_{tr} refers to city size time effects. Thus we compare more and less treated cells within these broad city size groups and allow the time trends in rents and allowances to differ by city size.

Figure B7 and Figure B8 report the event study-graphs that correspond to estimation Equation (10) with $\overline{\Delta pred_HA_1}$ and $\overline{\Delta pred_HA_2}$ respectively. In Figure B7 a one-euro increase in predicted HA maps almost one-for-one to the actual HA increase. This is very cleanly estimated with very narrow confidence intervals suggesting that also this treatment definition is strongly associated with actual variation in HA. For rents, we do not detect any changes between units with smaller and larger predicted HA changes. Moreover, this sample is substantially larger than our unit fixed-effects sample in Table 2 (we now use information on N=200,890 new rental contracts instead of N=22,346 in the fixed-effects specification), improving the overall validity of our findings.

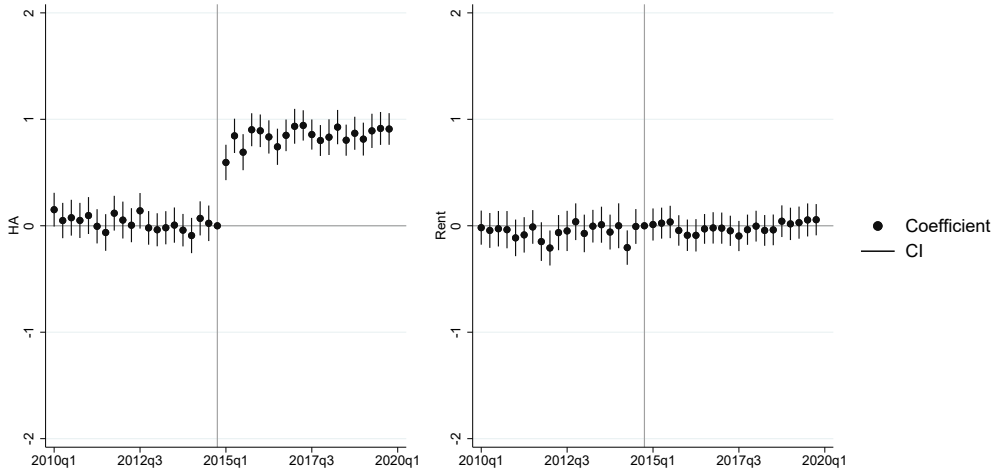


Figure B7: Average HA and rent regressions using treatments averaged by household size and unit characteristics.

Notes: The figure plots coefficients from an event study-regression, where the outcome (HA or rent) is regressed on cell fixed effects (where cell refers to unit and household type), quarter \times city size group fixed effects, zipcode fixed effects and treatment \times quarter fixed effects, omitting the last quarter before the reform and using treatment definition $\overline{\Delta pred_HA_1}$. Dots and whiskers illustrate the point estimate and the 95% confidence intervals of the treatment \times quarter coefficients. Standard errors are clustered at the zipcode level. We only include households with up to 3 members. N=200,890.

Results shown in Figure B8 are again consistent with our main results, although the first-stage results are less strong. A one-euro increase in predicted HA maps to approximately 0.6-0.7 euro increase in actual HA. For rents, again, we do not detect any changes between units with smaller and larger predicted changes.

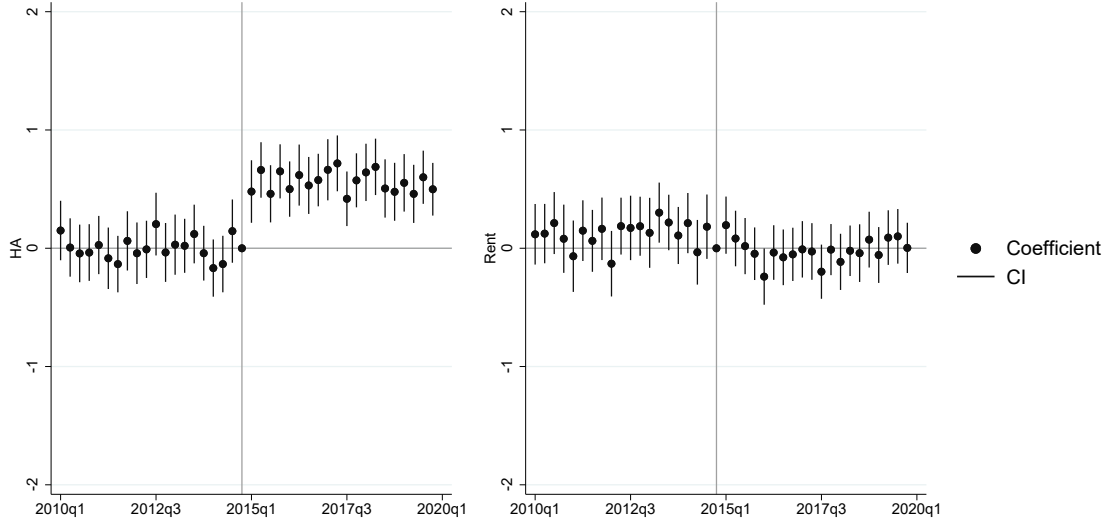


Figure B8: Average HA and rent regressions using treatments averaged by unit characteristics.

Notes: The figure plots coefficients from an event study-regression, where the outcome (HA or rent) is regressed on cell fixed effects (where cell refers to unit and household type), quarter \times city size group fixed effects, zipcode fixed effects and treatment \times quarter fixed effects, omitting the last quarter before the reform and using treatment definition $\overline{\Delta pred HA}_2$. Dots and whiskers illustrate the point estimate and the 95% confidence intervals of the treatment \times quarter coefficients. Standard errors are clustered at the zipcode level. We only include households with up to 3 members. N=201,531.

B.4 Rent effects by city size

This appendix section studies the heterogeneity and robustness of our results across different-sized cities.

Our main specification in Table 2 compares units with bigger treatments to units with smaller treatments. However, as Table B6 shows, the treatment was on average larger in large cities and smaller in small towns and rural areas. Therefore, part of the identifying variation comes from comparing changes in big cities to changes in less urban areas. To test whether results are robust to comparing more treated units to less treated units only within similar municipalities, we repeat the same analysis but include time effects at the city size group level in Table B1. (Our sample size in the unit fixed effects specification is not large enough to precisely estimate this specification separately by location.) For comparison, columns 1-3 report the results of Table 2. We note that the first stage still remains strong. The point estimates are again precisely estimated zeros.

Table B1: Continuous-treatment DID-IV estimates, with housing unit fixed effects.

	DID		IV	DID		IV
	(1)	(2)	(3)	(4)	(5)	(6)
	Allowance	Rent	Rent	Allowance	Rent	Rent
Predicted HA change	0.899 (0.0339)	0.0242 (0.0161)		0.821 (0.0353)	-0.0303 (0.0168)	
Allowance			0.0270 (0.0177)			-0.0369 (0.0209)
Month FEs	✓	✓	✓			
Quarter \times City Size FEs				✓	✓	✓
Unit FEs	✓	✓	✓	✓	✓	✓
Outcome mean	314.1	577.3	577.3	314.1	577.3	577.3
N	22346	22346	22346	22346	22346	22346
SE clustered by	Unit	Unit	Unit	Unit	Unit	Unit
First-stage F			704.9			540.1

Notes: The table reports results from DID and IV regressions where the treatment or instrument is defined to be a predicted change in HAs as described in Section 2.3. Columns 1-3 repeat our main results from Table 2. Columns 1 and 2 report coefficients from a regression of the outcome on our measure of predicted HA change \times post indicator. Column 3 reports the second-stage of an IV regression, where HAs are instrumented for by a predicted HA change \times post indicator. The first stage of this regression corresponds to column 1. Specifications in columns (1)-(3) contain month-by-year and housing unit fixed effects. Specifications in columns 4-6 repeat the analysis with quarter \times city size group fixed effects. Standard errors are clustered at the housing unit level.

Figure B9 replicates separately for each city size group the descriptive analysis of Figure 2, focusing only on new rental contracts. As expected based on Figure B6, the left-hand graph shows that the reform increased mean HA in small apartments especially in Helsinki metropolitan area (Panel (a)) and in other large cities (Panel (b)). The right-hand graph in turn shows again no visible effect on rents in any city size group.

Figure B10 repeats the event study style estimation shown in Figure B7, but splitting the sample by city size. In order to have a large enough sample size when splitting the data, we use our alternative treatment definition that averages the treatment exposure by household type and unit characteristics ($\overline{\Delta pred HA_1}$). With this treatment definition, we are able to include housing units that we observe only once to the estimation sample. The specification corresponds to Equation (10) with the

exception that quarter times city size group fixed effects are replaced by time fixed effects only, since the estimation is done separately for each city size group. Again, we confirm a strong first stage and no rent effects in all three city size groups. Moreover, more and less treated units have very similar parallel trends prior to the reform across all regions.



Figure B9: Mean HAs and rents by floor area and city size group, new contracts.

Notes: The figure shows mean monthly HA paid to recipient households and mean monthly rents paid by recipients in new contracts by floor area and city size group, aggregated to quarterly level. Panel (a) shows Helsinki Metropolitan area, Panel (b) shows six other large cities and Panel (c) the rest of Finland. The light gray shaded area refers to year 2015. For details on sample selection and identifying new rental contracts, see Appendix A.

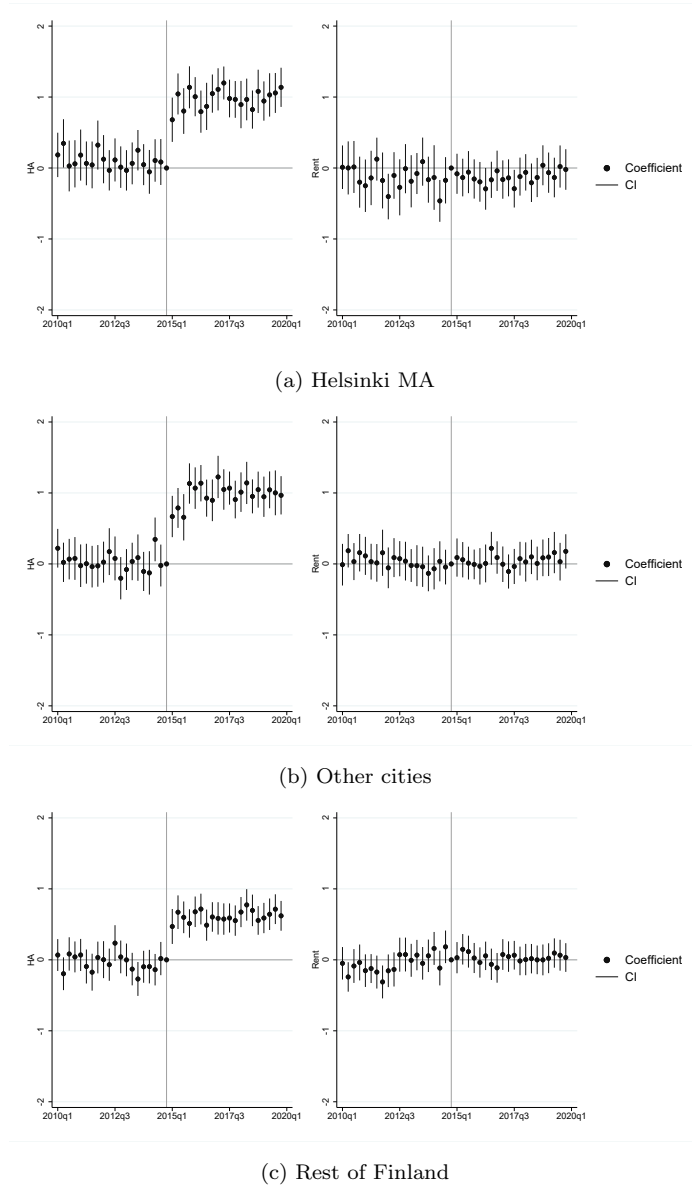


Figure B10: Comparison of monthly HAs and rents by city size group, new contracts.

Notes: The figure plots coefficients from an event study-regression that corresponds to Figure B7 but is split by city size group. The figure plots coefficients from an event study-regression, where the outcome (HA or rent) is regressed on cell fixed effects (where cell refers to unit and household type), zipcode fixed effects, quarter fixed effects and treatment \times quarter fixed effects, omitting the last quarter before the reform and using treatment definition $\overline{\Delta pred_HA}_1$. Dots and whiskers illustrate the point estimate and the 95% confidence intervals of the treatment \times quarter coefficients. Standard errors are clustered at the zipcode level. We only include households with up to 3 members. Panel (a) shows Helsinki Metropolitan area (N=36,646), Panel (b) shows six other large cities (N=58,552) and Panel (c) the rest of Finland (N=105,395).

B.5 Social assistance and rents

In this appendix section, we assess the robustness of our baseline results to potentially mitigating changes in social assistance (SA). Social assistance, or income support, is the last-resort form of financial assistance in the Finnish social security system. Households with very low incomes can receive social assistance if their income, including HA, is considered insufficient for covering their basic needs. As a result, for households eligible for SA, an increase in HA can be partly or entirely offset by a reduction in social assistance. Therefore, the rent effects of HA changes might be muted for this group.

The SA program consists of three parts: basic, supplementary, and preventive social assistance. The two latter programs are small relative to basic social assistance which we focus on. Basic social assistance can reimburse housing costs in full. It is calculated by adding together all household income, including HA, and subtracting housing costs as well as a "base amount" meant to cover necessary non-housing living costs.¹⁷ Basic assistance is only granted for 1–2 months at a time as opposed to HA which is usually granted for 12 months.

HA and SA in the population register data. The HA register data do not contain information on SA. However, using data from the full population register from Statistics Finland, we observe both the yearly HA payments and yearly SA payments. We can therefore verify that the total subsidies paid to households did actually change, and that the increases in HA payments were not fully offset by changes in social assistance.

In Figure B11, we compare housing allowance and social assistance payments across different floor area groups before and after the HA reform. The reported numbers are for households in multi-unit housing in unregulated rental market, and they include non-recipients as zeros (thus not conditioning on being HA recipient), but exclude students completely. The annual general HA payment to households in 15–25m² units increased by 416 euros more than for 35–45m² units from 2014 to 2016. Because this number is an average in the overall rental population, it is affected not

¹⁷There is a municipality-specific limit on the housing costs that can be covered. The limits are higher than the limits in HA system and less strictly enforced. If housing costs exceed the limit, the recipient may be instructed to seek more affordable housing. However, Kela may also reimburse the housing costs in full, if, for instance, affordable housing is not available. The declared housing costs are accepted in full in roughly 70% of the cases where housing costs exceed the municipality-specific limit.

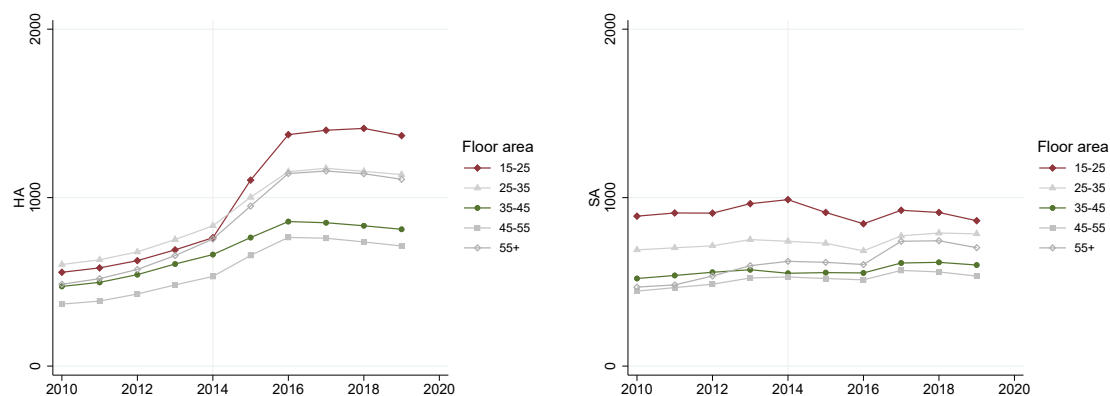


Figure B11: Housing allowances (left) and social assistance (right) in floor area groups on the unregulated rental market, including non-recipients as zeros.

Notes: Average HA and SA payments to households in the unregulated rental market. The graph includes all renters, including non-recipients as zeros, with the exception of students who are excluded from the analysis. Since the HA reform was rolled out during 2015 and the numbers summarize annual totals, the effect of the reform is fully visible only by the end of 2016. The SA measure does not contain supplementary SA but this part of the program is small relative to the other parts which are observed. There were some reporting changes in the social assistance payments in 2017 which could potentially explain the level increases in SA payments in 2017.

only by subsidies paid to a single household but also by changes in the number of recipients, as illustrated in Figure B1. The figure shows that the variation in HA is also substantial at the level of the overall rental market including non-recipients.

When we compare annual average SA payments in different floor area groups, we see that the increased HA in small units was indeed to some extent offset by lower SA payments. Basic social assistance decreased by 145 euros per year in 15–25m² units relative to 35–45m² units from 2014 to 2016. Together with the increase in HA payments, this implies that the *net* change in total subsidies (HA + SA) paid to households increased by 271 euros annually (including non-recipients) in 15–25m² units relative to 35–45m² units. That is, the total subsidy payments clearly increased in 15–25m² units and the differential HA changes were only moderately offset by social assistance changes.

Overall, we confirm that while the SA payments did decline as a response to HA increases, the changes in social assistance offset the effect of the HA only moderately. However, for those households who are eligible to SA, the SA program could have offset a large share of the HA changes. Therefore, we next verify that our rent effects are not sensitive to this.

Rent effects by recipient income. Since HA register data do not include information on social assistance, we test the robustness of our results with respect to social assistance status by splitting the sample to low- and high-income HA recipients, as the HA recipients with relatively higher incomes are unlikely to be eligible for social assistance. We divide households in our main estimation sample to two groups depending on whether household income per consumption units (excluding HA) is below or above the median in our sample. We exclude households with exactly the median income: In most years, the median household income appears to correspond to monthly basic unemployment allowance. Across the years, 41% of HA recipients have incomes strictly below median and 37% strictly above.¹⁸

Figure B12 shows the evolution of mean HAs and rents in different floor area groups, with Panel (a) describing HA recipients below and Panel (b) above median income. Although the levels of mean HA received in the different groups are different, the reform resulted in very similar variation in HA by floor area group. HA increased substantially in 15–25m² units and did not increase by much in 35–45m² units (with the exception of below median income households whose mean HA increased slightly also in 35–45m² units). The right-side graph shows that there are no visible differences in mean rents of those above and below median income.

We then run an analysis which corresponds to Figure B7 but where we split the sample by household income and use the treatment definition $\overline{\Delta pred HA}_1$. The resulting coefficients are reported in Figure B13. We again observe that average HA increased in both groups and that there are no meaningful rent effects in either group. Overall, even in the subset of households who are very unlikely to receive social assistance, rent effects are very similar to our main estimates.

Finally, we note that the moving patterns we observe in Figure 4 are driven by households with incomes above sample median, consistent with the social assistance program mitigating the incentives generated by the reform for the eligible households.

¹⁸We only look at households with at most 3 members. We construct consumption equivalence units as follows: adults in the household count for one and children under the age of 18 for 0.5 consumption units. We calculate median incomes in the sample annually.



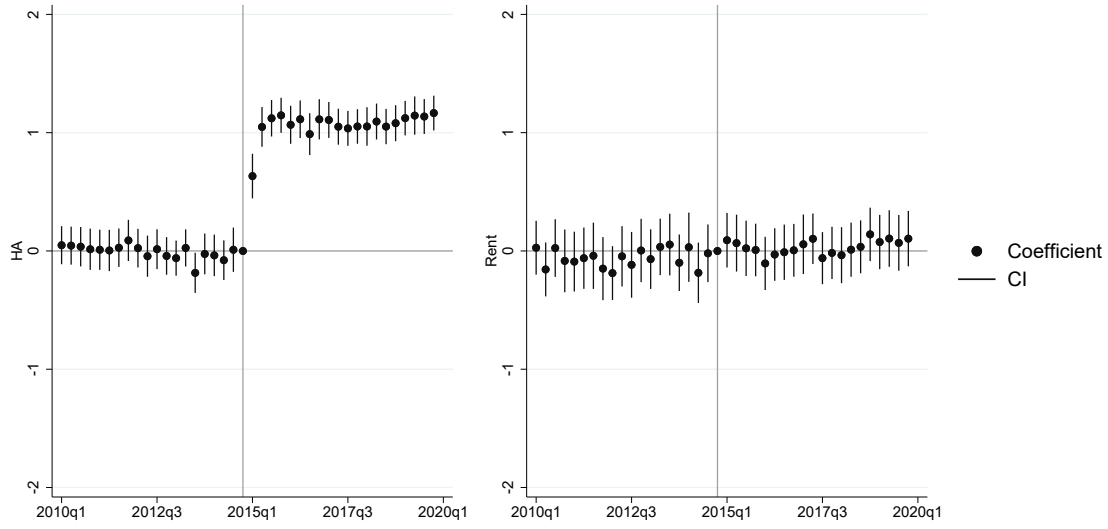
(a) New contracts of households with below-median incomes.



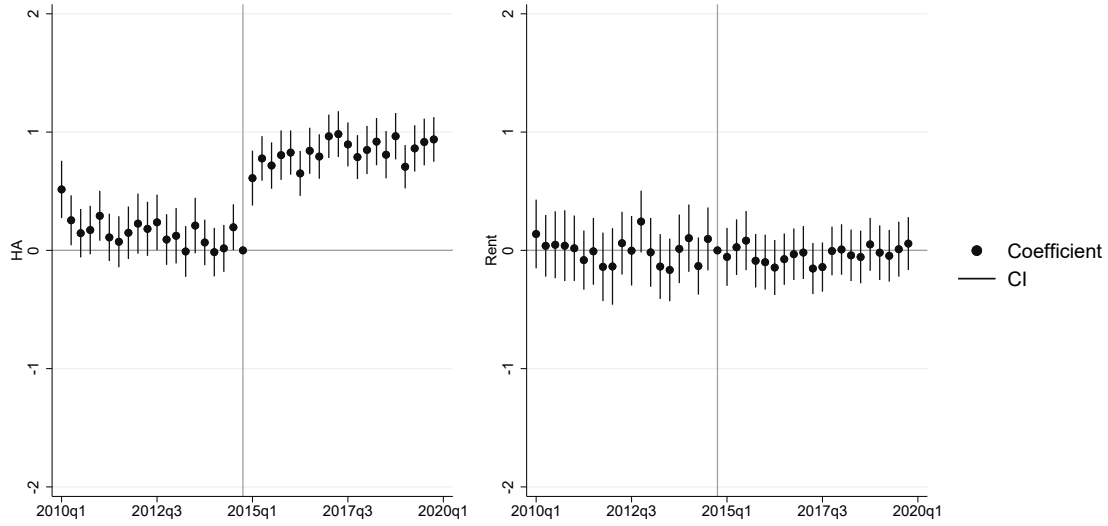
(b) New contracts of households with above-median incomes.

Figure B12: Mean HAs and rents by household income.

Notes: The figure shows mean monthly HA paid to recipient households and mean monthly rents paid by recipients in our estimation sample, aggregated to quarterly level. The light gray shaded area refers to year 2015. Panel (a) illustrates 1-3-member households with strictly below-median incomes. Panel (a) illustrates 1-3-member households with strictly above-median incomes.



(a) Households with below-median incomes



(b) Households with above-median incomes

Figure B13: Comparison of monthly HAs and rents by household income.

Notes: The figure plots coefficients from an event study-regression that corresponds to Figure B7 but is split by recipient income. Panel (a) illustrates 1-3-member households with strictly below-median incomes. Panel (b) illustrates 1-3-member households with strictly above-median incomes. The figure plots coefficients from an event study-regression, where the outcome (HA or rent) is regressed on cell fixed effects (where cell refers to unit and household type), quarter \times city size group fixed effects, zipcode fixed effects and treatment \times quarter fixed effects, omitting the last quarter before the reform and using treatment definition $\overline{\Delta pred_HA_1}$. Standard errors are clustered at the zipcode level. N=83,445 in Panel (a) and N=75,065 in Panel (b).

C Derivations for the Conceptual Framework

Consider a competitive rental market where $D(r)$ is aggregate rental demand and $S(r)$ aggregate rental supply as a function of the quality-adjusted rent r . Rental demand aggregates over demand by HA recipients (R) and non-recipients (N)

$$D(r) = D^R(r) + D^N(r).$$

Supply of rental housing $S(r)$ aggregates over three margins: the construction of new units (C), the (net) conversion of existing owner-occupied units to rental units (I), and the stock of existing rental units (O) net of depreciation (δ)

$$S(r) = S^C(r) + S^I(r) + S^O(1 - \delta).$$

Initially $D(r) = S(r) = q$ where q denotes the quantity consumed in the initial equilibrium.

Consider then a small change in the housing allowance, ds . This change affects the rent paid by HA recipients, $r + dr - ds$, rents paid by non-recipients, $r + dr$, as well as rent payments received by landlords, $r + dr$, where dr denotes the change in the equilibrium rent.

Taking into account changes in quantity demanded and supplied gives

$$D^R(r + dr - ds) + D^N(r + dr) = S^C(r + dr) + S^I(r + dr) + S^O(1 - \delta).$$

Rewriting and taking into account $D(r) = S(r)$ gives

$$D^{R'}(r) \cdot (dr - ds) + D^{N'}(r) \cdot dr = S^{C'}(r) \cdot dr + S^{I'}(r) \cdot dr$$

$$\frac{dr}{ds} = - \frac{D^{R'}(r)}{[S^{C'}(r) + S^{I'}(r)] - [D^{R'}(r) + D^{N'}(r)]}$$

Now, expressing terms on the RHS as $D^{R'}(r) = \frac{dD^R}{dr}$, and so on gives:

$$\frac{dr}{ds} = - \frac{\frac{dD^R}{dr} \frac{r}{q}}{[\frac{dS^C}{dr} \frac{r}{q} + \frac{dS^I}{dr} \frac{r}{q}] - [\frac{dD^R}{dr} \frac{r}{q} + \frac{dD^N}{dr} \frac{r}{q}]}$$

Finally, labeling the elasticities along the different margins, for example, $\epsilon_D^R = \frac{dD^R}{dr} \frac{r}{D^R}$, gives:

$$\frac{dr}{ds} = - \frac{\frac{D^R}{q} \epsilon_D^R}{[\frac{S^C}{q} \epsilon_S^C + \frac{S^I}{q} \epsilon_S^I] - [\frac{D^R}{q} \epsilon_D^R + \frac{D^N}{q} \epsilon_D^N]}$$

where q denotes the initial equilibrium quantity.

This highlights the relative importance of the different margins. First, even if new rental supply was very elastic, either due to construction (high ϵ_S^C) or conversions (high ϵ_S^I), the effect on the overall stock would depend on their initial importance $\frac{S^I}{q}$ and $\frac{S^C}{q}$.

This observation also highlights the difference between the effective short-run and long-run supply elasticities. While new construction is almost perfectly inelastic in the short run, conversion of units from owner-occupied units to rentals by investor-landlords ($\epsilon_S^I > 0$) could potentially be important also in the short run.

Second, the rent effect are strictly bounded from above as long as the demand of non-recipients*is not perfectly inelastic. For instance, with completely inelastic supply, we have

$$\frac{dr}{ds} = \frac{\epsilon_D^R}{\epsilon_D^R + \frac{D^N}{D^R} \epsilon_D^N}$$

Thus, for example, in the case where the price elasticities of demand of recipients and non-recipients are approximately similar, the rent effects are bounded from above at $\frac{dr}{ds} \approx -\frac{D^R}{q}$, even in the extreme case where supply is fully inelastic.