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The Incidence of Housing Allowances: Quasi-Experimental Evidence *

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Abstract

This paper studies the effects of housing allowances on rents. Our research design is based on a reform that made the allowance more generous for small housing units as a quasi-experimental setting. We find that large increases in housing allowances for small housing units have little or no effect on their rents relative to larger units. Thus, the incidence of the reform is largely on allowance recipients and not on their landlords. Consistent with very moderate rent effects, we do not find evidence of recipient households responding to the increased incentive to choose small units. A possible explanation is that optimization frictions and short expected allowance spell duration limited demand responses to the reform.

Key words: Rental housing, demand subsidies, housing allowance, rent, incidence, housing demand
JEL codes: H22, R28

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1 Introduction

Housing affordability, especially for low-income households, is a major concern in many cities throughout the world. According to the OECD, in the past 20 years there has been a policy shift from social housing and rent control policies toward housing allowances (or benefits) to tenants.[1] However, the effectiveness of housing allowances crucially depends on their incidence. That is, does the allowance benefit the tenants as intended or is it passed through to landlords in the form of higher rents?

This paper studies the effects of housing allowances (HA) on rents using Finnish register data on the universe of HA recipients and their housing units. Our research design is based on a major reform of the housing allowance system in 2015 that resulted in large variation in the way HA changed for different types of units. In particular, a ceiling on HA based on the monthly rent per square meter of the unit was replaced by a ceiling on the total monthly rent. This led to large increases in HA payments for households living in small units relative to larger units. We analyze how these changes affected the relative rents of different types of units, first by comparing rents in small (15-25m^2) and mid-sized (35-45m^2) units within geographic housing market areas using a difference-in-differences design (DID), and second, by using a continuous-treatment strategy which exploits the full variation in HA. We also analyze the mechanisms behind the rent effects by studying how HA recipients responded to the increased incentive to choose smaller units and how housing supply adjusted.

Starting with the two-group comparison, we find that the reform increased the monthly HA by some 70 euros in the treatment group compared to the control group, which is a 30 % increase relative to the treatment group pre-reform mean. Despite this sizable differential change in the HA, the difference in rents between small and mid-sized units remained nearly unaffected. This result persists when we control for unobserved housing quality through housing unit fixed effects. We also construct a continuous measure of treatment intensity by calculating the change in HA for a given housing unit caused by changes in policy parameters, holding constant the characteristics of the rental contract and the recipient. The results from our continuous treatment analysis are in line with the discrete treatment estimates. To quantify these findings in terms of a euro-on-euro effect of HA on rent, we re-interpret our DID

[1] In Finland, annual housing allowance outlays are 2.2 bn euros or 0.8% of GDP in 2020, whereas the OECD-25 country average is 0.3% of GDP. For more details, see the OECD Affordable Housing Database: https://www.oecd.org/housing/data/affordable-housing-database/.

estimates through instrumental variables regressions of rents on HA, instrumenting for HA with the variation generated by the reform. The point estimate for the effect of a one euro increase in HA on rent is roughly 0.01 euros in our preferred specification. The standard error is approximately 0.026, suggesting that we can rule out even moderate effects of the reform on rents with a high degree of confidence.

As we find that housing allowances increased in small units while their rents did not increase, the housing costs paid by HA recipients living in small units decreased substantially (by the order of magnitude of 15 percent). However, despite the increased incentive to choose small units, we find that HA recipients did not adjust their housing consumption after the reform. They are neither less likely to move out of small units nor are they more likely to move into one. A likely explanation is that optimization frictions, such as moving costs, are large in the housing market, at least relative to the financial incentives generated by the reform. In particular, a factor which is likely to contribute to the financial incentives to choose small units is the duration of the HA spells as they are typically shorter than tenancy spells.

Previous empirical research is inconclusive about the extent to which HAs are passed on to rents in housing benefit systems in Europe and similar systems in New Zealand and Israel. Some studies find that more than 50% of the HA accrued to landlords (Gibbons and Manning, 2006; Fack, 2006; Kangasharju, 2010; Viren, 2013)[2]. More recent studies have found more moderate but still economically significant rent effects (Hyslop and Rea, 2019; Sayag and Zussman, 2020) or very limited or zero rent effects (Brewer et al., 2019). In the Finnish context, the rent effects of HA have been studied recently by Eerola and Lyytikäinen (2021) who utilize discontinuities in HA at certain floor area and construction year cut-offs in the HA system that existed before 2015. While their main estimates are close to zero and statistically insignificant, the standard error (0.15) is not negligible, and economically significant rent effects cannot be ruled out with great confidence.[3]

In contrast to the European HA systems where the housing benefit is a universal entitlement, the US housing voucher system has a fixed budget, and only a minority of eligible households receives a voucher. There is an ongoing discussion on making vouchers a federal entitlement, highlighting the importance of empirical analyses of

[2]Eerola and Lyytikäinen (2021) report a failed attempt to replicate the findings of Kangasharju (2010).

[3]Assuming statistical power of 0.8 and p-value 0.05, the minimum detectable effect size is 0.42. In addition, the discontinuities used are not very large and not necessarily very salient for the tenants and landlords.

entitlement-style programs.[4] Another difference between European HA programs and the US housing voucher program is that in the US, households need to move to higher-quality housing in order to benefit from the voucher, whereas in Europe, the recipient status can change more flexibly for example in response to income shocks. Susin (2002), Eriksen and Ross (2015) and Collinson and Ganong (2018) analyze the incidence of the US housing voucher system.

We contribute to the existing literature on the rent effects of HA policies by utilizing a reform that caused large, salient and credibly exogenous variation in HA for different types of housing units in an entitlement-style housing subsidy program. Our large and detailed data on recipients and their housing units allows us to address endogeneity concerns like unobserved housing quality, and the key estimates are very precise relative to the standard of the literature.

Importantly, we provide one of the first analyses of demand and supply responses following the reform. The large variation in previous estimates of HA pass-through to rents suggests that the pass-through depends on properties of the HA program and the housing market, as well as on the type of variation used to identify the pass-through. Understanding the mechanisms via which housing allowances might increase rents is therefore essential in interpreting the results and in designing HA programs.

The key mechanism through which a change in HA can affect rents is by increasing housing demand by HA recipients. Yet, quasi-experimental evidence on the responsiveness of HA recipients to the incentives to choose certain kind of housing is almost non-existent. Gibbons et al. (2020) analyze a UK reform that reduced housing benefits for social housing tenants deemed to have a spare bedroom. The aim was to promote mobility and reallocation of the social housing stock. The policy was not successful in encouraging mobility, but those who moved became more likely to downsize. Similar to them, we also study the mobility and housing choices of recipient households, but we focus on households in private-market rentals instead of the social housing sector.

2 Data

Our main data source is the register of housing allowances from The Social Insurance Institute of Finland (Kela) for the years 2010–2019. The data cover the universe of

[4]https://www.bloomberg.com/news/articles/2021-10-07/biden-pledge-to-boost-section-8-housing-faces-congress

monthly recipient-level HA payments and include information on the characteristics of the recipient households and their housing units.

Our main focus is on private rental market and new rental contracts which are not subject to any controls and provide a credible benchmark for studying the rent effects of the reform. We do not directly observe new contracts in our data, but we define the contract as new if the recipient received HA in another address at most 4 months ago. We also report separately results for a sub-sample of repeat observations of the same housing unit under different tenant contracts. This sub-sample allows us to include unit-level fixed effects to control for unit-specific time-invariant unobservables.[5]

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

Table 1 provides summary statistics on our sample. Most HA recipients in private rental market are single-member households, especially in our sample of small housing units. For an overwhelming majority, the rent ceilings imposed by the HA program are binding, although, as expected, there is a clear drop in the treatment group after the reform.

In addition to the data from Kela, we use register data provided by Statistics Finland. First, we use the population-wide register data to relate the recipient households to the overall private rental market. The population-wide data contain less detailed information on HA and do not include information on rent, but include detailed information on the individuals and their housing units, and are therefore useful in studying the potential mechanisms behind our rent results. Second, we use data on the universe of residential buildings and housing units to describe the evolution of housing supply.

[5]We describe sample selection in more detail in Appendix A.

Table 1: Housing allowance recipient characteristics, private rental market

	All payments	New contracts	FE sample
	mean	mean	mean
Household size	1.1	1.2	1.2
Share single-member households	0.89	0.82	0.82
Apartment surface	36.7	38.1	38.3
Household income	740.5	757.5	728.0
Rent	508.0	544.2	529.7
Housing allowance received	276.6	288.7	288.4
Share rent ceiling binding	0.83	0.87	0.88
Observations	7337300	125590	30548

Notes: HA register 1/2010-12/2019, all monetary values in 2020 euros. Throughout, the sample is restricted to private rental market residents in apartments of floor area between 15 and 55 m^2. 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 third column summarizes the set of recipients living in units such that we observe the same unit with new rental contracts of at least two different recipients.

3 Institutional setting and research design

3.1 Housing allowance system and the 2015 reform

Roughly one third of Finnish households live in rental housing. The rental housing market consists of a private unregulated segment (70%) and a social housing segment (30%). We concentrate on the private rental market where rents are market-driven and can therefore be expected to respond to changes in housing allowances. The social housing sector, where rents are cost-based and should therefore not be affected by housing allowances, is separately analyzed in Appendix B.

Rents in new rental contracts in the private rental market are unregulated, but in existing rental contracts rent increases are typically tied to some publicly available index, such as the official cost of living index. Roughly 20% of renters move annually, and thus, we expect to see any rent effects of HA changes first in new contracts and in the whole rental stock with a delay.

The 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 fo-

cus on the general HA intended for working age households. In 2020, total outlays amounted to 1.57 bn euros (0.66% of GDP) and roughly 400,000 households (15% of all households) received the general housing allowance. When the reform was implemented in 2015, roughly 30% of tenants in the private rental market received HA. In addition to HA, housing costs of low-income households are also covered by the social assistance program. We discuss this program in more detail after we present our main results.

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

$$HA = 0.8[\min(Rent/m^2, MaxRent\_m^2) \cdot \min(FloorArea, Max\_m^2) - deductible], \tag{1}$$

where $MaxRent\_m^2$ denotes the ceiling on the monthly rent per square meter. This ceiling varied depending on construction year, heating system and location of the building. In practice, the ceiling on rent per square meter was binding for a large majority of recipient households (see Eerola and Lyytikäinen, 2021). Max_m^2 denotes the ceiling on the size of the unit which varied depending on household size.[6]

The 2015 reform simplified the formula and removed the dependence of HA on unit characteristics. The ceilings on rent per square meter and floor area were replaced with a single ceiling on total rent. Since January 2015, the HA is determined as

$$HA = 0.8[\min(Rent, MaxRent) - deductible], \tag{2}$$

where $MaxRent$ denotes a ceiling on the total monthly rent, $Rent$. In addition, municipalities are divided into four affordability groups depending on the local rent level. Both $MaxRent\_m^2$ and $MaxRent$ vary by affordability group.[7]

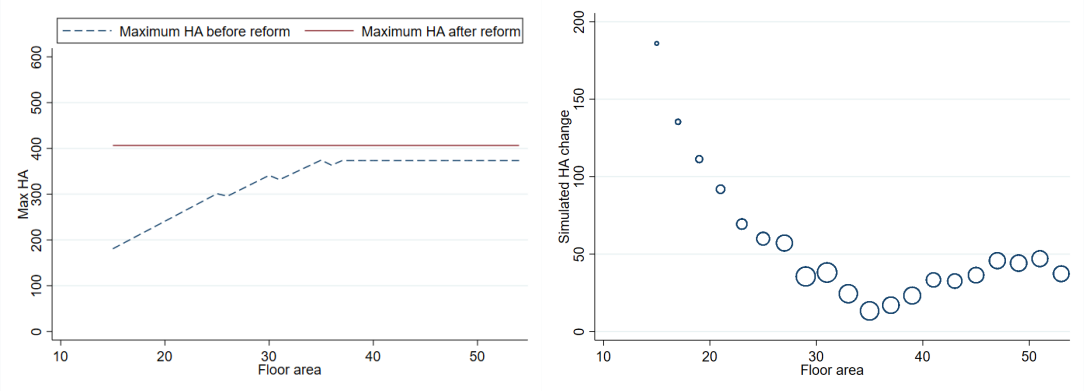
The HA program compensates 80% of what are considered acceptable housing costs for a household with no or very low income ($deductible = 0$). The deductible is zero up to a threshold income level which increases with household size. Above the threshold, each additional euro of income reduces monthly HA by some 0.4 euros. The reform also simplified the formula for calculating the deductible, but this had little practical relevance for actual HA levels. Furthermore, in September 2015, an earnings deduction of 300 euros/month was introduced. This made the HA more

[6]The ceiling on floor area was $37m^2$ for singles and $57m^2$ for two-person households.

[7]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 finally, all other municipalities consisting of small towns and rural municipalities.

generous for existing recipients with labor earnings and also enlarged the pool of eligible households. We do not exploit this variation in our analysis.

The left panel of Figure 1 illustrates the maximum HA before and after the reform for a single-member household in Helsinki with a zero deductible. To further illustrate how the reform affected actual HA amounts, we calculate the difference between HA determined by equation (1) and (2). We use pre-reform information on unit characteristics, tenant characteristics and rent together with pre-reform and post-reform program parameters to compute what would have been the HA before and after the reform in apartments that we observe both before and after the reform. The right panel of Figure 1 shows the resulting simulated change in HA as a function of unit size. As the figure shows, there is a systematic pattern in the variation generated by the reform that follows the change in the maximum HA. HA increased substantially in small housing units while the increase in HA was small or close to zero in larger units. This variation is the starting point for our empirical analysis.



(a) Maximum HA in Helsinki

(b) Simulated HA change

Figure 1: Maximum HA before and after the reform in Helsinki (left) and simulated change in HA in Kela data (right).

Notes: The left figure 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. The right figure illustrates a simulated estimate of change in HA in our fixed-effects sample as a function of unit size. The size of the circle is proportional to the number of units in each bin.

3.2 Research design

We start our analysis by dividing housing units to floor area brackets and describing the evolution of housing allowances and rents in these groups over time. This graphical analysis allows us to transparently examine and assess the magnitude of the changes

in the HA caused by the 2015 reform in these groups and possible coinciding changes in rents. We consider separately the pool of all contracts and the subset of new rental contracts as rents of continuing tenancies are unlikely to react to changes in housing allowances.

We then run the following event-study style regressions using the sample of new rental contracts:

$$y_{itz} = \sum_{\substack{s=2010q1 \\ s\neq 2014q4}}^{s=2019q4} 1_{\{Treat_i\}}\theta_s + \delta_t + \omega_z + 1_{\{Treat_i\}}\gamma + u_{itz} \tag{3}$$

where the outcome variable y_{itz} is either rent or housing allowance. Subscript i indexes rental contracts (a combination of a household and a housing unit), t time (quarter) and z the zipcode. $1_{\{Treat_i\}}$ is an indicator variable that takes value 1 for treated units (15-25m^2) and value 0 for units in the control group (35-45m^2). This grouping is based on Figure 1 from which we see that the small units (15-25m^2) get a much larger treatment than the larger units (35-45m^2).

We do not have a pure control group that does not get a treatment, but instead gets a much smaller dose of the treatment. We discuss this issue further after introducing the specifications where we fully exploit the variation in treatment dosage. The coefficients of interest in equation (3) are treatment group times quarter fixed effects $1_{\{Treat_i\}}\theta_s$. 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. δ_t are quarter fixed effects, ω_z zipcode fixed effects, and $1_{\{Treat_i\}}\gamma$ treatment group fixed effects.

To quantify the magnitude of the HA increases and the rent increases, we first run a standard pooled DID regression estimated using two-way fixed effects where the outcome variable, either HA or rent of a new rental contract, is regressed on time fixed effects, a treatment group indicator, zipcode-level fixed effects, and treatment times post-reform fixed effects. In this case, we replace the treatment group times quarter fixed effects with an interaction term $1_{\{Treat_i\}} \times post_t$, and the quarter fixed effects by month $\times$ year fixed effects. The identification of the treatment effect relies on the control group providing a good counterfactual for what would have happened to the treatment group had they not received the larger increase in HA.

Next, we discuss the interpretation of 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 of the incidence of changes in HA between tenants and their landlords

and also facilitates comparison with previous studies. Our regression of interest writes

$$Rent_{izt} = \alpha + \beta HA_i + \delta_t + \omega_z + D_i + \epsilon_{izt}, \tag{4}$$

where the outcome variable is monthly *Rent* in rental contract i in zipcode z in time t and the parameter of interest is β. Similar to the event study specification, we include fixed effects for the time period (δ_t), the zipcode (ω_z) and the treatment group (D_i). We also estimate a specification with unit-level fixed effects. We address the endogeneity concerns in the estimation equation by instrumenting HA with the treatment group indicator interacted with a post-reform indicator ($1_{\{Treat_i\}} \times post_t$). The coefficient β in this regression will simply be the DID-estimate for allowances divided by the DID-estimate for rents (like the typical IV estimator which boils down to rescaling 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 may 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: that the control group evolution of rents and allowances provides a good counterfactual for what would have happened in the treatment group in the absence of a treatment. We inspect the credibility of these parallel trends assumptions by comparing the pre-reform trends in the treated and non-treated units as a part of our event study-analysis.

In addition to the two-group strategy, we use a continuous treatment DID and DID-IV strategy, which exploits the full variation in HA changes induced by the reform. We use the reform to construct continuous doses of HA change for individual housing units. In order to measure the dose sizes, we proceed by taking a sub-sample of our fixed effects sample such that we observe each unit at least once before and after the reform. We then measure the change in HA induced by the reform by fixing the pre-reform rent and recipient characteristics, and by using program parameters for 2014 and 2015 to compute what would have been the HA in these years, had nothing else but the policy parameters changed. We take the difference in these simulated HAs and use that to measure the dose or intensity of treatment.

Then, we run the following regression:

$$Rent_{izt} = \beta \times dose_j \times post_t + \gamma_t + \omega_j + u_{izt}, \tag{5}$$

where i indexes the rental contract, j the housing unit, γ_t time-fixed effects, ω_j unit-level fixed-effects, and dose_j is the simulated HA change. This regression compares the change in the rent of a given housing unit from pre-treatment to post-treatment time period as a function of the change in HA induced by the reform. In this continuous treatment DID regression, we are interested in recovering a parameter indicating how much rents increase on average following a small increase in the treatment dose.

Following Callaway et al. (2021), we can interpret β as an average causal response to treatment on the treated under a strong parallel trends assumption, which demands that low-dose units provide a good counterfactual for what would have happened to outcomes in high-dose units had they received small doses.[8] The assumption would fail if, for example, small units received on average larger treatment doses and the effect of HA on rents was different for small units and for other units.

Finally, both DID approaches and the DID-IV approach 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 others. This assumption would be violated if, for example, housing allowances had increased for small units and this would have decreased the demand for medium-sized units, which is possible at least in principle. However, the failure of SUTVA assumption in our case would most likely lead to an upward bias in our estimate. If there was an important shift in demand away from the units in the control group, we would expect a rent decrease in this group, implying an upward bias in our estimate of the effect of HA on rents.

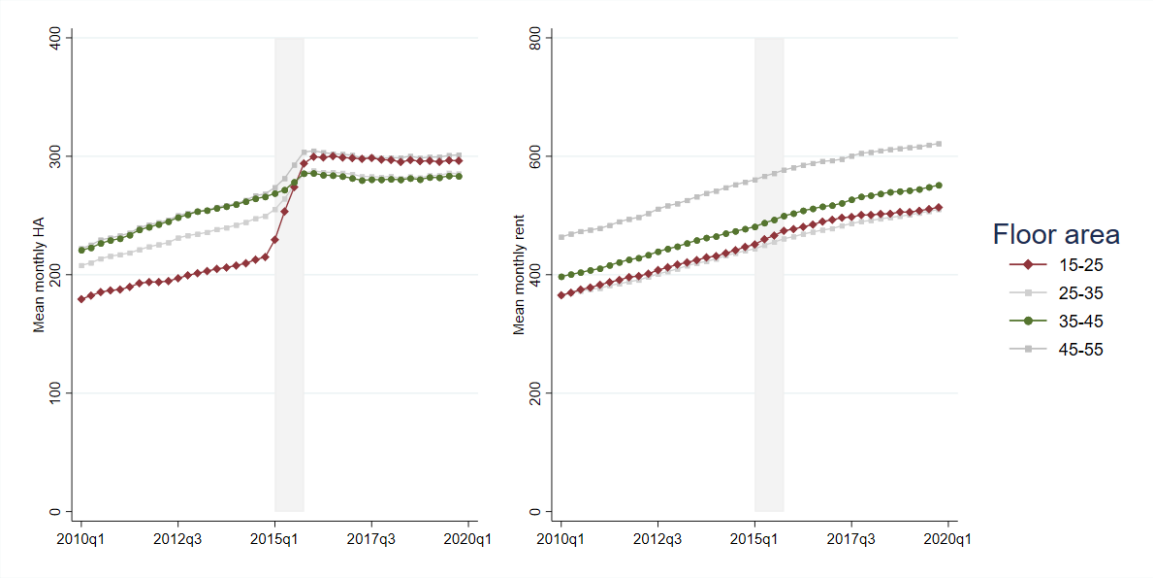
4 Results

4.1 Effects of housing allowance changes on rents

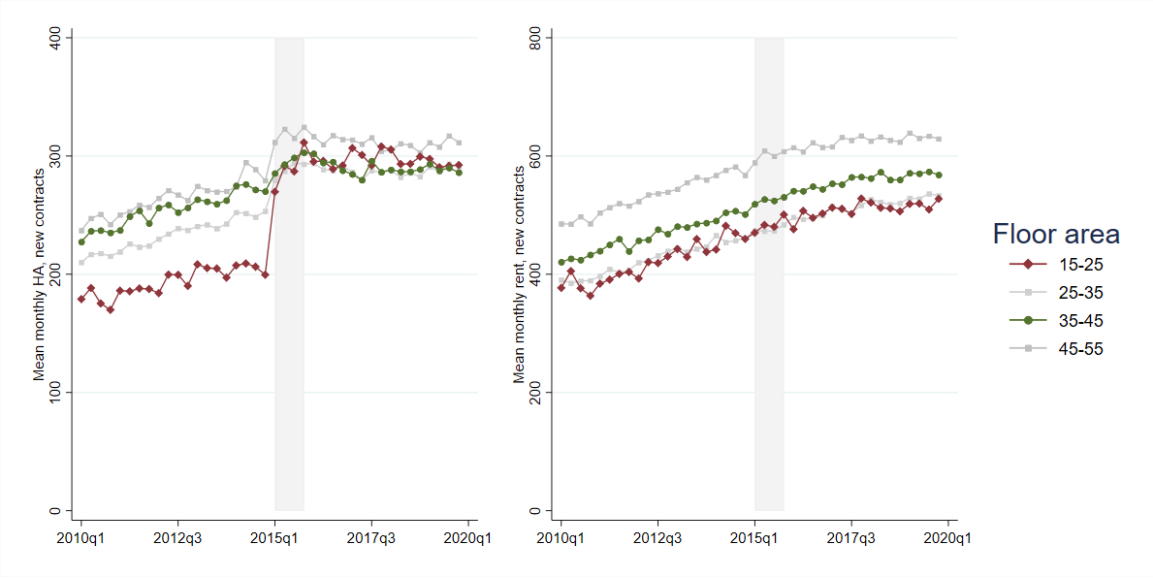
We start describing our results with a set of DID graphs in which we divide housing units into groups by their floor area. The top panel in Figure 2 shows the evolution of monthly HA payments to recipient households (left) and their rents (right) in the different floor area groups. The bottom panel in Figure 2 shows the evolution of monthly HA payments and mean rents in new rental contracts. As one would expect based on Figure 1, housing allowances in units of 15-25m^2 (red line) increased

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

dramatically in 2015, whereas in other floor area groups increases were moderate. Especially in medium-sized units (35-45m^2, green line), HA deviates only slightly from its pre-reform trend. The development of mean rents in the different floor area groups in turn is quite stable around the reform period and there are no visible differences between the groups suggesting that the reform had small if any effects on rents.



(a) All contracts.



(b) New contracts

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

Notes: Mean monthly HA paid to recipient households and mean monthly rents paid by recipients to their landlords in our estimation sample, aggregated to quarterly level. The light gray shaded area refers to year 2015. "All contracts" refers to all monthly 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.

In Figure 3, we focus on new rental contracts and floor area groups 15-25m^2 (treatment group) and 35-45m^2 (control group). The figure shows the event study

estimates comparing these groups. The figure indicates that HA and rents developed in parallel in these groups before the reform. After the reform, there was a sizable increase in HA in the treatment group, but again rents seem unaffected. One should keep in mind that since these are new contracts there are no legal or other reasons not to expect rent effects if HA increases are indeed passed through to rents.

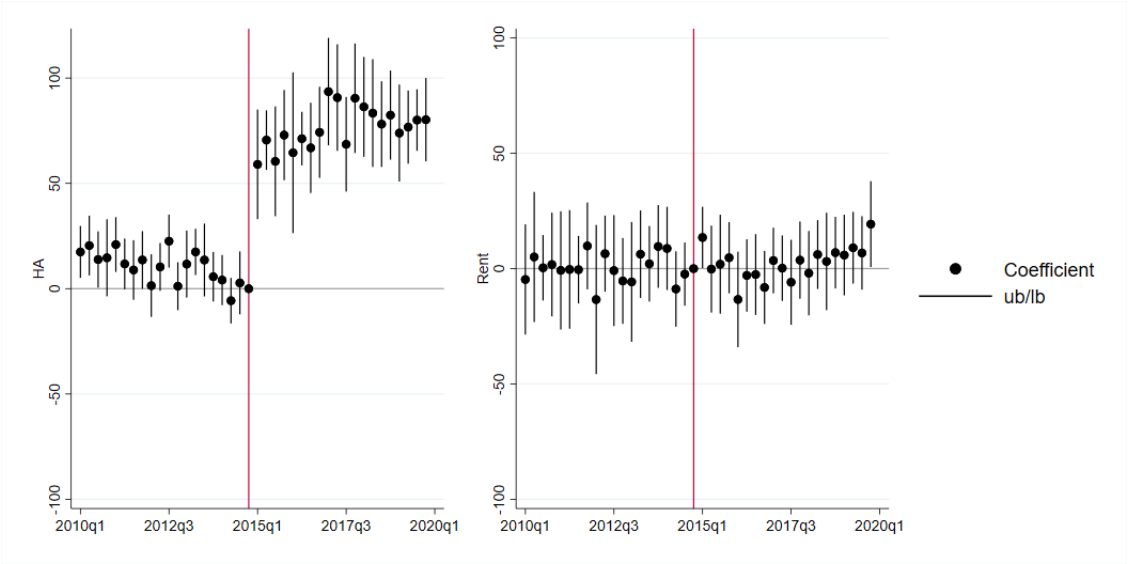


Figure 3: Comparison of monthly HAs and rents between treatment (15-25m^2) and control groups (35-45m^2) among new rental contracts.

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 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.

We next turn to Table 2 which reports the results from our DID-IV regressions. The first two columns report the DID estimates. We find that while the mean difference in HAs between the groups grew by 68 euros, the mean difference in rents grew only by 2 euros and this increase is statistically insignificant. These differences are the first stage and the reduced form of our DID-IV estimate in the third column. The DID-IV estimate implies that as the HA increases by one euro, rents increase by 0.03 euros. The standard error is roughly 0.05, implying that pass-through rates of above 0.15 are unlikely.

Table 2: DID-IV estimates using treatment (15-25m^2) and control group (35-45m^2) apartments.

	DID		DID-IV
	(1)	(2)	(3)
	Allowance	Rent	Rent
treat × post	68.12***	2.227	
	(7.518)	(3.382)	
Allowance			0.0327
			(0.0477)
Month × year FEs	✓	✓	✓
Zipcode FEs	✓	✓	✓
Outcome mean	275.1	515.4	515.4
N	45076	45076	45076
First-stage F			82.10
SE clustered by	Municipality	Municipality	Municipality

Notes: The table reports results from reduced-form regressions of HAs and rents on a treatment group indicator, year fixed effects and a treat×post indicator. The DID-IV column reports the result from an IV regression where HAs have been instrumented for by the treat×post indicator. Note that the DID-IV point estimate equals the second-stage DID coefficient divided by the first-stage DID coefficient. Robust standard errors are reported in parentheses. $^{*}\, p < 0.10$ $^{**}\, p < 0.05$, $^{***}\, p < 0.01$.

We next restrict our analysis to housing units for which we observe at least one new contract before and after the reform. This allows us to include unit-level fixed effects in the estimation.[9] These unit-level fixed effects help us control for unobserved unit characteristics and alleviate the concern that the estimate in Table 2 is driven by changes in group compositions over time.

Figure 4 first shows the event study estimates for the sub-sample of units with new contracts before and after the reform controlling for unit-level fixed effects. Confidence bands are wider than in Figure 3, but the patterns of HA and rents are very similar. After the reform HA increased substantially in the treatment group, but rents were unaffected.

[9]The procedure to identify repeated observations of same unit is described in Appendix A.

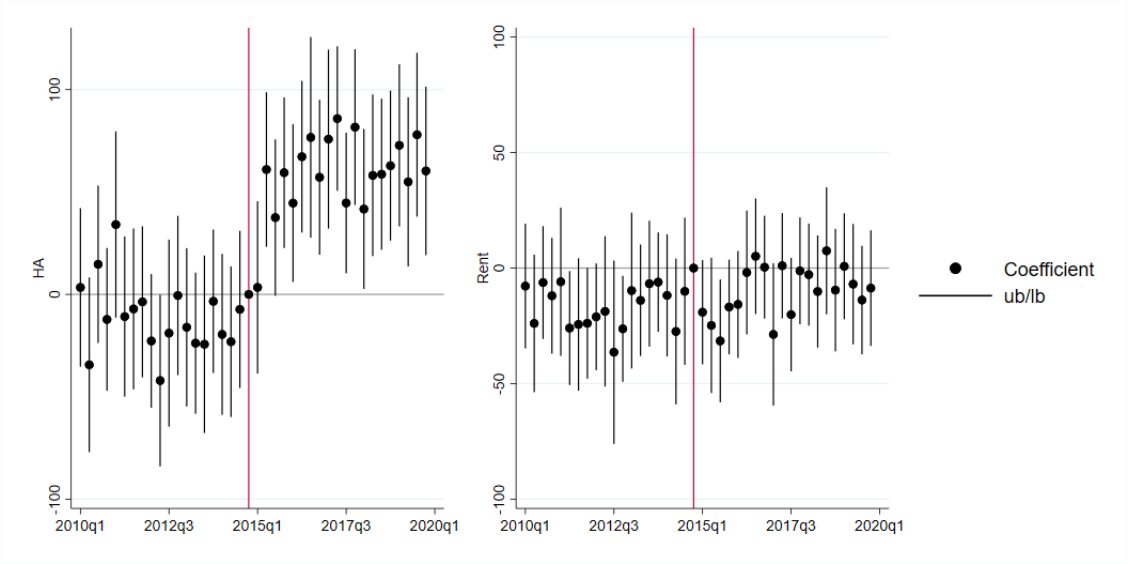


Figure 4: Comparison of monthly HAs and rents between treatment (15-25m^2) and control groups (35-45m^2) among new rental contracts with unit-level fixed effects.

Notes: The figure plots coefficients from an event study-regression in our fixed effects sample, where the outcome (HA or rent) is regressed on a treatment group indicator, quarter fixed effects, unit-level fixed effects and treatment × 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 × quarter coefficients. Standard errors are clustered at the apartment level.

Table 3 reports the DID-IV results when we include unit-level fixed effects in the estimation. For comparison, columns 1, 3 and 5 use the same regression specification as Table 2, but the fixed effects sample. They show very similar results to Table 2, indicating that our fixed effects sample is observationally similar to our baseline estimation sample. Columns 2, 4 and 6, in turn, document coefficients from otherwise same regressions when the unit-level fixed effects are included.

The average difference in HA change in the fixed effects specification is 72 euros (column 2), which is close to the estimated difference without fixed effects. The rent effect in the fixed effects specification is 6.3 euros (column 4) and the DID-IV estimate in column 6 implies that a one euro increase in the housing allowance increased rents by 0.09 euros. The standard error is approximately 0.05 implying that high pass-through of HA to rents seems unlikely.

Table 3: DID-IV estimates in the fixed effects sample, with and without unit-level fixed effects.

	DID				DID-IV	
	(1)	(2)	(3)	(4)	(5)	(6)
	Allowance	Allowance	Rent	Rent	Rent	Rent
treat × post	67.62***	71.58***	-0.321	6.314		
	(8.110)	(5.265)	(4.160)	(3.391)		
Allowance					-0.00474	0.0882
					(0.0615)	(0.0468)
Unit FEs		✓		✓		✓
Zipcode FEs	✓		✓		✓	
Month × year FEs	✓	✓	✓	✓	✓	✓
Outcome mean	274.3	274.3	501.2	501.2	501.2	501.2
N	10868	10868	10868	10868	10868	10868
First-stage F					69.53	184.8
SE clustered by	Municip	Unit	Municip	Unit	Municip	Unit

Notes: The table reports the results from reduced-form regressions of HAs and rents on treatment group × post indicators (columns 1-4) as well as from a regression of rents on HAs where HAs are instrumented for by the treat×post indicator (columns 5-6). The even columns (2, 4, 6) include unit-level fixed effects to control for time-invariant unit characteristics. Odd columns (1, 3, 5) use the same sample as even columns but run the regressions without the unit-level fixed effects. Standard errors are clustered at the municipality level for the odd columns and at unit level for the even columns. * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Finally, we turn our attention to the continuous-treatment DID analysis. This analysis compares changes in rents across housing units which received different-sized treatment doses, where the treatment dose is defined as a simulated change in HA assuming that only the parameters of the HA system changed but other features of the unit and the recipient remained the same (see section 3.2 for details). First, we report event-study style evidence about the continuous treatment specification in Figure 5. The left panel suggests that a one euro increase in our "simulated treatment" (treatment dose) led to a sizable increase in actual HA paid to the recipients, although the increase in actual HA is not exactly one euro. This means that we get significant exogenous variation in HAs, although we cannot perfectly predict the new HA received by the new tenant (as the tenant characteristics also changed, which

we do not control for, as did possibly the rent). The right panel suggests that an increase in the simulated treatment does not lead to sizable increases in rents paid by recipients.

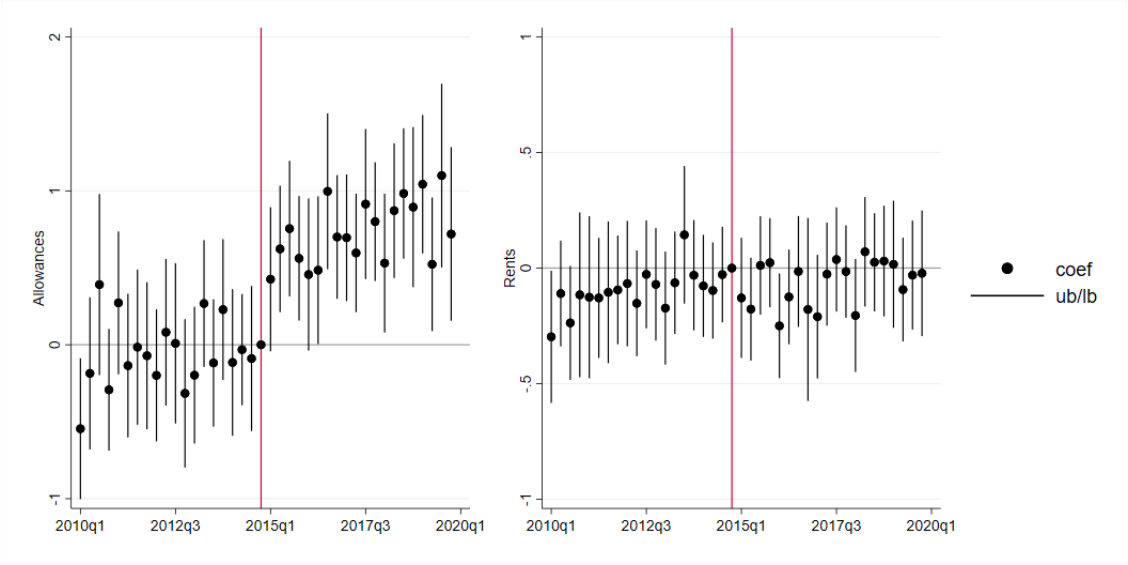


Figure 5: Comparison of monthly HAs and rents of units that received different-sized treatment doses, with unit-level fixed effects.

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, apartment-level fixed effects and treatment dose × 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×quarter coefficients. Standard errors are clustered at the unit level.

We then explore this variation through a continuous treatment DID regression, reported in Table 4. First, in column 1, we regress actual allowances on our measure of simulated HA change. The point estimate suggests that a one euro increase in the simulated HA change is associated with an increase in the actual HA increases of 0.75 euros. Thus, our simulated HA change is highly correlated with actual changes in 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 simulated HA was very modest, below one cent. The standard error 0.02 suggests we can rule out large effects on rents with a high degree of confidence.[10] For completeness, in column 3 of Table 4, we also estimate the outcome of an IV

[10]As shown by Callaway et al. (2021), we can interpret the point estimate as a weighted average over causal response parameters, but since the identification relies on comparing rents across apartments receiving different HA changes, the underlying identification argument is that the rent

regression in which the level of HA is instrumented for by our measure of continuous treatment. The estimated effect of HAs on rents, roughly 1 cent per an additional euro, is broadly in line with our other estimates of very low pass-through rates.

Table 4: Continuous-treatment DID-IV estimates, with unit-level fixed effects.

	DID		IV
	(1)	(2)	(3)
	Allowance	Rent	Rent
HA change	0.752***	0.00799	
	(0.0434)	(0.0198)	
Allowance			0.0106
			(0.0262)
Month × year FEs	✓	✓	✓
Apartment FEs	✓	✓	✓
Outcome mean	272.5	497.0	497.0
N	10962	10962	10962
SE clustered by	Unit	Unit	Unit
N of clusters	5481	5481	5481
First-stage F			300.6

Notes: The table reports results from DID and IV regressions where the treatment or instrument is defined to be a simulated change in HAs as described in Section 3.2. Columns 1 and 2 report coefficients from a regression of the outcome on our measure of continuous treatment × a post indicator. Column 3 reports the second-stage of an IV regression, where HAs are instrumented for by a continuous treatment × post indicator. The first stage of this regression corresponds to column 1. All specifications contain month-by-year and unit-level fixed effects. Standard errors are clustered at unit level. $^{*}\, p < 0.10$ $^{**}\, p < 0.05$, $^{***}\, p < 0.01$.

4.2 Robustness

As discussed above, housing costs of low-income households are also covered through social assistance. For social assistance recipients, a change in housing allowances may be partly or even entirely offset by compensatory effects on social assistance. We would therefore expect that the rent effects are more pronounced for households not eligible for social assistance.

changes in units with small doses provide a good counterfactual for the rent changes that would have been observed for units that received larger doses, had their doses been small too.

We address this issue in Appendix C. Ideally, we would like to replicate the estimation separately by social assistance status. Unfortunately, our data do not include information on social assistance status. We therefore divide the sample into two income groups using two different cut-offs reflecting eligibility for social assistance. The point estimates for the rent effect are slightly larger for the HA recipients who are not eligible for social assistance but the difference is not statistically significant. Therefore, it does not seem likely that our results are driven by the countervailing incentives generated by the social assistance program.

Roughly half of HA recipients live in social housing where the rents are based on maintenance and capital costs of the building. Therefore, changes in HA should not have any effects on the rents of the social housing tenants. For completeness, we repeat the estimation shown in Table 2 for social housing units in Appendix B. We do not find any evidence that the HA increases are passed through to rents of the social housing units.

Finally, a factor that could complicate the interpretation of our results is the potential effect of the reform on household sorting into different apartments. We note, however, that the changes to the HA system were to a large extent separable between apartment characteristics and household characteristics, so major changes in sorting patterns seem unlikely. We nevertheless study the issue in Appendix D and show that there were no large changes in household characteristics across treated and control units after the reform.

4.3 Mechanisms

Overall, our estimates of HA pass-through to rents are small. In this section, we discuss some of the issues related to the interpretation of the results and potential mechanisms behind the results. The mechanisms can be divided into demand-side and supply-side mechanisms. The rents of the housing units that get more generous HA after the reform can increase only if the demand for these apartments increases. At the same time, increases in demand may be followed by a supply increase, which would dampen any rent effects.

Starting with the demand side, it is important to understand whether HA is an important consideration for people when they are making housing consumption choices. We consider two ways to assess whether incentives generated by the reform affected residential mobility. First, we compare HA spell durations with tenure spell durations (the time a household spends in the same apartment). If HA spells are short

relative to tenure spells, other considerations besides HA should play a larger role in housing choices and changes in HA do not necessarily induce recipient households to move. Second, we analyze mobility patterns in the data directly.

Housing allowance spells seem slightly shorter than tenure spells in the data. Roughly 56% (39%) of HA spells in our sample still continue after one year (two years). According to the population-wide register data, roughly 67% (48%) of tenure spells continue after a year (two years) when focusing to the private rental market and to 15-55m^2 sized housing units. These numbers suggest that for many HA recipients, HA is a temporary subsidy covering part of their housing costs during a relatively short period of due to, for example, unemployment. This observation could go some way in explaining our finding of very small rent effects.

A more direct way to assess whether the demand of the HA recipients for small units increases is to analyze whether HA recipients move out from these units less frequently and whether, conditional on moving, they are more likely to move into small units after the reform. The left panel in Figure 6 shows an event-study graph using our treatment and control groups where the outcome is whether the tenant moves out from their unit at a given time. There seems to be no change in the probability to move out from a treatment group unit compared to moving out from a control group unit. The right panel of Figure 6 shows whether, conditional on moving, HA recipients move to units in the treatment group. Conditional on moving, there is a slight increase in the probability of choosing a unit in the treatment group compared to choosing a unit in the control group. However, this increase happens four years after the reform. There is no reason why there would be a three year lag in this response suggesting that this increase is due to some other changes that took place during this time period.

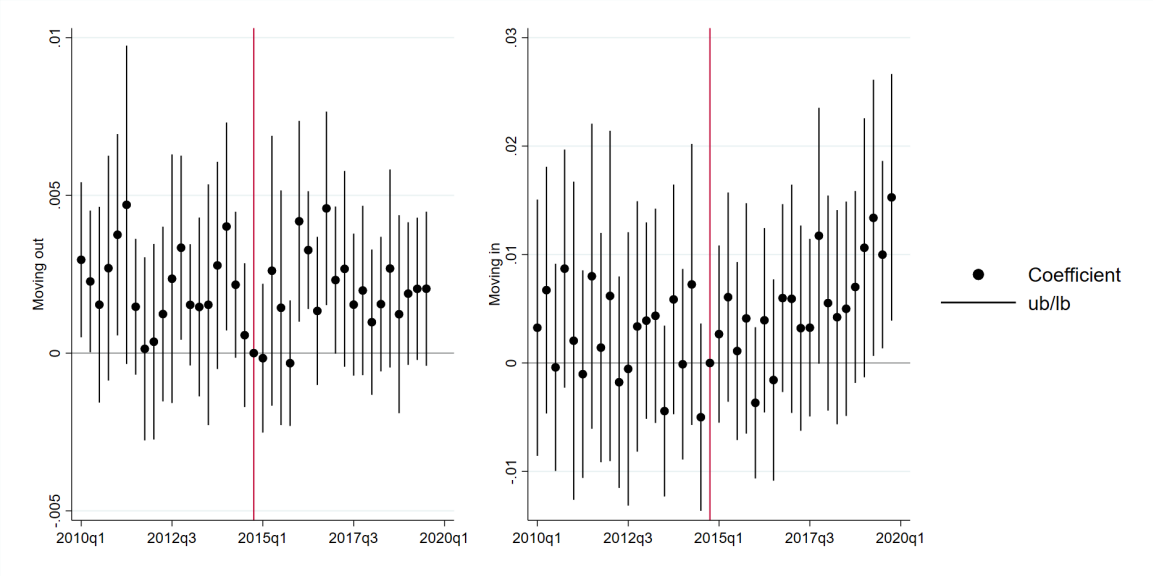


Figure 6: Comparison of probabilities of moving from treatment versus control units (left) and probability of moving to treatment units, conditional on moving (right).

Notes: The left panel ("Moving out") reports estimates from an event study regression where the outcome is an indicator for *moving out* from a unit, which is regressed on treatment group, quarter and zipcode fixed effects as well as a treatment group $\times$ quarter indicator, where the last quarter before the reform is omitted. Thus the figure indicates differences in the probability of moving from a treatment group unit relative to the probability of moving out from a control group unit. The individuals identified as movers receive HA both before and after the move. Sample consists of households living in treatment or control group units before the move. The zipcode fixed effect included is for the address before the move. Standard errors are clustered at municipality level.

The right panel ("Moving in") reports estimates from an event study regression indicating the probability to *move in* to a treatment group unit relative to the probability of moving in to any other type of unit in our main estimation sample, *conditional on moving*. The sample consists of individuals identified as movers in out main estimation sample, who receive HA both before and after the move and who reside in 15-55m^2 private-market rental units both before and after the move. Hence, the sample does not consist only of the treatment and control groups. The reported estimates are from a regression where the outcome variable, an indicator for if the unit that is *moved to* is a treatment group unit, is regressed on quarter and zip code fixed effects, but the last quarter before the reform is omitted. Thus, the figure describes the probability to move to a treatment group unit instead of other 15-55m^2 units over time. The zipcode fixed effect refers to the address before the move. Standard errors are clustered at the municipality level.

The quantitative importance of the reform depends not only on the HA increase for recipient households but also on the number of recipient households. If the share of HA recipients in the private rental market is large, any given increase in HA generosity can lead to larger shifts in the demand for different kinds of units and therefore to

larger rent effects. This would be especially true if the private rental market is highly competitive, that is, when all tenants pay the same quality-adjusted rent.

We use population-wide register data with less detailed information on HA (and no information on rents) to analyze the importance of HA for the private rental market. Table 5 shows that before the reform roughly 24% of households in the private rental market received HA (at least 100 euros during the year). The share of HA recipients increased after the reform and this increase was somewhat larger in small units (15-25m^2) in our treatment group than in the control group (35-45m^2). Mean annual HA of all households (including non-recipients as zeros) increased from 546 euros to 946 euros per year in the treatment group, while in the control group mean HA increased from 544 to 729 euros. The table suggests that an additional reason behind small rent effects could be that a large share of private rental market tenants are non-recipients. This implies that potential demand responses by HA recipients would have smaller effects on aggregate demand for rental housing.

Table 5: Share of HA recipients and mean annual HA in private rental market.

	2014			2016		
	Share of HA recipients	Mean annual HA	N	Share of HA recipients	Mean annual HA	N
All	0.238	609	523,507	0.295	896	567,329
15-25 m^2	0.275	546	24,411	0.351	946	25,131
35-45 m^2	0.234	544	84,581	0.280	729	94,639

Notes: Table shows the share of HA recipient households and mean annual HA in the private rental market in 2014 and in 2016. Households receiving at least 100 euros of HA during the year are classified as HA recipients. Mean annual HA is calculated for all households (non-recipients are included as zeros).

Next, we describe how the housing stock evolved before and after the reform in order to assess whether changes in housing supply might affect the pass-through of HA changes to rents. The left panel of Figure 7 shows the stock of housing units by floor area group in levels and the right panel as an index relative to 2014. There are no visible changes in trends of levels, but the stock of small units grew more slowly than mid-sized units between 2010 and 2014. Between 2014 and 2018 the stock of both small and mid-sized units grew by roughly 10 percent. Thus, it seems that while the construction of small units seems to increase after the reform this did not lead to a substantially faster growth of the stock of small units than the stock of mid-sized

units. It is not clear whether the acceleration of the construction of small units after the reform can be attributed to the reform. One would expect that an increase in the construction of small units would be preceded by an increase in rents of these units. The rent increases would then provide incentives for developers to build more of these kinds of units, but as we do not find evidence of significant rent effects, we argue that the change in housing supply may be caused by some other factors than the HA reform. Nevertheless, the increase in the supply of small units may contribute towards the small or zero rent effects.

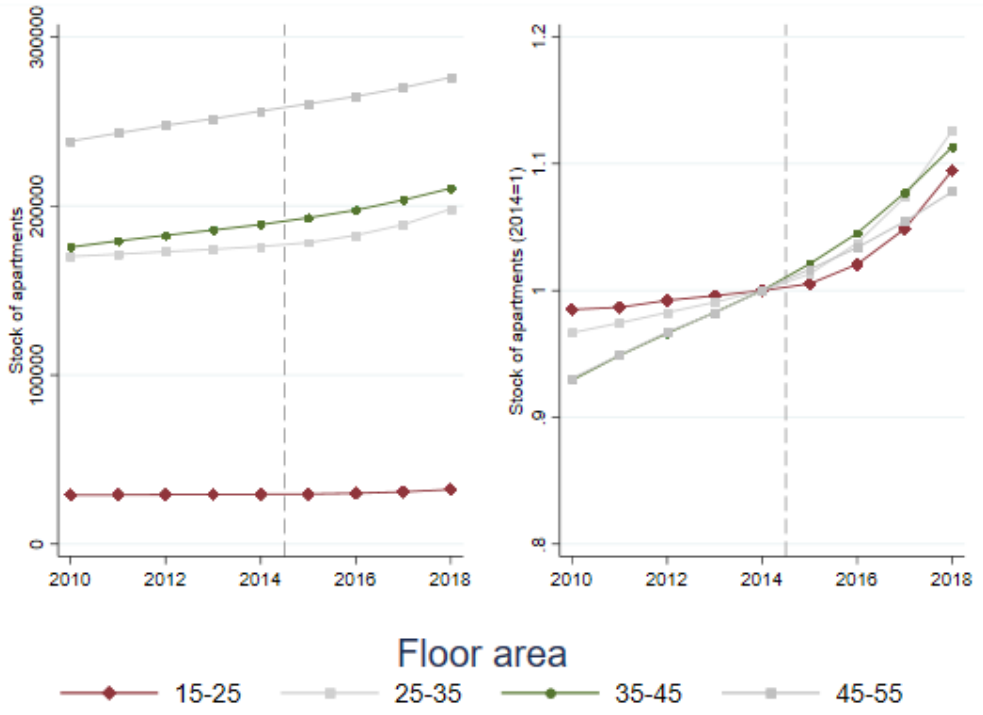


Figure 7: Housing stock by floor area group.

Notes: The figure shows the number of housing units by floor area group in multi-unit buildings in years 2010-2018. The figure is based on calculations using cross-sectional microdata on the total housing stock in 2018, which includes construction year of each building. Housing stock in earlier years 2010-2017 is imputed using the information on construction year.

4.4 Incidence

Thus far, we have focused on the recipient households and their rents. Our results imply that HA recipients benefited from HA increases, and their landlords received at most a small fraction of the HA increase in the form of higher rents. It is, how-

ever, possible that also non-recipients' rents are affected by HA changes. If so, their increased housing costs should be accounted for when assessing the overall incidence of HA changes.

We analyze the incidence of HA changes between HA recipient households, non-recipient tenants and landlords through a simple static back-of-the envelope calculation. The incidence of HA crucially depends on the competitiveness of the rental market. If the market is competitive, the rent of an housing unit does not depend on the characteristics of the tenant (see e.g. Gibbons and Manning, 2006). This means that the rents paid by non-recipient tenants and HA recipients are equally affected by HA changes. If the rental market is characterized by important frictions, rents paid by HA recipients may respond more strongly to HA changes than rents paid by non-recipients. We calculate the incidence under two extreme assumptions: 1) The private rental market is fully competitive and all tenants pay the same rent for similar units, and 2) the rental market is characterized by strong frictions, the rent of each unit depends on tenant, and the rents of non-recipient households are largely unaffected by HA changes.

We consider a case where 30% of the tenants in private rental market are HA recipients (see Table 5). The monthly HA is increased for all recipients by the same amount. We use the rent effect estimates from our continuous-treament DID-IV specification in Table 4, where the point estimate of the euro-on-euro effect of HA on rent was 0.01 euros. The upper bound of the 95% confidence interval (0.06 euros) is used as an alternative rent effect.

In a non-competitive market, HA increases affect the rents of recipient households only and the benefits are divided between HA recipients and their landlords. The point estimate of DID-IV specification implies that 99% of benefits go to the recipients and 1% to the landlords. Using the upper bound estimate implies that 94% of benefits are incident on the HA recipients and 6% go to their landlords.

In a fully competitive private rental market HA increases the rents paid by everyone. Consider a case where there are 100,000 tenants (30,000 HA recipients and 70,000 non-recipients). Table 6 shows how each additional euro of HA paid to HA recipients affects HA recipients, other tenants and landlords under alternative assumptions on the effect of a marginal increase in HA on rents. if the private rental market is competitive, using our point estimate of 0.01 implies that landlords capture a sum which is 3% of the increased allowance outlay. Most of this is paid by non-recipients through higher rents. Similarly, if we use the upper bound of the 95% confidence interval, landlords capture a sum that is 20% of the increased HA outlay.

Again, most of this sum comes from non-recipients tenants.

Table 6: Incidence of HA increase in a competitive rental market.

	Effect of one euro HA increase on rent	
	0.01 euros	0.06 euros
Government	$-30,000$ euros	$-30,000$ euros
HA recipients	$30,000 * 0.99 = 29,700$ euros	$30,000 * 0.94 = 28,200$ euros
Non-recipient tenants	$70,000 * (-0.01) = -700$ euros	$70,000 * (-0.06) = -4,200$ euros
Landlords	$100,000 * 0.01 = 1,000$ euros	$100,000 * 0.06 = 6,000$ euros

Notes: The table calculates the benefits and costs of a uniform 1 euro increase in HA. There are 30,000 HA recipient households and 70,000 non-recipient households. The rental market is competitive, and all rents are equally affected by the HA increase. The effect of a one euro increase in HA on rent is the point estimate of our DID-IV specification in Table 4 or the upper bound of the 95% confidence interval.

In sum, given the estimated rent effect, the extent that landlords benefit from HA increases depends on 1) the share of HA recipients relative to all private rental market tenants and 2) on how competitive the private rental market is. Using our population-wide register data, we are able to pin down the first, but evaluating the latter is beyond the scope of this analysis.

5 Conclusions

This paper addresses the question of how housing allowances affect rental levels by exploiting exogenous variation in housing allowances generated by a large policy reform in Finland and by using data that covers the universe of Finnish housing allowance recipients. The reform led to important increases in HA payments for households living in small units. We find that despite the increases in housing allowances, rents in small units evolved similarly to rents in larger units, and the reform had practically no effects on their rents. This suggests that the changes in housing allowances that were made in Finland in 2015 mainly benefited HA recipients rather than their landlords.

An open question in the literature on housing allowances is to identify the mechanisms via which housing allowances affect rents, and how the rent effects depend on the characteristics of the housing market and of the subsidy program. In Finland, the reform changed the out-of-pocket rents paid by recipients in different types of units,

since the formula became more generous for small units, but their rents did not increase. Despite this change in relative prices, we do not observe changes in recipient households' housing choices: Recipients neither stay longer in small units or start moving to small units more frequently. This suggests that demand for different sizes of units could be relatively inelastic, which might be a factor explaining our small rent effects. While it seems that following the 2015 Finnish reform, the changes in the relative prices of different units did not change households' housing consumption choices, it remains an open question whether the HA scheme affects choices between housing and non-housing consumption more generally.

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Appendix

A Data and sample selection

Kela HA register data

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

We make the following restrictions in selecting our sample. First, we only include regular monthly payments, thus excluding, for example, drawbacks of payments that had been too high. Second, we exclude the following observations: 1) observations from Åland Islands because 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 apartments with floor area either below 15m^2 or above 55m^2. Third, we leave out years 2008 and 2009 avoid any confounders stemming from the financial crisis.

Furthermore, we want to focus on regular rental contracts and therefore 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, exclude social housing where rents are regulated and determined based on the maintenance and capital costs. Finally, we exclude housing units that Kela has registered as shared. 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. Students with children were eligible for general HA even before 2017. After 2017, all students became eligible for general HA. Students are excluded using a separate Kela register on student allowance payments. In practice, we label all individuals who receive student allowance (opintoraha) at least once during the half-calendar year (January-June or July-December) as students for the six month period in question. This is because "student status" and therefore eligibility for student allowance is unlikely to change much in other periods than the end of year and summer. Thus, for example, someone who received student allowance in February 2018, will be excluded from our estimation sample throughout the first half of 2018.

The pensioners have a separate HA program. No general HA is 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 reform 2015 these families have been allocated to the general HA system. The government proposal estimated that the change concerns roughly 2,500 families with children. The change does not influence our data as we are focusing on small apartments with mostly single-member households.

We determine the sub-sample of "new rental contracts" before the above sample restrictions. We proceed as follows: If the individual received HA to another address at most 4 months before, we classify the first observation in the new address as a "new rental contract". Misclassification can occur in two ways: An individual who receives HA may move to a unit in which someone was also living and the rental contract of the existing tenant will not be renewed. Also, when an individual appears in the HA register for the first time, the observation will not be classified as new contract although the individual could well have moved at the same time.

We also identify repeat observations of apartment as part of our fixed effects analysis. Even if we do observe the exact street address including the apartment number, we also use 10 square meter floor area groups in determining apartment identifiers. This is because we apartment floor area is self-reported and we may have repeat observations of the same apartment street address where the reported floor area group varies.

In reporting summart statistics (table 1), we report all monetary values in 2020 euros, where the deflator is obtained from the Statistics Finland CPI.[11] To describe household incomes, we use variable "Household income" (*tulaymk*). This variable is used in the summary statistics table as well as in appendices C and D. To compute simulated HA change used in Table 4, we use variable "Household income net of deductions" (*tuyhmk*, net of *ansiotulovähennys*).

Statistics Finland population-wide data

We use the following Statistics Finland ready-made research data modules: basic, degree/qualifications, family, income, and employment. 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 (*astuki*),

[11] Official Statistics of Finland (OSF): Consumer price index [e-publication]. ISSN=1799-0254. Helsinki: Statistics Finland [referred: 7.6.2022]. Access method: http://www.stat.fi/til/khi/index$_e$*n.html*

pensioners' HA (*omaastuk*) and students' housing supplement (*asuli*). The ready-made modules and the tailored data are combined with secured unit identifiers.

Statistics Finland building data

We use Statistics Finland apartment and building data where analyzing the evolution of the housing stock. These data include information on construction year (*vavv*) as well as apartment level information including the floor area (*pala*).

B Analysis of social housing units

In the social housing sector, some 60% of the housing units are owned by municipalities and the rest by non-profit corporations and associations. The government and the municipalities subsidize the units through different programs. In the social housing sector, rents are cost-based and depend on the capital and maintenance costs of the building including land rent. This means that the rents in social housing units relative to market rents vary from one municipality and neighborhood to another. Tenant selection is based on the need of housing, income and wealth of the household.

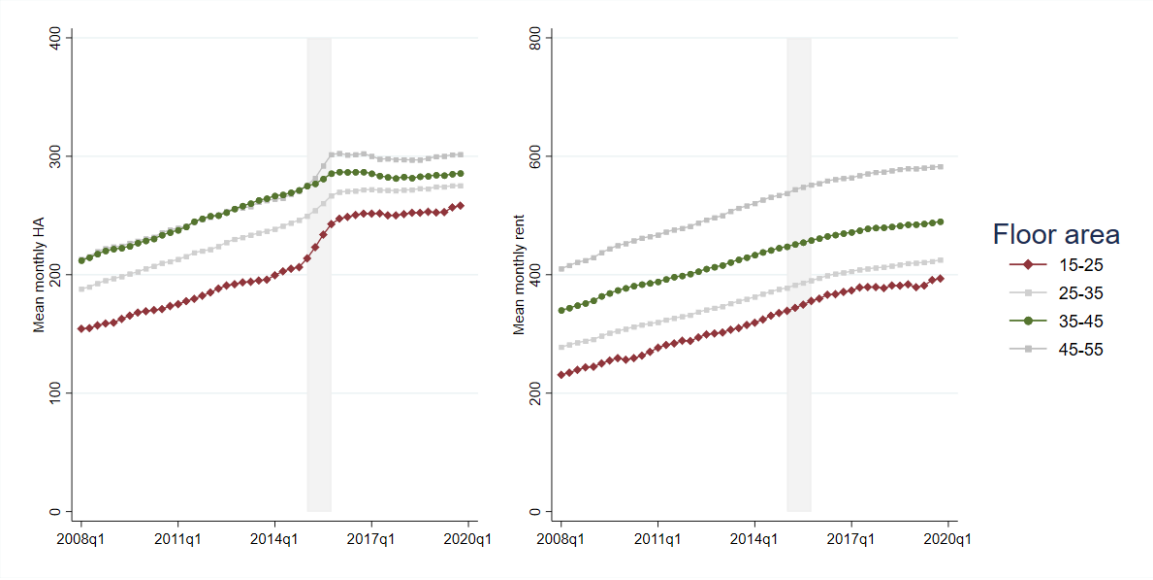
As rents are cost-based in the social housing sector, the HA increases should not have any effects on rents. In this section, we investigate whether this is the case. Table B1 provides some summary statistics on social housing tenants in the data.

Table B1: Housing allowance recipient characteristics, social housing

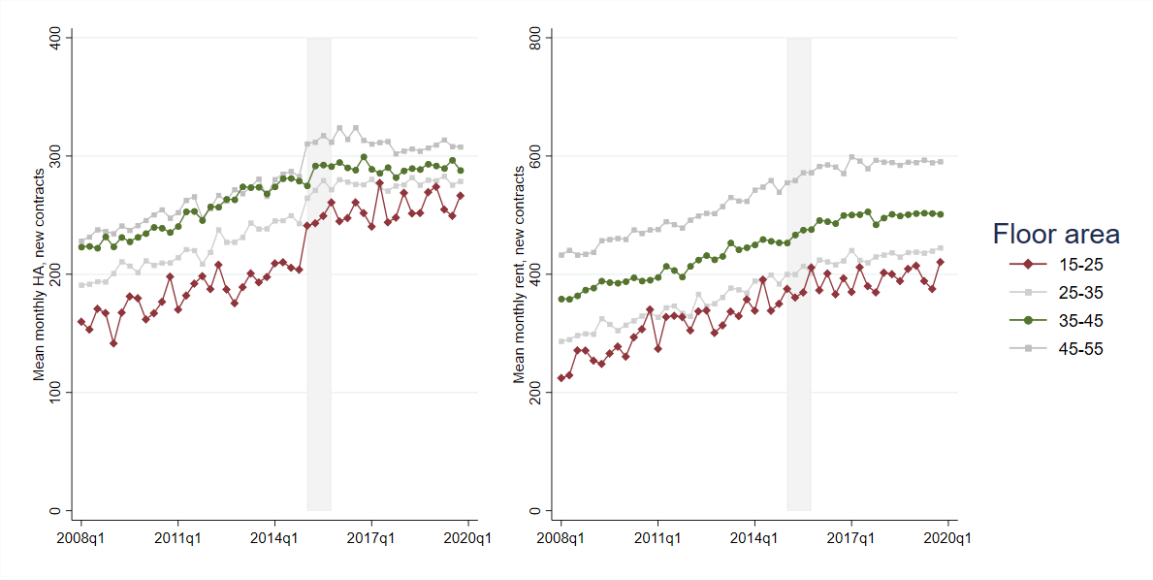
	All payments	New contracts
	mean	mean
Household size	1.1	1.2
Share single-member households	0.90	0.82
Apartment surface	40.1	41.1
Household income	722.0	733.5
Rent	474.4	501.0
Housing allowance received	276.8	288.7
Share rent ceiling binding	0.73	0.76
Observations	4949166	64629

Notes: HA register 1/2010-12/2019, all monetary values in 2020 euros. The sample consists of social housing residents in apartments of floor area between 15 and 55 m^2. The first column summarizes all month-by-household payments to HA recipients. The second column summarizes the subset of recipients with new rental contracts.

Figure B1 replicates the analysis of Figure 2 for social housing tenants. Increases in HA following the 2015 reform are much more modest for social housing tenants than for tenants in the private rental sector. This is to be expected as the social housing apartments are more affordable than the apartments in the private rental sector. Consistent with our main analysis, rents in the social housing apartments do not seem to be much affected either.



(a) All contracts.



(b) New contracts.

Figure B1: Mean HA and rents by floor area groups for social housing tenants.

Notes: Mean monthly HAs paid to recipient households in social housing units and their mean monthly rents, aggregated to quarterly level. The light gray shaded area refers to year 2015. "All contracts" refers to all monthly payments to recipients, and "new contracts" refers to the first payment made to a recipient who has changed addresses.

In Table B2 we repeat our two-group DID estimation for new contracts in social housing units. This corresponds to Table 2 in the main text. Consistent with our main results, we do not find any evidence of rent increases leading to HA increases.

Table B2: DID-IV estimates, 15-25m^2 and 35-45m^2 apartments, social housing.

	DID		DID-IV
	(1)	(2)	(3)
	Allowance	Rent	Rent
treat × post	32.55***	-0.383	
	(5.742)	(5.397)	
Allowance			-0.0118
			(0.165)
Month × year FEs	✓	✓	✓
Zipcode FEs	✓	✓	✓
Outcome mean	269.3	448.7	448.7
N	23077	23077	23077
First-stage F			32.14
SE clustered by	Municipality	Municipality	Municipality

Notes: The table reports results from reduced-form regressions of HAs and rents on a treatment group indicator, year fixed effects and a treat×post indicator. The DID-IV column reports the result from an IV regression where HAs have been instrumented for by the treat×post indicator. Robust standard errors are reported in parentheses. $^{*}\, p < 0.10$ $^{**}\, p < 0.05$, $^{***}\, p < 0.01$.

C Social assistance and housing costs

Social assistance program consists of three parts: basic social assistance managed by Kela, and supplementary and preventive social assistance provided by municipalities under specific conditions.[12] Like HA, the social assistance system sets an explicit limit on the housing costs to be covered. The limit is municipality-specific and much higher than the limits in HA system.[13] Unlike HA, social assistance can reimburse housing costs in full.

If the declared housing costs exceed the municipality-specific limit, Kela may instruct the recipient to seek more affordable housing. Kela may also continue to reimburse the housing costs in full, if affordable housing is not available or for other legitimate reason. The declared housing costs are accepted in full in roughly 70% of the cases where housing costs exceed the municipality-specific limit.

Our data do not include information on social assistance. In addition, because social assistance eligibility is based on the overall financial position of the household, it is not possible to predict eligibility with complete accuracy with the data we have.

Therefore, we consider two different ways to divide the sample. First, we use the so-called basic amount set in the social assistance system. The basic amount is a fixed sum of money towards the essential costs of daily living, such as food, clothing, public transportation etc. In 2020, it was 491 euros/month for a single-member household. Second, we divide the sample into two by using the median income.

We focus on single-member households because household income is straightforward to determine for them. Also, in our sample of new rental contracts in small units, over 80% are single-member households, so this restriction does not lead to a significant decrease in the sample size.

Figures C1 and C2 show the evolution of average HA and rent in the treatment and control groups by recipient income. The left graph shows that the average monthly HA is somewhat lower for households above the income limit. The increase in HA is quite similar and large in both income groups in our treatment group while there is no increase in the average HA in the control group. The right graph in turn does not indicate visible differences in the rent evolution of those above and below the income limit in the treatment group.

[12] Up until 2017, also the basic social assistance was managed by municipalities.

[13] For instance, for a single-member household in Helsinki in 2021, the maximum acceptable monthly rent was 521 euros in HA system and 694 euros in basic social assistance system.

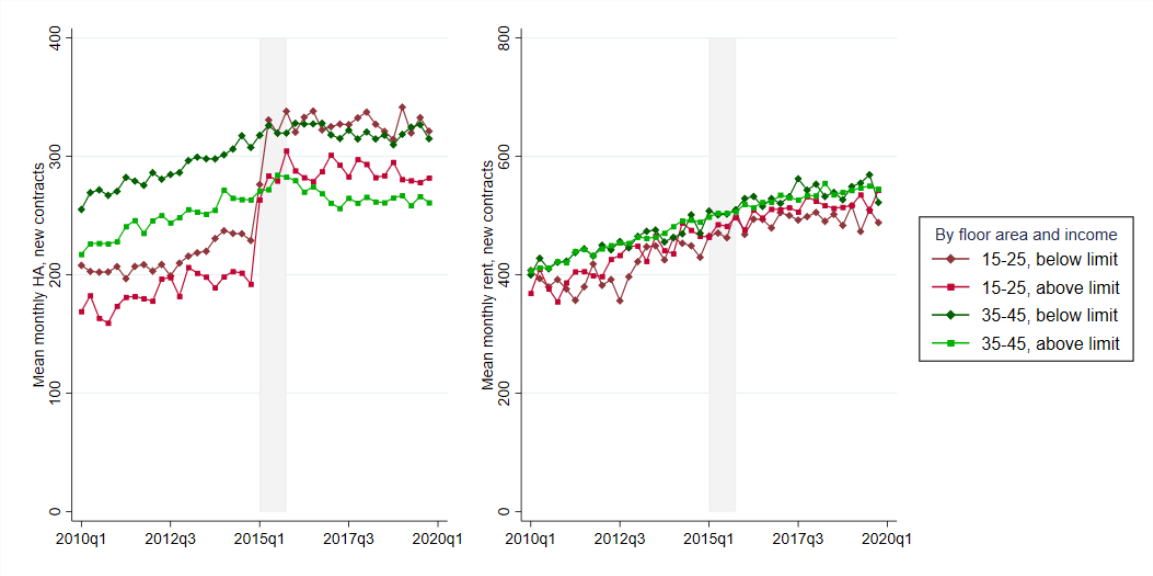


Figure C1: Mean HA and rents in 15-25m^2 and 35-45m^2 apartments by income group (below or above the basic amount of social assistance).

Notes: Mean monthly HA paid to recipient households and mean monthly rents for groups with income below and above the basic amount of social assistance, aggregated to quarterly level. The light gray shaded area refers to year 2015. New rental contracts, single-member households. For details on identifying new rental contracts, see Appendix A.

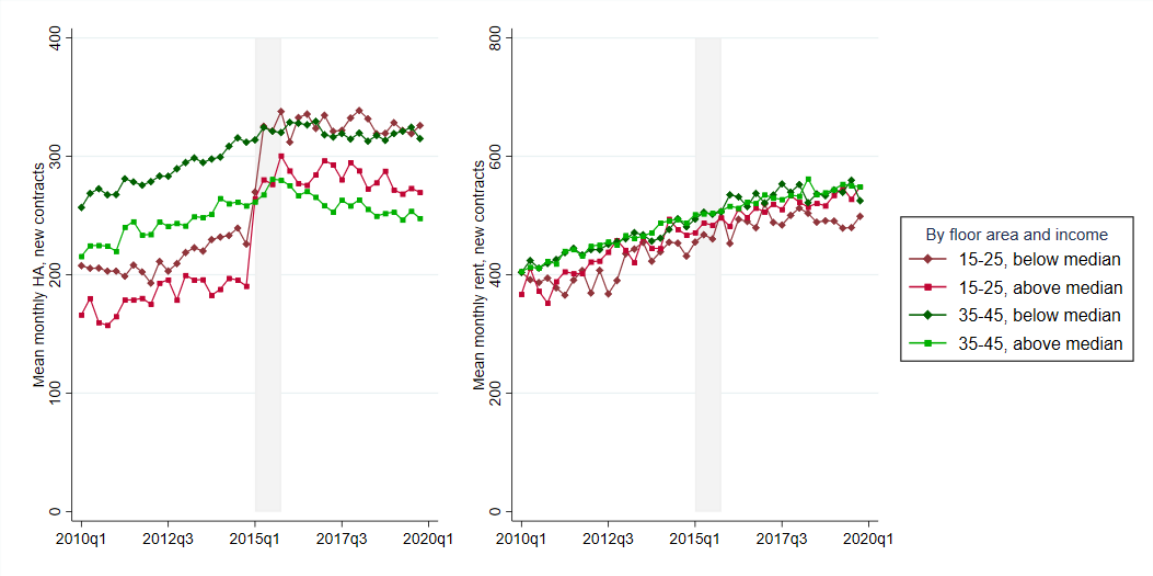


Figure C2: Mean HA and rents in 15-25m^2 and 35-45m^2 apartments by income group (below or above the sample median)

Notes: Mean monthly HA paid to recipient households and mean monthly rents for groups with below and above median income, aggregated to quarterly level. The light gray shaded area refers to year 2015. New rental contracts, single-member households.

Tables C1 and C2 repeat the same specification as in Table 2. In both tables, the left panel shows the results for the sub-sample with households eligible for social assistance and the right panel for non-eligible households. The first two columns report the estimates from a reduced-form regression of rents and HAs on a treat×post indicator. The third column reports the DID-IV estimate when the level of HA is instrumented for by using the treat×post indicator.

In both tables, the average difference in HA between the treatment and control group increased by roughly 80 euros/month. The mean difference in rents increased by 4.6 or 5.5 euros for households eligible for social assistance and by 7.5 or 7.1 euros non-eligible households depending on how we split the sample into two income groups. The differences between the point estimates are small relative to standard errors implying that the estimates are not statistically different.

Table C1: DID-IV estimates by social assistance eligibility status, comparing 15-25m^2 and 35-45m^2 apartments.

	Below assistance limit			Above assistance limit		
	DID Allowance	DID Rent	DID-IV Rent	DID Allowance	DID Rent	DID-IV Rent
treat × post	83.52***	4.603		79.90***	7.473*	
	(15.47)	(8.039)		(8.399)	(3.314)	
Allowance			0.0551			0.0935*
			(0.0888)			(0.0371)
Month × year FEs	✓	✓	✓	✓	✓	✓
Zipcode FEs	✓	✓	✓	✓	✓	✓
Outcome mean	301.7	491.5	491.5	259.1	499.6	499.6
N	7642	7642	7642	31263	31263	31263
First-stage F			29.16			90.50
SE clustered by	Municip	Municip	Municip	Municip	Municip	Municip

Notes: The table reports results from reduced-form regressions of HAs and rents on a treatment group indicator, year fixed effects and a treat×post indicator. The DID-IV column reports the result from an IV regression where HAs have been instrumented for by the treat×post indicator. The sample consists of single-member households in our main estimation sample. Columns 1-3 refer to single-member households below social assistance income limits (eligible), and columns 4-6 to individuals with incomes above the limit (not eligible). Robust standard errors are reported in parentheses.

Table C2: DID-IV estimates by income groups, comparing 15-25m^2 and 35-45m^2 apartments.

	Below median income			Above median income		
	DID Allowance	DID Rent	DID-IV Rent	DID Allowance	DID Rent	DID-IV Rent
treat × post	82.95***	5.502		80.85***	7.131	
	(14.39)	(5.590)		(8.694)	(3.694)	
Allowance			0.0663			0.0882*
			(0.0599)			(0.0415)
Month × year FEs	✓	✓	✓	✓	✓	✓
Zipcode FEs	✓	✓	✓	✓	✓	✓
Outcome mean	302.3	492.3	492.3	253.9	500.5	500.5
N	10952	10952	10952	27919	27919	27919
First-stage F			33.22			86.47
SE clustered by	Municip	Municip	Municip	Municip	Municip	Municip

Notes: The table reports results from reduced-form regressions of HAs and rents on a treatment group indicator, year fixed effects and a treat×post indicator. The DID-IV column reports the result from an IV regression where HAs have been instrumented for by the treat×post indicator. The sample consists of single-member households in our main estimation sample. Columns 1-3 refer to single-member households with below-median incomes, and columns 4-6 to individuals with above-median incomes. Robust standard errors are reported in parentheses.

D Household characteristics

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

First, Figure D1 describes the average household size in different floor area groups. The figure indicates that a majority of the recipients with new rental contracts in our treatment (15-25m^2) and control (35-45m^2) apartments were single-member households, and the reform did not have any effect on this.

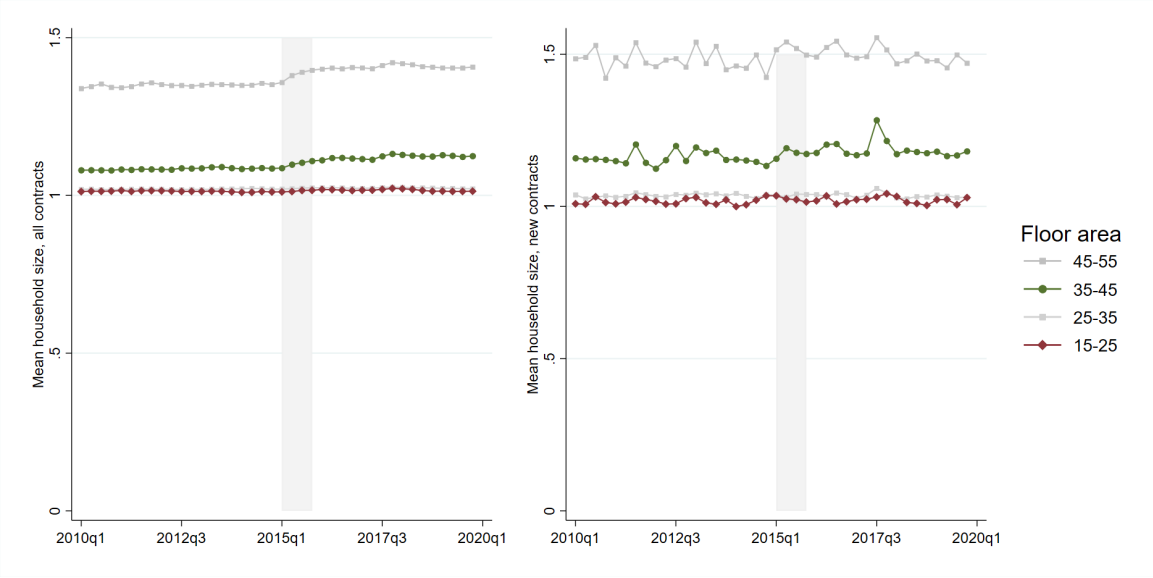


Figure D1: Mean household size in different floor area groups. All payments (left) and new rental contracts (right).

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.

Figure D2 describes the evolution of household incomes in different floor area groups. While average incomes do increase over time, there are no significant changes in the time trends after the reform. Together, these figures suggest that household composition in different types of apartments did not change significantly.

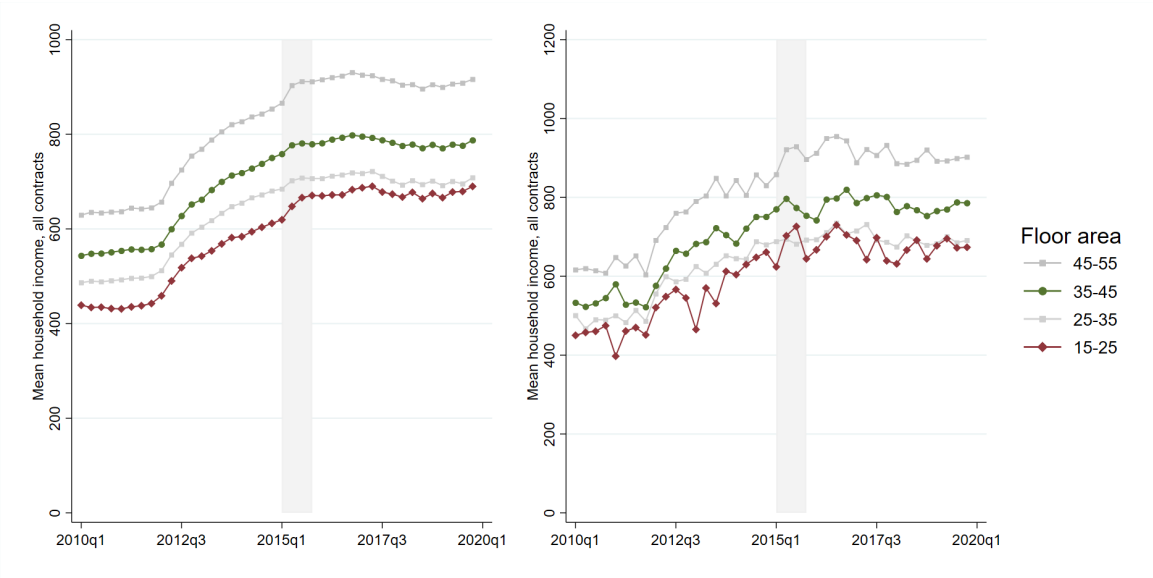


Figure D2: Mean household income in different floor area groups. All payments (left) and new rental contracts (right).

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.