Do Landlords Capitalize on Housing Allowance?

First Empirical Analysis of the Danish Housing Allowance Scheme

1 Introduction

Does housing allowance benefit tenants? Governments devote substantial resources to housing subsidies, making it crucial to understand whether and to what extent landlords capture part of these benefits through higher rents. This question has been examined empirically in several countries, yet no research has investigated it in a Danish context. Differences in housing markets, rent regulation, and institutional design between countries imply that findings from other settings cannot just be generalized. On the contrary, studies across countries vary greatly in their predicted rent capitalization effect of housing allowance. For this reason, the Danish case requires its own methodological approach and its own empirical analysis.

This thesis makes two contributions to the literature:

1. We provide the first empirical analysis of rent capitalization of the Danish housing allowance scheme, addressing a previously unstudied but policy-relevant question.
2. We are the first to apply a regression kink design to examine rent responses to housing subsidies, contributing a new empirical strategy to the rent-capitalization literature.

Previous international studies have relied primarily on a difference-in-differences approach. By contrast, our approach exploits a change in the marginal subsidy rate generated by the rule that single-person households receive allowance only for the first $65m^2$ of their dwelling. Because landlords observe dwelling size, this kink creates an incentive channel through which potential rent capitalization can occur — making it well suited for a quasi-experimental design.

We contribute to this literature by providing the first empirical evidence on rent capitalization of housing allowance in Denmark. Our approach exploits a unique feature of the Danish system: only a limited share of apartment size is eligible for subsidy calculation. For single-person households, only the first 65 m^2 are included in the allowance computation. This rule induces a discrete change in the marginal subsidy once apartment size exceeds 65 m^2 and crucially because landlords know the size of their dwellings this is a feasible way for them to capitalize on the housing allowance. This is what forms the basis for our empirical strategy.

We exploit this kink to estimate whether landlords capitalize housing allowance into higher rents. Specifically, we employ a fuzzy regression kink design (RKD) using dwelling size as the running variable and the 65 m^2 threshold as the kink point. Under the identifying assumptions of the RKD, this setting provides quasi-experimental variation in housing allowance intensity. We estimate a local average treatment effect using a two-stage least squares specification, where the kink-induced change in the slope of the allowance schedule serves as an instrument for housing allowance.

Our results paint a somewhat mixed picture when considering the full sample. While we find no robust evidence of rent capitalization at the national level, the pattern changes markedly when focusing on Denmark's largest cities. In urban areas—where housing supply is more inelastic and rent regulation less prevalent—we find a consistently positive and statistically significant effect of housing allowance on rent across bandwidths. In contrast, for the rest of the country, the estimated effects are generally insignificant. These results suggest that rent capitalization may occur primarily in markets with tighter supply constraints, aligning with theoretical predictions.

Given the volatility of our estimates, we refrain from interpreting the precise magnitude of the rent pass-through. Instead, we emphasize the methodological contribution of our study. This thesis makes two key contributions. First, we demonstrate how a regression kink design can be used to identify potential rent seeking behavior in the rental housing market - an approach not previously used in this literature. Second, we provide empirical motivation for further research on housing allowance effects in Denmark by showing evidence consistent with rent capitalization, at least in parts of the country. Future studies could build on our framework to more precisely estimate the extent and underlying mechanisms of such effects. By developing a methodological framework tailored to the Danish context, we hope to lay the foundation for future research on housing allowance in Denmark.

In the housing allowance literature, empirical evidence on the effect of housing allowances on rent is mixed. Several earlier studies from the 2000s document substantial effects. Using French data, Fack (2005) finds that up to 78% of the housing allowance incidence falls on landlords. Gibbons and Manning (2006) report effects of comparable magnitude in the UK, while Kangasharju (2010), using Finnish data, estimates an incidence of 60–70%. All three studies employ a difference-in-differences (DiD) research design.

More recent studies tend to find little or no effect of housing allowances on rent. Brewer et al. (2019), using UK data and a DiD design, find that in the short run, 90% of the allowance benefit falls on tenants rather than landlords. Eerola and Lyytikäinen (2019), using a regression discontinuity design to Finnish data, find no significant rent discontinuities across multiple cut-off points in the housing allowance scheme. Finally, Eerola et al. (2024) find minimal effects on rents in Finland and argue that this is due to low demand- and supply-side responsiveness to variation in housing allowance.

This thesis makes an important contribution to the literature by extending the analysis of housing allowance incidence to the Danish context. While most existing studies employ DiD approaches, we instead estimate a local treatment effect using a regression kink design. Methodologically, this approach is closely related to the RDD framework used by Eerola and Lyytikäinen (2019). Our design is also inspired by Berger et al. (2020), who apply an RKD to study subsidies for homeownership. However, we are the first to employ an RKD to examine subsidy effects on the rental housing market.

2 Institutional Setting

2.1 Rental Market in Denmark

The rental market in Denmark consists of private housing and social housing. In 2024, 49% of the rental dwellings was social housing (refrence: https://bl.dk/viden-kartotek/boliger/). By Danish law, social housing must be run non-profit meaning that the rent is determined only by the landlord's costs of the dwelling. Therefore, the rent in social housing will typically not vary with the rent prizes in the private market. This rent-restriction means that we don't expect rents in social housing to be affected by differences in housing allowance.

In the private rental sector there are three ways the rent can be determined: cost determined rent, *the rental value* and free rent setting. As a baseline, the rent of dwellings in properties occupied before 1992 is determined by the landlord's costs of the dwelling. This is similar to social housing, with the exception that a fixed return on capital for the landlords is priced in. A dwelling in a property occupied before 1992 can be transferred to *the rental value* if it is modernized by the landlord. *The rental value* means that the rent is determined by the general rent level of other dwellings under *the rental value* in the same area and with a similar size, quality etc. *The rental value* was introduced in 1996 and since then many dwellings have transferred from cost determined rent to *the rental value* as a result of landlords modernizing dwellings. For dwellings in properties occupied after 1991 the rent is determined freely between the landlord and tenant resulting in a market-based rent setting.

These three rent setting rules have different implications for the possibility of landlord rent seeking. Cost determined rent can by construction not be affected by differences in housing allowance. - Can there be manipulation in the apartments under rent control, by renovating certain apartments? *The rental value* should not be affected either since landlords are bound by a general rent level. Perhaps a spill-over effect from the market-based rents is possible, but we don't expect *the rental value* to be adjusted to specific housing allowance rules. However, the rent of dwellings under free rent setting can be exposed to rent seeking, since it is possible for landlords to price differences in housing allowance into the rent.

2.2 Housing Allowance Scheme

In Denmark, housing policy has been a part of the Scandinavian welfare model where it has been a goal to ensure payable quality housing for everyone (reference: https://lex.dk/boligpolitik). One aim to achieve this goal is through subsidy schemes such as housing allowance. Besides playing a role in the housing market, housing allowance also functions as income support as it is directly tied to household income. In 2018, 600.255 Danish households received housing allowance and the allowance is therefore not confined to only the very poor, but rather serves a broader redistributive goal (reference: https://www.dst.dk/da/Statistik/emner/sociale-forhold/social-stoette/boligstoette).

In 2018, housing allowance expenditures in Denmark amounted to 14.780.000.000 kr., taking up 2.02% of the total social spending of the Danish government. Throughout our period of analysis, 2007-2018, the housing allowance's share of total social spending was stable at around 2% (footnote to table). This implies that housing allowance for many years has been a significant part of the Danish welfare scheme.

In Denmark, there's an allowance scheme for both homeowners and renters. The housing allowance for renters is split into two schemes with separate rules. There's a general allowance targeted at working-age citizens (boligsikring) and there's a specific allowance for pensioners (boligydelse). Throughout this thesis, we examine only the general allowance for renters, boligsikring.

The Danish housing allowance scheme for a single person dwelling:

$$HA_i = min\left[HAlimit, 0.6\left[min\left[ER_i, rentlimit\right]\right] - deductible_i\right] \tag{1}$$

$$ER_i = \begin{cases} rent_i, & \text{if } m^2 \leq 65 \\ rent_i \cdot \dfrac{65}{m^2}, & \text{if } m^2 > 65 \end{cases}, \tag{2}$$

where HA is housing allowance and ER is effective rent.

The scheme consists of two parts: the rent share and the deductible.

The rent share is what gets added to a tenants housing allowance and it depends on the size of the rent. As a baseline, the rent share is given by 0.6 times the rent. However, a tenant can only receive housing allowance for 65 m^2, so if the dwelling exceeds this size, the effective rent used to calculate the HA is 65 times the rent per m^2. It is this m^2-limit of 65 that creates a kink in the housing allowance scheme, which we are going to use later in our empirical analysis. For each additional tenant in a household the m^2-limit is raised by 20, e.g. a two person household can receive housing allowance for 85 m^2 of the dwelling size. These floor area limits have been implemented since 1991 (KILDE). The rent share is calculated by the effective rent up to a certain rent limit.

The deductible depends both on the household income and assets. For incomes below a certain income limit the deductible is zero. For incomes above the limit there's a deduction in the HA depending on the size of the income. This ensures that the housing allowance is generally higher for poorer households. If the assets of a household exceed a certain limit, there is a further deduction in the housing allowance. Since this limit is fairly high (1.011.700 kr. in 2025), it will not be relevant for many receivers of housing allowance.

When both rent, income and assets have been accounted for, the housing allowance still can't exceed a certain housing allowance limit, HAlimit. The HA, rent, income and asset limits are changed yearly to account for inflation.

3 Theory

In this section, we set up a theoretical framework that will enable us to make predictions about the rent capitalization effect of housing allowance. First, we discuss the assumptions of our setup. Next, we build a simple model for the housing allowance in the Danish rental market.

3.1 Perfect competition assumptions

To simplify the setup for our theoretical predictions, we will consider how a subsidy affects the price in a market characterized by perfect competition. We will discuss the assumptions underlying this perfect competition setup in the context of the rental housing market.

Under perfect competition homogeneous goods is assumed. In a rental housing market context this assumption seems unrealistic, since dwellings have unique characteristics such as its physical structure and location. We will, however, think of a m^2 as the quantity unit in our setup where a m^2 is identical across the market. We make this assumption since the housing allowance in Denmark is effectively a per m^2-subsidy. Under perfect competition we also assume price taking behavior, which seems more realistic in the rental market, since there are typically many landlords and tenants. Perhaps there are some areas with only a few landlords, where price taking behavior seems less propable. Finally, under perfect competition we assume that there is one unit price in the market. This assumption is not realistic, since one extra m^2 might change in price for different dwelling sizes.

We try to accommodate for the criticism of these assumptions by the following: Instead of thinking of the rental market as one single market, we can imagine that the housing market is divided into submarkets within different locations and size ranges. The price of a m^2 can then vary between submarkets. Within each submarket, there will typically be many tenants and landlords, which could validate the assumption of price taking behavior. Dwellings are more similar within local areas and sizes so the homogeneous goods assumption seem more probable within submarkets. If housing allowance rules imply different subsidy levels across submarkets this could result in subsidy-induced differences in rent per m^2, which is exactly what we wish to examine.

Additional criticisms of assuming perfect competition in the rental housing market:

- Friction in the housing market. Moving to a new dwelling can be costly and searching for one can take time. This means there are a lot of additional costs tied to changing the consumption of m^2's which we don't account for in our setup.
- If tenants are not fully aware of the housing allowance rules, they might not be pricing the housing allowance into higher willingness to pay for dwellings that are subsidized more generously.
- There is not equal access to dwellings for tenants. Dwellings might not be allocated solely according to willingness to pay, but also do to for example nepotism.

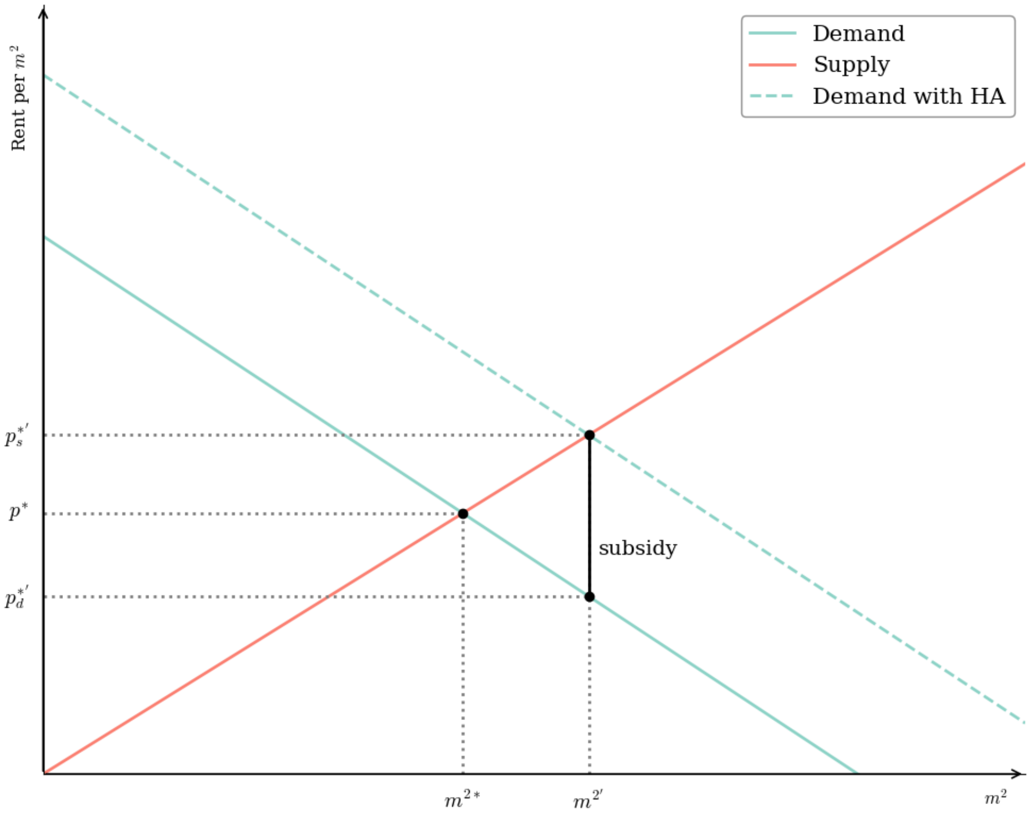
There is not equal access to dwellings for tenants. Dwellings might not be allocated solely according to willingness to pay, but also do to for example nepotism. There can be a lot of friction in the housing market. Moving to a new dwelling can be quite costly and searching for one can take time. This means there are a lot of additional costs tied to changing the consumption of m^2's which we don't account for in our setup. If tenants are not

Acknowledging the simplifications we impose, we proceed to look at the effect of a subsidy under perfect competition.

3.2 Subsidy incidence

In Denmark, housing allowance is a subsidy paid by the government to renters in the form of a cash transfer. Therefore, we can think of it as a Pigou-subsidy granted per m^2 consumed. Microeconomic theory predicts that such a subsidy will shift the demand curve upward by exactly the amount of the subsidy. This shift reflects a higher willingness to pay for each m^2 that is subsidized. This is illustrated in Figure 1.

Figure 1: Subsidy incidence



The subsidy creates two effective prizes in the housing market. p_s^* corresponds to the rent and p_d^* to the rent minus subsidy, i.e. the effective prize that renters have to pay per m^2 post subsidy. We can formalize the determinants of how the subsidy affects the relative prizes of consumers and producers:

Assume a supply- and demand function $S(p)$, $D(p)$ and a subsidy $a \geq 0$. Such a subsidy will create two effective prizes in the housing market, p_d^* and p_s^*, as shown in Figure 1. These new equilibrium prizes both depend on a, where $p_s^* - p_d^* = a$. The equilibrium condition in the market is:

$$S(p_s^*) = D(p_d^*) \Leftrightarrow$$
$$S(p_s^*) = D(p_s^* - a)$$

We differentiate both sides wrt. a using the chain rule, since p_d^* and p_s^* depend on a:

$$S'(p_s^*)\frac{\partial p_s^*}{\partial a} = D'(p_s^* - a)\left(\frac{\partial p_s^*}{\partial a} - 1\right)$$

We isolate the derivative of the producers' prize:

$$\frac{\partial p_s^*}{\partial a} = -\frac{D'(p_s^* - a)}{S'(p_s^*) - D'(p_s^* - a)}$$

We now evaluate this derivative at $a = 0$, i.e examining how the producers' prize changes when we introduce a tiny subsidy:

$$\left.\frac{\partial p_s^*}{\partial a}\right|_{a=0} = -\frac{D'(p^*)}{S'(p^*) - D'(p^*)} \Leftrightarrow$$
$$\left.\frac{\partial p_s^*}{\partial a}\right|_{a=0} = -\frac{D'(p^*)\frac{p^*}{q^*}}{S'(p^*)\frac{p^*}{q^*} - D'(p^*)\frac{p^*}{q^*}} \Leftrightarrow$$
$$\left.\frac{\partial p_s^*}{\partial a}\right|_{a=0} = -\frac{D'(p^*)\frac{p^*}{D(p^*)}}{S'(p^*)\frac{p^*}{S(p^*)} - D'(p^*)\frac{p^*}{D(p^*)}}$$

We see that these are just the formulas for the prize elasticities of demand and supply:

$$\left.\frac{\partial p_s^*}{\partial a}\right|_{a=0} = -\frac{\varepsilon_d}{\varepsilon_s - \varepsilon_d} \Leftrightarrow$$
$$\left.\frac{\partial p_s^*}{\partial a}\right|_{a=0} = \frac{|\varepsilon_d|}{\varepsilon_s + |\varepsilon_d|} \equiv \kappa$$

This expression tells us that when introducing a subsidy, the effect on the producers' prize depends on the relative prize elasticities of demand and supply. In our simplified setup, this corresponds to the effect of housing allowance on rent. κ will only be an approximation of this, since we're evaluating the derivative at $a = 0$.

$$\kappa \in [0,1]$$

A perfectly inelastic supply implies that $\kappa = 1$ meaning all of the incidence fall on the landlords. In contrast, a perfectly elastic supply implies $\kappa = 0$ meaning that all incidence falls on tenants. Landlord incidence depends negatively on the supply elasticity and positively on the demand elasticity. A high κ implies that housing allowance translates a lot into higher rent, i.e. a large degree of rent capitalization.

3.3 Rent restrictions

The structure of the rental market in Denmark implies that not all rents will be able to respond to housing allowance. For both social housing, cost-dependent private rental units and dwellings where rent is determined by *the rental value*, landlords will not be able to freely adjust rents according to the housing allowance rules. This means that a share of the rental market will by construction not respond to changes in housing allowance. Call this share of dwellings θ. The share of dwellings where the rent can actually be affected by differences in housing allowance will be $(1 - \theta)$.

3.4 Rent capitalization

Denote the effect from a change in housing allowance onto rent by ρ. Given the perfect competition assumption and the structure of the Danish rental market, an approximation of the theoretical rent capitalization will be:

$$\rho = \Delta a \cdot \kappa \cdot (1 - \theta) \tag{3}$$

We see that for a given change in housing allowance, Δa, the effect on rent depends on the relative elasticities of housing demand and supply, as well as on the share of dwellings with free rent setting. If either housing supply is elastic relative to demand or many dwellings are rent restricted, ρ will go towards zero.

From this setup we can hypothesize both a significant rent capitalization effect and a small and negligible effect, depending on κ and $(1-\theta)$. Specifically, for a 1 DKK increase in housing allowance, $\Delta a = 1$, the theoretical rent capitalization effect is:

$$\rho \in [0{,}1] \tag{4}$$

We should expect that areas with a more inelastic housing supply, high κ, have a stronger rent capitalization effect. For a given outward shift in demand, as in Figure 1, markets characterized by inelastic housing supply cannot expand the housing stock very much in response. As a result, the adjustment occurs primarily through prices rather than quantities, leading to a larger increase in rents compared with markets where supply is more elastic. On this basis, we hypothesize that ρ will be larger for areas with a more inelastic housing supply. We test this hypothesis in the empirical analysis.

We also expect areas with a large share of dwellings with free rent setting, high $(1-\theta)$, to have a stronger rent capitalization effect. Unfortunately, we don't have access to data that allows us to look specifically at the $(1-\theta)$ share. We hope for future research to empirically examine the rent capitalization effect for the $(1-\theta)$ share of dwellings.

4 Presentation of the Data

In this section we explain the data foundation of the analysis, as well as describe some of the characteristics of the main variables. We further graphically investigate the first stage identifying kink, to test for the validity of the research design implemented in the thesis.

4.1 Data

The thesis is based on data from Statistics Denmark and is obtained through access to micro data via Statistics Denmark's researcher access. Here variables across multiple registers can be combined and analyzed. The core variables needed for the analysis, are sourced from the *BOSA* (housing allowance) register, where we have variables for rent, housing allowance and apartment size in discrete m^2. We furthermore have variables for both municipality and parish (sogn) from the Danish address register, *DAR.* We additionally have a variable for age from the population register *BEF* and an income variable from the income register *IND.*

On the basis of DAR, it is also identified how many people live on the address of a given person. As the identifying rule kink in the housing allowance scheme is only valid for people, who live alone, we focus on one person households for this analysis. Of the households in Denmark, that receive housing allowance, 50 % consists of one person - at least in our sample period. The data spans across 12 years from 2007-2018, and there are between 33.000 and 71.000 observations for each year. This in total leaves us with a dataset of 707.000 observations. The data set contains people, that receive housing allowance in all of the years, some of the years and in only one year. As described earlier, the housing allowance limits rises throughout the years as it is adjusted to account for inflation.

As can be seen from Table 1, the mean monthly rent for the entire sample is around 3900 DKK, while the mean monthly housing allowance is close to 600 DKK. That means, that the average coverage rate of the housing allowance in the full sample is about 15%. Both the rent and housing allowance clearly spans wide, with the dataset containing both renters with very cheap apartments and receiving very little in housing allowance as well as someone living in both very expensive apartments and receiving a lot in housing allowance. When someone is observed to have a housing allowance above the limit for one person household (which was 1008 DKK in 2018), it stems from them being eligible for extra benefits e.g. because of disability-related expenses. As shown in Table 1, the mean floor area is $59m^2$, which is close to the cutoff of $65m^2$, where the full amount of the rent stops being subsidized. Descriptive statistics on the covariates age and income is also reported in the table.

Table 1: Descriptive statistics

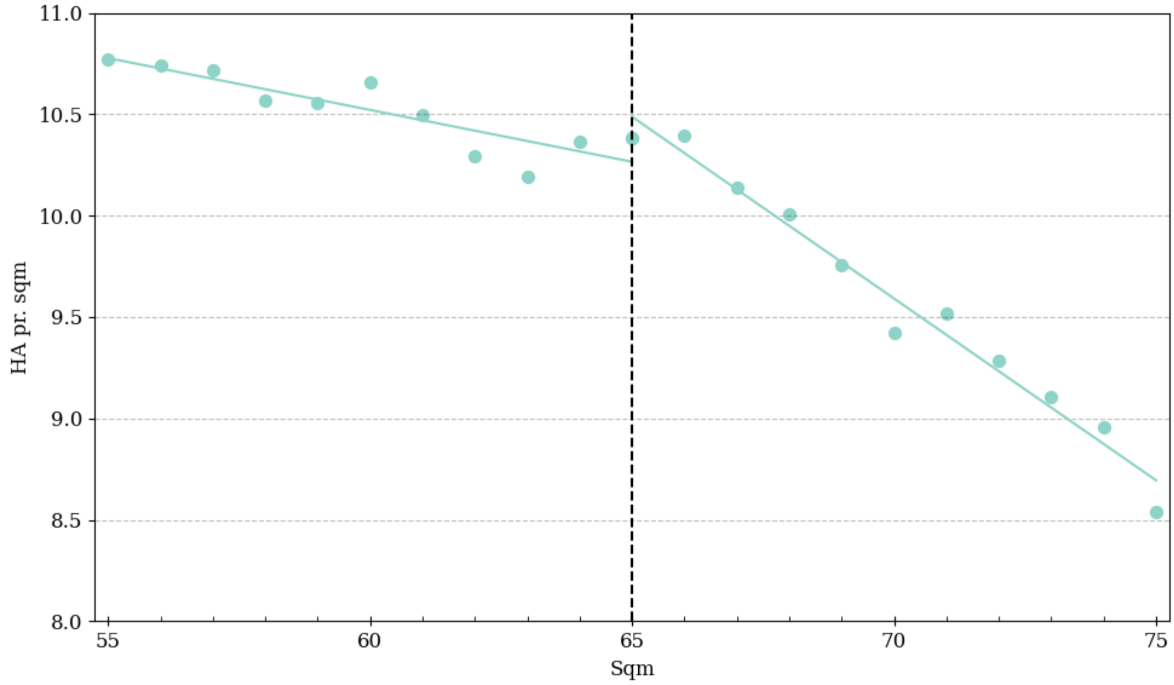
Variable	Mean	Sd	P1 mean	P99 mean
Rent (monthly)	3896 DKK	2.061 DKK	1879 DKK	10.662 DKK
Housing allowance (monthly)	595 DKK	269 DKK	255 DKK	2.643 DKK
Floor area	59 m^2	20 m^2	23 m^2	134 m^2
Age	36	14	18	65
Income (monthly)	14.894 DKK	6.770 DKK	4.930 DKK	37.200 DKK

The data employed in this thesis is based on available data through our student jobs. This means, that not every data register at DST was available for us to use. We for example didn't have access to the Danish Housing register *BBR Enhed*, which can distinguish social housing from private rental housing. We also didn't have access to any dwelling characteristics.

4.2 Identifying kink

The necessary condition for a regression kink design is the first stage identifying kink. As this analysis is based on a fuzzy first stage kink, it is necessary to investigate, that the first stage kink is evident. In this analysis, the identifying kink is the reduction in the housing allowance per m^2, that is imposed on apartments above the cutoff of $65m^2$ following the rule described in **section 2.2**. To visually check this dynamic, we inspect the scatter plot of housing allowance per m^2 for apartment sizes just under and just over the cutoff. Figure 2 shows housing allowance per m^2 plotted across different m^2's. We see a clear kink in the housing allowance at the $65m^2$ cutoff. As mentioned, this is expected since the housing allowance scheme contains a discontinuous change in the marginal subsidy at that threshold. There so seems to be visual evidence of a kink in our first stage identifying kink.

Figure 2: First stage scatter plot: Housing Allowance per m^2



5 Empirical Strategy

The causal relationship of interest is the effect of housing allowance on rents. In the ideal experiment, we would hand out varying amounts of housing allowance to randomly selected households. Furthermore, we would inform each landlord exactly how much housing allowance is received by each household. This experiment would ensure that no specific characteristics are reflected in the housing allowance receivers meaning there would be exogenous variation in housing allowance receipt. The experiment also ensures that rent seeking is possible for the landlords (assuming rents aren't bound by legal restrictions). In the real world however, housing allowance isn't distributed randomly and landlords are not informed of the households' housing allowance payments. We therefore need an empirical strategy that can work around these issues. We first present the identification issues using a simple OLS strategy. Next, we propose a fuzzy regression kink design as our empirical strategy.

5.1 Identification issues using OLS

If an OLS strategy were to be implemented, it would be of the following form:

$$rent_i = \delta_0 + \sum_{k=1}^{4} \delta_k I_{ki} + \delta_5 X_i + \delta_6 HA_i + \varepsilon_i \tag{5}$$

where, for household i, r_i is the rent,I_{ki} is a dummy equal to one if household i's income belongs to the k'th quantile, X_i is a set of other household characteristics and HA_i is the housing allowance received by the household. δ_6 is the parameter of interest.

There are two ways that this OLS strategy could lead h_i to be endogenous, potentially biasing the estimate of δ_6.

1. **Reversed causality**: Because the housing allowance is partly determined by the household's rent, any unobserved factors that increase rent will also mechanically increase the housing allowance, leading to an upward bias in δ_6. This also opens the possibility of a feedback effect where a shock to rent will affect housing allowance which in turn will affect rent and so on. However, reverse causality is only relevant for part of the sample, since there is an upper limit on the amount of rent that can influence the housing allowance. Thus, only households with rents below this threshold would experience an effect of rent on the allowance.

2. **Unobserved confounders**: Although the specification in equation (1) controls for income and other observable household characteristics, non-observable factors may still remain. For example, households with higher rents may be more proactive in securing housing allowance. In such a case, higher rent would be associated with higher allowance not only because of landlord rent-seeking, but also due to unobserved household traits, again biasing δ_6 upward.

5.2 Regression kink design

In order to estimate a causal effect from housing allowance onto rent, we need to exploit exogenous differences in housing allowance across households. Equation (2) from the Danish housing allowance scheme specifies a kink in the effective rent used to calculate a household's housing allowance payment. Given certain assumptions, this kink presents exogenous variation in housing allowance that can be used to examine the rent capitalization effect. We use this kink in the HA scheme to employ a regression kink design.

5.2.1 Fuzzy RKD

The regression kink design exploits a discontinuity in the slope of the treatment assignment rule with respect to a running variable at a known cutoff. This kink creates a quasi-experimental setting where individuals just below and just above the cutoff face different marginal changes in treatment intensity. If the kink in the treatment rule is exogenous - meaning it affects the outcome only through its impact on the treatment variable - then any corresponding kink in the outcome at the same point can be interpreted causally. The causal effect of the treatment is identified by the ratio of the change in the slope of the outcome variable to the change in the slope of the treatment variable at the cutoff. In our setting, where housing allowance is the treatment and rent is the outcome, the local average treatment effect is identified as:

$$\rho = \frac{lim_{m^2 \downarrow 65} \frac{\partial E[Rent|m^2]}{\partial m^2} - lim_{m^2 \uparrow 65} \frac{\partial E[Rent|m^2]}{\partial m^2}}{lim_{m^2 \downarrow 65} \frac{\partial E[HA|m^2]}{\partial m^2} - lim_{m^2 \uparrow 65} \frac{\partial E[HA|m^2]}{\partial m^2}} \tag{6}$$

This just says that as we approach the cutoff of $65m^2$, the change in slope of the conditional mean of rent relative to the change in slope of the conditional mean of housing allowance is the local average treatment effect of housing allowance on rent. This ρ is what we try to estimate empirically.

Notice that the denominator in equation (5) includes the conditional mean of housing allowance. This is because housing allowance is not a deterministic function of m^2. Equation (1) shows two limits in the HA scheme, the HA-limit and the rent limit. Households that are at either of these limits will not experience a kink in their HA-scheme. This imply that not all households with $m^2 > 65$ experience a change in treatment intensity. Since we have a scheme where only some households change in treatment intensity above the cutoff, we need to employ a fuzzy regression kink design. The fuzzy regression kink design will account for the fact that for a given household there's only a probability of a kink in the slope of the treatment assignment rule at the cutoff.

5.2.2 Identifying assumptions

A key identifying assumption in an RKD is that potential outcomes are smooth around the cutoff. This means that in the absence of a kink in the treatment intensity, there should be no kink in the outcome. In our case, this assumption holds if in the absence of the $65m^2$ rule, the rent per m^2 is smooth through that threshold. An argument in favor of this assumption is that the $65m^2$ rule is specific to the housing allowance scheme. To our knowledge, no other rules in the housing market are built around the $65m^2$ dwelling size.

We cannot formally test the assumption of smooth potential outcomes. We can, however, use placebo cutoffs to test that kinks in rent per m^2 do not arise at other values of the running variable close to the $65m^2$ policy cutoff. This would strengthen the assumption of smooth potential outcomes at the true cutoff.

Another identifying assumption of the RKD is that covariates are smooth around the cutoff. If a kink arises in the covariates at the cutoff, we will not be able to separate the effect of these kinks on the outcome from the effect of the treatment kink on the outcome. In our case, if there is a kink in the covariates, we will not be able to identify a kink in the rent per m^2 as a direct effect of differences in housing allowance. This could happen if the $65m^2$ rule affects behavior in other variables. We test this assumption in our empirical analysis.

Another crucial assumption is that there's no manipulation of the running variable. If individuals can affect the running variable in response to the cutoff they can select themselves into a specific treatment intensity. Then the individuals we compare just below and above the cutoff will not necessarily be comparable and we get a biased ρ-estimate. To test this assumption we can look for bunching in the density of the running variable. In our case, we can check whether the slope of the density of households across m^2 seem to change sharply at the cutoff.

5.3 The Model

In order to estimate ρ from equation (5), we will use the following two regressions, the first stage and the reduced form of our fuzzy regression kink design:

$$HA_i = \beta_0 + \beta_1(m_i^2 - 65) + \beta_2\mathbf{1}(m^2 > 65)\cdot(m_i^2 - 65) + \omega X_i + \varepsilon_i \tag{7}$$
$$R_i = \alpha_0 + \alpha_1(m_i^2 - 65) + \alpha_2\mathbf{1}(m^2 > 65)\cdot(m_i^2 - 65) + \mu X_i + \xi_i \tag{8}$$

The running variable is centralized at 0 around the $65m^2$ cutoff, so that the estimates reflect slope changes at that cutoff. HA_i is housing allowance per m^2 and R_i is rent per m^2. β_1 and α_1 measures the slopes before the cutoff. β_2 measures the change in slope of housing allowance at the $65m^2$ cutoff. α_2 measures the corresponding change in slope of rent at the $65m^2$ cutoff. Covariates are included in X_i. The ratio of α_2 and β_2 will be our local estimate of the effect from housing allowance on rent:

$$\widehat{\rho} = \frac{\widehat{\alpha_2}}{\widehat{\beta_2}} \tag{9}$$

Equation (6) and (7) correspond to the linear specification of the slope with regards to floor area. In our empirical analysis we will also use a quadratic specification, where squared terms of the slope on each side of the cutoff is included. The estimates of interest will still be $\widehat{\beta_2}$ and $\widehat{\alpha_2}$, since we are looking for changes in the first derivative.

In order to get the right standard errors, we use a 2sls estimator, where the change in slope around the $65m^2$ cutoff is used as an instrument for housing allowance.

$$R_i = \delta_0 + \delta_1(m_i^2 - 65) + \rho\widehat{HA}_i + \mu X_i + \xi_i \tag{10}$$

This is the second stage of our fuzzy RKD which uses the predicted values of housing allowance from equation (6) to estimate ρ, the local effect of housing allowance on rent.

5.3.1 Bandwidth selection

As shown in equation (5), the local average treatment effect is identified as we get arbitrarily close to the cutoff from both sides of the running variable. Therefore, we would ideally like to only use observations just around the cutoff when estimating ρ. This motivates the use of bandwidths in the RKD. The idea is in the estimation to only use observations within a narrow sample window to prevent observations far away from the cutoff to bias the ρ-estimate. However, if the sample window gets too narrow we end up with very few observations and the standard errors will be too big to estimate any effect. This is known as the bias-variance trade-off.

There is no universally optimal method for choosing the bandwidth. However, one method to address the bias-variance trade-off is to use the mean-squared-error (MSE) optimal bandwidth (Imbens and Kalyanaraman (2012). The MSE-optimal bandwidth minimizes the mean squared error of the local estimator by balancing the bias of larger bandwidths with the variance of smaller bandwidths. The MSE-optimal bandwidth is the solution to the following minimization problem:

$$h_{MSE} = arg\min_h \left(Bias(\hat{\rho}(h))^2 + Var(\hat{\rho}(h))\right) \tag{11}$$

In practice, it is computed through a plug-in procedure that estimates the conditional variance and the curvature of the conditional expectation function using local polynomial fits.

While the initial choice of bandwidth should be selected thoughtfully, the most important thing is to show robustness across multiple bandwidths. The convention in the literature is to choose a bandwidth, h, and report estimates using h, half of h and double of h. In our analysis, h corresponds to the MSE-optimal bandwidth, and we therefore report estimates of our model using $0.5 \cdot h_{MSE}$, $1 \cdot h_{MSE}$, and $2 \cdot h_{MSE}$.

Since we use bandwidths, estimation of equation (6) and (9) with a first order polynomial of the floor area will correspond to a local linear regression. Including a second order polynomial of the floor area corresponds to a local quadratic regression. We choose the uniform kernel in all specifications since it is the convention in the RKD literature.

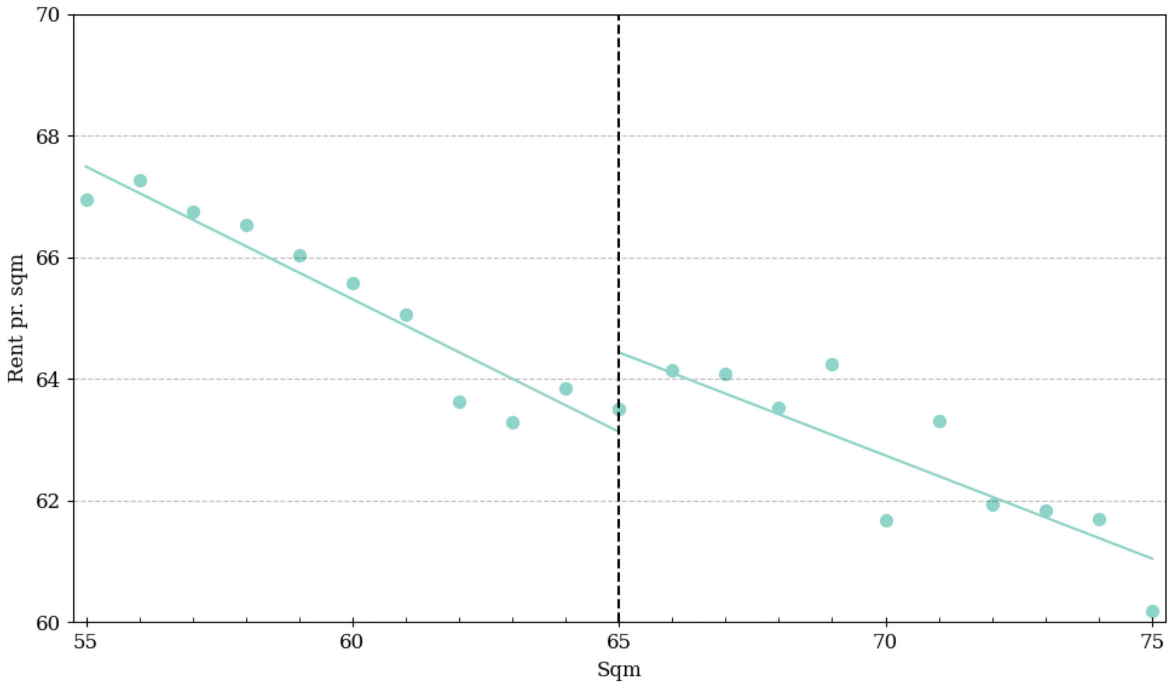
6 Results

6.1 Main results

As shown in section 4, there seems to be an identifying kink, which makes the setup of the analysis valid. We now investigate graphically whether a corresponding kink seems to appear in rent per m^2 around the 65 m^2 cutoff.

Figure 3 shows rent per m^2 plotted across different m^2's. Looking at the data points close to the cutoff, it's difficult to see whether a kink appears, since the data shows some irregular behavior. There is some indication of a jump in the rent per m^2 which we don't expect given the structure of the housing allowance scheme. In the robustness section, section 6.3, we examine this possible jump. When we look at a bandwidth of 10 with a linear fit, the slope doesn't seem to change. Since the data behaves a little different close to the cutoff, we will also rely on a local quadratic regression to account for possible non-linearities. All in all, we can't rule out a rent capitalization effect at the 65 m^2 threshold from our graphical inspection though it doesn't look to be robust across bandwidths. We will rely on our estimations to more precisely discern whether a rent capitalization occurs.

Figure 3: Reduced form scatter plot: Rent per m^2



In Figure 3, there seem to be a lot of noise in the rent per m^2 data. Therefore, estimation is likely to be sensitive to the choice of bandwidth, especially since we have a discrete running variable. This makes it extra crucial to report results for multiple bandwidths. As mentioned in section 5.3.1, we use the MSE-optimal bandwidths of both of our polynomial specifications for out main results. We also report half and double of the MSE-optimal bandwidths.

Table 2 shows the sample windows of the MSE-optimal bandwidths as well as the number of observations in each sample window. Since floor area can only be integers the $0.5 \cdot h_{MSE}$ bandwidths are rounded up to 3 and 4. As mentioned, with a discrete running variable and noisy data our estimate becomes quite bandwidth sensitive. For small bandwidths, we have very few values of the running variable on each side of the cutoff, so sensitivity to the data noise will be high. Therefore, we are careful at interpreting the estimate for the smallest bandwidths. The sample windows include observations $N \in \{104.007, 385.613\}$. These are fairly big samples, which means we have a lot of support on each value of the running variable. At the same time, measuring significant changes in first derivatives can require a lot of data, so samples need to be big.

Table 2: MSE-optimal bandwidths

Bandwidth	Floor area		Number of observations	
	Local linear	Local quadratic	Local linear	Local quadratic
h_{MSE}	60 - 70 m^2	58 - 72 m^2	167.944	221.174
$0.5 \cdot h_{MSE}$	62 - 68 m^2	61 - 69 m^2	104.007	132.995
$2 \cdot h_{MSE}$	55 - 75 m^2	51 - 79 m^2	295.767	385.613

MSE-optimal bandwidths are estimated using the rdbwselect command in Stata.

Table 3 shows our main results. We estimate the first and second stage of the IV-regression in equation (6) and (9) using four different models: A local linear and local quadratic regression with and without covariates and fixed effects. All models are estimated using the MSE-optimal bandwidths reported in Table 3. Models (1)-(4) measure the change in the marginal housing allowance per m^2 from crossing the $65m^2$ threshold. Models (5)-(8) measure the local average effect of a 1 DKK increase in housing allowance on rent.

Table 3: Effect of HA on rent at the $65m^2$ cutoff, IV-estimates

Bandwidth	First stage, β_2				Second stage, ρ			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
h_{MSE}	-0.208***	-0.333***	-0.213***	-0.343***	0.741	1.189	0.531	2.034**
	(0.048)	(0.116)	(0.043)	(0.105)	(1.171)	(1.599)	(0.826)	(1.012)
$0.5 \cdot h_{MSE}$	-0.287***	-0.376	-0.287***	-0.318	1.332	1.890	2.887***	4.635
	(0.095)	(0.292)	(0.088)	(0.276)	(1.451)	(3.245)	(1.057)	(3.134)
$2 \cdot h_{MSE}$	-0.129***	-0.179***	-0.132***	-0.190***	-1.003	-1.766	-0.959	-1.115
	(0.019)	(0.048)	(0.016)	(0.040)	(1.012)	(1.964)	(0.697)	(1.235)
Polynomial order of floor area	First	Second	First	Second	First	Second	First	Second
Covariates & fixed effects	No	No	Yes	Yes	No	No	Yes	Yes

All models are estimated using the MSE-optimal bandwidth of 5 and $7m^2$ to each side of the cutoff for the linear and quadratic regression respectively. The models are also estimated with half and double (rounded up) of the MSE-optimal bandwidths. The number of observations for the different models are $N \in \{104.007, 385.613\}$. See Table 2 for the specific information. We use the uniform kernel in all specifications. The covariates include income, income squared, age and age squared. Fixed effects are by municipality, parish, year and parish x year. The first four columns show the first stage results from the IV-regression, i.e β_2 in equation (5). Columns (5)-(8) show the second stage estimates, i.e ρ in equation (9). (1)-(2) and (5)-(6) don't include any covariates or fixed effects. Robust standard errors clustered at the parish level. *, **, and *** indicate significance at the 10%, 5%, and 1% level.
Result: Across bandwidths and polynomial specifications there is a clear kink in the housing allowance at the 65 m^2 cutoff. There is some indication of an overall effect from housing allowance on rent. It is, however, not robust across bandwidths and imprecisely estimated.

Model (1)-(4) all show a clear first stage kink. All but the local quadratic regression with a bandwidth of $0.5 \cdot h_{MSE}$ show a kink significant at the 1%-level. Figure 8 in the appendix shows that the first stage kink is robust across bandwidths and polynomial order. The main estimate in Model (3) using h_{MSE} is -0.213 which implies that when crossing the $65m^2$ threshold the housing allowance per m^2 is reduced by 0.213 DKK for every extra m^2. At the cutoff, this means a marginal reduction in the housing allowance of 14 DKK.

For the second stage estimates, we first notice that in Models (5) and (6) without covariates and fixed effects all estimates are insignificant do to very large standard errors. This suggests that we might need covariates and fixed effect to discern a possible rent capitalization effect. We turn our focus to Model (7) and (8). The main estimate in Model (7) using h_{MSE} is 0.531 which implies that a 1 DKK increase in housing allowance leads to a 0.531 DKK increase in rent, a rent capitalization effect of approximately 50%. The estimate is, however, insignificant. The main h_{MSE}-estimate from Model (8) is unrealistically high at 2.034, but only borderline significant at the 5%-level.

We see that the results don't seem robust across bandwidths. The ρ-estimate is volatile and overall the standard errors are of a magnitude, where precise parameter interpretation seem nonsensical. For this reason we refrain from interpreting on the exact magnitude of the rent capitalization effect and focus more on the sign and significance of the effect. It is in this context reassuring that the two significant ρ-estimates in Model (7) and (8), though unrealistic in magnitude, have 95% confidence bands within zero to one - the theoretical range of ρ.

Overall the results in Table 3 seem to suggest weak signs of landlords capitalizing on housing allowance through rent. The estimates are, however, imprecise and not very robust across bandwidths which Figure 3 also seem to suggest. The variation in estimates across bandwidths and functional forms, together with the wide confidence intervals, may reflect that any effect, if present, could be too small to be detected in our data. We examine bandwidth sensitivity more thoroughly in the robustness section.

In Table 3, we choose to cluster standard errors on the parish level for two reasons. First, we expect observations to be correlated within parishes, since both the ability to rent seek for landlords are most likely locally similar and since dwellings in the same parish will share location characteristics, affecting the rent. Second, clustering at the parish level yields larger standard errors than using robust SE's and we want to be conservative regarding the inference of our estimates. In Table 6 in the Appendix, we use different types of standard errors and see that the inference is not sensitive to different levels of clustering. Using conventional or robust standard errors does, however, turn estimates more significant.

6.2 Internal validity

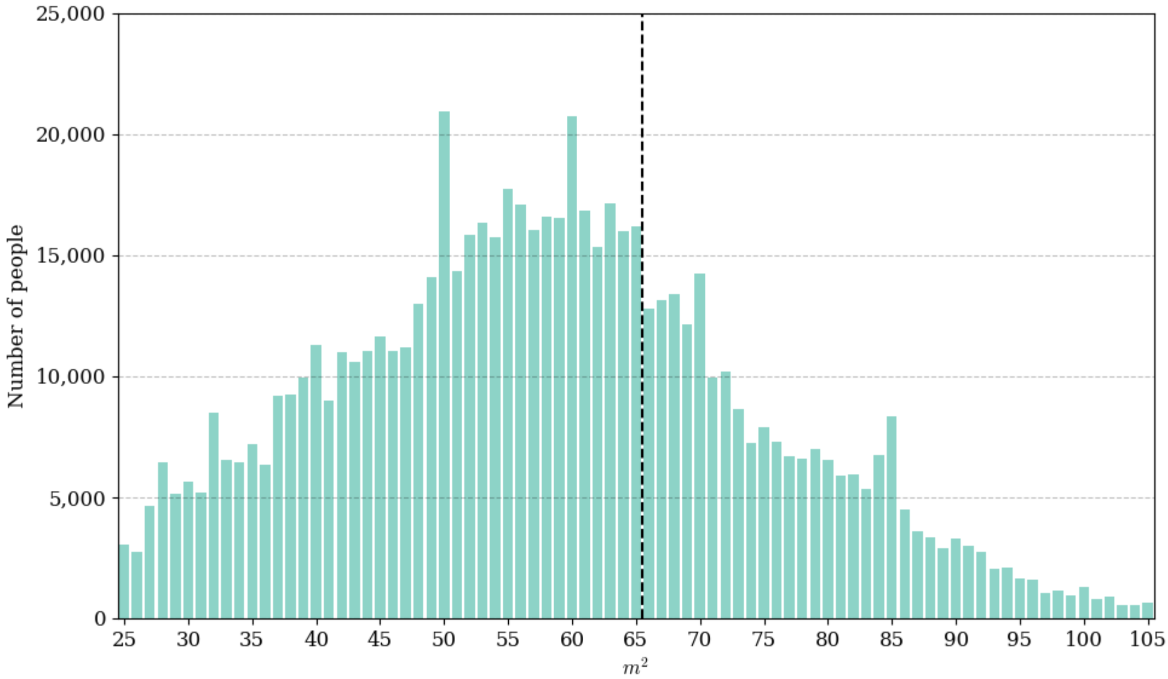
For our research design to measure the causal effect of housing allowance on rent the identifying assumptions in Section 5.2.2 must hold. We test those assumptions through the following specification checks: bunching, smoothness of covariates and placebo cutoffs.

6.2.1 Bunching

An important assumption in the RKD is no manipulation of the running variable, since this secures that individuals are distributed as good as randomly locally around the cutoff. To test this assumption we look at the density histogram of the running variable. If there are clear signs of bunching just below the cutoff this could indicate selection into treatment intensity, making households below and above the cutoff incomparable.

Figure 4 shows that the density of the running variable seems to be slightly higher below the cutoff. Since it's an RKD, we are looking for a change in slope of the density, which Figure 4 seems to show some indication of. One explanation for this could be if landlords tend to manipulate the reported floor area in order to exploit the housing allowance rules. Looking at the density of 85 m^2 dwellings we see a similar spike, which could reflect exploitation of the 85 m^2 limit for two-person dwellings. Although there is no kink in the housing allowance at 85 m^2 for one-person households, this would still affect the general supply of rental housing around that cutoff.

Figure 4: Manipulation of the running variable



A possible explanation for the observed bunching below the 65 m^2 cutoff might be landlords misreporting the floor area so that tenants receive housing allowance for the whole apartment, since this could be capitalized into higher rents. Such manipulation could violate identification and possibly bias our estimates upward, as the inflated rents of misreported dwellings below the cutoff would mechanically increase the apparent kink in rent. In this case, we would not only estimate the direct effect of the policy but also the effect of this illegal landlord behavior.

Another explanation for the slight bunching could be if new properties are built around the housing allowance rules. New dwellings might tend to be built more often below 65 m^2 to ensure more favorable housing allowance eligibility. We would then have a larger share of dwellings that can actually be affected by the housing allowance rules (the ones built after 1991) below the threshold. If this is the case, the households just below and above the threshold will not be as good as randomly distributed, and our estimate could be biased. The direction of this potential bias is most likely upwards: Newer buildings have free rent setting, so these dwellings will be associated with higher rent per m^2 leading to a sharper kink.

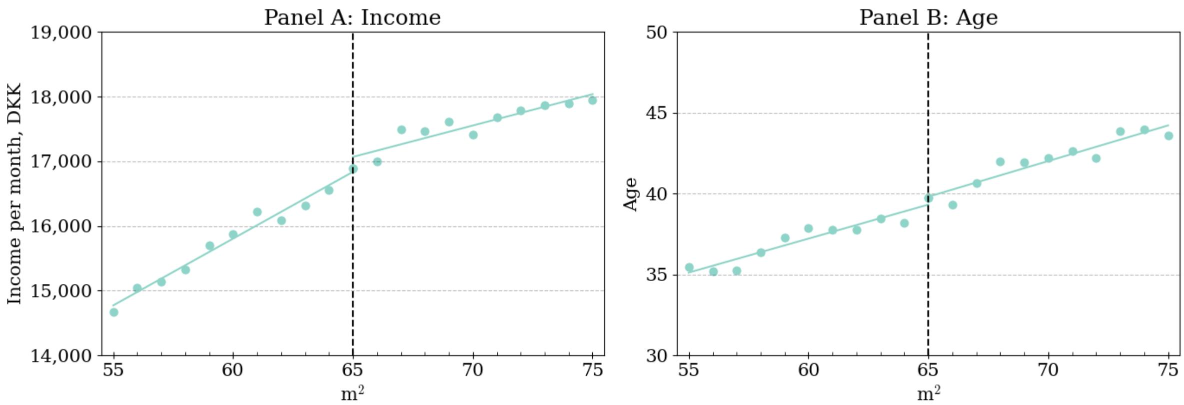
The spike in density at 65 m^2 might just result from floor areas being reported or constructed in round numbers, such as 60, 65, or 70 m^2. In Figure 4 we see that most spikes in the density arise at m^2 values divisible by 5, e.g. 40, 50, 55, 60, 65, 70, 85. This could explain why the distribution seems to jump at 65 m^2. If this pattern simply reflects mechanical rounding rather than strategic behavior, we shouldn't expect identification to be broken, although the additional measurement noise could lead to larger standard errors.

If the observed bunching arises from strategic misreporting or construction responses to the housing-allowance rule, households below and above the cutoff might not be comparable, potentially biasing our estimate of rent capitalization. In contrast, if the pattern reflects mechanical rounding of floor area, the design most likely remains valid though our standard errors might be larger due to noise in the sample.

6.2.2 Smoothness of covariates

As mentioned, an identifying assumption in the regression kink design is that covariates are smooth around the cutoff. If a kink appears in the covariates, we will no longer estimate a causal effect, since the kink in rent could be affected by the kink in the covariates. Figure 5 shows a plot of the two covariates income and age around the $65m^2$ cutoff in Panel A and B, as well as the estimates of a kink in Panel C.

Figure 5: Smoothness of covariates



Panel C: Test of a kink in income and age at the $65m^2$ cutoff

Bandwidth	Income		Age	
	(1)	(2)	(3)	(4)
h_{MSE}	-124.130**	-238.058*	0.230*	0.736**
	(53.350)	(125.703)	(0.130)	(0.351)
Polynomial order of floor area	First	Second	First	Second

Fixed effects included in all specifications are by municipality, parish, year and parish x year. Robust standard errors clustered at the parish level. *, **, and *** indicate significance at the 10%, 5%, and 1% level.
Result: There's a negative kink in income, however only significant with a bandwidth of 20. For age there's a positive kink, significant for the narrow bandwidths. Overall, we can't reject that kinks appear in our covariates, causing potential bias to our ρ-estimates.

Panel A shows signs of a negative kink in income around the $65m^2$ cutoff. It's difficult to see whether the data reflects a kink or a nonlinearity in income. In Panel C, we estimate a significant kink in income in Model (1) which is the local linear regression. The local quadratic regression in Model (2) shows only significance on the 10%-level. This could indicate that the apparent kink in Panel A arises because of a non-linear relationship between rent per square meter and income. We do however need to stress that the possibility of a kink in a covariate is problematic for our analysis, since identification relies on an assumption of smooth covariates.

In Panel B, age doesn't look to kink at the $65m^2$ cutoff. The estimate from Model (3) in Panel C does, however, measure a weak kink, significant at the 10%-level. Using the local quadratic regression, we see a more significant kink in age.

Since identification in our RKD relies on no kinks in the covariates, these results are worrying for our analysis. If these are actual kinks in income and age, our ρ-estimate will not only capture the direct effects from housing allowance, but also the effect that these slope changes have on rent.

As income and rent are highly correlated, a kink in this relationship is worrisome. At the same time, we expect that a lot of the correlation between age and rent arises through differences in income across age. We therefore focus on how a kink in income could distort the estimated effect of housing allowance on rent.

The kink in income is negative. Because rent tends to increase with income, this negative kink should push our ρ-estimate upward. The idea is that incomes just above the cutoff become lower compared to a scenario with no kink. As a result, rents just above the cutoff also end up lower than they would be in the no-kink scenario, which makes the kink in rent look sharper. If this is true, then ρ measures the effect of both a change in the marginal housing allowance and lower marginal incomes above the cutoff. A reason we could see a negative kink in income at the $65m^2$ cutoff is if households adjust their income downward in response to the decreased marginal housing allowance. Since income is part of the deductible of the Danish housing allowance scheme, households might compensate for the fall in marginal housing allowance by decreasing income slightly, which in turn would increase the housing allowance.

6.2.3 Placebo tests

To assess whether the kink we estimate at $65m^2$ is truly driven by the housing allowance rules, we implement placebo cutoff tests at points where no change in the treatment rule exists. If the RKD is valid, we should not observe slope changes at these placebo cutoffs. Finding no significant placebo kinks supports the assumption that any kink in rent per m^2 at the $65m^2$ cutoff is caused by the policy rather than nonlinearities of the outcome variable.

For the placebo cutoff tests we use the method introduced by Imbens & Lemieux (2008). We split our sample into two subsamples at each side of the cutoff. We define two pseudo cutoffs as the medians of each subsample. We then run the RKD for each subsample using the two pseudo cutoffs. The advantage of this approach is that none of the subsamples will contain the true cutoff. In order to have enough observations in each subsample we divide the $2 \cdot h_{MSE}$ samples into two. Table 4 shows the result of the placebo tests.

Table 4: Placebo tests: Test of a kink at placebo cutoffs

	Local linear regression			Local quadratic regression		
	(1)	(2)	(3)	(4)	(5)	(6)
Cutoff	$60m^2$	$65m^2$	$70m^2$	$58m^2$	$65m^2$	$72m^2$
ρ	9.787	0.531	-31.557	23.982	2.034**	-2.646
	(15.118)	(0.826)	(231.764)	(52.138)	(1.012)	(5.818)

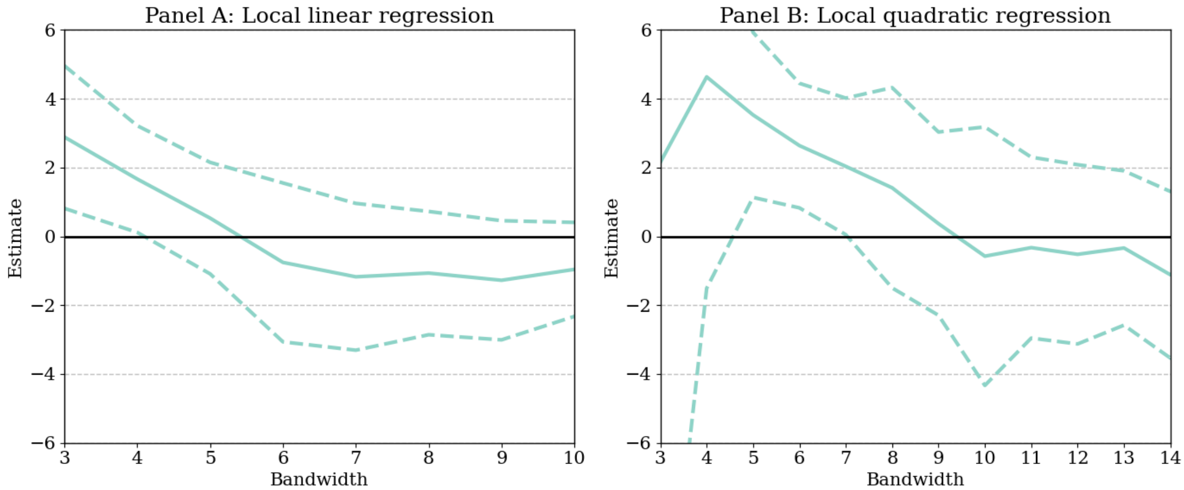
Fixed effects included in all specifications are by municipality, parish, year and parish x year. Robust standard errors clustered at the parish level. *, **, and *** indicate significance at the 10%, 5%, and 1% level.
Result: There's a negative kink in income, however only significant with a bandwidth of 20. For age there's a positive kink, significant for the narrow bandwidths. Overall, we can't reject that kinks appear in our covariates, causing potential bias to our ρ-estimates.

We have reported results for the two pseudo cutoffs as well as the true cutoff for both the local linear and local quadratic regression. The bandwidth used for Model (1)-(3) is +/- $5m^2$ around the (pseudo)cutoff and for Model (4)-(6) it is +/- $7m^2$. When we examine the estimates at the pseudo cutoffs in Models (1), (3), (4), and (6), we see that the standard errors are extremely large. This pattern indicates that there is no meaningful evidence of a simultaneous kink in the first stage and the reduced form. The standard errors at the true cutoffs are much smaller which suggests that what we are capturing at the $65m^2$ threshold is actually tied to the housing allowance policy and not just reflecting nonlinearities in rent per m^2.

6.3 Robustness checks

To check the robustness of our results, we wish to examine how sensitive our main estimate and standard errors are to varying bandwidths. Figure **??** shows our main estimate, ρ, across bandwidths for both the local linear and local quadratic regression.

Figure 6: ρ-estimates across bandwidths



The local linear regression shows a clear pattern. The ρ-estimate starts out positive and significant, whereafter it declines and from a bandwidth of 6 remains fairly constant at a negative and insignificant value. For the local quadratic regression the estimate starts out insignificant for the smallest bandwidths and is thereafter positive and significant through bandwidth 5-7. Finally, it stabilizes close to zero and highly insignificant for the larger bandwidths.

Overall, we see that standard errors for the local linear regression are smaller than for the local quadratic. For small bandwidths, both specifications yield unrealistically high estimates, and for larger bandwidths the local linear estimates seem unrealistically low though insignificant.

The instability and large standard errors of the ρ-estimates, that we see in Figure **??**, is quite worrying. Firstly, the volatility of the estimates are monumental given that we theoretically expect a ρ-estimate between zero and one. Secondly, the standard errors are of a magnitude where a realistic ρ-estimate would always turn out insignificant. That we see this imprecision and variance across bandwidths is to some extent invalidating for our empirical results. We therefore emphasize that our results are associated with a lot of uncertainty and should be interpreted carefully.

In Figure 3, we saw a possible discontinuous jump in rent per m^2 at the $65m^2$ cutoff. From the Danish housing allowance scheme we have no reason to expect a jump in rent at the cutoff. Therefore, if we measure a significant jump, this could break the smoothness of potential outcomes assumption, since it would indicate an effect on rent at the cutoff coming from outside the housing allowance scheme.

Table 5: Testing for a discontinuous jump at the $65m^2$ cutoff

Bandwidth	Discontinuity, $D(m^2 > 65)$		Second stage, ρ	
	(1)	(2)	(3)	(4)
h_{MSE}	0.537	-0.262	0.666	2.022**
	(0.469)	(0.463)	(0.798)	(1.022)
Polynomial order of floor area	First	Second	First	Second

Fixed effects included in all specifications are by municipality, parish, year and parish x year. Robust standard errors clustered at the parish level. *, **, and *** indicate significance at the 10%, 5%, and 1% level.
Result: There's a negative kink in income, however only significant with a bandwidth of 20. For age there's a positive kink, significant for the narrow bandwidths. Overall, we can't reject that kinks appear in our covariates, causing potential bias to our ρ-estimates.

For Table 5, we included in equation (10) a dummy for the floor area being larger than $65m^2$, $D(m^2 > 65)$. This variable captures possible jumps in rent per m^2 at the cutoff. Table 5 shows estimates of this jump as well as the second stage estimates when $D(m^2 > 65)$ is included in the regression. Model (1) and (2) suggest no significant jump in rent per m^2 when using h_{MSE}. The sign of the jump even flips between the local linear and local quadratic regression. This suggests that we do not observe a discontinuous jump at the $65m^2$ cutoff. Looking at the ρ-estimates in Model (3) and (4), we see almost no change compared to Table 3. Therefore, our results don't seem influenced by a jump in rent per m^2.

6.4 Mechanisms

In order to better understand the mechanisms behind our weak finding of rent capitalization, we try to examine the importance of the elasticity of housing supply. In Section 3, we showed that the theoretical prediction is that areas with a more inelastic housing supply will have a higher degree of rent capitalization from housing allowance. To test this prediction, we run our RKD on areas associated with lower and higher housing supply elasticities. A proxy for the elasticity of housing supply is the vacancy rate. A lower vacancy rate will typically imply that housing supply is unable to respond to increases in demand, i.e. a low supply elasticity. The vacancy rates in the municipalities of the big Danish cities are much lower compared to the rest of the country, with vacancy rates near zero in areas like Copenhagen (IMF, 2016, p. 14). This indicates that the biggest Danish cities have a more inelastic housing supply compared to the rest of the country.

For this reason, we split the dataset up into two parts, one containing the municipalities of the four biggest cities in Denmark, and one containing the other 94 municipalities. The four biggest municipalities are those of Copenhagen (23.0% of the sample), Århus (9.7% of the sample), Odense (8.9% of the sample) and Aalborg (5.6% of the sample) - these in total make up 47.2% of the sample.

Figure 7: ρ-estimates for big cities vs no big cities across bandwidths

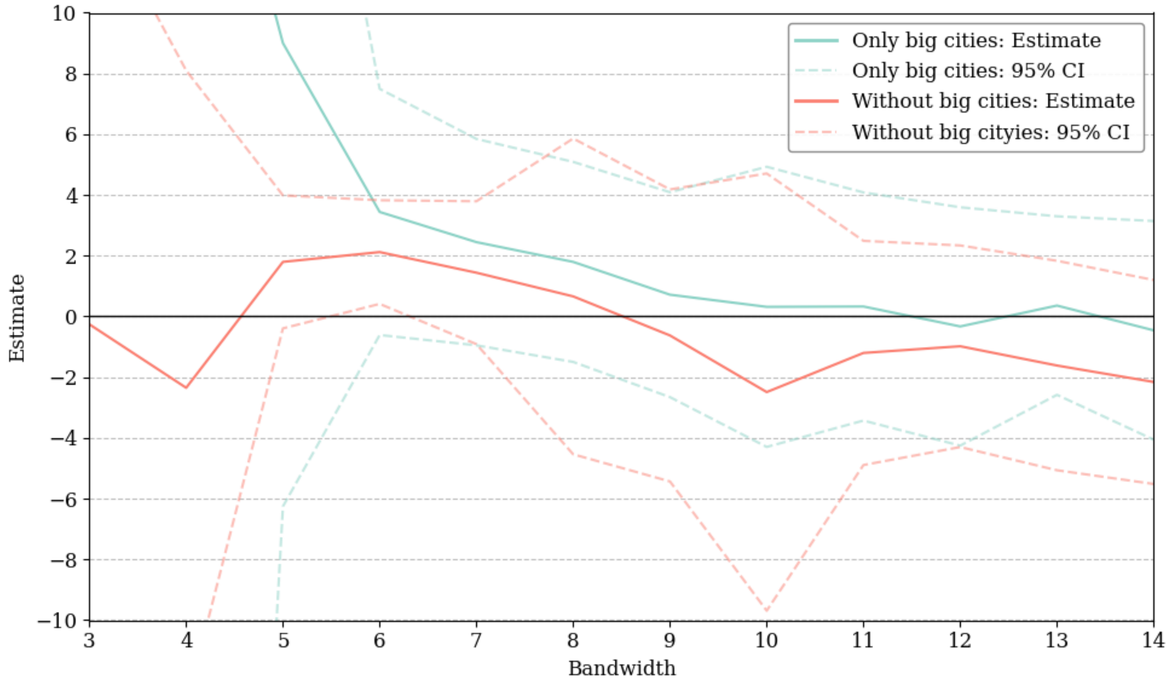


Figure 7 shows the ρ-estimates of the local quadratic regression across bandwidths including only the municipalities of the four biggest cities in Denmark and excluding them. We see that the estimated rent capitalization effect is larger across all bandwidths for the biggest cities compared to the rest of the country. This means that we generally predict housing allowance to affect rents more in the big cities compared to other areas. This result is in line with our theoretical prediction: rent capitalization of housing allowance is more present in areas with an inelastic housing supply.

It is important to note the very large confidence bands of from both samples. There are no bandwidths where the ρ-estimate for no big cities lies outside the 95%-confidence bands of the big cities. Therefore, we can not from Figure 7 conclude that the rent capitalization is significantly stronger in the big cities compared to the rest of the country. We only observe that there seems to be a pattern of higher estimated average effects in the big cities. The estimates from the local linear regression paint a more muddled picture, however not in contradiction with Figure 7 (Figure 9 in the Appendix).

Making this analysis we are somewhat constraint in terms of data. It's possible that big cities have less rent restricted properties. In such case, what we are measuring in Figure 7 might not only be the effect of differences in housing supply elasticities, but also the fact that the rent of more dwellings can be affected by housing allowance in the big cities. As mentioned earlier, we would ideally have liked to be able to separate households in terms of rent setting, but we instead propose it as an area of further possible research.

7 Discussion and closing remarks

7.1 Identification considerations

A central element of our empirical strategy is the identification of the causal effect of housing allowance on rent through the kink at 65 m^2. While the analysis is structured around what seems to be quasi-experimental variation, multiple parts of the analysis and institutional setting should be discussed.

Two of the internal validity tests give indications that warrant some caution. First, the density of the running variable shows some bunching just below the cutoff, see Figure 4. This could reflect strategic reporting or construction patterns and may introduce a modest upward bias, as newer buildings on avereage are more expensive to rent. The bunching pattern could also resemble the general rounding of floor area to round fives elsewhere seen in the distribution.

Second, we observe indications of kinks in covariates, and particularly income is worrying, see Figure 5. Since income is correlated with rent and mechanically is a deductible in the housing allowance formula, such a kink could mechanically create a slight upward bias in the estimated rent kink. The magnitude is however limited, and the significance depends on the chosen functional form. Both of these results could so result in an upwards in the estimate.

A further point deserving attention when it comes to the validity of the identification is the limited size of the first-stage kink, see Table 3. Although the change in marginal housing allowance is precisely estimated, its size limits the amount of quasi-experimental variation available for identification in the subsequent parts of the analysis. A small first-stage kink implies that the possible small effect of the decrease in housing allowance on rent will be difficult to detect in the presence of substantial noise in the rent data. The reduced form is considerable noisy, see 3. Additionally, the fact that the running variable is discrete means that fewer observations lie close to the cutoff. The heightens the trade-off between local identification and sufficient support on each side of the cutoff. This naturally makes slope changes in the reduced form harder to estimate, and leads to sensitivity towards bandwidth choice with volatile estimates.

7.2 Imprecision in estimates

From the main results in Section 6.1, as well as from the bandwidth graph in Figure 6, it is clear, that the estimates are highly imprecise. Apart from the fact, that the identifying kink may be to weak, there could be other reasons for there being so large standard errors on the estimates.

One limitation is that we cannot separately identify the dwellings that are exposed to potential rent seeking. The sample mixes different rent-setting regimes, and without information on which apartment type each individual lives in, we can't isolate the apartments where rent capitalization is most likely to happen from the ones, where it is improbable. If one could tell the different housing types apart, analyzing the different rental markets simultaneously could strengthen the identification. In particular, cost-based social housing would serve as a natural control group, since no kink in rent is expected at the cutoff for this segment. Such an approach could maybe also help address some of the uncertainties related to smoothness in covariates or bunching around the threshold.

As mentioned, the reduced form is clearly affected by considerable noise. This noise could likely have been reduced by including more detailed information on dwelling characteristics, which might have enabled us not only to determine the sign of the effect more clearly but maybe also to assess its magnitude.

Another potential improvement would be to explore multiple cutoffs within the housing allowance formula. Using multiple identification points could provide additional robustness and help validate whether the patterns observed at 65 m^2 are similar elsewhere. As single person households are half of the households in the dataset, analysis of other cutoffs for bigger households at 85, 105 and so on would likely be possible to do.

A final point involves the role of market elasticity. In the analysis, we conducted an initial and somewhat rough segmentation of the housing market based on differences in supply elasticity, primarily distinguishing between the four largest cities and the rest of the country. This exercise weakly indicated that capitalization patterns may vary meaningfully across markets with different elasticities, which aligns with theoretical predictions. However, this binary split only captures broad differences and far from showed a significant difference. A more refined segmentation such as separating markets by neighborhood characteristics or vacancy rates could shed further light on how rent capitalization might be different between markets with different elasticities of housing supply. A more detailed analysis might offer a better indication of whether capitalization varies across markets with different elasticities.

7.3 Closing remarks

- Weak estimates leading to the conclusion, that the rent capitalization from renters are minimal. We can, though, neither exclude the possibility of the being both 0 and 100 % rent capitalization.
- The imprecision itself suggests no effect - we can't rule out, that there is en effect, but we find only find limited evidence of an effect.
-

8 Appendix

Figure 8: First stage estimate across bandwidths, β_2

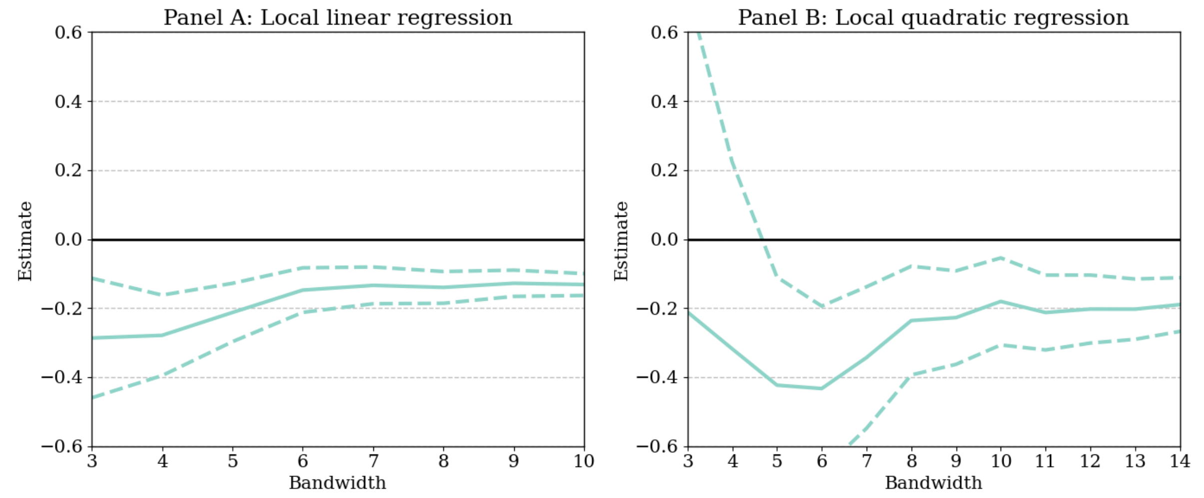


Table 6: Effect of HA on rent across different standard errors

Standard errors:	Clustered at parish level		Clustered at municipality level		Clustered at m^2		Robust		Conventional	
Bandwidth	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
h_{MSE}	0.531	2.034**	0.531	2.034**	0.531	2.034***	0.531**	2.034***	0.531	2.034***
	(0.826)	(1.012)	(0.903)	(0.803)	(1.121)	(0.729)	(0.256)	(0.381)	(0.474)	(0.636)
$0.5 \cdot h_{MSE}$	2.887***	4.635	2.887**	4.635	2.887***	4.635***	2.887***	4.635***	2.887***	4.635*
	(1.057)	(3.134)	(1.463)	(3.976)	(0.317)	(0.660)	(0.465)	(1.541)	(1.013)	(2.484)
$2 \cdot h_{MSE}$	-0.959	-1.115	-0.959	-1.115	-0.959	-1.115	-0.959***	-1.115***	-0.959***	-1.115***
	(0.697)	(1.235)	(0.734)	(1.465)	(0.756)	(1.076)	(0.216)	(0.352)	(0.272)	(0.424)
Polynomial order of floor area	First	Second	First	Second	First	Second	First	Second	First	Second

All models are estimated using a bandwidth of 5, 10 and 20. We use the uniform kernel in all specifications. The covariates included in all specifications are income, income squared, age and age squared. Fixed effects included in all specifications are by municipality, parish, year and parish x year. Robust standard errors clustered at the parish level. *, **, and *** indicate significance at the 10%, 5%, and 1% level. **Result**: Our results are robust to using different kinds of standard errors.

Figure 9: ρ-estimate for big cities vs no big cities, local linear regression