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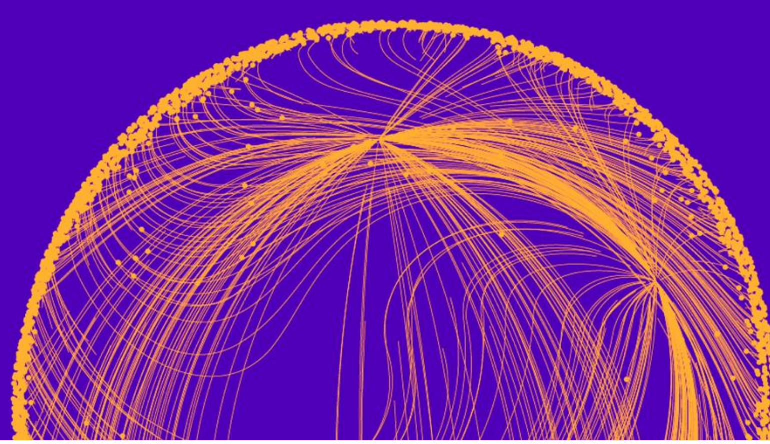

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The dynamics of long-term subletting: Evidence from apartment-seeking university students in Sweden*

Theo Herold† Fredrik Kopsch‡

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Abstract

Due to stringent regulation on the Swedish primary rental market, many individuals turn to long-term sublets to satisfy their housing demand. A lack of accessible data has made study of this market difficult. Using a unique dataset, we exploit weekly temporal variation over multiple treatment windows to analyze rent levels around the start of the Swedish university semester. We uncover strong effects, with monthly rent increasing between 3.31 percent and 4.22 percent during the treatment period. The effects peak immediately after the university semester has started and are concentrated to the tails of the distribution of living space. The results are robust to a range of alternative specifications and sample sizes. [112 words]

Keywords: rental market, long-term subletting, subletting market, difference-in-differences, rent control

JEL Codes: D4, R1, R21, R31

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1 Introduction

A unintended consequence of rent control is that unregulated market segments are indirectly affected, often leading to increased demand and rising rents (Skak & Bloze, 2013; Diamond, McQuade, & Qian, 2019; Breidenbach, Eilers, & Fries, 2022; Mense, Michelsen, & Kholodilin, 2023). If rent control is broad enough to cover the entire supply of rental housing, alternative institutions such as different forms of subletting markets will naturally emerge to meet the available demand. In the case of Sweden, rental housing is distributed through a centralized housing queue and dwellings are subject to strict vacancy control[1]. Individuals without enough time-in-queue are forced to demand rental housing on the market for long-term sublets, which are distributed according to willingness-to-pay and rent is negotiated directly between landlord and tenant. Average rent for long-term sublets is considerably higher than on the primary market, which particularly affects younger households with low-incomes (Herold, 2019; Kopsch, 2021; Stockholm Chamber of Commerce, 2022).

Considering that rent control has returned to the forefront of policy debate in several countries, understanding the dynamics of long-term subletting becomes increasingly important. Yet, due to data limitations in Sweden and elsewhere, little attention has been given to long-term subletting. The contemporary literature has primarily focused on short-term subletting through sharing economy-platforms such as Airbnb (Gouveia, Nilsson, & Berggren, 2020; Combs, Kerrigan, & Wachsmuth, 2020; Koster, van Ommeren, & Volkhausen, 2021; Reichle, Fidrmuc, & Reck, 2023; Hill, Pfeifer, & Steurer, 2023).[2] In the case of Sweden, there is no monitoring of the market of long-term subletting and

[1]Initial rents are based on a utility-value system which in practice means that rents should equal the rent for other similar dwellings. Rent increases are negotiated between tenant association and landlord. Vacancy control implies that new tenants will in principle pay the same rent as previous tenants based on this utility value system.

[2]This literature has grown immensely in the past decade. Frequently analyzed aspects include the effect of platform companies on residential property values and rents (Einav, Farronato, & Levin, 2015; Horn & Merante, 2017; Chang, 2020; Koster et al., 2021; Reichle et al., 2023), segregation and discrimination (Ellen & Ross, 2018; Kakar, Voelz, Wu, & Franco, 2018; Marchenko, 2019; Gouveia et al., 2020), and crowding-out effects (Combs et al., 2020; Hill et al., 2023).

available data is typically centered on the capital region of Stockholm.

Platform companies for short-term rentals provide a fundamentally different service than that of long-term subletting. The average stay duration globally for Airbnb sublets is 4.2 days, according to the organization Rental Scale-up, while the average stay duration on the Swedish subletting market is approximately a year (Stockholm Chamber of Commerce, 2022). Sharing economy-platforms provide the researcher with accessible data and, in return, the literature has provided important insights into this relatively new phenomenon. However, the lack of inquiry into long-term subletting runs the risk of excluding an important component in the analysis of rental markets.

The purpose of this study is to investigate the market dynamics of long-term subletting, which is done by analyzing how students' demand affects rent levels around the start of the Swedish university semester. We use a unique data set over the market for long-term sublets between March 2015 and October 2016 that covers the entirety of Sweden. We compare a treatment group of student cities to a control group containing all other, 'non-student cities'. We follow recent developments within the difference-in-difference literature to identify the causal relationship of interest, by exploiting temporal variation over multiple, reoccurring treatment windows.[3]

We find that rent levels react strongly to demand shocks, with observed treatment effects in student cities between 3.31 percent and 4.22 percent. In real terms, this implies that students have to pay 3 449 sek (304 usd)[4] more per year compared to individuals who sign their lease at other periods throughout the year. This is a considerable difference, especially when accounting for the low income of students. The effects peak immediately after the university semester has started, reaching as high as 4.98 percent and indicating that students have to hold out some time before they are able to satisfy their demand. These rent increases are most pronounced in either tail of the distribution of living space,

[3]Recent developments include, but are not limited to, de Chaisemartin and D'Haultfoeuille (2017); Goodman-Bacon (2021); Borusyak, Jaravel, and Spiess (2023); Roth (2022); Roth and Sant'Anna (2023); Roth, Sant'Anna, Bilinski, and Poe (2023)

[4]Exchange rate for 28 August 2023.

i.e., for relatively small and relatively large dwellings, most likely as the demand side largely consists of students with low incomes (and tight budget constraints) and groups of students with higher aggregated incomes (and therefore higher budget constraints).

We conduct a range of tests to assess the robustness of our uncovered results, model specification and assumed identifying variation. We confirm the existence of shared common trends between our treatment and control group in three different ways: by visual inspection, by leading our time variable and by investigating higher-order (linear, quadratic and cubic) pre-trends.[5] We assess the sensitivity of our identifying assumption by allowing for municipality-specific linear time trends, with the exploited variation instead coming from deviations from this trend. We assess our model specification by using a range of different samples with differing population sizes, for instance by excluding neighboring municipalities and including larger metropolitan areas. We lastly conduct placebo tests in the form of randomized treatment groups and randomized treatment periods.[6]

The uncovered results come with policy implications. Younger households, who tend to dominate the demand side for long-term subletting, are shown to have to pay considerable rent increases conditional on when the lease is signed. Households on the primary rental market, which tend to be older and have higher incomes (Donner & Kopsch, 2021), are completely protected from such fluctuations due to rent control. Policy makers are strongly advised to account for the indirect effects on unaffected segments when assessing potential rent control schemes. In this vein, classic welfare analyses can greatly help when assessing the pros and cons of regulatory mechanisms on rental markets.

The institutional setting of the present paper shares similarities with the empirical literature on rent control, as regulation in the form of vacancy control covers the entire Swedish primary rental market.[7] Contemporary analyses usually exploit within-market

[5] For the latest developments on pre-trend analysis, see the excellent body of work in Roth (2018, 2022); Roth et al. (2023).

[6] For the latest on placebo sampling, see for instance Eggers, Tuñón, and Dafoe (2021); Ye, Chen, and Zhang (2022); Roth et al. (2023)

[7] Rent control on the primary rental market in Sweden has been in effect in one way or another since the second world war.

variation to assess the effect of rent control on unaffected segments (Skak & Bloze, 2013; Deschermeier, Seipelt, & Voigtländer, 2017; Diamond et al., 2019; Breidenbach et al., 2022; Mense et al., 2023). These contributions assess the implementation of rent control, which is contrary to the present paper where we instead analyze the dynamics of the unaffected segment long after the implementation has taken place. Nonetheless, understanding the effects of rent control is vital in order to understand the context in which long-term subletting takes place, both in Sweden and elsewhere. Other strands of the empirical literature on rent control looks at the effect of rent control on maintenance and construction activities (Heskin, Levine, & Garrett, 2000; Glaeser & Luttmer, 2003; Skak & Bloze, 2013; Donner & Kopsch, 2018), on mobility, segregation and discrimination (Auspurg, Hinz, & Schmid, 2017; Donner & Kopsch, 2021; Bratu & Bolotnyy, 2023), and on land and property values (St. John, 1990; Rosen, 2018).

The literature presents a tangible gap with regards to subletting markets. This paper contributes by expanding the perspective from short-term platform rentals to long-term subletting using a unique data set. We are able to analyze how rents for long-term sublets react over multiple periods of hypothesized fluctuations in supply and demand, and break these dynamics down over different dwelling sizes and dwelling types.

The reminder of this paper is structured as follows: Section 2 presents the institutional setting, forms the testable hypotheses and provides a stylized example over the subletting market. Section 3 presents the identification strategy and econometric approach. Section 4 presents the data material, summary statistics and balance across group and treatment status. Section 5 presents the results, robustness checks and placebo tests. Section 6 offers some concluding remarks and avenues for future research.

2 Background

Institutional setting

Primary market rental housing in Sweden is distributed through a centralized housing queue, akin to the distribution of certain social- and public housing in other parts of Europe. While the vast majority of landlords on the primary market are public and private companies, private property owners can in theory rent out their dwellings on the primary market through the housing queue. The initial primary market rent is set to follow the rent of similar dwellings and rent increases are negotiated between tenant and landlord associations. The tenant association must explicitly accept any rent increase before it can take effect, which means that tenant associations can hold out on any proposal until it matches their expectations. The queuing system in conjuncture with the asymmetry in bargaining power between tenant association and landlord has lead to a steadily increasing time-in-queue, most visible in the capital city of Stockholm.[8] Several adverse effects have been documented as a result of these regulations, such as a shortage of rental housing and housing misallocation (Kopsch, 2019), as well as segregation based on education level, age and background (Fridell & Brogren, 2007; Enström Öst, Söderberg, & Wilhelmsson, 2014; Donner & Kopsch, 2021; Bratu & Bolotnyy, 2023)

As a consequence of the constrained primary market, the market for long-term subletting has naturally developed out of necessity to aid individuals without enough time-in-queue to find appropriate housing. Long-term sublets are distributed according to willingness-to-pay and rent is directly negotiated between landlord and tenant. The Swedish National Board of Housing, Planning and Construction (2018) estimates that approximately 11 percent of renting households lived in a sublet in 2015, a number that most likely has increased since then. There is however a high degree of uncertainty attached to this number due to a lack of market monitoring and reliable data.

There are two primary types of long-term sublets in Sweden. Private property owners wishing to rent out their dwelling can do so on the subletting market by *owner-subletting*. Housing cooperatives are the norm in Sweden, meaning that individuals purchase a stake

[8]The Stockholm Housing Agency, owned by the municipality of Stockholm, reports that time-in-queue exceeds 9 years in Stockholm county, 12 years in Stockholm municipality and 18 years in the central parts of Stockholm city. The yearly welfare costs of rent control in Sweden has been estimated to be between 10 billion sek (1 billion usd) and 20 billion sek (2 billion usd) (The Swedish National Board of Housing, Planning and Construction, 2013; Kopsch, 2021).

in the cooperative which in turn grants them the right to a dwelling. In a technical sense, while private individual has exclusive rights to the dwelling, it is still owned by the cooperative. This means that anyone wishing to owner-sublet their dwelling must apply for permission of the housing cooperative before subletting can take place.

The other form of long-term subletting is *rental-subletting*, i.e., when a primary market tenant chooses to sublet their dwelling. As with owner-subletting, rental-subletting requires permission, but in this case from the landlord. Long-term rental-subletting is only allowed when the primary tenant temporarily moves, for instance for work or studies. Any long-term subletting without permission from the housing cooperation or from the landlord is prohibited.

Subletting with the intention to maximize profits is also prohibited, however, this is not an issue in practice. Individuals who owner-sublet are allowed to cover operating costs, but as "operating costs" is vaguely defined, they have considerable leeway in negotiations with tenants. The rent of rental-sublets must equal primary market rent, but individuals are allowed to add an additional 10 to 15 percent to the rent if the dwelling is furnished. Multiple studies have shown that rent often exceeds the 15 percent threshold for rental sublets.[9] Subletters have considerably more bargaining power compared to sublettees during rent negotiations, which is further amplified by the immense demand surplus that spills over from the rent controlled primary market. Sublet rent in Sweden should *in theory* be controlled but is in *in practice* set freely for both rental sublets and owner sublets.[10] As a result, long-term sublets have considerably higher rents than the rent controlled primary market.

Previous contributions show that rent in the controlled segment is negatively correlated with rent in unaffected segments (Skak & Bloze, 2013; Mense, Michelsen, & Cholodilin, 2017; Mense et al., 2023), i.e., that the suppression of rents on one segments leads to higher

[9] See for instance The Swedish National Board of Housing, Planning and Construction (2018); Herold (2019); Stockholm Chamber of Commerce (2022)

[10] This phenomenon of higher-than-allowed sublet rents is well-documented. Swedish authorities have recently clamped down on and prosecuted actors for selling subletting leases, but little has been done with regards to the systematically higher-than-allowed sublet rent.

rents on an unaffected segment. This is shown to be true when the unaffected segment consists of long-term sublets. Table 1 shows previous results over the difference between primary market and subletting market rent per square meter. The most contemporary result comes from Stockholm Chamber of Commerce (2022), who finds that the nation-wide rent per square meter for owner- and rental-sublets is 79 percent higher compared to the primary market. The data used in this study shows a difference of over 100 percent.

Table 1: Average rent per square meter on the primary market and subletting market in Sweden.

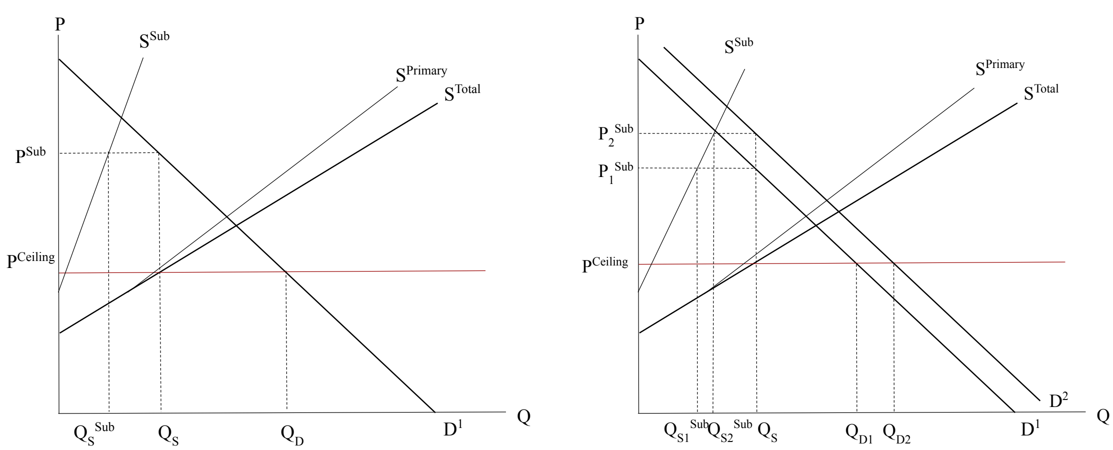
Reference	Primary market	Sublet market	%-difference
Herold & Kopsch (2023) *Period March 2015 - October 2016*			
Nation average	87 sek (8.04 usd)	176 sek (17.10 usd)	102%
Stockholm county	101 sek (9.33 usd)	240 sek (22.19 usd)	138%
Stickholm municipality	105 sek (9.70 usd)	266 sek (24.57 usd)	153%
Stockholm Chamber of Commerce (2022) *Period 2021*			
Nation average	100 sek (9.27 usd)	178 sek (16.50 usd)	79%
Stockholm county	111 sek (10.25 usd)	221 sek (20.41 usd)	99%
Stockholm municipality	118 sek (10.90 usd)	294 sek (27.16 usd)	149%
Boverket (2018) *Period 2009-2017*			
Nation average	82 sek (7.57 usd)	136 sek (12.56 usd)	66%
Stockholm county	94 sek (8.68 usd)	183 sek (16.90 usd)	95%

Note: The reported primary market rents comes from Statistics Sweden. The table aggregates rent levels for owner and rental sublets. The Swedish National Board of Housing, Planning and Construction (2018) does not provide an estimate for Stockholm municipality.

Long-term subletting: a stylized example

Based on a framework originally presented in The Swedish National Board of Housing, Planning and Construction (2018), figure 1 presents a stylized example of an entirely rent controlled primary market and an unaffected subletting market. While a share of the long-term sublet supply consists of rental-sublets, not all primary market dwellings are made available as long-term sublets. Similarly, part of the supply on the subletting market consists of owner-occupied dwellings in the form of owner-sublets that are never made available in the centralized queue on the primary market. Therefore, the intercept and slope of $S^{Primary}$ and S^{Sub} differ. $S^{Primary}$ and S^{Sub} together form the total supply curve S^{Total}. The total demand for rental housing is given by D. Given the price ceiling $p^{Ceiling}$, quantity supplied on the primary market is Q_S and demand is Q_D in city j. The rent for long-term sublets is given by p^{Sub}, which is higher than both $p^{Ceiling}$ and the hypothetical equilibrium rent.

Figure 1: The rental market consisting of both a primary and subletting rental market, before (left) and after (right) a sudden demand shock.



An exogenous shock shifts quantity demanded from Q_{D1} to Q_{D2}. Under an institutional setting where the entire primary market is rent controlled, the sudden demand increase will channel through the subletting market exclusively. This leads to a rent increase from p_1^{Sub} to p_2^{Sub} and a subsequent increase in the supply of sublets from Q_{S1}^{Sub} to Q_{S2}^{Sub}. The goal of this paper is to successfully identify and estimate $p_2^{Sub} - p_1^{Sub}$. In appendix A, we show that the magnitude of the rent increases uncovered in this paper is presumably higher compared to a counterfactual situation where the primary market is free from rent control. Stockholm Chamber of Commerce (2022) shows this phenomenon empirically, where tighter rent control on the primary market, i.e., a bigger difference between the controlled rent $p^{Ceiling}$ and the hypothetical market rent, leads to a higher long-term sublet rent p^{Sub}.

University semesters to gauge market dynamics

The present paper uses the start of the university semester to study the dynamics of long-term subletting. This setting is ideal to identify and estimate the size of $p_2^{Sub} - p_1^{Sub}$, simply because students moving to a new city, without time-in-queue and who are unable to purchase a dwelling, have to utilize the market for long-term sublets. This influx of market participants should be clearly visible in the data, especially in the more popular student cities across Sweden.

The difficulty of finding long-term sublet housing is well known, inclining tenants to start their search as soon as possible. For students, this implies that the search for long-term sublets should commence as soon as admittance is inferred. However, note in figure 1 that $Q_{D2} - Q_{D1} > Q_{S2}^{Sub} - Q_{S1}^{Sub}$, i.e., the increase in quantity supplied on the subletting market is smaller than the increase in total quantity demanded after a sudden demand shock. This is because of the spillover from the rent controlled primary market to the subletting market, leading to congestion when more tenants are looking for sublet housing than available demand. If this sudden demand shock happens prior to some terminal date, for instance right before the start of the university semester, some individuals will not be able to satisfy their demand until after the university semester has started. It is not certain which effect dominates, i.e., whether demand for sublets is higher right after admittance but before the semester starts, or during the weeks right after the semester has started. A greater effect after the terminal date would indicate that the spillover of tenants from the primary market to the subletting market is great enough to disrupt normal market mechanisms. To test which effect that dominates, we split the observed treatment effect $p_2^{Sub} - p_1^{Sub}$ into an anticipation and delayed response period. Ex-ante, we expect significant treatment effects in both periods.

Living space is marginally decreasing over rent levels, but the hypothesized treatment effect (higher rent levels) must not necessarily marginally decrease over living space. Consider the case with only two available sublet dwellings that have the same living space but different rent levels – individuals will simply demand the less expensive option. It is only when individuals find the difference in rent to be smaller than the expected utility gain from increased living space that demand for the more expensive unit will increase. It is not uncommon for Swedish students to group together to find sublet housing, which for two students with identical incomes doubles the budget constraint while simultaneously increasing demand for more living space. This leads us to the hypothesis that the observed treatment effect $p_2^{Sub} - p_1^{Sub}$ is the most pronounced closer to the the affordable segment in the left tail (students acting individually) and the more expensive segment in the right tail (students grouping together) of the distribution of living space.

To summarize, the hypotheses to be tested can be formulated as

H1: $p_2^{Sub} - p_1^{Sub}$ exists and is a statistically significant rent increase for long-term sublets around the start of the university semester.

H2: The rent increase is subject to anticipation and delayed response behavior among tenants.

H3: The rent increase is most pronounced in the tails of the distribution of living space.

3 Empirical framework

Identification strategy

By using the start of the university semester to analyze the dynamics of long-term subletting, we are able to utilize three separate treatment periods as the university semester has two yearly starting dates and reoccurs three times over the sample period. Each admission round is split into two parts: the first admission round ends in the middle of July for the Fall semester and in the middle of December for the Spring semester. The second and final admission round ends approximately a month prior to the start of the university semester. The university semester formally starts on the same day across the country, which is usually the last week of August and the third week in January, but the year-on-year starting date can differ. We exploit this temporal variation in the starting dates and set the treatment window to be 5 weeks prior to the start of the university semester. The length of the treatment window is thus long enough to capture the demand effect when both rounds of admission have passed and leaves room to properly conduct a pre-trend analysis.

Multiple treatment periods will most likely produce heterogeneous treatment effects from treatment window to treatment window (de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2021; de Chaisemartin & D'Haultfoeuille, 2022; Borusyak et al., 2023). To this end, we view difference-in-differences estimation as the best approach for disentangling treatment effects. Alternative empirical designs such as a regression discontinuity in time would also allow for identification of the treatment effect between the two groups, but analyzing potential pre-event and post-event effects becomes difficult due to the method's focus on a narrow cutoff point. Differently put, the method runs the risk of omitting information as the treatment effects can manifest multiple periods before or after the cutoff point. On the contrary, difference-in-differences is not affected by this shortcoming.

A key assumption is the existence of a shared common trend prior to treatment, which is crucial to compare potential treatment effects to the counterfactual had there not been any treatment. If the common trends assumption fails, the difference-in-difference estimator is biased. Trend differences between our treatment and control group can arise if, for instance, landlords are aware of the influx of new students and the accompanying higher demand, as they can then withhold supply, creating a trend difference prior to treatment. Alternatively, a trend difference could arise if more and more students move away throughout the semester, pressing rents downwards over the pre-treatment period. We undertake rigorous testing of the common trends assumption by investigating linear, quadratic and cubic pre-trends, and by plotting estimated treatment effects and their confidence intervals.[11]

However, we still wish account for the possibility that there are some underlying trend differences between our treatment and control groups. Municipality fixed effects controls for these kinds of differences, insofar as they are time invariant, whereas allowing for location-specific time trends can capture such phenomena given their dependence on time. We therefore fit municipality-specific weekly linear time trends as a form of robustness, which also switches the identifying assumption to deviations from this trend.

The observed treatment effect could equally be the result of the proximity of the treatment group to some other group of municipalities that experience part of, or the entire, treatment effect. Failing to account for this leads to the false conclusion that the treatment group reacts to the treatment, when the observed treatment effect in actuality is caused by confounding bias. We asses spillover effects by using alternative samples with differing population sizes between our treatment and control groups, for instance by controlling for neighbouring municipalities and larger metropolitan areas. To assess our overall model specification and identifying assumption, we conclude the analysis by conducting placebo tests through randomized treatment periods and randomized treatment groups over subsamples of the data.[12]

Treatment and control groups

Table 2 presents some preliminary information regarding the biggest student cities in Sweden. The municipalities of Stockholm, Gothenburg and Malmö are home to multiple higher education institutions, but are also much larger than the other included municipalities. These large metropolitan areas have large business districts and function as cultural centers, with plenty of tourism and international trade. The adverse effects of long-lasting rent control are the most severe in these metropolitan areas. This is particularly true in

[11] For the latest developments on pre-trend analysis, see the excellent body of work in Roth (2018, 2022); Roth et al. (2023).

[12] For the latest on placebo sampling, see for instance Eggers et al. (2021); Ye et al. (2022); Roth et al. (2023)

Stockholm, with record queuing times and a steady decline in the number of rental units since the 1970's. Average monthly rent over the sample period is also higher in Stockholm municipality (11 402 sek), Gothenburg (7 901 sek) and Malmö (7 613 sek), compared to the 11 largest student cities (6 542 sek). Taken together, it is particularly difficult to compare Stockholm to other cities around Sweden, which is why we choose to exclude Stockholm municipality altogether from the analysis. Similar arguments apply to a lesser extent to Gothenburg and Malmö, which is why we perform the main analysis without these municipalities but re-run the main analysis over a full sample as a form of robustness and to gauge potential spillover effects.

Table 2: Population and number of students enrolled in Sweden's largest student cities.

Institution	**Municipality**	**Population**	**Students**
Larger student cities			
Uppsala University	Uppsala	242 140	47 378
Lund University	Lund	128 384	40 706
Linneaus University	Växjö	97 137	34 974
Linköping University	Linköping	166 673	31 528
Smaller student cities			
Umeå University	Umeå	132 235	30 547
Mid Sweden University	Sundsvall	99 361	18 228
Karlstad University	Karlstad	96 466	16 508
Luleå Institute of Technology	Luleå	79 244	15 770
Borås College	Borås	114 445	15 670
Mälardalen University	Västerås	158 653	14 346
Örebro University	Örebro	158 057	13 501
Not included in main analysis			
Stockholm University	Stockholm	984 748	54 848
Gothenburg University	Gothenburg	596 841	47 017
Malmö University	Malmö	357 377	20 466
KTH Royal Institute of Technology	Stockholm	975 551	16 857
Chalmers Institute of Technology*	Gothenburg	596 841	12 041
Total		3 411 761	430 385

Note: Source: Statistics Sweden (2022), Swedish Council for Higher Education (2021/2022).

To analyze the behavior of rents for long-term sublets due to a demand shock, we compare student cities with the rest of Sweden. Defining student cities is not trivial. In recent years, Swedish education policy has consisted of increased access to higher education across the country and today there exists approximately 50 higher education institutions (public and private) across Sweden's 20 counties. The question thus becomes which of the cities with higher education institutions (excluding Stockholm, Gothenburg and Malmö) are comparable to the control group, i.e., the rest of Sweden.

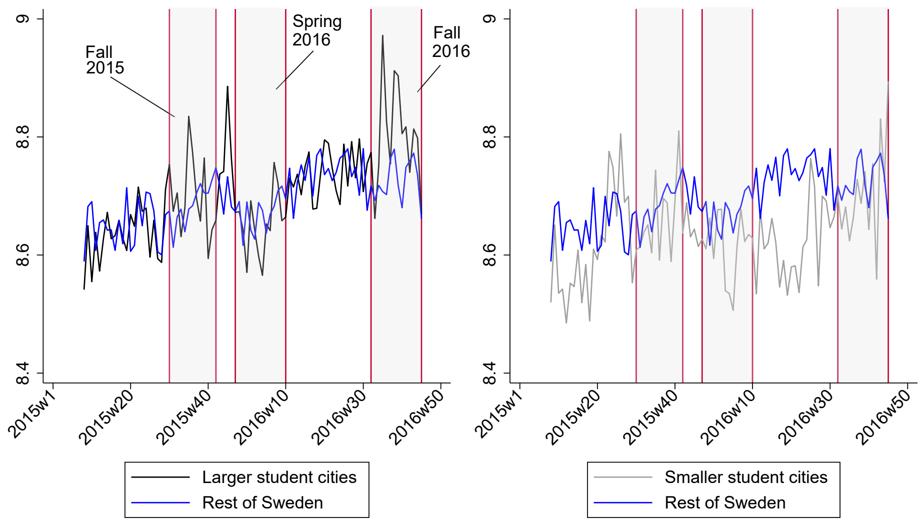
As a starter, the fifth largest student city in terms of number of enrolled students is Umeå, which stands out from the four largest student cities by being one of the northernmost cities in Sweden and far removed from larger metropolitan areas. In fact, the four largest student cities are generally more accessible and closer to metropolitan areas than many of the smaller student cities, with Uppsala being close to Stockholm and Lund being close to Malmö. Exceptions within the smaller student city group include Borås and Västerås, which are close to Gothenburg and Stockholm, respectively. The four largest student cities together enroll almost 155 000 students, which is higher than the following seven student cities combined, which enroll approximately 125 000 students. The present data also shows a considerably difference of market activity between these two groups: larger student cities have approximately 1 000 more total observations than their smaller counterpart. This is true even though some of the smaller student cities have higher general population numbers.

As is apparent in figure 2, the log of monthly rent in larger student cities follows the rest of Sweden closely during the pre-treatment periods. This is contrary to smaller student cities, which deviate quite noticeably from the trend in the rest of Sweden before both Fall semesters. To guarantee that any potentially observed treatment effect can be attributed to a deviation from an otherwise shared common trend, we divide the student cities into two groups when testing for pre-trends.

Estimation strategy

We assume that the rent follows a hedonic pricing framework, meaning that the rent of a dwelling sums up to the monetary value of relevant characteristics. Using the log of monthly rent as our dependent variable, we adopt a semi-log specification which allows for interpretation of marginal changes in the dependent variable while accounting for

Figure 2: Log of monthly rent over entire sample period.



Note: See appendix E for results of log of square meters and number of rooms over calendar and event weeks.

non-linearities. In a linear specification, the added utility of an additional unit of some characteristic is proportional to the utility of the previous unit of the same characteristic. It is however reasonable to assume that tenants' utility is marginally decreasing in the included covariates. We estimate variants of a baseline model with pooled treatment windows of the form

$$ln(rent_{imt}) = \phi\, studentcity_{im} \;+\; \gamma\, treatment^{\rho}_{mt} \;+\; \delta\, studentcity_{im} \times treatment^{\rho}_{mt} \;+ \\ city\ FE_m \;+\; time\ FE_t + \mathbf{x}_i^{'}\beta \;+\; u_{it} \quad (1)$$

The above specification uses the dependent variable $ln(rent_{imt})$, which is log of monthly rent for observation i in municipality m at week t. $studentcity_{im}$ indicates whether observation i in municipality m is a student city excluding Stockholm, Gothenburg and Malmö, as presented in table 2. Its coefficient ϕ captures the group effect relative to the control group. There is potential heterogeneity of treatment effects over time when analyzing multiple treatment windows (de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2021; de Chaisemartin & D'Haultfoeuille, 2022; Borusyak et al., 2023) – for instance, the 2015 Fall semester started on the 31st of August (week 26) while the 2016 Fall semester started on the 29th of August (week 35). We therefore assign treatment relative to the treatment window, with the treatment period defined to be five weeks before up to five weeks after the start of the university semester. Thus, $treatment_{mt}$ takes the value 1 if an observation in municipality m at week t is posted $\rho = -5, ..., 0, ..., 5$ weeks before or after the week when the university semester starts ($\rho = 0$) for a given semester. This specification pools all the weeks during our treatment window into one, allowing us to assess systematic differences before and after treatment between our treatment and control groups. γ captures the treatment effect during the treatment window relative to all other periods in the sample. For one specification, we split the treatment window into an anticipatory period for $\rho = -5, ..., -1$ and a delayed response period for $\rho = 1, ..., 5$. $studentcity_{im} \times treatment^{\rho}_{mt}$ is the interaction term and its coefficient δ captures the difference between treatment and control group for periods $\rho = -5, ..., 0, ..., 5$ relative to all other periods. $\mathbf{x}_i^{'}\beta$ is a $1 \times K$ vector containing individual-level covariates. $time\ FE_t$ includes daily, monthly and yearly fixed effects, and $city\ FE_m$ are municipality-specific fixed effects. u_{it} is a normally distributed, zero mean and constant variance, error term. We cluster standard errors at the municipality level throughout the analysis.

We break down the baseline specification into each week separately in order to investigate the common-trends assumption. This specification takes the form

$$\begin{aligned} ln(rent_{imt}) = city\ FE_m\ +\ time\ FE_t + \mathbf{x}_i^{'}\beta\ +\ \phi studentcity_{im}\ +\ \sum_{\rho=-12}^{8} \gamma^{\rho} treatment^{\rho}_{mt} \\ +\ \sum_{\rho=-14}^{5} \delta^{\rho}(studentcity_{im} \times treatment^{\rho}_{mt})\ +\ I_{mt} +\ u_{it} \quad (2) \end{aligned}$$

This specification now defines $\rho = -14, ..., 0, ..., 8$, with each week entering separately, allowing us to estimate the week-for-week treatment effect between our treatment and control groups. γ now captures the treatment effect relative to some ex-ante specified baseline period. Following Schmidheiny and Siegloch (2023) and Mense et al. (2023), we define I_{jt} as a binned indicator capturing units outside of $\rho = -14, ..., 0, ..., 5$. As Swedish students receive their notice of acceptance little over a month prior to the start of the semester, we deem this as an appropriate window-length to capture pre-trends. It is however not trivial to decide upon the lengths of the pre-and post-treatment windows, as when the post-treatment window grows large enough, it will coincide with the pre-treatment of the next treatment window. We choose to include a pre-treatment window of 9 weeks ($\rho = -14, ..., 6$), as $\rho = -15$ for the 2016 Spring semester coincides with $\rho = 5$ for the 2015 Fall semester.

4 Data and descriptive statistics

Data treatment and variables

The present paper utilizes list data from the Swedish website Blocket with observations running from March 2015 to October 2016. Listings with clearly incorrectly specified information as well as listings that do not specify monthly rent, size in square meters and the total number of rooms available, are dropped. We are unable to identify unique listings and dwellings over time with the given data set, which gives rise to some potential concerns. For one, since the university semester is reoccurring, landlords can time the lease duration to the cyclicity of the semester to charge higher rents. The same listings can thus potentially reappear around the same time every year in the data, causing selection bias. This should be little cause for concern as tenants can terminate the contract at an earlier-than-agreed-upon date. A landlord would therefore have to wait until the start of the university semester in order to time the market. Another issue is that the same listing can reappear in the data over a short time span if the landlord updates the listed rent due to changes in demand. To combat this, we drop duplicate observations from the same year but allow for repeated observations in different years. This restricts the same listing to reappear at most three times over the sample period. In the case of a duplicate in the same year, we choose to retain the most recent observation as it best reflects the landlord's latest preferences.

Dwelling, geographic and location characteristics are created by quantifying free text that landlords have entered in the "other comments"-section for a listing. The descriptive statistics of these variables are found in appendix C. When creating a variable for listings that mention the existence of a balcony, listings that explicitly mention that there does not exists a balcony might accidentally be included. An intermediate step is therefore to create multiple tease-variables such as a dummy for "no balcony", which we then subtract from the dummy "balcony".

In total, we include 2 continuous variables for dwelling size (square meters and number of rooms) and 18 dummy variables based on the information provided by the landlord: 4 variables for the type of dwelling (apartment, semi-detached, spare room, overnight apartment), 3 variables for dwelling characteristics (balcony, furnished, parking), 3 variables for the condition of the dwelling (recently constructed, recently renovated, if the dwelling can be considered modern), 3 variables for spatial information (if the apartment is close to campus; the central station; the city center), 2 variables for lease duration (indefinite, short-term), and 3 variables indicating the preferred tenant to whom the landlord wishes to rent (student, male, female tenant).

There are two additional shortcomings worth highlighting. As with all list data, the disclosed information might not reflect actual market outcomes. An example of this is that the listed rent might not equal the actual rent level. There is little reason to believe that the deviation between listed and actual rent is considerable: Stockholm Chamber of Commerce (2022) finds the deviation between listed and actual rent to be 0,9 percent for the entirety of Sweden, indicating only a small downward-adjustment. Listings on Blocket might differ from listings on other rental websites, which could limit the generalization of our results. This issue should not be a cause for concern as Blocket is the most popular platform for long-term sublets in Sweden (The Swedish National Board of Housing, Planning and Construction, 2018).

Data balance and distribution

Table 3 present means and standard deviations over treatment and group statuses for some of the variables of interest.[13] Column 1 and 2 compares the control group with and without Gothenburg and Malmö included, whereas column 3 and 4 concern the control group without Gothenburg and Malmö conditional on treatment. We split larger student cities and smaller student cities into two separate samples, presented in column 5 and 6 for the former and column 7 and 8 for the latter.

Unsurprisingly, the inclusion of Gothenburg and Malmö showcases higher rents and somewhat smaller living space, which follows from the fact that dwellings tend to be smaller in metropolitan areas. When excluding Gothenburg and Malmö, nominal monthly rent actually decreases after treatment in the control group (6 722 sek to 6 656 sek), whereas it increases in both larger student cities (from 6 685 sek to 7 105 sek) and smaller student cities (6 199 sek to 6 289 sek). The descriptive difference-in-differences is therefore 486 sek (47.96 usd) and 156 sek (15.40 usd) in the respective student city groups compared to the control. The variation in the mean of square meters and number of rooms (and their logs) is small within each group – with regards to square meters before and after treatment, the control group varies between 76.18 and 75.30, larger student cities vary between 52.73 and 54.48, and smaller student cities vary between 60.42 and 61.94. The negligible difference in living space within each group indicates that it is not a sudden increase in the supply of larger dwellings that is causing the increase in monthly rent when treatment is active.

Kernel density plots over the logarithm of monthly rent are provided in appendix D; the eye test indicates a normal distribution of our dependent variable with a small difference

[13] See section 3 for a detailed definition of the treatment variable.

in means. Larger student cities conditional on treatment show increased density on the right side of the distribution, highlighting the rent increase for larger student cities that we observe in table 3. The same pattern cannot be observed for the control group or smaller student cities.

Table 3: Average values of continuous variables with and without treatment (standard deviations in parenthesis).

Treatment	.	.	**Yes**	**No**	**Yes**	**No**	**Yes**	**No**
Large student cities	.	.	**No**	**No**	**Yes**	**Yes**	**No**	**No**
Small student cities	.	.	**No**	**No**	**No**	**No**	**Yes**	**Yes**
Gothenburg and Malmö	**Yes**	**No**	**No**	**No**	**No**	**No**	**No**	**No**
Nominal monthly rent (sek)	7688.06	6643.19	6655.58	6722.10	7105.15	6684.52	6289.26	6199.33
	(4874.09)	(3691.34)	(3775.57)	(3746.14)	(4478.16)	(3489.67)	(3135.88)	(3238.99)
Square meters of the dwelling	65.53	69.29	75.30	76.18	54.48	52.73	61.94	60.42
	(45.45)	(46.58)	(47.14)	(48.13)	(43.92)	(38.53)	(47.83)	(39.70)
The number of rooms of the dwelling	2.42	2.52	2.73	2.74	2.11	2.01	2.21	2.20
	(1.46)	(1.49)	(1.53)	(1.53)	(1.37)	(1.23)	(1.40)	(1.32)
Observations	62,086	37,474	9,545	15,037	2,075	4,869	1,950	3,998

Note: See appendix C for summary statistics of all included covariates.

5 Results

Baseline results - assessing pre-trends

We start by first plotting equation 2 in figure 3, i.e., each separate week starting from $\rho = -14, ..., 0, ..., 5$ (conditional on covariates and fixed effects), with the last week before the treatment window $\rho = -6$ entering as baseline. Panel A and B are the same model where we interact the treatment indicator with both larger and smaller student cities to assess the validity of including the smaller student cities as a treatment group. While the pre-trends seem stable across treatment and control groups, $\rho = -8$ shows a small but statistically significant ($0.05<p<0.1$) treatment effect in smaller student cities and the rest of Sweden. When we instead re-estimate the model with smaller student cities included in the control group over 5 week and 9 week pre-treatment windows, as is the case in panel

C and panel D, we see that the statistical significance of the trend deviation vanishes.

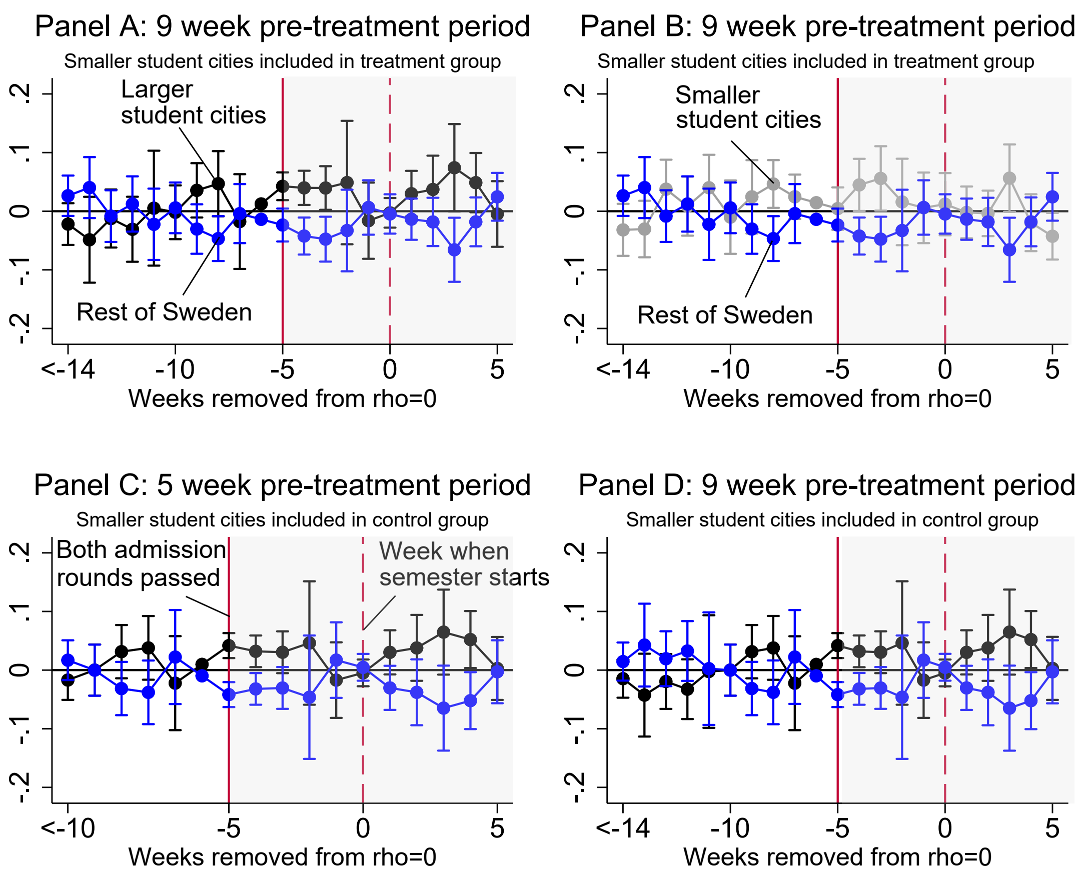
We further interact the treatment indicator with both student city groups over the pre-treatment period when including linear, quadratic and cubic time trends. There is no sign of significant pre-trends between the larger student cities and the control group, nor within the control group in relation to both treatment groups. However, we uncover small but statistically significant ($0.05<p<0.1$) linear and quadratic trends for smaller student cities. Refitting the model and including smaller student cities in the control group yields stable estimates and no signs of higher order pre-trends; see appendix F for these results. These results hold even when estimating higher-order pre-trends conditional on covariates. We thus view it as appropriate to include smaller student cities in the control group for the rest of the analysis. For a more straight-forward interpretation, we simply refer to 'student cities' when referencing the treatment group containing what was previously defined as larger student cities.

Doing so and focusing on a 9 week pre-treatment period in panel D shows immediate treatment effects in the included student cities after both admission rounds have passed. For $\rho = -5, -4$, the average treatment effect reaches 3.69 percent and, with some delay, the next spike for $\rho = 4$ showcases a treatment effect of 5.21 percent. This points towards significant anticipation and delayed response effects, with somewhat larger effects in the latter period. These effects remain intact even when including a larger binned indicator and 5 week pre-treatment periods in panel C.

Baseline results - treatment, anticipation and delayed response effects

We continue by setting treatment for observation i in municipality m to equal 1 for $\rho = -5, ..., 0, ..., 5$ and estimating 4 separate models. Aggregating the treatment weeks like this allows us to assess systematic differences before and after treatment conditional on fixed effects and covariates, without having to rely on a single baseline period. The covariate estimates in appendix G show negative coefficients for the student city indicator, meaning that rent over the sample period is smaller in student cities compared to the rest

Figure 3: Estimated treatment effect based on the specification in equation 2.



Note: $t - 6$ enters as baseline. Plotted are the 90 percent confidence intervals. Heteroscedasticity-robust standard errors are clustered on the municipality level. The model specification follows the baseline model from table 4.

of Sweden. This similarly holds for the coefficient estimates of the treatment indicator, implying that rent is generally lower around the start of the university semester compared to other periods. It is only when the interaction happens, i.e., when listings in student cities during the treatment period are posted, that there is a positive and significant treatment effect. The observed treatment effects of the interaction terms are reported in table 4.

The results show that the rent increase $p_2^{Sub} - p_1^{Sub}$ as put forth in section 2 does exist and is considerably protruding during the treatment period. We first estimate a barebones model in column 1 including only the number of rooms and the log of square meters as covariates, as well as time and municipality fixed effects, to probe the sensitivity of the covariates we generated by quantifying the free text for each listing. The observed treatment effect is 3.31 percent, which is somewhat smaller compared to the baseline model in column 2 that includes all time-invariant covariates and showing an observed treatment effect of 4.01 percent. Swapping from municipality to county fixed effects in column 3 does not affect the result in a meaningful way, increasing the observed treatment effect slightly to 4.22 percent.

In column 4, we break down the treatment period into an anticipation (after admittance but before the semester starts) and delayed response (the weeks right after the semester has started) period. The observed treatment effect is 3.72 percent during the anticipation period and 4.98 percent during the delayed response period. As speculated earlier, the well known difficulty of finding sublet housing in Sweden rather naturally leads to the assumption that the observed anticipation effect should be greater than the delayed response effect. This is because the probability of finding a long-term sublet increases the earlier one starts searching for housing. The opposite seems to hold true which indicates that the demand surplus spillover from the primary market, and its subsequent congestion, forces many students to wait until the university semester has passed in order to satisfy their housing demand.

Table 4: Estimated treatment effects from the model in equation 1.

	(1)	(2)	(3)	(4)
Model specification	Barebones model	Baseline model	County FE	Anticipation and delayed response
Student cities × treatment	0.0331*** (0.0127)	0.0401*** (0.0118)	0.0422*** (0.0117)	
Student cities × anticipation				0.0372** (0.0152)
Student cities × delayed response				0.0498*** (0.0131)
Observations	37,474	37,474	37,474	37,474
R^2	0.559	0.571	0.509	0.571
Covariates	No	Yes	Yes	Yes
City FE	Yes	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. Heteroscedastic-robust standard errors clustered on the municipality level in parenthesis. The anticipation period is for $\rho = [-5, ..., -1]$ and a delayed response for $\rho = [1, ..., 5]$. Time FE consists of yearly, monthly and daily fixed effects. See appendix G for estimated effects of covariates.

Dynamics over dwelling size and type

To have a more complete picture of the dynamics of long-term subletting, we look at the observed treatment effects over different dwellings sizes and types. Table 5 and table 6 estimate the treatment effect over subsamples of square meter sizes and the number of rooms. At a first glance, the hypothesis of greater observed treatment effects in the tails of the distribution of living space finds support in the results. Dwellings of 20-29 square meters show a statistically significant treatment effect of 6.42 percent, with the next statistically significant group being 60-69 square meters with an effect of 9.80 percent. Dwellings equal to or greater than 70 square meters show a comparably small, statistically significant effect of 3.15 percent. This trend of treatment effects being the most protruding in the tails can similarly be seen over the number of rooms.

Interestingly, there is no statistically significant treatment effect for the smallest dwelling size of 0-19 square meters. This could potentially be the result of the absence of demand for these dwellings among students. Such an explanation is however unsatisfactory. The more reasonable explanation is that demand for these dwellings is high across both treatment and control groups, thus muddling out the observed treatment effect. In fact, when inspecting dwellings of 0-10 square meters, we uncover a statistically significant ($p<0.1$) treatment effect of 8.27 percent. However, there is some uncertainty attached to this result as the sample size is small with only 527 observations. The group of dwellings between 11 and 19 square meters shows a statistically insignificant effect with 2,593 observations, which seemingly negates the significant effect from the group containing dwellings of 0-10 square meters.

As is apparent in table 7, apartments are the dominant type and the only type showing a statistically significant effect, of magnitude 4.4 percent. This result is unsurprising as the majority of observations are apartments. Furthermore, students tend to want to live closer to campus and the city center – in areas that predominantly consist of apartments – which can explain the negative, albeit statistically insignificant, effects for both villas and semi-detached dwellings.

Table 5: Estimated treatment effect over subsamples of square meters.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	0-19m^2	20-29m^2	30-39m^2	40-49m^2	50-59m^2	60-69m^2	>69m^2
Student cities × treatment	0.0171	0.0642**	0.0191	0.0525	0.0398	0.0980***	0.0315*
	(0.0194)	(0.0275)	(0.0279)	(0.0519)	(0.0257)	(0.0211)	(0.0162)
Observations	3,120	3,246	3,305	3,490	4,011	4,448	15,854
R^2	0.283	0.373	0.428	0.473	0.383	0.320	0.346
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-specific linear time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Heteroscedastic-robust standard errors clustered on the municipality level in parenthesis. See appendix G for estimated effects of covariates.

Table 6: Estimated treatment effect over subsamples of the number of rooms.

	(1)	(2)	(3)	(4)	(5)
	1 room	2 rooms	3 rooms	4 rooms	5+ rooms
Student cities × treatment	0.0204***	0.0561***	0.0300	0.0873*	0.131
	(0.00743)	(0.0176)	(0.0322)	(0.0460)	(0.160)
Observations	11,923	10,046	7,855	3,958	1,651
R^2	0.390	0.403	0.290	0.353	0.428
Covariates	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Municipality-specific linear time trend	Yes	Yes	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. Heteroscedastic-robust standard errors clustered on the municipality level in parenthesis. See appendix G for estimated effects of covariates.

Table 7: Estimated treatment effects over dwelling type.

	(1)	(2)	(3)
	Apartment	Villa	Semi-detached
Student cities × treatment	0.0459***	-0.0219	-0.0553
	(0.0148)	(0.0427)	(0.0558)
Observations	30,698	6,015	671
R^2	0.543	0.569	0.717
Covariates	Yes	Yes	Yes
City FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Municipality-specific linear time trend	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. Heteroscedastic-robust standard errors clustered on the municipality level in parenthesis. See appendix G for estimated effects of covariates.

Robustness: Alternative specifications and samples

We reassess our uncovered results over different specifications and sample sizes; the output is presented in table 8. We provide the baseline estimate in column 1 for easier comparisons. For starters, all specifications show statistical significance on some conventional level. In column 2, we fit municipality-specific weekly linear time trends. As previously mentioned, this linear time trend accounts for the possibility that there are some underlying trend differences between our student cities and the rest of Sweden, which switches the identifying assumptions to deviations from this trend. This model shows an estimated treatment effect of 3.76 percent, which is arguably comparable to the baseline estimate of 4.23 percent without such a time trend.

The models in column 3 through 5 asses the time variation of the underlying sample, with the model in column 3 utilizing a sample running from 2015 week 9 to 2015 week 52 and showing an observed treatment effect of 3.09 percent. Running the model for the entirety of 2016 in column 4, starting from 2016 week 1 to 2016 week 42, gives a somewhat larger observed treatment effect of 4.1 percent. The Fall semester commences the new academic year and considerably more students are accepted around this period. We therefore run a separate model for only the Fall semesters, pooling together observations between 2015 week 9 to 2015 week 42 and 2016 week 9 to 2016 week 42, and uncover an observed treatment effect of 4.87 percent.

The models in column 6 through 8 assess spillover effects. The exclusion of smaller student cities from the control group in column 6 shows a treatment effect of 4.26 percent, which is equal to the spillover effect when omitting neighboring municipalities. Thus, while these models show evidence of some spillover, the effect is rather small compared to the baseline model. When including the full sample with both Gothenburg and Malmö, the estimated treatment effect decreases to 2.43 percent. However, given that the sample size in this specification almost doubles, it is not apparent that the previously assumed common trends assumption holds. We therefore assess pre-trends of different orders and re-estimate the baseline results over the full sample including Gothenburg and Malmö in appendix B. We uncover no systematic deviations from a shared common trend and conclude that our results are robust to including Gothenburg and Malmö in the analysis, with some accompanying spillover effects when doing so.

Table 8: Estimated treatment effects over dwelling type.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Model specification	Baseline model	City time trend	2015	2016	Fall semesters	Small student excluded	Neighbors excluded	Full sample
Student cities × treatment	0.0401***	0.0349***	0.0309**	0.0410***	0.0487***	0.0426***	0.0426***	0.0243**
	(0.0118)	(0.0111)	(0.0152)	(0.0140)	(0.0168)	(0.0121)	(0.0121)	(0.0111)
Observations	37,474	37,474	18,003	19,471	28,704	32,200	34,840	62,086
R^2	0.571	0.574	0.576	0.572	0.573	0.583	0.569	0.596
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. Heteroscedastic-robust standard errors clustered on the municipality level in parenthesis. The baseline estimate comes from column 2 in table 4. See appendix G for estimated effects of covariates.

Robustness: Placebo testing

We have accounted for confounding factors, fixed effects, potential trend differences and different samples to assess the validity of our estimates. In this section, we employ two placebo tests to further test the validity of our results.[14] In the first test, we compare 120 real truncated samples with 120 truncated placebo samples with fake treatment groups, where we randomly assign 18.53 percent of observations into a fake student city group to mimic the actual data. A statistically significant finding would in this case indicate that the results are driven by some underlying difference in trends between the municipalities over time, thus violating the common trends assumption. In the second test, we compare the estimated treatment effect of the 120 real truncated samples with 120 truncated placebo samples with fake treatment periods, where we randomly assign a fake treatment period to 36.21 percent of the observations to mimic the actual data. This test assesses potential irregularities in our uncovered results with regards to treatment, for instance if

[14]For the latest on placebo sampling, see for instance Eggers et al. (2021); Ye et al. (2022); Roth et al. (2023).

the observed treatment effect is common over the sample period or if there are any other detrimental mistakes in our assumed specification.

Differently put, statistically significant effects in either test raises concern of unmeasured confounding bias, whereas statistically insignificant effects strengthen the causal conclusion of the analysis.

Table 9 show average treatment effects and t-values, as well as the number of iterations with statistical significance on any conventional level. The baseline estimate, which comes from column 2 in table 4, shows an observed treatment effect of 4.01 percent (t-value = 3.59). The 120 real truncated samples show an average coefficient estimate not far off from the baseline with 3.91 percent (t-value = 2.90). For comparison, the placebo treatment group and placebo treatment period samples have average treatment effects of 0.09 percent (t-value = 0.10) and -0.03 percent (t-value = -0.03), respectively. Of the 120 real truncated samples, 104 show significance on any conventional level and the majority (73 iterations) on the 1 percent level. The placebo treatment group samples show 10 iterations of statistical significance, whereas the placebo treatment period samples are slightly more with 26 significant iterations.

These results are visiually presented in figure 4, with the estimated treatment effects plotted vertically and t-values reported horizontally. Panel A compares the real truncated samples with the placebo treatment groups and panel B with the placebo treatment periods. Contrarily to the baseline and real truncated samples, some of the placebo tests show negative coefficients. The real truncated samples are closely scattered around the baseline estimate, with both placebo tests being significantly skewed downards and to the left, far removed from the baseline estimate.

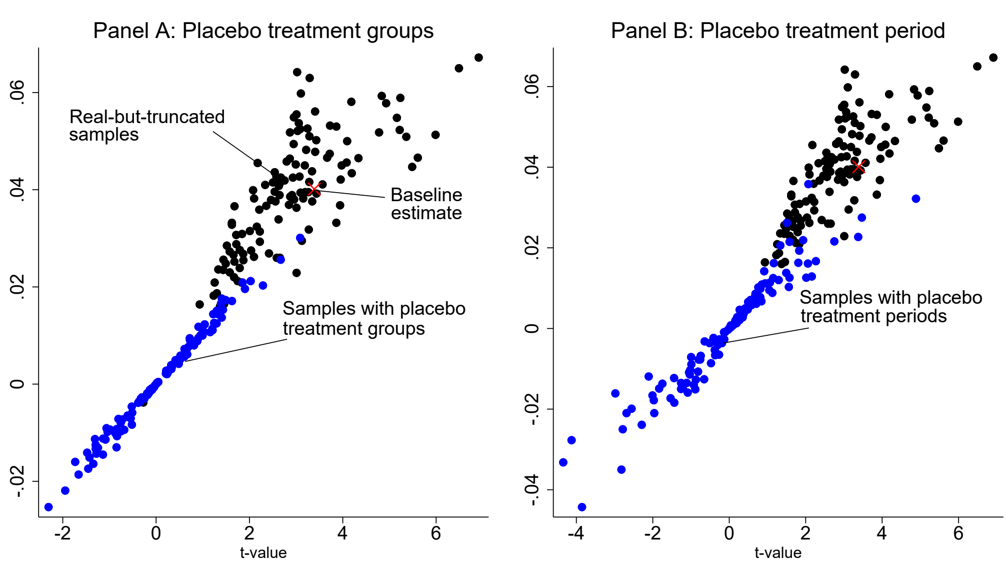
Based on these and other tests performed throughout the present analysis, we view our uncovered results as robust to both common irregularities and differences in trends between the treatment and control group.

Table 9: Average coefficient estimates, t-values and number of observations over 120 placebo tests.

	Average treatment effect	Average t-value	Number of samples with p<0.01	p<0.05	p<0.1
Baseline estimate	0.0401	3.39			
Real truncated sample	0.0391	2.90	73	18	13
Placebo treatment group	0.0009	0.10	2	3	5
Placebo treatment period	-0.0003	-0.03	11	10	5

Note: *** p<0.01, ** p<0.05, * p<0.1. All estimated models follow the model in column 1 in table 4. Heteroscedasticity-robust standard errors are clustered on the municipality level.

Figure 4: Analysis with placebo treatment groups and placebo treatment periods.



All estimated models follow the model in column 2 in table 4. Heteroscedasticity-robust standard errors are clustered on the municipality level.

6 Conclusions

Many individuals in Sweden without enough time-in-queue on the rent controlled primary market have to satisfy their demand on the market for long-term sublets, but a lack of data has made rigorous study of this market difficult. Considering that rent control is making its way back into policy debate, it is increasingly important to understand the dynamics of long-term subletting. Insofar as the empirical literature has analyzed subletting markets, focus has been on short-term rentals through sharing-economy platforms such as Airbnb (Gouveia et al., 2020; Combs et al., 2020; Koster et al., 2021; Reichle et al., 2023; Hill et al., 2023). This is a fundamentally different market compared to long-term subletting and the lack of inquiry into long-term subletting in Sweden and elsewhere runs the risk of excluding an important component in the analysis of rental markets. This paper has aimed to fill this gap by analyzing how rents for long-term sublets react over multiple periods of hypothesized fluctuations in supply and demand.

We have utilized a novel dataset consisting of long-term sublet rentals between March 2015 and October 2016. We find that rent levels react strongly strongly to demand shocks, with observed treatment effects in student cities between 3.31 percent and 4.22 percent, depending on model specification. The effects peak immediately after the university semester has started, reaching as high as 4.98 percent and indicating that students have to hold out some time before they are able to satisfy their demand. These rent increases are most pronounced in either tail of the distribution of living space, most likely as demand consists of students with low incomes (and tight budget constraints) or groups of students with higher aggregated incomes (and therefore higher budget constraints). Our findings are highly robust to alternative specifications, such as when including municipality-specific time trends, utilizing alternative sample sizes, and conducting placebo tests.

An average observed treatment effect of 4.22 percent in our baseline estimate implies that average monthly rent for long-term sublets in student cities increases from 6 810 sek

(619 usd) to 7 097 sek (645 usd)[15] if the lease is signed around the start of the university semester. Seeing as how the lease generally runs for a year, this implies that tenants in student cities pay 3 449 sek (304 usd) more per year compared to tenants in the rest of the country who sign their lease outside the treatment period. This is a considerable difference, especially when accounting for the low income of students. Primary market tenants tend to face rents well below market level due to rent control. Households on the primary market are generally older and display higher-than-average incomes, which generally follows from the high correlation between tenant age and increased time-in-queue on the primary market (Donner & Kopsch, 2021). The uncovered results show that younger households, who tend to dominate the demand side for long-term subletting, have to pay considerably higher rent conditional on when the lease is signed. Households on the primary rental market are completely protected from such fluctuations due to rent control.

As we discussed in section 2 and show in appendix A, the uncovered results are likely higher than what they would be given a market-based rent level. More lenient rent control could potentially benefit younger tenants by decreasing the magnitude of the rent increases on the market for long-term sublets at the expense of households on the primary market. We advise policy makers to take such considerations into account when designing measures for rent control. In this vein, classic welfare analyses can greatly help when assessing the pros and cons of such regulatory mechanisms.

Future research should first and foremost focus on analyzing long-term subletting in different countries and institutional contexts to improve the general understanding of its mechanisms and dynamics. A closely related topic is to assess how the socioeconomic status of households on subletting markets is affected due certain policy changes, such as more stringent rent control. The estimation of welfare losses due to imposed rent control, when taking into account potential (adverse) indirect effects on alternative markets such as subletting markets, should similarly be of interest to the literature.

[15] Exchange rate for 29 August 2023.

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A Derivation of the market for long-term subletting

In an institutional setting where the entire primary market is rent controlled and long-term subletting is unaffected, two key results can be inferred:

1. Tightening of regulation on the primary market causes rents on the subletting market to rise.
2. The difference $p_2^{Sub} - p_1^{Sub}$ as uncovered in the main analysis in section 5 is higher compared to an institutional setting with no rent control.

The mechanism resulting in proposition 1 is discussed in Skak and Bloze (2013) and Mense et al. (2017, 2023) with regards to second generation rent controls, i.e., when a subset of the primary rental market supply is rent controlled. We prove theoretically that this holds even with first generation rent controls, i.e., when the entire primary market is rent regulated and subsequent secondary markets (such as subletting markets) are left directly unaffected.

Proof. The market for rental housing as defined in figure 1 is given by

$$\begin{aligned}
S^{Primary} &= a + bp && \text{(Primary market supply)} \\
S^{Sub} &= c + dp^{Sub} && \text{(Long-term subletting supply)} \\
D^1 &= e - fp && \text{(Quantity demanded before shock)} \\
D^2 &= g - hp && \text{(Quantity demanded after shock)}
\end{aligned}$$

with $D^2 \geq D^1$ and $a,\ b,\ c,\ d,\ e,\ f,\ g$ and h being parameters.

Under perfect competition, there exists no subletting market and all supply is provided on the primary market, implying that $S^{Sub} = 0$ and in equilibrium, $S^{Total} = S^{Primary} = D^1$ giving $p^* = \frac{e-a}{f+b}$. If regulation is imposed, restricting the rent from p^* to $p^{Ceiling}$ on the primary market while leaving p^{Sub} unaffected, supply on the subletting market is no longer zero and total supply becomes

$$S^{Total} = (a + bp^{Ceiling}) + (c + dp^{Sub})$$

To find household's maximum willingness to pay on the subletting market under $p^{Ceiling}$ on the primary market, we set all supply under $p^{Ceiling}$ and solve for p^{Sub} when supply equals demand

$$(a + bp^{Ceiling}) + (c + dp^{Ceiling}) = e - fp^{Sub}$$

$$p^{Sub} = \frac{e - a - c - (b + d)p^{Ceiling}}{f}$$

Taking the partial derivative with respect to $p^{Ceiling}$ gives

$$\frac{\partial p^{Sub}}{\partial p^{Ceiling}} = -\frac{b+d}{f}$$

implying that decreases (increases) in primary market rents lead to increases (decreases) in sublet market rents, in line with proposition 1. This result is empirically proven with regards to second generation rental control in Skak and Bloze (2013); Mense et al. (2017, 2023), and similarly for long-term sublets in Stockholm in Stockholm Chamber of Commerce (2022).

To prove proposition 2, we infer what happens in the event of a demand shock for the rent of sublets p^{Sub} under rent control and the rent of primary dwellings p^* without rent control. A sudden positive demand shock occurs such that demand shifts from $D^1 = e - fp^{Sub}$ to $D^2 = g - hp^{Sub}$ for sublets. By definition, $\Delta p^{Sub} = p_1^{Sub} - p_2^{Sub} = \frac{e-a-c-(b+d)p^{Ceiling}}{f} - \frac{g-a-c-(b+d)p^{Ceiling}}{h} \leq 0$. Now consider the same positive change in demand in the absence of rent control, which gives $\Delta p^* = p_1^* - p_2^* = \frac{e-a}{b+f} - \frac{g-a}{b+h} \leq 0$. If the rent increase for sublets is higher with the primary market entirely rent controlled, compared to a situation with no rent control, then $\Delta p^{Sub} = p_1^{Sub} - p_2^{Sub} \leq p_1^* - p_2^* = \Delta p^*$ must be true.

We have

$$\frac{e-a-c-(b+d)p^{Ceiling}}{f} - \frac{g-a-c-(b+d)p^{Ceiling}}{h} \leq \frac{e-a}{b+f} - \frac{g-a}{b+h} \qquad \text{(flip the terms over)}$$

$$\frac{g-a-c-(b+d)p^{Ceiling}}{h} - \frac{e-a-c-(b+d)p^{Ceiling}}{f} \geq \frac{g-a}{b+h} - \frac{e-a}{b+f} \qquad \text{(set } f = h\text{)}$$

$$\frac{g-a-c-(b+d)p^{Ceiling} - e + a + c + (b+d)p^{Ceiling}}{h} \geq \frac{g-a-e+a}{b+h} \qquad \text{(rearrange)}$$

$$\frac{g-e}{h} \geq \frac{g-e}{b+h}$$

By definition, $g \geq e$ for a positive demand shock, and $b, h \geq 0$ by design, as positive values are required for a negative slope of $D^2 = g - hp$ and positive slope of $S^{Primary} = a + bp$. The cases where the above equation is not true is if the slopes of D^1 and D^2 differ, which happens if $f \neq h$.[16] Rent increases on the subletting market given primary market rent control are thus at least as great as rent increases under no rent control, while the reverse is not true. Note that $p^{Ceiling}$ only has an effect on this relationship if $p^{Ceiling}$ is allowed to adjust between demand shocks.

The implications of proposition 1 and proposition 2 is that the uncovered treatment effect in the present paper is larger than it would be on a completely unregulated rental market. Both propositions offer avenues for further research into the potential (adverse) effects of rent control on unaffected segments – in particular with regards to unaffected subletting markets. ■

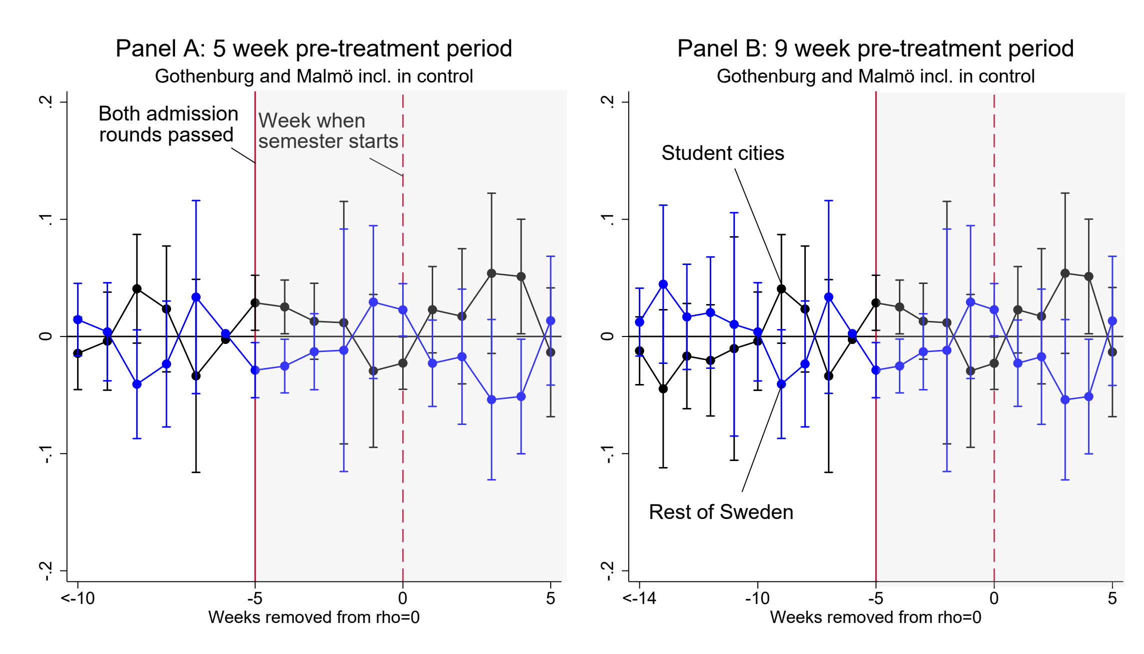
[16] One can experiment with cases where this is the case for some interesting market dynamics.

B Additional robustness check of baseline models with Gothenburg and Malmö included

To corroborate our findings, we perform robustness checks by including both Gothenburg and Malmö in the sample, increasing the number of observations from 37 474 to 62 086. There is no statistically significant deviation when testing for shared linear, quadratic and cubic time trends during the pre-treatment period; see appendix F. Figure 5 plots each separate week and the 90-percent confidence intervals according to equation 2 and following the baseline model from table 4. The results are largely identical to the baseline results, with no significant pre-treatment effects. The absence of any systematic deviations from the shared common trend can be taken to highlight a general comparability even when including Gothenburg and Malmö in the control group. We interpret the results uncovered here as favorable to the overall robustness of the results in the main analysis.

Table 10 shows the regression output when aggregating the treatment windows to compare difference before and after treatment. As expected, the inclusion of Gothenburg and Malmö decreases the treatment effect: The baseline estimate in column 1 showcases an effect of 2.25 percent compared to 4.01 percent for the baseline model in table 4. Changing from municipality to county fixed effects in column 2 leads to a larger observed treatment effect, whereas including municipality-specific linear time trends marginally decreases the observed treatment effect. The magnitude of the observed treatment effect when assessing spillovers is small, in line with the main results.

Figure 5: Estimated treatment effect based on the specification in equation 2 with Gothenburg and Malmö included.



Plotted are the 90 percent confidence intervals with $\rho = -6$ entering as baseline. Heteroscedasticity-robust standard errors are clustered on the municipality level.

Table 10: Estimated coefficients over the baseline specification and other select models using the full sample with Gothenburg and Malmö included.

	(1)	(2)	(3)	(4)	(5)
Model specification	Baseline Model	County FE	Municipality-specific linear trend	Small student cities excluded	Neighbors excluded
Student cities × treatment	0.0243**	0.0334***	0.0216**	0.0238**	0.0261**
	(0.0111)	(0.0108)	(0.0100)	(0.0112)	(0.0114)
Observations	62,086	62,086	62,086	56,812	55,050
R^2	0.596	0.537	0.598	0.599	0.597
Covariates	Yes	Yes	Yes	Yes	Yes
City FE	Yes	No	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Municipality-specific linear time trend	No	No	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. Heteroscedastic-robust standard errors clustered on the municipality level in parenthesis. See appendix G for estimated effects of covariates.

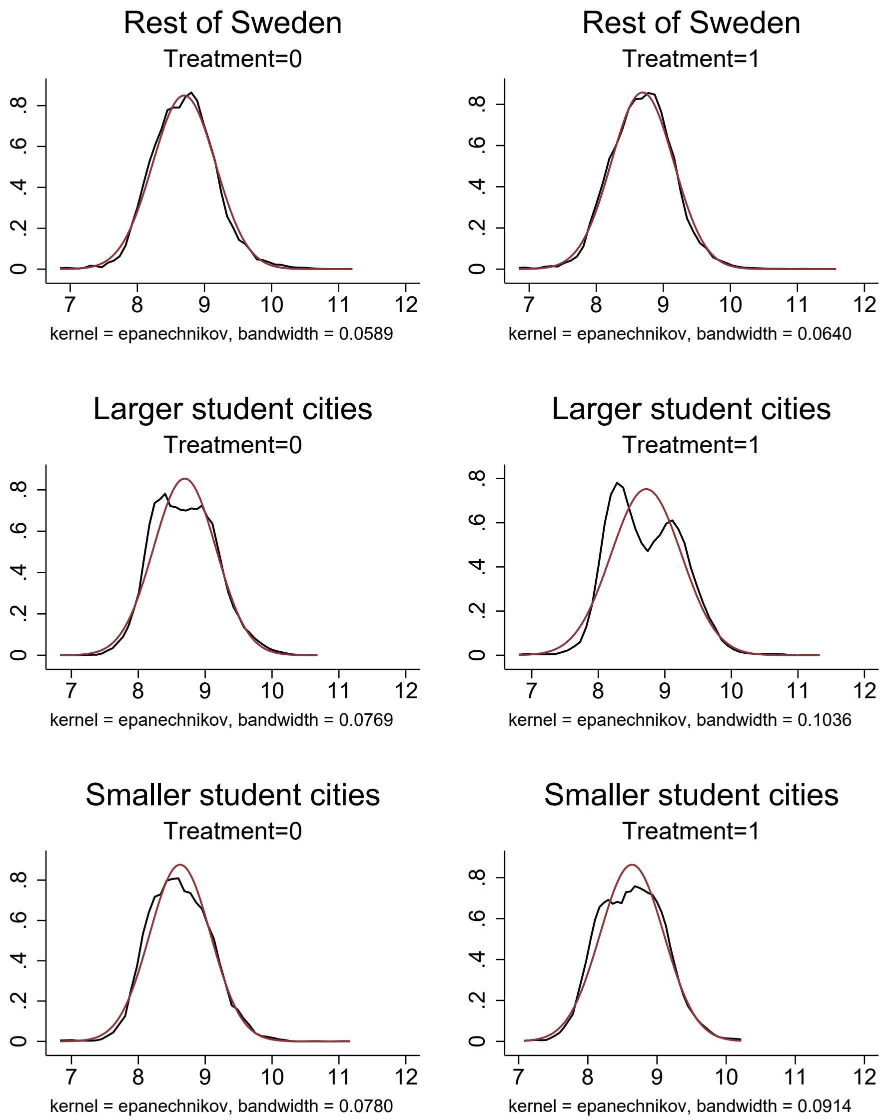
C Summary statistics

Summary statistics for covariates.

Variable	Obs	Mean	Std. dev.	Min	Max
Log of nominal monthly rent	37,474	8.69	0.47	6.91	11.51
Square meters	37,474	69.29	46.58	4	980
Log of square meters	37,474	4.03	0.69	1.39	8.97
Number of rooms	37,474	2.52	1.49	1	11
Log of number of rooms	37,474	0.76	0.57	0	2.40
The dwelling has a balcony	37,474	0.02	0.13	0	1
The dwelling has parking	37,474	0.00	0.03	0	1
The dwelling is furnished	37,474	0.03	0.16	0	1
The dwelling is recently constructed	37,474	0.00	0.06	0	1
The dwelling is recently renovated	37,474	0.04	0.19	0	1
The dwelling is considered modern	37,474	0.00	0.05	0	1
The dwelling is an apartment	37,474	0.82	0.38	0	1
The dwelling is a villa	37,474	0.16	0.37	0	1
The dwelling is semi-detached	37,474	0.02	0.13	0	1
The dwelling is a spare room	37,474	0.00	0.06	0	1
The dwelling is an overnight apartment	37,474	0.00	0.06	0	1
The dwelling is near the train station	37,474	0.00	0.04	0	1
The dwelling is centrally located	37,474	0.12	0.33	0	1
The dwelling is near campus	37,474	0.00	0.06	0	1
The lease is indefinite	37,474	0.01	0.08	0	1
The lease is short-term	37,474	0.00	0.05	0	1
The listing asks for a student	37,474	0.02	0.13	0	1
The listing asks for men	37,474	0.01	0.08	0	1
The listing asks for women	37,474	0.01	0.08	0	1

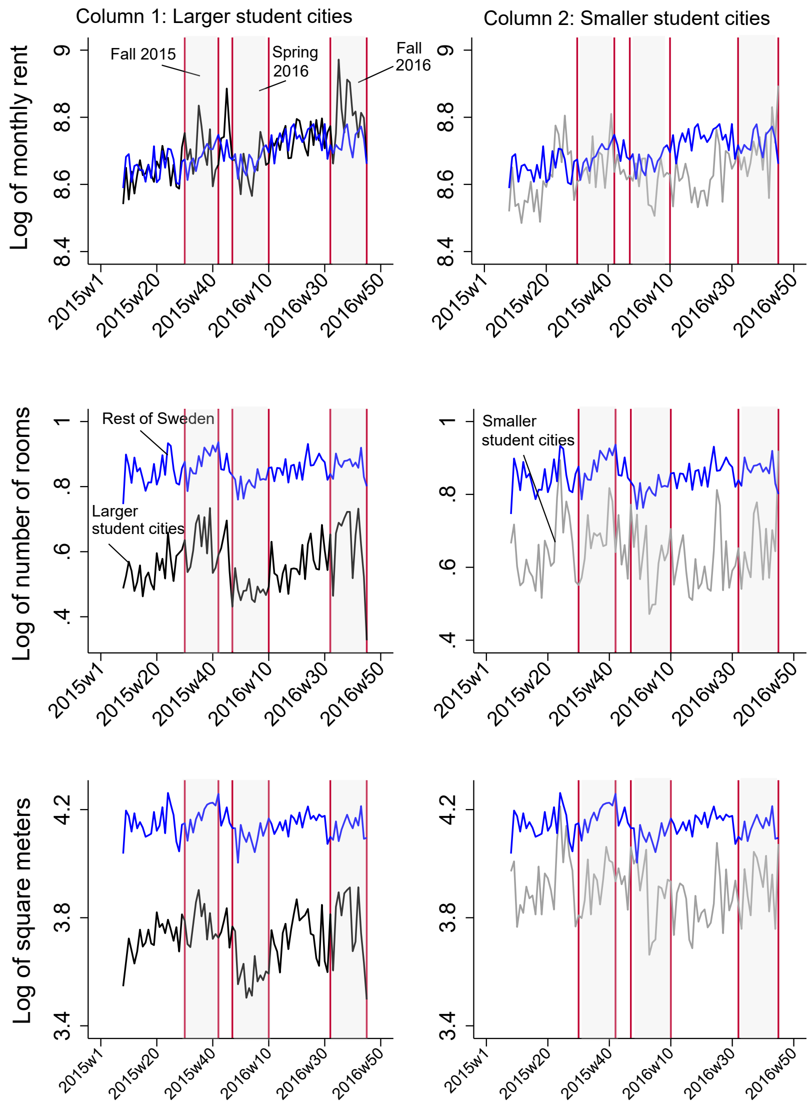
D Kernel density plots

Kernel density plots over the log of monthly rent for treatment and groups status.



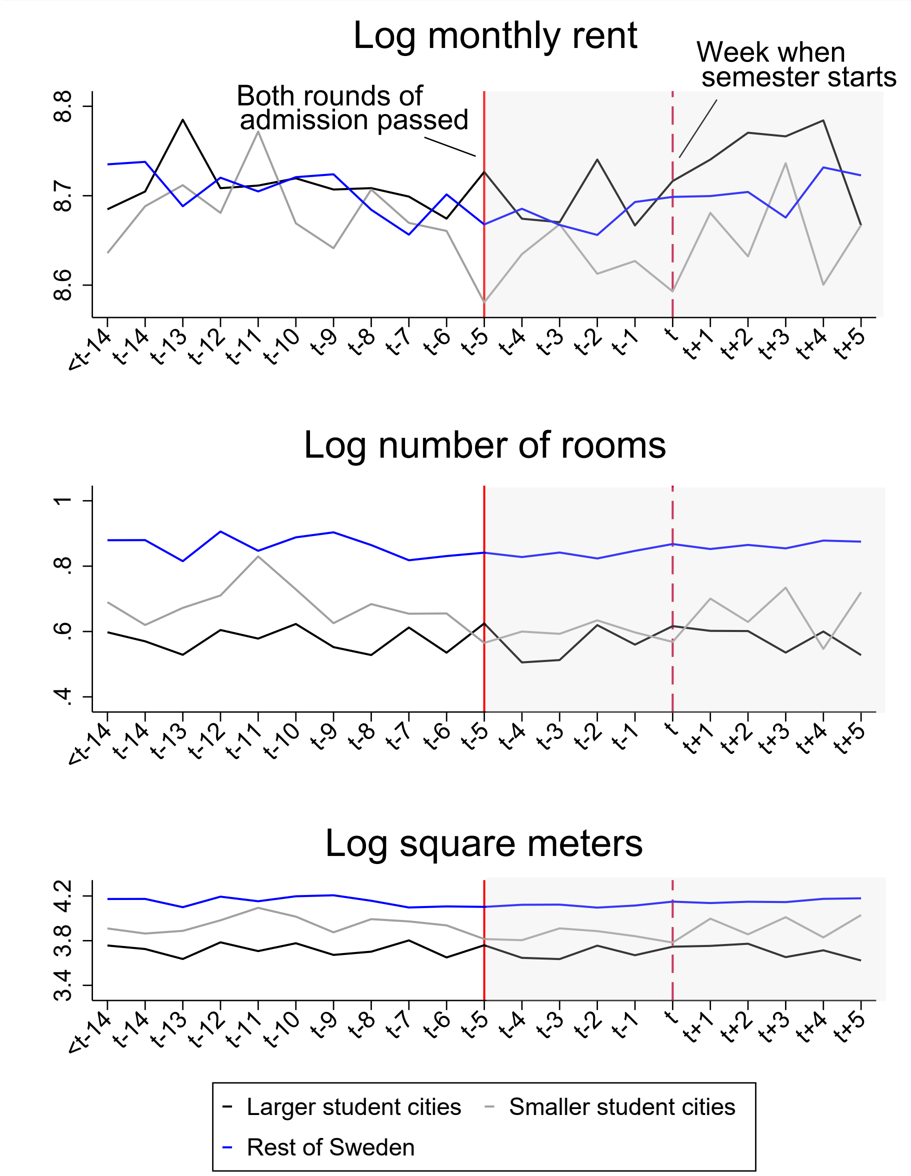
E Shared common trends

Pre-trends over calendar weeks with separate treatment windows.



Note: Column 1 plots large student cities and column 2 plots small student cities, both in relation to the control group. Row 1 plots the log of monthly rent, row 2 plots the log of number of rooms and row 3 plots the log of square meters.

Pre-trends over event weeks with aggregated treatment windows.



Note: Both rounds of admission have passed in t-5, with the university semester starting in week t.

F Analysis of higher order pre-trends

Linear, quadratic and cubic pre-trends for the log of monthly rent with larger and smaller student cities as treatment groups.

	Linear trend	Linear + quadratic trend	Linear + quadratic + cubic trend
Panel A: Treatment groups			
Larger student cities × linear trend	0.00198	0.00828	-0.0245
	(0.00729)	(0.0321)	(0.104)
Larger student cities × quadratic trend		-0.000635	0.00705
		(0.00268)	(0.0218)
Larger student cities × cubic trend			-0.000510
			(0.00137)
Smaller student cities × linear trend	0.00828	0.0470**	0.131**
	(0.00592)	(0.0234)	(0.0584)
Smaller student cities × quadratic trend		-0.00385*	-0.0234*
		(0.00213)	(0.0130)
Smaller student cities × cubic trend			0.00129
			(0.000834)
Observations	12,001	12,001	12,001
R^2	0.002	0.002	0.002
Period	$\rho = -14, ..., -6$	$\rho = -14, ..., -6$	$\rho = -14, ..., -6$
Panel B: Control group			
Rest of Sweden × linear trend	-0.00467	-0.0259	-0.0494
	(0.00505)	(0.0208)	(0.0701)
Rest of Sweden × quadratic trend		0.00213	0.00762
		(0.00188)	(0.0151)
Rest of Sweden × cubic trend			-0.000364
			(0.000949)
Observations	12,001	12,001	12,001
R^2	0.001	0.001	0.001
Period	$\rho = -14, ..., -6$	$\rho = -14, ..., -6$	$\rho = -14, ..., -6$

Note: *** p<0.01, ** p<0.05, * p<0.1. Heteroscedastic-robust standard errors clustered on the municipality level in parenthesis.

Linear, quadratic and cubic pre-trends for the log of monthly rent with smaller student cities included in control group.

	Linear trend	Linear + quadratic trend	Linear + quadratic + cubic trend
Panel A: Treatment group			
Larger student cities × linear trend	0.000376	-0.00104	-0.0498
	(0.00711)	(0.0320)	(0.103)
Larger student cities × quadratic trend		0.000139	0.0116
		(0.00263)	(0.0215)
Larger student cities × cubic trend			-0.000758
			(0.00135)
Observations	12,001	12,001	12,001
R^2	0.000	0.000	0.001
Period	$\rho = -14, ..., -6$	$\rho = -14, ..., -6$	$\rho = -14, ..., -6$
Panel B: Control group			
Rest of Sweden × linear trend	-0.000376	0.00104	0.0498
	(0.00711)	(0.0320)	(0.103)
Rest of Sweden × quadratic trend		-0.000139	-0.0116
		(0.00263)	(0.0215)
Rest of Sweden × cubic trend			0.000758
			(0.00135)
Observations	12,001	12,001	12,001
R^2	0.000	0.000	0.001
Period	$\rho = -14, ..., -6$	$\rho = -14, ..., -6$	$\rho = -14, ..., -6$

Note: *** p<0.01, ** p<0.05, * p<0.1. Heteroscedastic-robust standard errors clustered on the municipality level in parenthesis.

Linear, quadratic and cubic pre-trends for the log of monthly rent over the full sample (Gothenburg and Malmö included).

Table 11: Higher-order pre-trends estimated in the pre-event period for the full sample.

	Linear trend	Linear + quadratic trend	Linear + quadratic + cubic trend
Panel A: Treatment group			
Larger student cities × linear trend	0.00104	0.00490	-0.0393
	(0.00677)	(0.0317)	(0.101)
Larger student cities × quadratic trend		-0.000388	0.00997
		(0.00258)	(0.0210)
Larger student cities × cubic trend			-0.000687
			(0.00132)
Observations	20,349	20,349	20,349
R^2	0.005	0.005	0.005
Period	$\rho = -14, ..., -6$	$\rho = -14, ..., -6$	$\rho = -14, ..., -6$
Panel B: Control group			
Rest of Sweden × linear trend	-0.00104	-0.00490	0.0393
	(0.00677)	(0.0317)	(0.101)
Rest of Sweden × quadratic trend		0.000388	-0.00997
		(0.00258)	(0.0210)
Rest of Sweden × cubic trend			0.000687
			(0.00132)
Observations	20,349	20,349	20,349
R^2	0.005	0.005	0.005
Period	$\rho = -14, ..., -6$	$\rho = -14, ..., -6$	$\rho = -14, ..., -6$

Note: *** p<0.01, ** p<0.05, * p<0.1. Heteroscedastic-robust standard errors clustered on the municipality level in parenthesis.

G Regression output for covariates

Estimated coefficients from table 4.

	(1)	(2)	(3)	(4)
Model specification	Barebones model	Baseline model	County FE	Anticipation and delayed response
Constant	6.979***	6.971***	7.057***	6.970***
	(0.0373)	(0.0444)	(0.0396)	(0.0445)
Treatment	-0.0178**	-0.0178**	-0.0196**	
	(0.00703)	(0.00731)	(0.00800)	
Anticipation				-0.0188**
				(0.00794)
Delayed response				-0.0172*
				(0.00934)
Week when semester starts				-0.00830
				(0.0124)
Student cities	-0.141***	-0.152***	0.112***	-0.152***
	(0.00537)	(0.00522)	(0.0292)	(0.00524)
Log of square meters	0.400***	0.383***	0.358***	0.383***
	(0.0104)	(0.0111)	(0.00989)	(0.0111)
Number of rooms	0.0528***	0.0639***	0.0669***	0.0639***
	(0.00379)	(0.00401)	(0.00413)	(0.00402)
The dwelling has a balcony		0.0497***	0.0473**	0.0497***
		(0.0172)	(0.0187)	(0.0172)
The dwelling is furnished		0.0561***	0.0813***	0.0560***
		(0.00853)	(0.0115)	(0.00849)
The dwelling has parking		0.0587	0.0977*	0.0585
		(0.0485)	(0.0506)	(0.0486)
The dwelling is recently constructed		0.158***	0.187***	0.158***
		(0.0267)	(0.0306)	(0.0268)
The dwelling is recently renovated		0.105***	0.103***	0.105***
		(0.00951)	(0.0105)	(0.00953)
The dwelling is considered modern		0.107***	0.142***	0.107***
		(0.0315)	(0.0287)	(0.0315)
The dwelling is an apartment		0.0519***	0.0603***	0.0520***
		(0.0123)	(0.0165)	(0.0122)
The dwelling is semi-detached		0.0737***	0.106***	0.0737***
		(0.0166)	(0.0196)	(0.0166)
The dwelling is a spare room		-0.264***	-0.243***	-0.264***
		(0.0312)	(0.0299)	(0.0312)
The dwelling is an overnight apartment		-0.0797***	-0.0834***	-0.0796***
		(0.0221)	(0.0248)	(0.0221)
The lease is indefinite		0.0748***	0.0881***	0.0749***
		(0.0165)	(0.0185)	(0.0164)
The lease is short-term		-0.0221	0.00898	-0.0218
		(0.0341)	(0.0319)	(0.0340)
The listing asks for a student		-0.113***	-0.0949***	-0.113***
		(0.0186)	(0.0208)	(0.0187)
The listing asks for male tenant		-0.00807	0.0260	-0.00809
		(0.0340)	(0.0374)	(0.0341)
The listing asks for female tenant		-0.204***	-0.198***	-0.204***
		(0.0406)	(0.0389)	(0.0407)
The dwelling is near the train station		0.0961**	0.123***	0.0960**
		(0.0414)	(0.0452)	(0.0415)
The dwelling is centrally located		0.0739***	0.0656***	0.0739***
		(0.00679)	(0.00824)	(0.00678)
The dwelling is near campus		-0.0669***	-0.0237	-0.0670***
		(0.0231)	(0.0316)	(0.0230)
Observations	37,474	37,474	37,474	37,474
R^2	0.559	0.571	0.509	0.571
Covariates	No	Yes	Yes	Yes
City FE	Yes	Yes	No	Yes
Time FE	Yes	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. Heteroscedastic-robust standard errors clustered on the municipality level in parenthesis. Note that as apartments and villas make up the absolute majority of observations, the latter is dropped to avoid multicollinearity.

Estimated coefficients from table 5

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	0-19m^2	20-29m^2	30-39m^2	40-49m^2	50-59m^2	60-69m^2	>69m^2
Constant	7.758***	6.558***	6.837***	6.545***	6.719***	7.441***	7.440***
	(0.0911)	(0.152)	(0.205)	(0.409)	(0.371)	(0.428)	(0.154)
Treatment	-0.00477	-0.00875	-0.00351	-0.0151	-0.0705***	0.000998	-0.00924
	(0.0175)	(0.0213)	(0.0241)	(0.0153)	(0.0211)	(0.0202)	(0.0135)
Student cities	-0.0507***	-0.159***	-0.131***	-0.116***	-0.191***	-0.203***	-0.126***
	(0.00796)	(0.0132)	(0.0158)	(0.0216)	(0.0137)	(0.00953)	(0.00621)
Log of square meters	0.0758**	0.534***	0.418***	0.477***	0.483***	0.269**	0.274***
	(0.0320)	(0.0437)	(0.0566)	(0.103)	(0.0941)	(0.108)	(0.0369)
Number of rooms	0.0113*	0.000712	0.0591***	0.0871***	0.0567***	0.0585***	0.0717***
	(0.00623)	(0.0142)	(0.0167)	(0.0102)	(0.0176)	(0.0153)	(0.00807)
The dwelling has a balcony	0.0108	0.0724*	0.0559*	0.0501**	0.0125	0.0425*	0.0473***
	(0.0387)	(0.0393)	(0.0295)	(0.0238)	(0.0346)	(0.0240)	(0.0164)
The dwelling is furnished	0.0594***	0.00965	0.0833***	0.0416*	0.0456*	0.0899***	0.0624**
	(0.0210)	(0.0303)	(0.0298)	(0.0229)	(0.0248)	(0.0225)	(0.0259)
The dwelling has parking	0.0736	0.468***	-0.0155	0.0914***	-0.0807	0.138	0.0339
	(0.0766)	(0.0233)	(0.0318)	(0.0326)	(0.0882)	(0.0837)	(0.0929)
The dwelling is recently constructed	0.0843	0.148***	0.235***	0.157	0.0565	0.0587	0.167**
	(0.0597)	(0.0280)	(0.0685)	(0.0964)	(0.136)	(0.0910)	(0.0779)
The dwelling is recently renovated	0.0626	0.162***	0.0985***	0.115***	0.0948***	0.168***	0.0758***
	(0.0517)	(0.0426)	(0.0248)	(0.0177)	(0.0201)	(0.0216)	(0.0136)
The dwelling is considered modern	-0.0163	0.0204	0.184***	0.0910	0.148***	0.0807	0.104*
	(0.0615)	(0.0495)	(0.0639)	(0.130)	(0.0458)	(0.0628)	(0.0542)
The dwelling is an apartment	0.0200	-0.0280	-0.00611	0.0510	0.0776***	0.0804***	0.0459***
	(0.0290)	(0.0423)	(0.0383)	(0.0397)	(0.0263)	(0.0235)	(0.0174)
The dwelling is semi-detached	-0.00491	-0.0818	-0.139*	-0.0887	-0.0561	0.0637	0.0942***
	(0.0304)	(0.0784)	(0.0823)	(0.0746)	(0.110)	(0.0655)	(0.0199)
The dwelling is a spare room	0.0269**	0.0281	-0.251*	-0.499***	-0.458***	-0.343***	-0.582***
	(0.0133)	(0.0660)	(0.150)	(0.166)	(0.0483)	(0.0760)	(0.0564)
The dwelling is an overnight apartment	-0.0454	-0.0323	-0.0129	-0.0130	0.0801	0.0482	-0.446*
	(0.0446)	(0.0416)	(0.0356)	(0.116)	(0.188)	(0.179)	(0.230)
The lease is indefinite	0.0379***	0.208***	0.0896**	0.00994	0.0998**	0.0386	0.0496**
	(0.0119)	(0.0372)	(0.0451)	(0.0735)	(0.0438)	(0.0665)	(0.0250)
The lease is short-term	0.0583*	0.0428	-0.0897	0.0306	-0.00582	0.180	-0.149**
	(0.0323)	(0.0438)	(0.105)	(0.0781)	(0.0679)	(0.143)	(0.0679)
The listing asks for a student	-0.0132	-0.00656	-0.0316	-0.126***	-0.248***	-0.270***	-0.474***
	(0.0153)	(0.0192)	(0.0249)	(0.0438)	(0.0740)	(0.0619)	(0.0522)
The listing asks for male tenant	-0.164	-0.0100	-0.0946	-0.0128	-0.0550	-0.0555	0.0207
	(0.139)	(0.0409)	(0.126)	(0.0808)	(0.0340)	(0.143)	(0.0508)
The listing asks for female tenant	-0.0324	-0.123***	-0.217**	-0.164	-0.854**	-0.652***	-0.607***
	(0.0276)	(0.0295)	(0.106)	(0.128)	(0.378)	(0.145)	(0.0535)
The dwelling is near the train station	0.0380	0.178***	-0.0476	0.0549	0.244**	0.0358	0.175***
	(0.0319)	(0.0505)	(0.0954)	(0.0655)	(0.0997)	(0.228)	(0.0572)
The dwelling is centrally located	0.0297*	0.0434***	0.0841***	0.0932***	0.0629***	0.0765***	0.0812***
	(0.0159)	(0.0160)	(0.0149)	(0.0119)	(0.0111)	(0.0151)	(0.0101)
The dwelling is near campus	-0.0518**	0.0209	-0.0144	0.0650	-0.0374	0.0790**	-0.323***
	(0.0219)	(0.0164)	(0.0196)	(0.0435)	(0.0843)	(0.0362)	(0.0650)
Observations	3,120	3,246	3,305	3,490	4,011	4,448	15,854
R^2	0.283	0.373	0.428	0.473	0.383	0.320	0.346
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-specific linear time trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. Heteroscedastic-robust standard errors clustered on the municipality level in parenthesis.

Estimated coefficients from table 6

	(1)	(2)	(3)	(4)	(5)
	1 room	2 rooms	3 rooms	4 rooms	5+ rooms
Constant	7.075***	6.939***	6.485***	6.982***	7.544***
	(0.0695)	(0.0735)	(0.145)	(0.174)	(0.288)
Treatment	-0.00593	-0.0279*	-0.0273*	-0.00601	-0.0421
	(0.0115)	(0.0155)	(0.0145)	(0.0296)	(0.0497)
Student cities	-0.117***	-0.168***	-0.0418***	-0.235***	-0.220**
	(0.00509)	(0.00744)	(0.0117)	(0.0135)	(0.0989)
Log of square meters	0.267***	0.417***	0.488***	0.474***	0.355***
	(0.00982)	(0.0177)	(0.0360)	(0.0360)	(0.0582)
Number of rooms	0.220***	0.0368*	0.0530*		0.00386
	(0.0376)	(0.0189)	(0.0298)		(0.0198)
The dwelling has a balcony	0.0880***	0.0232	0.0256	0.103***	0.380***
	(0.0238)	(0.0198)	(0.0157)	(0.0306)	(0.0807)
The dwelling is furnished	0.0690***	0.0565***	0.0681***	0.121***	0.300*
	(0.0137)	(0.0147)	(0.0250)	(0.0428)	(0.170)
The dwelling has parking	0.154***	-0.0600	0.0616	0.142***	
	(0.0562)	(0.0629)	(0.108)	(0.0458)	
The dwelling is recently constructed	0.149***	0.179***	0.207***	-0.0156	0.384
	(0.0481)	(0.0504)	(0.0637)	(0.219)	(0.435)
The dwelling is recently renovated	0.110***	0.125***	0.0784***	0.0982***	-0.0345
	(0.0143)	(0.0131)	(0.0152)	(0.0253)	(0.0909)
The dwelling is considered modern	0.108**	0.0951**	0.140*	-0.00486	0.0854
	(0.0502)	(0.0397)	(0.0786)	(0.102)	(0.130)
The dwelling is an apartment	0.0622**	0.0556***	0.0720***	0.0460*	-0.115
	(0.0281)	(0.0177)	(0.0165)	(0.0263)	(0.0895)
The dwelling is semi-detached	-0.00312	-0.0231	0.0772**	0.121***	0.0371
	(0.0321)	(0.0605)	(0.0299)	(0.0342)	(0.0985)
The dwelling is a spare room	-0.0806***	-0.346***	-0.480***	-0.723***	-1.416***
	(0.0256)	(0.0665)	(0.0555)	(0.0493)	(0.126)
The dwelling is an overnight apartment	-0.0549**	-0.0684	-0.482	-0.533***	
	(0.0263)	(0.0571)	(0.350)	(0.0391)	
The lease is indefinite	0.0998***	0.0935***	0.0408	0.0562	0.0416
	(0.0243)	(0.0357)	(0.0623)	(0.0503)	(0.0853)
The lease is short-term	0.0129	-0.00458	-0.0710	-0.0754	
	(0.0327)	(0.0661)	(0.104)	(0.0508)	
The listing asks for a student	-0.0396***	-0.133**	-0.240***	-0.445***	-0.674**
	(0.0139)	(0.0520)	(0.0736)	(0.0953)	(0.264)
The listing asks for male tenant	-0.0152	-0.111**	0.0383	0.103***	-0.0981
	(0.0252)	(0.0531)	(0.0538)	(0.0352)	(0.0726)
The listing asks for female tenant	-0.101***	-0.406**	-0.688***	-0.535***	-1.045***
	(0.0162)	(0.164)	(0.0795)	(0.183)	(0.0908)
The dwelling is near the train station	0.111***	0.0940*	0.00798	0.378***	
	(0.0271)	(0.0540)	(0.106)	(0.0782)	
The dwelling is centrally located	0.0683***	0.0815***	0.0706***	0.0879***	0.0465
	(0.0106)	(0.00858)	(0.0144)	(0.0206)	(0.0524)
The dwelling is near campus	-0.0214	-0.00773	-0.154***	-0.742***	0.0912
	(0.0188)	(0.0390)	(0.0519)	(0.0378)	(0.0669)
Observations	11,923	10,046	7,855	3,958	1,651
R^2	0.390	0.403	0.290	0.353	0.428
Covariates	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Municipality-specific linear time trend	Yes	Yes	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. Heteroscedastic-robust standard errors clustered on the municipality level in parenthesis. Note that each class is bounded above by the other class, so that the class of "1 room" includes listings that have specified 1.5 rooms. As no listing between 4 and 5 specified 4.5 rooms, the number of rooms variable is dropped for this specification.

Estimated coefficients from table 7

	(1) Apartment	(2) Villa	(3) Semi-detached
Constant	7.016***	7.058***	7.230***
	(0.0394)	(0.0764)	(0.243)
Treatment	-0.0243***	0.0308	-0.0903
	(0.00695)	(0.0209)	(0.0986)
Student cities	-0.130***	-0.267***	-0.345***
	(0.00655)	(0.0156)	(0.0368)
Log of square meters	0.381***	0.366***	0.336***
	(0.0104)	(0.0207)	(0.0712)
Number of rooms	0.0673***	0.0583***	0.105***
	(0.00602)	(0.00899)	(0.0312)
The dwelling has a balcony	0.0482***	0.180	-0.155*
	(0.0172)	(0.243)	(0.0835)
The dwelling is furnished	0.0583***	0.0339	-0.0520
	(0.00854)	(0.0468)	(0.105)
The dwelling has parking	0.0663	0.390***	-0.157
	(0.0488)	(0.0350)	(0.170)
The dwelling is recently constructed	0.148***	0.307**	0.284***
	(0.0245)	(0.145)	(0.0687)
The dwelling is recently renovated	0.113***	0.0457	0.0482
	(0.0107)	(0.0340)	(0.0451)
The dwelling is considered modern	0.133***	0.0913	-0.0214
	(0.0212)	(0.0909)	(0.0568)
The dwelling is a spare room	-0.249***	-0.491***	
	(0.0311)	(0.177)	
The dwelling is an overnight apartment	-0.0820***	-0.121	
	(0.0231)	(0.112)	
The lease is indefinite	0.0810***	0.0484	-0.00104
	(0.0156)	(0.0457)	(0.121)
The lease is short-term	-0.00718	-0.0718	-0.602***
	(0.0341)	(0.128)	(0.0934)
The listing asks for a student	-0.0975***	-0.211**	-0.342**
	(0.0218)	(0.0864)	(0.152)
The listing asks for male tenant	-0.0143	0.0330	0.309***
	(0.0379)	(0.0401)	(0.0441)
The listing asks for female tenant	-0.188***	-0.356**	-0.293
	(0.0379)	(0.177)	(0.260)
The dwelling is near the train station	0.101**	-0.116***	
	(0.0400)	(0.0285)	
The dwelling is centrally located	0.0734***	0.0702***	0.0509
	(0.00690)	(0.0233)	(0.0517)
The dwelling is near campus	-0.0720***	0.0215	-0.255**
	(0.0238)	(0.0758)	(0.114)
Observations	30,698	6,015	671
R^2	0.543	0.569	0.717
Covariates	Yes	Yes	Yes
City FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Municipality-specific linear time trend	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. Heteroscedastic-robust standard errors clustered on the municipality level in parenthesis.

Estimated coefficients from table 8

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Model specification	Baseline estimate	City time trend	2015	2016	Fall semesters	Small student excluded	Neighbors excluded	Full sample
Constant	6.971***	6.971***	6.894***	7.115***	6.988***	6.945***	6.954***	6.982***
	(0.0444)	(0.0453)	(0.0643)	(0.0465)	(0.0437)	(0.0415)	(0.0486)	(0.0327)
Treatment	-0.0178**	-0.0164**	-0.0156	-0.0178	-0.00609	-0.0146*	-0.0188**	-0.00800
	(0.00731)	(0.00747)	(0.0138)	(0.0125)	(0.00996)	(0.00750)	(0.00764)	(0.00670)
Student cities	-0.152***	-0.122***	-0.0805***	-0.219***	-0.184***	-0.151***	-0.153***	-0.137***
	(0.00522)	(0.00351)	(0.00446)	(0.00776)	(0.00604)	(0.00570)	(0.00547)	(0.00494)
Log of square meters	0.383***	0.383***	0.389***	0.378***	0.388***	0.385***	0.386***	0.388***
	(0.0111)	(0.0113)	(0.0168)	(0.0117)	(0.0109)	(0.0113)	(0.0120)	(0.00858)
Number of rooms	0.0639***	0.0634***	0.0632***	0.0635***	0.0619***	0.0666***	0.0630***	0.0663***
	(0.00401)	(0.00403)	(0.00642)	(0.00401)	(0.00397)	(0.00415)	(0.00411)	(0.00339)
The dwelling has a balcony	0.0497***	0.0487***	0.0418**	0.0567***	0.0518***	0.0517***	0.0447**	0.0835***
	(0.0172)	(0.0169)	(0.0181)	(0.0193)	(0.0159)	(0.0193)	(0.0183)	(0.0156)
The dwelling is furnished	0.0561***	0.0558***	0.0619***	0.0501***	0.0534***	0.0614***	0.0528***	0.0429***
	(0.00853)	(0.00843)	(0.0126)	(0.0132)	(0.0107)	(0.0101)	(0.00859)	(0.00834)
The dwelling has parking	0.0587	0.0548	0.0789	0.0488	0.0517	0.00657	0.0776	0.0816**
	(0.0485)	(0.0504)	(0.0869)	(0.0556)	(0.0494)	(0.0442)	(0.0504)	(0.0391)
The dwelling is recently constructed	0.158***	0.155***	0.185***	0.133***	0.146***	0.137***	0.149***	0.165***
	(0.0267)	(0.0272)	(0.0361)	(0.0308)	(0.0257)	(0.0252)	(0.0265)	(0.0165)
The dwelling is recently renovated	0.105***	0.106***	0.113***	0.0995***	0.0946***	0.101***	0.107***	0.126***
	(0.00951)	(0.00970)	(0.0178)	(0.0108)	(0.00905)	(0.0103)	(0.00995)	(0.00953)
The dwelling is considered modern	0.107***	0.103***	0.0908**	0.117**	0.133***	0.0991***	0.115***	0.127***
	(0.0315)	(0.0322)	(0.0387)	(0.0514)	(0.0341)	(0.0336)	(0.0316)	(0.0268)
The dwelling is an apartment	0.0519***	0.0506***	0.0773***	0.0297**	0.0521***	0.0539***	0.0552***	-0.00606
	(0.0123)	(0.0123)	(0.0162)	(0.0138)	(0.0134)	(0.0128)	(0.0131)	(0.0128)
The dwelling is semi-detached	0.0737***	0.0723***	0.105***	0.0434**	0.0720***	0.0704***	0.0736***	0.0501***
	(0.0166)	(0.0167)	(0.0200)	(0.0210)	(0.0183)	(0.0154)	(0.0176)	(0.0147)
The dwelling is a spare room	-0.264***	-0.263***	-0.288***	-0.246***	-0.281***	-0.249***	-0.268***	-0.314***
	(0.0312)	(0.0313)	(0.0465)	(0.0396)	(0.0408)	(0.0359)	(0.0326)	(0.0253)
The dwelling is an overnight apartment	-0.0797***	-0.0776***	-0.0526	-0.109***	-0.0850***	-0.0841***	-0.0773***	-0.0634**
	(0.0221)	(0.0227)	(0.0341)	(0.0335)	(0.0267)	(0.0251)	(0.0222)	(0.0265)
The lease is indefinite	0.0748***	0.0759***	0.0670***	0.0864***	0.0456**	0.0779***	0.0697***	0.124***
	(0.0165)	(0.0168)	(0.0237)	(0.0273)	(0.0178)	(0.0185)	(0.0166)	(0.0156)
The lease is short-term	-0.0221	-0.0220	-0.0202	-0.0176	-0.0369	-0.0122	-0.0126	-0.0803***
	(0.0341)	(0.0341)	(0.0488)	(0.0490)	(0.0400)	(0.0430)	(0.0350)	(0.0257)
The listing asks for a student	-0.113***	-0.113***	-0.108***	-0.118***	-0.114***	-0.113***	-0.112***	-0.130***
	(0.0186)	(0.0186)	(0.0282)	(0.0193)	(0.0183)	(0.0205)	(0.0191)	(0.0152)
The listing asks for male tenant	-0.00807	-0.00356	0.0282	-0.0291	-0.0276	-0.0127	-0.0389	-0.0363
	(0.0340)	(0.0330)	(0.0415)	(0.0372)	(0.0324)	(0.0341)	(0.0253)	(0.0362)
The listing asks for female tenant	-0.204***	-0.205***	-0.213***	-0.196***	-0.211***	-0.213***	-0.204***	-0.257***
	(0.0406)	(0.0407)	(0.0463)	(0.0457)	(0.0444)	(0.0471)	(0.0414)	(0.0235)
The dwelling is near the train station	0.0961**	0.0981**	0.143*	0.0304	0.119**	0.0944**	0.0815*	0.00120
	(0.0414)	(0.0410)	(0.0759)	(0.0272)	(0.0571)	(0.0434)	(0.0440)	(0.0471)
The dwelling is centrally located	0.0739***	0.0737***	0.0698***	0.0763***	0.0763***	0.0673***	0.0752***	0.0684***
	(0.00679)	(0.00684)	(0.00770)	(0.00851)	(0.00570)	(0.00666)	(0.00702)	(0.00803)
The dwelling is near campus	-0.0669***	-0.0664***	-0.0482	-0.0823**	-0.0653***	-0.104*	-0.0665***	-0.0632***
	(0.0231)	(0.0232)	(0.0315)	(0.0365)	(0.0203)	(0.0552)	(0.0234)	(0.0220)
Observations	37,474	37,474	18,003	19,471	28,704	32,200	34,840	62,086
R^2	0.571	0.574	0.576	0.572	0.573	0.583	0.569	0.596
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. Heteroscedastic-robust standard errors clustered on the municipality level in parenthesis.

Estimated coefficients from table 10

	(1)	(2)	(3)	(4)	(5)
Model specification	Baseline Model	County FE	City time trend	Small student cities excluded	Neighbors excluded
Constant	6.982***	7.028***	6.981***	6.968***	6.971***
	(0.0327)	(0.0329)	(0.0337)	(0.0312)	(0.0373)
Treatment	-0.00800	-0.0115	-0.00677	-0.00499	-0.0105
	(0.00670)	(0.00728)	(0.00680)	(0.00684)	(0.00719)
Student cities	-0.137***	0.0860**	-0.111***	-0.135***	-0.138***
	(0.00494)	(0.0342)	(0.00298)	(0.00530)	(0.00521)
Log of square meters	0.388***	0.368***	0.388***	0.389***	0.389***
	(0.00858)	(0.00862)	(0.00863)	(0.00865)	(0.00967)
Number of rooms	0.0663***	0.0669***	0.0660***	0.0684***	0.0659***
	(0.00339)	(0.00374)	(0.00339)	(0.00345)	(0.00365)
The dwelling has a balcony	0.0835***	0.0914***	0.0833***	0.0879***	0.0784***
	(0.0156)	(0.0166)	(0.0156)	(0.0166)	(0.0174)
The dwelling is furnished	0.0429***	0.0635***	0.0430***	0.0438***	0.0378***
	(0.00834)	(0.00998)	(0.00826)	(0.00909)	(0.00928)
The dwelling has parking	0.0816**	0.118***	0.0820**	0.0634	0.0927**
	(0.0391)	(0.0405)	(0.0399)	(0.0405)	(0.0415)
The dwelling is recently constructed	0.165***	0.186***	0.164***	0.158***	0.164***
	(0.0165)	(0.0188)	(0.0164)	(0.0164)	(0.0172)
The dwelling is recently renovated	0.126***	0.123***	0.126***	0.125***	0.123***
	(0.00953)	(0.00978)	(0.00959)	(0.0101)	(0.0100)
The dwelling is considered modern	0.127***	0.141***	0.126***	0.124***	0.131***
	(0.0268)	(0.0287)	(0.0274)	(0.0279)	(0.0308)
The dwelling is an apartment	-0.00606	0.0354**	-0.00749	-0.00821	-0.000475
	(0.0128)	(0.0171)	(0.0128)	(0.0132)	(0.0137)
The dwelling is semi-detached	0.0501***	0.0743***	0.0480***	0.0473***	0.0486***
	(0.0147)	(0.0174)	(0.0147)	(0.0148)	(0.0158)
The listing asks for a student	-0.130***	-0.113***	-0.130***	-0.133***	-0.132***
	(0.0152)	(0.0165)	(0.0151)	(0.0160)	(0.0158)
The dwelling is a spare room	-0.314***	-0.293***	-0.312***	-0.313***	-0.312***
	(0.0253)	(0.0255)	(0.0255)	(0.0271)	(0.0267)
The dwelling is an overnight apartment	-0.0634**	-0.0830***	-0.0613**	-0.0663**	-0.0692**
	(0.0265)	(0.0288)	(0.0270)	(0.0299)	(0.0273)
The lease is indefinite	0.124***	0.141***	0.125***	0.127***	0.121***
	(0.0156)	(0.0164)	(0.0157)	(0.0162)	(0.0175)
The lease is short-term	-0.0803***	-0.0473*	-0.0789***	-0.0834***	-0.0704***
	(0.0257)	(0.0259)	(0.0259)	(0.0282)	(0.0259)
The listing asks for male tenant	-0.0363	-0.0280	-0.0324	-0.0403	-0.0570
	(0.0362)	(0.0432)	(0.0358)	(0.0367)	(0.0353)
The listing asks for female tenant	-0.257***	-0.254***	-0.257***	-0.261***	-0.252***
	(0.0235)	(0.0277)	(0.0237)	(0.0243)	(0.0259)
The dwelling is near the train station	0.00120	0.0205	0.00272	-0.000620	-0.0206
	(0.0471)	(0.0456)	(0.0467)	(0.0479)	(0.0516)
The dwelling is centrally located	0.0684***	0.0538***	0.0680***	0.0634***	0.0721***
	(0.00803)	(0.00852)	(0.00808)	(0.00816)	(0.00818)
The dwelling is near campus	-0.0632***	-0.0144	-0.0626***	-0.0977**	-0.0629***
	(0.0220)	(0.0313)	(0.0220)	(0.0493)	(0.0221)
Observations	62,086	62,086	62,086	56,812	55,050
R^2	0.596	0.537	0.598	0.599	0.597
Covariates	Yes	Yes	Yes	Yes	Yes
City FE	Yes	No	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Municipality-specific linear time trend	No	No	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. Heteroscedastic-robust standard errors clustered on the municipality level in parenthesis.