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Do rent caps lead to discrimination? The case of Vienna.

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Abstract English

Rent caps lead to discrimination against certain population groups in the rental housing market – this argument is often used to oppose rent caps. However, there are hardly any empirical studies on whether and to what extent this discrimination actually takes place and which population groups are affected by it. This research paper attempts to quantify the extent of this discrimination using the Viennese housing market as an example. The Viennese housing market is an excellent experimental laboratory for this analysis due to its unique, historically evolved legislation on rent caps. Using a spatial regression discontinuity design, I compare neighbouring census districts where different rent caps apply. With the data and methodology used in this paper, I cannot find clear evidence of the existence of any such discrimination. The Viennese rent cap does, however, have an effect on the rent level: Flats for which an additional location surcharge of 1 EUR per square metre can be charged have a rent that is on average 0.73 EUR per square metre higher. While this value is around 1 EUR in the outer districts, it is significantly lower with 0.57 EUR in the inner districts.

Abstract German

Mietpreisdeckel führen zu Diskriminierung gewisser Bevölkerungsgruppen auf dem Mietwohnungsmarkt – so lautet ein häufig vorgebrachtes Argument gegen Mietpreisdeckel. Empirische Untersuchungen, ob und inwieweit diese Diskriminierung tatsächlich stattfindet und welche Bevölkerungsgruppen davon betroffen sind, gibt es allerdings kaum. Diese Forschungsarbeit versucht am Beispiel des Wiener Wohnungsmarktes das Ausmaß dieser Diskriminierung zu quantifizieren. Der Wiener Wohnungsmarkt eignet sich aufgrund seiner einzigartigen, historisch gewachsenen Gesetzgebung zu Mietpreisdeckeln hervorragend als Versuchslabor für diese Analyse. Mittels eines räumlichen Regression-Discontinuity-Designs werden aneinander angrenzende Zählsprengel, in denen verschiedene Mietpreisdeckel gelten, miteinander verglichen. Mit den Daten und der Methodik, welche dieser Forschungsarbeit zugrunde liegen, kann ich keinen eindeutigen Beleg für die Existenz dieser Art von Diskriminierung finden. Der Wiener Mietpreisdeckel hat allerdings durchaus einen Effekt auf die Höhe des Mietzinses: Wohnungen, in denen pro Quadratmeter 1 EUR mehr Lagezuschlag verrechnet werden darf, haben im Durchschnitt pro Quadratmeter eine um 0.73 EUR höhere Miete. Während dieser Wert in den äußeren Bezirken bei ungefähr 1 EUR liegt, liegt er in den inneren Bezirken mit 0.57 EUR deutlich darunter.

1 Introduction

Rent caps lead to discrimination against certain population groups – this statement seems to be undisputed in the economic research literature. Already in 1995, Arnott writes in a widely cited research paper: "The textbook analysis of a rent freeze will be familiar to any reader of this journal. [...] The below-market rental prices lead to excess demand for housing, which causes various phenomena: [i.a.] an increase in discrimination, since disfavored groups are rationed out" (Arnott, 1995, p. 103). A few years later, the urban economist Edward Glaeser asks "Does Rent Control Reduce Segregation?" Glaeser writes: "By creating an excess of demand over supply, rent control ensures that apartments will be allocated on the basis of landlord preferences, which may in fact be segregationist." He argues: "After all, what will landlords look for? Tenants who make the building more attractive to other tenants. In general, this will mean tenants who resemble the existing stock of tenants, or just richer tenants. This will tend to exacerbate segregation." His research findings lead him, among other things, to the following conclusion: "This [result] lends some credence to the view that rent control sometimes works against minorities, perhaps because the allocation mechanism favors whites." (Glaeser, 2003, p. 179-192).

This research paper attempts to quantify the discrimination caused by rent caps. Furthermore, it aims to answer the question of which population groups are particularly affected by it. For this purpose, the Viennese housing market is an excellent object of study: The Austrian Landlord and Tenant Act (Mietrechtsgesetz – MRG) creates a situation with many different rent caps within the same city and even within the same districts. A key role in this context is played by the so-called "location surcharge", which – depending on the census district (German: Zählsprengel) in which a flat is located – may be added on top of the existing rent caps in varying amounts. The resulting zones of varying rent caps are analysed in this research paper by using a spatial regression discontinuity design.

This is not the first research paper addressing this issue. The most closely related paper to mine is the one by Pilchmair (2012), which also focuses on the Viennese housing market. In this study, Pilchmair (2012) investigates structural effects of the Viennese housing regulations and analyses, among other things, the so-called "Gründerzeitviertel". The term refers to census districts in which a predominant part of the building stock was constructed between 1870 and 1917 and which at the time of construction consisted predominantly of small, poorly equipped flats (equipment category D). According to the Guide Value Act (German: Richtwertgesetz), the location in such areas is to be classified as average at most and therefore no location surcharge can be charged (§2 Abs. 3 RichtWG). Pilchmair (2012) finds a higher proportion of foreigners and a lower proportion of high school graduates in those

"Gründerzeitviertel". Whether this is the result of a discriminatory tenant selection is left open by the researcher. In contrast to Pilchmair (2012), my research analyses the borders between different location surcharge zones, and thus goes beyond the analysis of "Gründerzeitviertel". Furthermore, to my knowledge, this study is the first to analyse the cutoff between different location surcharge zones using income data.

Another study related to mine is that by Öst, Söderberg and Wilhelmsson (2014), which compares Stockholm's rent-controlled housing segment with its free market segment. The authors find that geographical segregation is lower in the rent-controlled segment in terms of income, but not in terms of ethnicity, education, age or number of children. In other words, rent control enables people with lower incomes to find housing in the city centre, but this does not apply to the same extent to people with foreign backgrounds and low education levels. Following these findings by Öst et al. (2014), I also start my research with the hypothesis that migrants and people with a lower level of education have greater difficulties finding rent-controlled housing.

As a kind of starting point for my research, I consider a paper by Glaeser and Luttmer (2003), which examines the housing market in New York City. In their paper "The misallocation of housing under rent control", the authors make the assumption that housing demand between population subgroups is constant over space. They then compare the consumption patterns of different subgroups in places with a free market with those in rent-controlled New York City. The main focus is on consumption patterns in terms of the number of rooms rented. The researchers find that a significant amount of housing in New York City appears to be misallocated, for example, such that tenants live in larger flats than they would in the absence of rent control. Glaeser and Luttmer (2003) name two reasons for this misallocation: first, rent control creates incentives not to move out of rent-controlled, low-cost flats. Second, the allocation is no longer determined by a market price, but randomly or by some other allocation mechanism. This is the starting point of my research, which assumes that these alternative mechanisms allow landlords to discriminate without financial disadvantages.

Other studies on rent caps often focus on the impact on housing supply and maintenance. Diamond, McQuade and Qian (2019), for example, find lower housing supply due to rent controls in San Francisco, in addition to lower mobility of renters. Sims (2007) also finds that rent control reduces housing supply and maintenance in Massachusetts. Both studies exploit a sudden change in tenancy law where rent control was either suddenly introduced or abolished. In addition to the research topics already mentioned, there exist also studies on the effects of rent caps on rental prices (e.g. Early, 2000), as well as studies on the effects on consumption of non-housing goods (e.g. Olsen, 1972).

Besides the research literature on the effects of rent caps, there is a range of research that investigates discrimination in the housing market in Europe and in the US, for example, Carpusor and Loges (2006), Ahmed and Hammarstedt (2008), Bosch, Carnero and Farré (2010), Hanson and Hawley (2011), Ewens, Tomlin and Wang (2014) as well as Bartoš, Bauer, Chytilová and Matějka (2016). All these studies mentioned conduct correspondence tests: the researchers send out various applications to landlords, which differ insofar as some are sent in the name of a majority-sounding name, and others under that of a minority-sounding name. Even though the study designs are always slightly different, in all of these studies significantly more applications receive a (positive) response, if they are sent in the name of a majority-sounding name. A distinction is often made between statistical discrimination and taste-based discrimination. In the first case, landlords try to obtain information about the tenant through the name, for example about his or her income. In the second case, the discrimination is based on racism and the like. For example, Bosch et al. (2010) find less discrimination when applications provide positive information about the applicant's socioeconomic status, however, some degree of taste-based discrimination still remains. A good overview of further studies on this topic can be found in Flage (2018). A meta-analysis of different field experiments, including not only correspondence tests but also personal audits, is conducted by Auspurg, Schneck and Hinz (2018). Here, too, the authors confirm the occurrence of ethnic discrimination in the housing market.

I would like to mention one more paper by Musil, Brand, Huemer and Wonaschütz (2022), as it provides interesting insights into the Viennese housing market. The authors try to quantify the gentrification dynamics by measuring the conversion of historic rental houses into new rental houses or condominiums in Vienna. Both are methods to avoid rent regulation, as rent regulation only applies to historic rental houses. In their study, the authors identify where the gentrification hotspots are located in Vienna and which population groups are particularly affected by it. Interestingly, the gentrification hotspots are not characterised by people with high incomes displacing people with low incomes, but rather by people with high educational qualifications displacing people with low qualifications. This is particularly interesting because there is a strong correlation between income and level of education, but in the case of Musil et al. (2022) only the level of education increases.

The further structure of this paper is as follows: the next chapter *Theory* elaborates on the research question and its hypotheses and gives an overview of the Viennese housing market. The chapter *Data* follows with a description of the data used in this research. The *Methods* chapter then deals with the regression discontinuity design, and the obtained results are then presented in the chapter *Results*. Finally, I summarise my research in the *Conclusion*.

2 Theory

Does a rent cap lead to discrimination against certain population groups? If landlords have to offer their flats below the market price, they are confronted with high demand. The high demand allows landlords to choose between different interested tenants and gives them the opportunity to discriminate. For example, landlords might favour people with high incomes, as they believe they are less likely to default on rent.

Hypothesis 1: In flats with a lower rent cap, there are more people with ***higher incomes*** *compared to flats with a higher rent cap, assuming that the flats differ only in their level of the rent cap.*

As the landlord often has no information about the tenant's income, the landlord might use the tenant's academic title as a proxy for the tenant's income.

Hypothesis 2: In flats with a lower rent cap, there are more people with an ***academic degree*** *compared to flats with a higher rent cap, assuming that the flats only differ in their level of the rent cap.*

It is also conceivable that landlords discriminate against foreigners and prefer Austrians – similar to the studies mentioned above by Carpusor and Loges (2006), Ahmed and Hammarstedt (2008), Bosch et al. (2010), Hanson and Hawley (2011), Ewens et al. (2014), Bartoš et al. (2016) and Auspurg et al. (2018).

Hypothesis 3: In flats with a lower rent cap, there are more people with an ***Austrian citizenship*** *compared to flats with a higher rent cap, assuming that the flats differ only in their level of the rent cap.*

In order to test these hypotheses, the Viennese housing market is an excellent object of study, as it constitutes a kind of experimental laboratory.

2.1 Austrian Landlord and Tenant Act (MRG)

The Austrian Landlord and Tenant Act (German: Mietrechtsgesetz – MRG) has evolved historically and strongly regulates the Viennese housing market. The MRG provides not only one, but several different rent caps at varying levels. However, at the same time there is also a free housing market in Vienna, in which no rent caps exist at all. The Viennese housing market can therefore be seen as a kind of natural experiment in which the effects of different rent caps can be studied.

The share of rental housing in Vienna is traditionally high: 77% of the people living in Vienna are renters. The rental flats can roughly be divided into two categories: On the one hand, there are the city-owned flats (German: Gemeindewohnungen) and co-operative flats (German: Genossenschaftswohnungen), which make up approximately 57% of the rented flats. The remaining 43% is the private rental housing market, which, however, is in many parts also highly regulated and on which this paper focuses (Statistics Austria, 2022, p. 27-30).

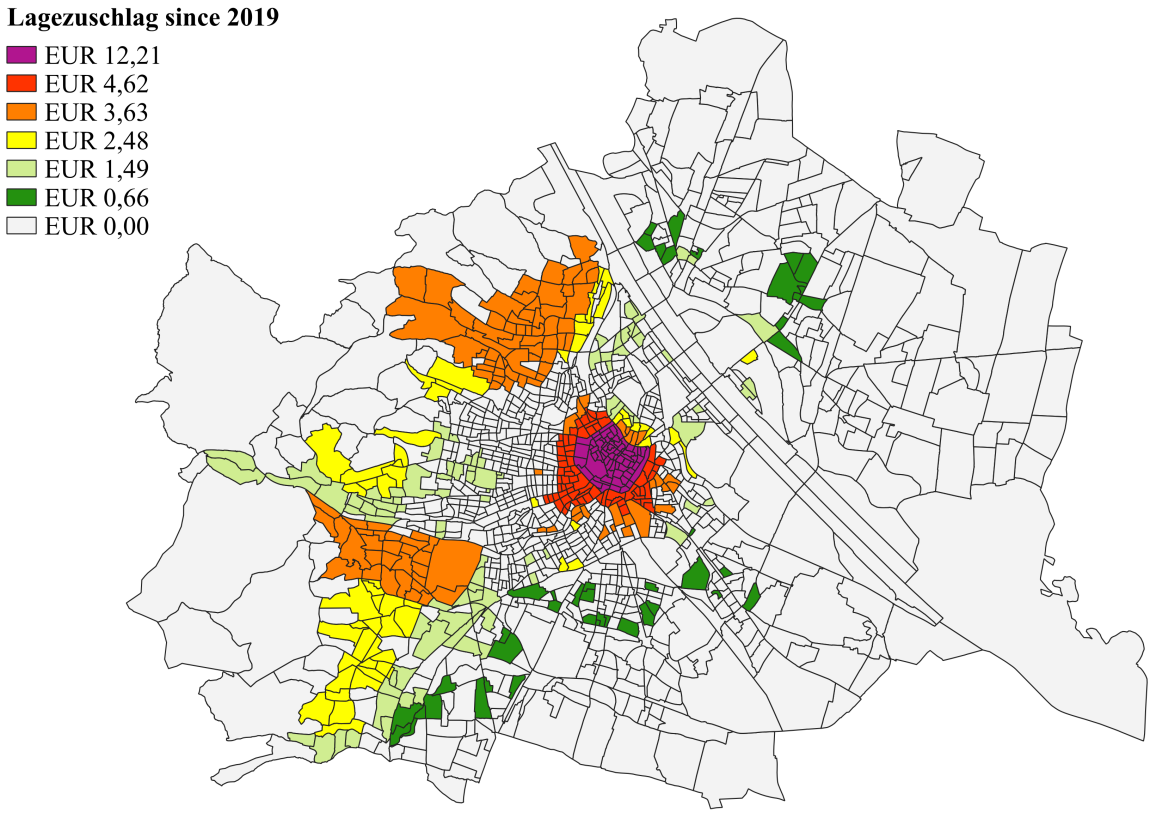
In the private rental housing market, landlords are often not allowed to set the rent freely, especially if the flat is located in an old building – defined as a building built before 8.5.1945 (§16 Abs. 1 Z 2 MRG).[1] For these flats, the Guide Value Act (German: Richtwertgesetz) instead defines a maximum rental price per square metre that may be charged (§16 Abs. 2 MRG). If the landlord charges more rent than permitted under the MRG, the tenant may reclaim overpaid rent, even years later and after moving out of the flat (§27 Abs. 3 MRG). The calculation of the maximum rental price is as follows: A base price is fixed for the whole city of Vienna (currently 5.81 EUR/m^2), to which various deductions and surcharges can be added and subtracted. The largest of these surcharges is the location surcharge (MA 25, City of Vienna, 2019b).

[1] Around 31% of the building stock in Vienna was built before 1945 (Statistics Austria, 2017a).

2.2 Location surcharge

The location surcharge that may currently be charged is shown in Figure 1.

Figure 1: "Location surcharge" map of Vienna since 2019.



Source: Own illustration. Data: MA 25, City of Vienna, 2019a.

For example, landlords in the area marked in red are allowed to add a location surcharge of EUR 4.62/m^2 on top of the base price of EUR 5.81/m^2. The rent cap for these flats is therefore 10.43 EUR/m^2, provided that no other surcharges and deductions apply. What is striking are the sharp borders between the different location surcharge zones: For example, in the grey area no location surcharge may be charged at all, although there are many places where it directly borders the red area. Neubaugasse in the seventh district, for example, separates the red zone from the grey zone. On the right side of the street the location is considered good and a location surcharge of 4.62 EUR/m^2 may be charged, whereas on the left side the location is considered average and no location surcharge may be charged at all. It is hard to imagine that the location on the left and right side of the same street differ from each other in terms of quality. Rather, it is due to the calculation method on which the location surcharge map is based that, for the sake of simplification, it is always census districts and not individual properties that are considered. The geographical borders of those census districts

therefore automatically result in the random-looking borders of the location surcharge zones. The census districts are only statistical units and have no further meaning for the people living there. In the empirical part of this paper, a spatial regression discontinuity design (RDD) is used to exploit these cutoffs.

At this point it must be mentioned that the location surcharge map in Figure 1 is not legally binding – under certain circumstances it may be legal to charge more location surcharge – but the map does have a legal character: If the landlord charges more than indicated on the map and the tenant claims the overpaid rent back, the case will be dealt with by the so-called "conciliation board" (German: Schlichtungsstelle – MA 50) in first instance (MA 50, City of Vienna, 2022). The conciliation board assesses whether a location surcharge is permissible primarily on the basis of the location surcharge map (Mietervereinigung Österreichs, 2018). If one of the involved parties does not agree with the decision of the conciliation board, they can take the case to court, which can overrule the decision of the conciliation board (MA 50, City of Vienna, 2022). The court usually commissions an expert to make an assessment of the location of the specific object. Especially in recent years, the number of such cases handled by the court has risen sharply (Putschögl, 2021). It can therefore be assumed that the cutoff in this RDD is not perfectly sharp, as some landlords are allowed to charge a higher location surcharge based on these individual assessments.

The location surcharge map is adjusted and republished approximately every 2 years. On one hand, the amounts in the respective surcharge categories change. On the other hand, some census districts change their zone and either move up into a higher category or down into a lower one. The historical location surcharge maps are shown in Figure A.1 in the appendix. Until 2017, the location surcharge zones remained relatively constant. Following a Supreme Court decision in November 2017 (5Ob74/17v), the location surcharge map calculation method had to be changed: Previously, it was based solely on the basic cost shares (German: Grundkostenanteile), that is, the land price of the respective census district. Since then, in addition to the basic cost share, five other characteristics are taken into account, namely: the distances to *public transport*, *educational institutions*, *medical care*, *shops* and *green spaces* (MA 25, City of Vienna, 2019b, p. 3). Due to this new calculation basis, the location surcharge map changed abruptly in 2018, after which it remained constant again. It would be exciting to examine this sudden change in 2018 with a difference-in-difference design. However, as I only have access to rental data up to 2017 (see chapter *Data*), this thesis concentrates on the period up to 2017. Focusing on this period also has the advantage that the individual assessments described above – and thus the number of landlords who can legally charge a higher location surcharge than is shown on the map – were less frequent at that time and I can therefore conduct a sharper RDD (Putschögl, 2021).

2.3 Other surcharges and deductions

Besides the location surcharge, there are also additional surcharges and deductions that can be applied. For example, for a flat with a balcony, up to 10% of the base price may be added on top. At the currently valid base price of 5.81 EUR/m^2 this corresponds to a permissible balcony surcharge of 0.58 EUR/m^2. For flats without a cellar compartment, 2.5% must be deducted from the base price, and the same applies if bathroom and toilet are in the same room. The complete list of possible surcharges and deductions is defined in the guidelines published by the City of Vienna (Richtlinien für die Ermittlung des Richtwertzinses gem. §16 Abs. 2 MRG). What is important to note is that all surcharges and deductions are independent of the location surcharge, they are all only linked to the base price. The incentive for a landlord to invest in a building in order to be able to apply more surcharges is therefore the same on both sides of the location surcharge cutoff.[2]

2.4 Limitations of the MRG

The Guide Value Act (German: Richtwertgesetz) is not applicable to all objects. As mentioned, it does not apply to new buildings, that is, buildings that were constructed after 08.05.1945. Also, a flat is only subject to the Guide Value Act if it is smaller than 130m^2 and if it has a (usable) water tap and a (usable) toilet inside the flat.[3] Also attic flats are often excluded.

[2] One reasoning why this might not be the case is the argument of diminishing marginal utility of money: private landlords on the more expensive side of the cutoff, who can already rent out their flats at a high price, would according to this argument have less incentive to invest. However, if we consider that some of the landlords are also commercial landlords for whom the argument does not apply, I think this objection is negligible.

[3] Around 95% of the flats in Vienna are smaller than 130m^2 (Statistics Austria, 2017a).

3 Data

This research is based on data from three different sources:

Firstly, I create a dataset containing the 1368 Viennese census districts (German: Zählsprengel) with the corresponding location surcharges. The currently valid location surcharges are available in the form of a "location surcharge map" on the website of the City of Vienna (MA 25, City of Vienna, 2019a). Using a geographic information system software, I read out the data from the map and convert it into a standard data format. I also read out the historical location surcharge data from historical location surcharge maps, which were depicted in old newspaper articles (namely: Kurier, die Presse, der Standard, Wiener Zeitung). However, I cannot find such location surcharge maps for the period prior to 2010, which is why this research limits its focus to the period from 2010 onwards. Figure A.1 in the appendix shows the location surcharge maps since 2010 including their sources.[4]

Secondly, data from a real estate database containing rental advertisements.[5] Although the dataset does not include the exact addresses of the advertised flats, it does show in which census district a flat is located and the date on which an advertisement was posted. With these two pieces of information, I can link this dataset to the location surcharge dataset described above, thus I know the maximum location surcharge for each flat. I exclude advertisements that were posted before 2010 from the dataset, as I do not know the location surcharge for this period. The real estate dataset ends in 2017, hence this paper covers only the period from 2010 to 2017. The dataset also contains a range of information about the advertised flats, such as the *net rent*, the *number of square metres* and the *year of construction*.[6] I further clean the dataset as follows: I exclude flats larger than 130m^2, as well as new buildings (built after 08.05.1945) and attic flats, because – as describe above – the Landlord and Tenant Act (MRG) is not equally applicable in these cases. I also exclude the few observations with unrealistic prices (< 3 EUR/m^2 and > 50 EUR/m^2).[7] Moreover, I exclude – as thoroughly as possible – duplicates: many flats were posted at several times and therefore appear more than once in

[4] The City of Vienna also provides a "Location Surcharge Calculator" on its website, which lists the current and all historical location surcharges for each census district. After a close examination of this data, however, it turns out that the data on which the calculator is based seems to be faulty: the location surcharges often do not correspond to the location surcharges shown in the location surcharge maps. Also, the location surcharges described by Pilchmair (2012, p. 124) do not correspond to the calculator, whereas they do correspond to the location surcharge maps. After I contacted the City of Vienna about the possible error in their calculator, I was informed that the page would be revised. I therefore assume that the location surcharge maps show the correct location surcharges, which is why I decided to work with them and not with the location surcharge calculator.

[5] Unfortunately, I am prohibited from citing the source of the dataset. In order to enable a validation of the dataset nevertheless, I make the dataset available upon request.

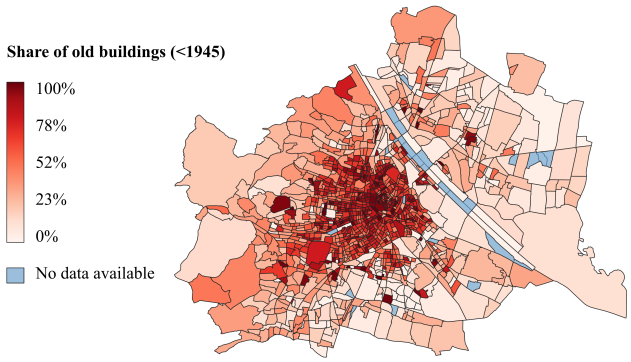
[6] Furthermore, the dataset contains the following information about the advertised flats: *condition*, *furnished* or *unfurnished*, *floor*, *garage*, *basement compartment*, *lift*, *balcony*, *number of toilets* and *number of bathrooms*.

[7] Excluding observations with unrealistic prices reduces my data set by approximately 3%.

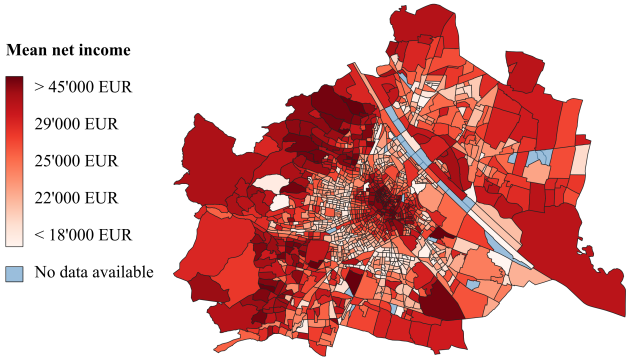
the dataset. The cleaned dataset contains approximately 4'112 observations at the cutoff. However, about 30 percent of these observations do not contain any information on the variable *floor*, so that in the end there are 2,897 complete observations. In order not to lose 30 percent of the data, I will run my regression both with and without the *floor* variable.

Thirdly, three different datasets from Statistics Austria at the census district level: A. building data (Statistics Austria, 2017a), B. income data (Statistics Austria, 2017b) and C. demographic data (Statistics Austria, 2017c). The survey date for all three datasets is 2017. With these datasets, I can calculate, for example, the share of old buildings, the average income, the share of foreigners and the share of university graduates for each census district in Vienna. Figure 2 plots this data on the map of Vienna.

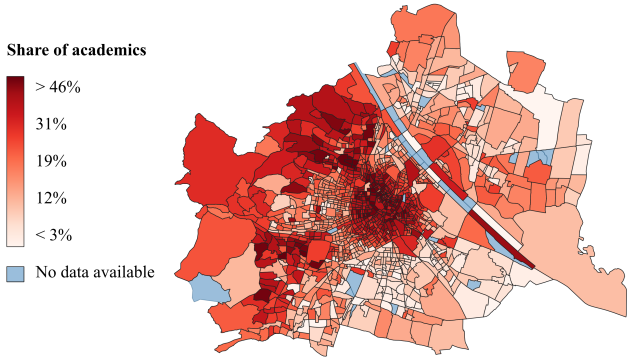
Figure 2: Building, income, and demographic data per census district in 2017.



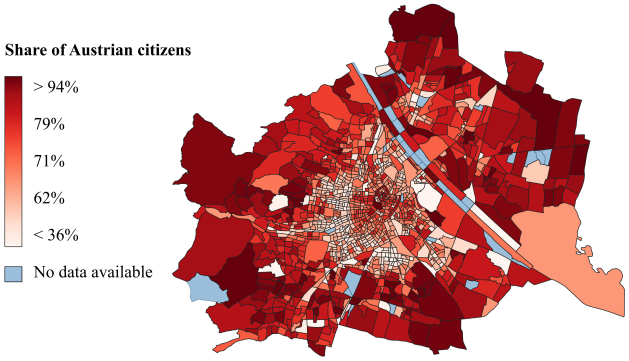
(a) Share of buildings built before 1945.



(b) Average annual net income after transfer payments.



(c) Share of people with academic degrees.



(d) Share of people with Austrian citizenship.

Source: Own illustration. Data: a) Statistics Austria, 2017a; b) Statistics Austria, 2017b; c) Statistics Austria, 2017c; d) Statistics Austria, 2017c.

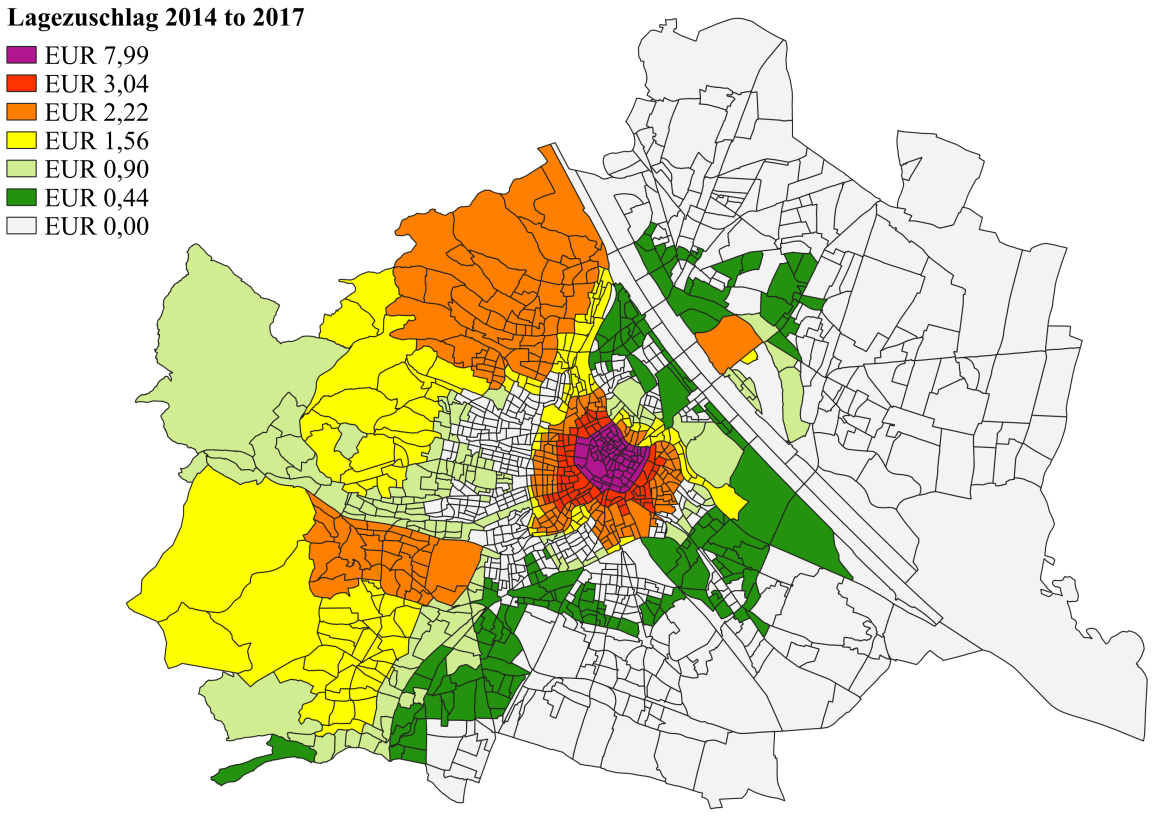
Map (a) in Figure 2 shows that the share of old buildings is highest in the city center and decreases towards the outer districts. Map (b) shows that the annual average net income is highest within the so-called "Gürtel" (meaning in the inner districts), and also on the outskirts, especially in the west. The lowest average net income is in between, that is, between the "Gürtel" and the suburban areas. In map (c) you can see that people with academic degrees live mainly in the city center and in some of the wealthy census districts in the west. Map (d) shows the share of Austrian citizens, which is particularly high on the outskirts of the city.

4 Method

The Austrian Landlord and Tenant Act (MRG), with its many rent caps at different levels and the seemingly random cutoffs between different location surcharge zones, is well suited for a spatial regression discontinuity design (RDD). The intuition behind this design is that of a natural experiment: if the assumption holds that houses located along the cutoff differ only in terms of their location surcharge, then the differences in the demographic composition of households can be explained by the differences in the rent caps.

Figure 3 shows the location surcharge map for the period 2014-2017, on which this paper is focused. The location surcharge map shows the maximum allowed location surcharge for each census district. As described in the chapter *Data*, I also have information on average income, demographic composition (education level, citizenship, etc.) and on the building stock in each census district.

Figure 3: "Location surcharge" map of Vienna from 2014 to 2017.



Source: Own illustration. Data: Rösner, 2014.

As you can see in Figure 3, there are different treatment intensities: the difference in location surcharge between the cheap and expensive side of the cutoff varies depending on where the cutoff is located.

Instead of using a dummy variable indicating whether a flat is on the more expensive or cheaper side of the cutoff, I therefore include the location surcharge in its respective amount directly into the regression. The baseline regression equation is thus as follows:

$$y_i = \alpha + \beta \cdot \text{location surcharge}_i + \varepsilon_i$$

This regression is run multiple times with the following dependent variables y_i: 1. *net income after transfer payments* 2. *share of university graduates* and 3. *share of Austrian citizens*. The regressor *location surcharge* is the location surcharge applicable in the respective census district in the period 2014 to 2017.

Since there is not a single cutoff, but many cutoffs across the city, and since I want to compare census districts with the neighbouring census district and not with a census district on the other side of the city, I extend my baseline equation with a neighbour parsing. To do this, I proceed as follows: For each pair of neighbouring census districts that are located in different location surcharge zones, I create a "box" b. Two census districts are defined as neighbours if they share at least 100 meters common border. Census districts that do not border any location surcharge cutoff are excluded. All created "boxes" are then included into the regression as fixed effects. Since some census districts are located at several cutoffs and therefore also appear in several "boxes", I face the problem of having more fixed effects than observations in the regression. I solve this problem by stacking the data, meaning that I include the same observation as many times in the data set as it occurs in boxes. In order not to underestimate the standard errors of the stacked dataset, I cluster the standard errors by census districts. This procedure follows Spenkuch and Toniatti (2018, p. 1992). The regression equation including the neighbour parsing is as follows:

$$y_i = \alpha + \beta \cdot \text{location surcharge}_i + \text{FE}_b + \varepsilon_i$$

FE_b denotes the fixed effects, namely the "boxes" described above.

My RDD works, of course, only under certain assumptions: Firstly, buildings on both sides along the cutoff may not differ systematically, except with regard to the amount of the permissible location

surcharge. Appendix Table A.1 therefore shows the summary statistics of the buildings along the cutoff.[8] Secondly, the RDD obviously only works if rents in the higher location surcharge zones are actually higher. One reason why this might not be the case is that the rent caps may be set at a level that is above the market price. In this case, the rent caps would not be binding, in other words, they would have no effect. Another reason might be that landlords overcharge and do not comply to the rent cap. To what extent such an overcharge is illegal, cannot be clearly determined, since the possibility of assessments opens up a legal grey area, as described in the chapter *Theory*. In order to examine whether, along the border, rents are actually higher in zones with a higher location surcharge, I analyse the rental price data described in the chapter *Data*. To do so, I regress the m2-price on the location surcharge and control for the observables:

$$
\begin{aligned}
\text{m2 price}_\text{i} = \alpha &+ \beta_1 \cdot \text{Location surcharge}_\text{i} \\
&+ \beta_2 \cdot \text{Condition}_\text{i} \\
&+ \beta_3 \cdot \text{Furnished}_\text{i} \\
&+ \beta_4 \cdot \text{Basement}_\text{i} \\
&+ \beta_5 \cdot \text{Number of toilets}_\text{i} \\
&+ \beta_6 \cdot \text{Balcony}_\text{i} \\
&+ \beta_7 \cdot \text{Terrace}_\text{i} \\
&+ \beta_8 \cdot \text{Flat size}_\text{i} \\
&+ \beta_9 \cdot \text{Flat size squared}_\text{i} \\
&+ \beta_{10} \cdot \text{Distance to CBD}_\text{i} \\
&+ \beta_{11} \cdot \text{Floor}_\text{i} \\
&+ \beta_{12} \cdot \text{Elevator}_\text{i} \\
&+ \beta_{13} \cdot \text{Floor}_\text{i} * \text{Elevator}_\text{i} \\
&+ \beta_{14} \cdot \text{Year}_\text{i} \\
&+ \beta_{15} \cdot \text{Year squared}_\text{i} + \varepsilon_i
\end{aligned}
$$

[8] The share of old buildings is slightly higher on the cheaper side of the cutoff; probably that is why flats on this side also tend to be smaller and are more often located in a building with several flats. Ideally, I would only want to compare the demographic structure of people living in houses built before 1945. Unfortunately, I cannot determine from the demographic data available whether a person lives in an old or a new building.

In this case, the variable *location surcharge* is the location surcharge at the time the flat was advertised. If the coefficient of interest β_1 turns out to be significantly positive, I can conclude that the flats in higher location surcharge zones actually have a higher rent.[9]

To check the soundness of my RDD, I run several placebo tests. For this purpose, I assign a placebo surcharge randomly to the census districts at the location surcharge cutoffs. The placebo surcharge has the same levels and frequencies as the location surcharge, but is randomly distributed across census districts. Furthermore, I also run a placebo test not only on the intensive margin but also on the extensive margin: In this case, I not only change the surcharges at the location surcharge cutoffs, but also change the cutoffs themselves. For this, I distribute a placebo surcharge randomly across the city and run my RDD along these newly created cutoffs. Also in this case, I distribute the placebo surcharge with the same levels and frequencies as the location surcharge, however, randomly across the city. Furthermore, I also run a placebo test on the intensive margin for my rental price regression. In all three placebo tests, I replace the variable of interest *location surcharge* in my models with the newly created variable *placebo surcharge*. Since the placebo surcharge is randomly assigned, I expect to obtain no significant surcharge coefficients in my placebo tests.

[9] As a robustness check, I would ideally want to test another specification of my rental price model by including the neighbour parsing here as well. Unfortunately, the observations in my rental price dataset are unevenly distributed across the city, such that I have rental price observations in only 61% of the census districts at the cutoff. Therefore, in contrast to my demographic model, I do not include the neighbour parsing in my rental price model.

5 Results

5.1 Effects on the level of rental prices

First, let us test the assumption that rent levels are actually higher on the side of the cutoff, where a higher location surcharge can be charged. I obtain the following results:

Table 1: The Location Surcharge has a significant, positive effect on the rent level.

	(1)		(2)		(3)	
	m2 price		m2 price		m2 price	
Location surcharge	0.729***	(12.23)	0.665***	(8.90)	0.778***	(13.76)
Condition	1.220***	(13.32)	1.170***	(12.65)	1.204***	(13.11)
Furnished	0.565***	(8.21)	0.563***	(7.88)	0.551***	(7.98)
Basement	-0.350***	(-3.82)	-0.316***	(-3.51)	-0.400***	(-4.57)
Number of toilets	1.087***	(5.59)	1.162***	(6.08)	1.160***	(5.95)
Balcony	0.747***	(5.45)	0.652***	(4.91)	0.734***	(5.37)
Terrace	1.262***	(4.98)	1.257***	(5.09)	1.266***	(5.19)
Flat size	-0.0650***	(-5.83)	-0.0713***	(-6.39)	-0.0689***	(-6.21)
Flat size squared	0.000247***	(3.55)	0.000279***	(4.03)	0.000271***	(3.93)
Distance to CBD	-0.169***	(-4.59)	-0.242**	(-2.89)	-0.161***	(-4.42)
Elevator	-0.105	(-0.27)	-0.0320	(-0.08)	-0.101	(-0.26)
Year	0.237*	(2.56)	0.234*	(2.55)	0.199*	(2.21)
Year squared	-0.0303**	(-2.89)	-0.0280**	(-2.68)	-0.0265**	(-2.60)
Constant	5.249***	(7.88)	5.859***	(6.02)	5.421***	(8.18)
Floor FE	Yes		Yes		Yes	
Floor∗Elevator	Yes		Yes		Yes	
District dummies	No		Yes		No	
1st district excluded	No		No		Yes	
Observations	2897		2897		2828	
R^2	0.304		0.320		0.282	

Notes: Regression examining the effect of the location surcharge on rental prices. *m2 price* and *Location surcharge* in EUR, *Condition* on a scale from 1 to 4 (4=very good), *Furnished* on a scale from 1 to 3 (3=fully furnished), *Basement* and *Elevator* as binary variables (Yes=1), *Number of toilets*, *Balcony* and *Terrace* in the corresponding number, *Flat size* in m2, *Distance to CBD* in km, *Year* as continuous variable. Data from a real estate database containing rental advertisements. t-statistics in parentheses; standard errors are heteroskedasticity robust. Significant effects are as follows: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 1 shows that in all three models the coefficient of the location surcharge is positive and significant. Depending on the specification, the coefficient is between 0.67 and 0.78, which is lower than might

be expected. If landlords were to pass on the full location surcharge to tenants, I should observe a location surcharge coefficient of 1. Interestingly, when I consider only the outer districts (Districts 10-23), I indeed observe a location surcharge coefficient of 1.04, as displayed in Table 2, Model 5. The opposite is true for the inner districts (Districts 1-9), where the location surcharge is even lower with 0.57, as shown in Table 2, Model 4.

Table 2: The effect is weaker for the inner districts (Model 4) compared to the outer districts (Model 5).

	(4)		(5)	
	m2 price		m2 price	
Location surcharge	0.574***	(7.47)	1.038***	(6.01)
Condition	1.244***	(10.43)	1.063***	(7.70)
Furnished	0.651***	(7.57)	0.446***	(4.10)
Basement	-0.351**	(-3.17)	-0.388*	(-2.36)
Number of toilets	1.156***	(6.60)	0.970	(1.33)
Balcony	0.759***	(4.95)	0.691*	(2.39)
Terrace	1.235***	(3.83)	0.804*	(2.26)
Flat size	-0.0703***	(-4.66)	-0.0892***	(-4.43)
Flat size squared	0.000244**	(2.72)	0.000458**	(3.15)
Distance to CBD	-0.795***	(-6.41)	-0.0152	(-0.29)
Elevator	-0.510	(-1.15)	1.779*	(2.18)
Year	0.266*	(2.35)	0.252	(1.67)
Year squared	-0.0341**	(-2.68)	-0.0223	(-1.30)
Constant	6.712***	(7.37)	5.726***	(5.35)
Floor FE	Yes		Yes	
Floor∗Elevator	Yes		Yes	
District dummies	No		No	
1st district excluded	No		No	
Only districts 1-9	Yes		No	
Only districts 10-23	No		Yes	
Observations	2026		871	
R^2	0.302		0.283	

Notes: Regression examining the effect of the location surcharge on rental prices for the inner districts 1-9 (Model 4) and for the outer districts 10-23 (Model 5). *m2 price* and *Location surcharge* in EUR, *Condition* on a scale from 1 to 4 (4=very good), *Furnished* on a scale from 1 to 3 (3=fully furnished), *Basement* and *Elevator* as binary variables (Yes=1), *Number of toilets*, *Balcony* and *Terrace* in the corresponding number, *Flat size* in m2, *Distance to CBD* in km, *Year* as continuous variable. Data from a real estate database containing rental advertisements. t-statistics in parentheses; standard errors are heteroskedasticity robust. Significant effects are as follows: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

As already mentioned, there are two possible reasons for the location surcharge coefficient beeing smaller than 1: Either landlords on the more expensive side do not charge the full location surcharge or landlords on the cheaper side are overcharging. Both reasons might explain, why the location surcharge coefficient is smaller in the inner districs: On the one hand, the feasible location surcharges in the inner districts are especially high, for example 7.99 EUR in the first district. It might be, that the rent cap in this area is above the market price and has thus no effect. On the other hand, it could also be that landlords can overcharge more easily in the inner districts due to the high demand for apartments in this popular area. Unfortunately, I cannot determine which of the two explanations is true based on my available data. Even though my rental price data set contains many variables, it does not contain all relevant variables needed to determine whether the charged rent is within the legally permitted range or not.

Table A.2 in the appendix shows the same models as Table 1, without the variable *floor*. As described above, this gives me considerably more observations. The results in Table A.2 are consistent with those in Table 1, with a slightly lower location surcharge coefficient.

In Table A.3 in the appendix, I perform a placebo test in which I randomly assign a *placebo surcharge* to the census districts at the location surcharge cutoffs. The surcharges correspond in amount and frequency to the location surcharges, but are randomly distributed across census districts. As expected, the coeffcients of the placebo surcharges are insignificant in all four models. This underlines the results of Table 1.

Even though the coefficient for the location surcharge is smaller than 1 in Table 1, it could be shown that the rent along the cutoff is considerably higher in zones with a higher permissible location surcharge. Since I was able to show that this assumption holds, I can now examine the effects of the different levels of location surcharges on the demographic structure.[10]

[10] The coefficients of the control variables in Table 1, Table 2, Table A.2 and Table A.3 seem plausible, only the variable *basement* surprises with its negative value.

5.2 Effects on the demographic structure

As shown in Table 3, Model 1, the location surcharge coefficient is positive and significant. This means that the average income at the cutoff is higher on the more expensive side. Hypothesis 1, namely that there are richer people living on the cheaper side of the cutoff, must therefore be rejected — I cannot confirm that this case of discrimination takes place. Rather, it seems that lower-income households are simply outpriced and cannot afford the flats on the more expensive side. Even if I control for the distance to the city centre (defined as St. Stephen's Cathedral), this result does not change, as can be seen in Model 2.

Table 3: The average income is higher on the more expensive side of the cutoff.

	(1)		(2)		(3)		(4)	
	Net Income		Net Income		Academics		Austrian	
Location Surcharge	3.519***	(5.37)	3.833***	(5.76)	0.0453***	(11.71)	0.0345***	(11.17)
Distance to CBD			0.546***	(7.67)	-0.0114***	(-8.09)	0.0214***	(11.65)
Constant	22.33***	(40.10)	19.64***	(27.57)	0.218***	(26.49)	0.562***	(58.93)
Observations	830		830		826		826	
R^2	0.324		0.345		0.418		0.187	

Notes: Spatial regression discontinuity design (RDD) at the location surcharge cutoff. *Net income* after transfer payments, in thousand EUR and annually, *Academics* and *Austrian* in percentage, *Location Surcharge* in EUR per square metre, *Distance to City Centre (CBD)* in km. Data from Statistics Austria (2017b, 2017c). t-statistics in parentheses; standard errors are heteroskedasticity robust. Significant effects are as follows: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Let us now look at Model 3 and Model 4 in Table 3. The share of people with an academic degree is significantly higher on the more expensive side of the cutoff (Model 3), as well as the share of Austrian citizens (Model 4). Hypothesis 2 and 3, namely that there are more people with an academic degree and more people with an Austrian citizenship living on the cheaper side of the cutoff, must therefore also be rejected. Why are the hypotheses not correct? The first explanation could of course be the same as for hypothesis 1: Since academics and Austrians have a higher income on average and Models 1 and 2 show that people with higher incomes tend to live on the more expensive side, the higher share of academics and Austrians on the more expensive side could also simply be a consequence of this outpricing effect. However, it is also be conceivable that another kind of discrimination is taking place: Landlords on the cheaper side might actually prefer tenants without academic degrees and migrants, believing that they are less familiar with the Austrian rental law and less likely to challenge an excessive rent in court. This assumes, of course, that landlords on the cheaper side are more

likely to charge an excessive rent than landlords on the more expensive side, which seems reasonable since their rent cap is further away from the market price. Which of these two possible explanations is correct and which has the greater effect cannot be determined on the basis of my data. The overcharge of rent is difficult to measure, not only because the rent caps are defined by many factors, but also because some landlords are legally allowed to charge a higher rent through individual assessments, as described in the chapter *Theory*.

Although not core to this research, I would like to mention the effect of the distance to city centre (CBD). The coefficients in Model 2 and 4 are positive and significant: per kilometre distance to the city centre, the average annual net income increases by approximately 546 EUR and the share of Austrians increases by approximately 2%. The coefficient in Model 3 is also significant, however negative: per kilometres distance to the city centre, the share of people with an academic degree decreases by approximately 1%. Thus, in Vienna, people with higher incomes tend to live further away from the city centre, although the price per square metre decreases with the distance to the city centre (Table 1). However, this does not mean that people with higher incomes pay less rent, as it seems reasonable to assume that they live on the outskirts of the city in comparatively large flats with many square metres. Furthermore, I can unfortunately not distinguish from the data available whether the people live in rented flats or condominiums. I consider it likely that many of the high-income people in my data who live far from the city centre live in condominiums. The increasing share of Austrian citizens with increasing distance from the city centre could be explained by their higher income too, as income and citizenship are correlated. Interestingly, the proportion of people with academic degrees increases towards the city centre, although income and education also correlate with each other. At least in the case of Vienna, people with academic degrees seem to have higher preferences for living close to the city center.

To examine possible regional differences within the city, I repeat my analysis from Table 3, but now I consider only the inner districts in Table 4 and only the outer districts in Table 5.

Table 4: In the Inner Districts (1-9), the Location Surcharge coefficients tend to be smaller.

	(1)		(2)		(3)		(4)	
	Net Income		Net Income		Academics		Austrian	
Location Surcharge	3.574***	(3.98)	3.265**	(3.16)	0.0206***	(4.53)	0.0215***	(6.36)
Distance to CBD			-1.165	(-1.69)	-0.0362*	(-2.40)	0.0143	(1.60)
Constant	21.33***	(16.78)	24.33***	(8.63)	0.339***	(9.20)	0.612***	(28.64)
Observations	308		308		308		308	
R^2	0.304		0.308		0.343		0.111	

Notes: Spatial regression discontinuity design (RDD) at the location surcharge cutoff for the inner districts (districts 1-9). *Net income* after transfer payments, in thousand EUR and annually, *Academics* and *Austrian* in percentage, *Location Surcharge* in EUR per square metre, *Distance to City Centre (CBD)* in km. Data from Statistics Austria (2017b, 2017c). t-statistics in parentheses; standard errors are heteroskedasticity robust. Significant effects are as follows: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 5: In the Outer Districts (10-23), the Location Surcharge coefficients are larger.

	(1)		(2)		(3)		(4)	
	Net Income		Net Income		Academics		Austrian	
Location Surcharge	4.797***	(13.59)	4.138***	(10.25)	0.0818***	(13.10)	0.0635***	(6.65)
Distance to CBD			0.621***	(6.13)	-0.00607**	(-3.18)	0.0223***	(7.40)
Constant	22.07***	(93.01)	19.00***	(38.44)	0.154***	(14.02)	0.533***	(36.73)
Observations	522		522		518		518	
R^2	0.337		0.386		0.286		0.240	

Notes: Spatial regression discontinuity design (RDD) at the location surcharge cutoff for the outer districts (districts 10-23). *Net income* after transfer payments, in thousand EUR and annually, *Academics* and *Austrian* in percentage, *Location Surcharge* in EUR per square metre, *Distance to City Centre (CBD)* in km. Data from Statistics Austria (2017b, 2017c). t-statistics in parentheses; standard errors are heteroskedasticity robust. Significant effects are as follows: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

If you compare Table 4 and Table 5, you can see that the location surcharge coefficients are larger in the outer districts compared to the inner districts. The difference is particularly large with regard to the share of academics: In Table 4, census districts with a 1 EUR higher location surcharge have on average a share of academics that is 2% higher, whereas in Table 5 it is 8%. That there is a difference between inner and outer districts is consistent with my results from Table 2, in which I analyzed the rental price data and showed a larger effect of the location surcharge on rents in the outer districts compared to the inner districts.

As a robustness check, I extend my model from Table 3 with a neighbour parsing. As described in chapter *Method*, I therefore create boxes for all pairs of neighbouring census districts if they are located in different location surcharge zones and then include the boxes as fixed effects in my regression. The neighbour parsing allows me to directly compare census districts with their neighbouring census districts. The results of this extended model are shown in Table 6.[11] In census districts, where 1 EUR more location surcharge can be charged, the average annual net income is on average 3'825 EUR higher (Model 1), the share of academics is on average approximately 2.5% higher (Model 3) and the share of Austrians 4% higher (Model 4). Except for the smaller coefficient on academics, these results are similar to those in Table 3. Again, I cannot find any discrimination in favour of high-income households, university graduates or Austrian citizens.

Table 6: Robustnesscheck with Neighbour Parsing yields similar results.

	(1)		(2)		(3)		(4)	
	Net Income		Net Income		Academics		Austrian	
Location Surcharge	3.825**	(3.17)	3.879**	(3.25)	0.0251***	(5.68)	0.0401***	(7.30)
Distance to CBD			0.479*	(2.07)	-0.0112**	(-2.62)	0.0335***	(6.46)
Constant	22.09***	(17.22)	19.97***	(23.09)	0.239***	(12.31)	0.508***	(21.03)
Neighbour Parsing	Yes		Yes		Yes		Yes	
Observations	1207		1207		1202		1202	
Within R2	0.342		0.350		0.144		0.229	

Notes: Spatial regression discontinuity design (RDD) at the location surcharge cutoff. *Net income* after transfer payments, in thousand EUR and annually, *Academics* and *Austrian* in percentage, *Location Surcharge* in EUR per square metre, *Distance to City Centre (CBD)* in km. *Neighbour parsing* refers to "boxes", each of which contains two neighbouring census districts and which are included in the regression as fixed effects. Data is stacked such that each observation occurs as often as it occurs in boxes. Data from Statistics Austria (2017b, 2017c). t-statistics in parentheses; standard errors are clustered by census districts. Significant effects are as follows: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

In Table A.4 in the appendix, I perform a placebo test to check the soundness of my model. The four regressions are identical to those in Table 3, only the variable *Location Surcharge* is replaced by a *Placebo Surcharge*. For that, I randomly assign each census district at the location surcharge cutoff a placebo surcharge. Those placebo surcharges are identical in amounts and frequencies to the location surcharges, however, they are regionally randomly distributed. In all four models in Table A.4, the placebo coefficient is statistically insignificant.

[11] The larger numbers of observations in Table 6 come from the fact that the data is stacked in order to conduct the neighbour parsing.

In Table A.5, I perform another placebo test, now not only on the intensive, but also on the extensive margin. Thus, I do not only change the surcharges at the location surcharge cutoffs, but I also change the positions of the cutoffs. This is achieved by randomly distributing a placebo surcharge across all census districts (not only those at the original cutoffs), and then running the regression discontinuity design at these new *Placebo cutoffs*. The placebo surcharges are again distributed with the same frequency and amount as the location surcharges. As Table A.5 shows, all coefficients of the placebo surcharge are insignifcant, which underlines my results from Table 3.

6 Conclusion

This paper studies to what extent rent caps lead to discrimination against certain population groups in the rental housing market. The Viennese rental housing market is chosen as an "experimental laboratory", since there exist many different rent caps within the same city and even within the same districts. The sharp cutoffs separating zones with different rent caps form the basis of my spatial regression-discontinuity model. After analysing a database containing rental housing advertisements, I find that the average rent is significantly higher on those sides of the cutoff where the location surcharges and therefore also the rent caps are higher. This result may seem trivial, but considering that there is a legal grey area around the location surcharges, it does show that the legal regulation is having an effect. However, this effect is not 1: In zones where 1 EUR more may be charged as a location surcharge, the rent is only 0.73 EUR higher on average. If I break down the results regionally, I see that the effect in the outer districts is actually around 1 EUR (at 1.04 EUR) while in the inner districts the effect is significantly lower, at 0.57 EUR. Especially in the inner districts, the landlords on the more expensive side either do not pass the location surcharge on to the tenants, or the landlords on the cheaper side charge a location surcharge that is too high. Even though I consider the second explanation more likely, a closer examination of this result would be an exciting subject for a future research project.

Do more people with higher incomes, more university graduates or more Austrian citizens live in the, ceteris paribus, cheaper flats because they are preferred by the landlords? No evidence of such form of discrimination can be found in this study. Instead, I find a significantly higher average income in the, ceteris paribus, more expensive flats. This effect is particularly pronounced in the outer districts. However, I consider it likely that the higher average income is a simple outpricing effect and that people with low incomes simply cannot afford the more expensive rent. Since income is correlated with education and nationality, it is hardly surprising that also the proportion of university

graduates and Austrian citizens is higher in the, ceteris paribus, more expensive flats. However, another explanation would also be conceivable, namely that landlords overcharge on the cheaper side of the cutoff and believe that non-academics and foreigners are less likely to take them to court. In that case, I would have misjudged the direction of the discrimination. In summary, with the data and methodology used in this paper, I cannot find evidence of discrimination as a result of rent caps. However, further research with more comprehensive and accurate data would be needed to reach a final conclusion in this discussion. In particular, more comprehensive rental price data could answer the question of whether the location surcharge coefficient is smaller than 1 because landlords overcharge on the cheaper side of the cutoff. This information would then also help in the discussion of whether landlords discriminate in their tenant selection.

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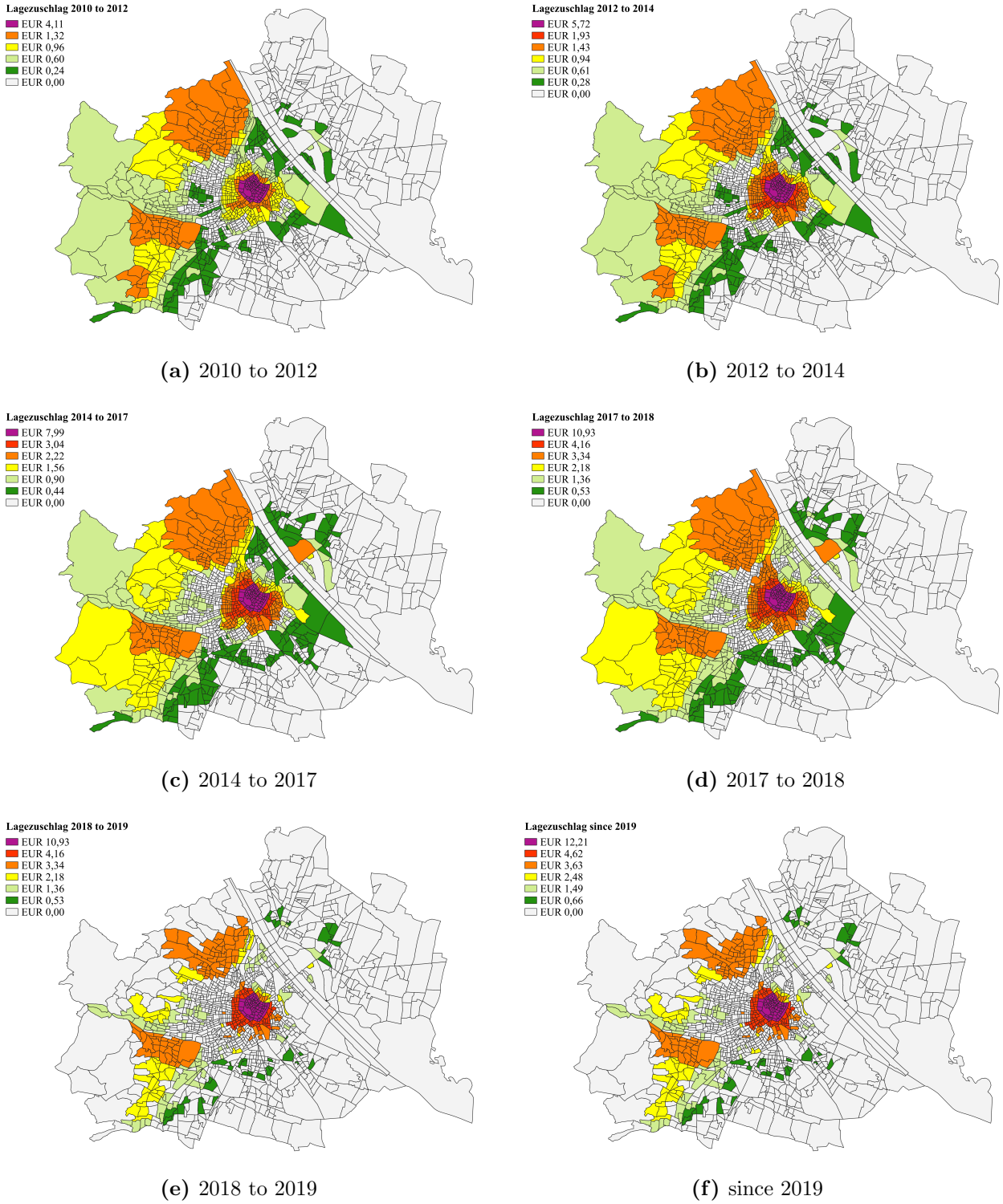
A Appendix

Figure A.1: Location surcharge maps since 2010.



(a) 2010 to 2012

(b) 2012 to 2014

(c) 2014 to 2017

(d) 2017 to 2018

(e) 2018 to 2019

(f) since 2019

Source: Own illustration. Data: a) Mösenlechner, 2022; b) Hofbauer, n.d.; c) Rösner, 2014; d) Putschögl, 2018; e) Nothegger, 2018; f) MA 25, City of Vienna, 2019a.

Table A.1: The share of old buildings is slightly higher on the cheaper side of the cutoff which probably also explains the differences in flat sizes.

	(1)		(2)		(3)	
	lower side		higher side		difference	
	mean	sd	mean	sd	diff	t
Share of buildings with type:						
Built before 1945	0.57	0.28	0.50	0.32	0.07***	(3.36)
Residential (1 flat)	0.13	0.23	0.17	0.26	-0.04*	(-2.47)
Residental (2 flats)	0.01	0.03	0.02	0.04	-0.01*	(-2.38)
Residental (>2 flats)	0.64	0.29	0.57	0.32	0.08***	(3.58)
Residental other	0.01	0.02	0.01	0.02	-0.00	(-0.50)
Hotel	0.01	0.03	0.01	0.04	-0.00	(-0.86)
Office	0.05	0.07	0.07	0.12	-0.02*	(-2.32)
Commercial	0.03	0.05	0.03	0.06	-0.00	(-0.71)
Traffic	0.01	0.02	0.01	0.02	-0.00	(-0.01)
Industrial	0.03	0.06	0.02	0.05	0.01*	(2.13)
Culture and education	0.02	0.05	0.03	0.07	-0.01*	(-2.01)
Other	0.03	0.07	0.04	0.09	-0.01	(-1.56)
Type unknown	0.03	0.04	0.03	0.04	0.00	(0.53)
Share of flats with size:						
<45m^2	0.23	0.13	0.17	0.13	0.05***	(5.83)
45-60m^2	0.21	0.09	0.20	0.11	0.01	(1.74)
60-75m^2	0.18	0.07	0.18	0.08	0.00	(0.65)
75-90m^2	0.15	0.07	0.16	0.08	-0.00	(-0.40)
90-120m^2	0.12	0.07	0.14	0.09	-0.02***	(-4.12)
120-150m^2	0.04	0.05	0.06	0.06	-0.02***	(-4.78)
150-200m^2	0.02	0.03	0.03	0.05	-0.01***	(-4.92)
>200m^2	0.01	0.05	0.02	0.05	-0.01*	(-2.57)
Size unknown	0.02	0.04	0.03	0.05	-0.00	(-0.69)
Observations	413		417		830	

Notes: Summary statistics comparing buildings along the location surcharge cutoff. Data from Statistics Austria (2017a). t-statistics in parentheses. Significant effects are as follows: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A.2: Also without controlling for the variable *floor*, the location surcharge has a significant, positive effect on the rent level.

	(1)		(2)		(3)	
	m2 price		m2 price		m2 price	
Location surcharge	0.668***	(14.77)	0.587***	(9.83)	0.685***	(15.04)
Condition	1.151***	(14.48)	1.105***	(13.76)	1.132***	(14.27)
Furnished	0.432***	(7.10)	0.435***	(7.02)	0.425***	(6.97)
Basement	-0.260***	(-3.47)	-0.240**	(-3.20)	-0.296***	(-4.12)
Number of toilets	1.021***	(6.79)	1.075***	(7.13)	1.056***	(6.91)
Balcony	0.696***	(6.14)	0.664***	(5.98)	0.696***	(6.15)
Terrace	2.138***	(17.43)	2.110***	(16.89)	2.201***	(18.07)
Flat size	-0.0592***	(-6.57)	-0.0631***	(-6.96)	-0.0609***	(-6.78)
Flat size squared	0.000224***	(3.98)	0.000242***	(4.28)	0.000236***	(4.19)
Distance to CBD	-0.169***	(-5.82)	-0.252***	(-4.04)	-0.170***	(-5.84)
Elevator	0.295***	(3.36)	0.313***	(3.54)	0.274**	(3.12)
Year	0.251***	(3.33)	0.243**	(3.25)	0.234**	(3.16)
Year squared	-0.0304***	(-3.61)	-0.0271**	(-3.23)	-0.0284***	(-3.43)
Constant	5.128***	(9.46)	5.962***	(7.66)	5.267***	(9.75)
District dummies	No		Yes		No	
1st district excluded	No		No		Yes	
Observations	4112		4112		4019	
R^2	0.320		0.332		0.301	

Notes: Regression examining the effect of the location surcharge on rental prices. *m2 price* and *Location surcharge* in EUR, *Condition* on a scale from 1 to 4 (4=very good), *Furnished* on a scale from 1 to 3 (3=fully furnished), *Basement* and *Elevator* as binary variables (Yes=1), *Number of toilets*, *Balcony* and *Terrace* in the corresponding number, *Flat size* in m2, *Distance to CBD* in km, *Year* as continuous variable. Data from a real estate database containing rental advertisements. t-statistics in parentheses; standard errors are heteroskedasticity robust. Significant effects are as follows: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A.3: Placebo test with randomly assigned location surcharges on an intensive margin.

	(1)		(2)		(3)	
	m2 price		m2 price		m2 price	
Placebo Surcharge	0.0385	(1.70)	0.0412	(1.80)	0.0325	(1.49)
Condition	1.344***	(14.23)	1.213***	(13.03)	1.293***	(13.74)
Furnished	0.655***	(9.38)	0.583***	(8.23)	0.635***	(9.18)
Basement	-0.360***	(-3.73)	-0.341***	(-3.76)	-0.438***	(-4.85)
Number of toilets	1.131***	(5.57)	1.172***	(6.05)	1.219***	(6.10)
Balcony	0.831***	(5.81)	0.684***	(5.06)	0.879***	(6.15)
Terrace	1.290***	(4.91)	1.284***	(4.99)	1.339***	(5.12)
Flat size	-0.0645***	(-5.62)	-0.0715***	(-6.32)	-0.0632***	(-5.56)
Flat size squared	0.000280***	(3.92)	0.000297***	(4.23)	0.000268***	(3.80)
Distance to CBD	-0.407***	(-10.41)	-0.445***	(-5.26)	-0.346***	(-9.30)
Elevator	-0.288	(-0.72)	-0.0735	(-0.19)	-0.198	(-0.50)
Year	0.308**	(3.19)	0.286**	(3.08)	0.264**	(2.86)
Year squared	-0.0208	(-1.91)	-0.0185	(-1.77)	-0.0170	(-1.62)
Constant	5.258***	(7.66)	9.579***	(11.05)	5.290***	(7.81)
Floor FE	Yes		Yes		Yes	
Floor*Elevator	Yes		Yes		Yes	
Distict dummies	No		Yes		No	
1st district excluded	No		No		Yes	
Observations	2897		2897		2828	
R^2	0.234		0.299		0.237	

Notes: Placebo test: Regression examining the effect of a *Placebo Surcharge* on rental prices. *Placebo Surcharges* are identical in amount and frequency to the original Location Surcharges in their respective period, but are randomly assigned to the census districts at the location surcharge cutoff. *m2 price* and *Placebo Surcharge* in EUR, *Condition* on a scale from 1 to 4 (4=very good), *Furnished* on a scale from 1 to 3 (3=fully furnished), *Basement* and *Elevator* as binary variables (Yes=1), *Number of toilets*, *Balcony* and *Terrace* in the corresponding number, *Flat size* in m2, *Distance to CBD* in km, *Year* as continuous variable. Data from a real estate database containing rental advertisements. t-statistics in parentheses; standard errors are heteroskedasticity robust. Significant effects are as follows: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A.4: Placebo test with randomly assigned location surcharges on an intensive margin.

	(1)		(2)		(3)		(4)	
	Net Income		Net Income		Academics		Austrian	
Placebo Surcharge	-0.0793	(-0.52)	-0.0735	(-0.48)	-0.000896	(-0.40)	0.00154	(0.65)
Distance to CBD			-0.170	(-1.10)	-0.0200***	(-11.54)	0.0148***	(7.97)
Constant	26.30***	(66.48)	27.02***	(28.39)	0.306***	(37.80)	0.626***	(72.69)
Observations	830		830		826		826	
R^2	0.000		0.003		0.166		0.073	

Notes: Placebo test: Spatial regression discontinuity design (RDD) at the location surcharge cutoff with *Placebo Surcharges*. *Placebo Surcharges* are identical in amount and frequency to the 2014-2017 Location Surcharges, but are randomly assigned to the census districts at the location surcharge cutoff. *Net income* after transfer payments, in thousand EUR and annually, *Academics* and *Austrian* in percentage, *Placebo Surcharge* in EUR per square metre, *Distance to City Centre (CBD)* in km. Data from Statistics Austria (2017b, 2017c). t-statistics in parentheses; standard errors are heteroskedasticity robust. Significant effects are as follows: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A.5: Placebo test with randomly assigned location surcharges on an extensive and intensive margin.

	(1)		(2)		(3)		(4)	
	Net Income		Net Income		Academics		Austrian	
Placebo Surcharge	-0.0697	(-0.50)	-0.0869	(-0.62)	-0.00235	(-1.48)	-0.0000689	(-0.04)
Distance to CBD			-0.266*	(-2.38)	-0.0216***	(-18.45)	0.0188***	(15.11)
Constant	26.85***	(86.26)	28.09***	(38.45)	0.320***	(50.02)	0.616***	(86.73)
Observations	1301		1301		1296		1296	
R^2	0.000		0.007		0.218		0.128	

Notes: Placebo test: Spatial regression discontinuity design (RDD) at the location surcharge cutoff with *Placebo Surcharges*. *Placebo Surcharges* are identical in amount and frequency to the 2014-2017 Location Surcharges, but are randomly distributed across the city. Only census districts at the new cutoffs between *placebo location surcharge* zones are included in the regression. *Net income* after transfer payments, in thousand EUR and annually, *Academics* and *Austrian* in percentage, *Placebo Surcharge* in EUR per square metre, *Distance to City Centre (CBD)* in km. Data from Statistics Austria (2017b, 2017c). t-statistics in parentheses; standard errors are heteroskedasticity robust. Significant effects are as follows: $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.