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"Does Rent Control Reduce Supply on the Housing Market?

The Effect of the 1982 Tenancy Law Reform in Austria on Vacancies"

verfasst von / submitted by

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Abstract

Während Ökonomen weitgehend darin übereinstimmen, dass Mieterschutzregelungen zu verschiedensten Ineffizienzen führen können, beschäftigt sich überraschend wenig empirische Literatur speziell mit dem Effekt einer Mietzinsobergrenze auf das Angebot bereits vorhandenen Wohnraums auf dem Mietwohnungsmarkt. Der neoklassischen mikroökonomischen Theorie entsprechend sollten bei Mietpreisen, die unter dem Marktniveau liegen, manche potentiellen Vermieter alternative Nutzungsmöglichkeiten für ihre Wohnungen bevorzugen, was zu einem Anstieg nicht vermieteter Wohnungen, und damit Leerständen, führt. In diesem Paper suche ich anhand eines Pseudo-Experiments in Österreich aus dem Jahre 1982 nach empirscher Evidenz für diese Frage: die Einführung des neuen Mietrechtsgesetzes brachte Mietzinsobergrenzen für Wohneinheiten in vor 1945 erbauten Gebäuden mit sich, während die Mieten in jüngeren Gebäuden nach wie vor grundsätzlich nicht reguliert waren. Auf Basis eines Difference-in-Differences Ansatzes und Daten aus der österreichischen Gebäude- und Wohnungszählung prüfe ich, ob die Leerstandsraten für Altbauten (Versuchsgruppe) im Vergleich zu denen in Neubauten (Kontrollgruppe) nach der Mietrechtsreform steigen. Da dies tatsächlich nicht der Fall ist, bestätigt meine empirische Analyse die oben beschriebene theoretische Vorhersage nicht.

Abstract

Whereas economists widely agree that rent control can lead to various kinds of inefficiencies, surprisingly few empirical literature is dedicated specifically on the effect of a rental price ceiling on the supply of already existing dwelling units on the rented housing market. Neoclassical microeconomic theory, of course, predicts that at below-equilibrium rental prices, some potential landlords will prefer alternative uses of their dwellings, leading to an increase in dwelling units that are not rented out – thus vacant. In this paper, I look for empirical evidence on the question by exploiting a natural experiment that took place in Austria in 1982, when a new tenancy law was introduced that involved a rental price ceiling for buildings constructed before 1945 whilst leaving rents in younger buildings free of regulation. Applying a difference-in-differences approach and using data from the Austrian Buildings and Dwellings Census, I test whether the vacancy rate of old buildings (treatment group) increased after the policy break, compared to the vacancy rate of new buildings (control group). As this is not the case, my empirical analysis does not support the above described theoretical result.

Contents

1 Introduction

When I first moved to Vienna a few years ago, my rent for a room in a shared flat was 360 Euro per month. As opposed to this, currently, as of February 2020, I am paying only 300 Euro for the same room. This unexpected twist arose as a results of my flatmates and me challenging the rental price after we had become aware that the rent we had been paying exceeded the legally determined rental price ceiling for the kind of flat we lived in, and we eventually got our rent reduced. Whereas this experience made me personally a big fan of the rental price ceiling that certain buildings are subject to in Austria, it also made me ponder the economic backgrounds and implications of this form of rent control. Concretely, in this paper I examine whether the rental price ceiling system in Austria causes higher vacancy rates among the dwellings that are subject to it by exploiting the 1982 tenancy law reform as a natural experiment.

The housing market has been a preferred target of regulatory governmental interventions in the recent decades, especially in the European countries (see O'Sullivan and De Decker (2007) and Cuerpo, Kalantaryan, and Pontuch (2014)), which can be justified by the peculiarities of housing as an economic good as Kunner and Baumgartner (2012) point out: firstly, housing is highly heterogeneous, in the sense that no two dwelling units are identical in all of their characteristics, in particular their location, so each supplier can be thought of as a monopolist on the market for the specific dwelling unit he owns. Also, there is typically an information asymmetry, as suppliers tend to know not only their object, but also the general market situation better than demanders. It can consequently be doubted that the equilibria observed in unregulated real-world markets are indeed efficient - prices might be well above equilibrium prices on ideal markets while supply would be artificially restricted. Finally, housing is a very basic human need while at the same time it does not have any substitutes, so accommodating the demand for it is crucial.

On the other hand, economists widely agree that rent control in the form of rigorous price ceilings (so-called first generation rent control) does more harm than good (see Navarro (1985), Jenkins (2009) and Block, Horton, and Shorter (1998) who even refer to rent control as an "economic abomination"). Rent control is associated with misallocation of housing (Glaeser & Luttmer, 2003), retarded maintenance, a reduction of mobility and fewer new buildings being constructed (Arnott (1995) and Turner and Malpezzi (2003)). Kruse, Ozdemir, Thompson, et al. (2005) even suggest to simulate the market for rented housing in a classroom experiment because it is so well suited to demonstrate the dead-weight loss induced by a rental price ceiling: One of the most elementary building blocks of neoclassical microeconomics is the theoretical result that if a price ceiling lower than the equilibrium price is implemented on ideal markets, demand is higher and supply is lower than in equilibrium, some transactions do not take place anymore and total welfare consequently decreases. For the rented housing market, this implies not only that fewer new buildings are constructed due to lower expected revenues from renting them out, but also that already existing dwelling units are less likely to be rented out because their owners benefit relatively more from alternative uses (provided that the real-world rented housing market meets the assumptions of the ideal market to a sufficient extent). In other words, vacancy rates should increase.

Surprisingly little empirical research has been dedicated specifically to the issue of vacancies in the context of rent control: Wilhelmsson, Andersson, and Klingborg (2011) find that the Swedish rent control system indeed has an effect on vacancies, although their approach is quite different from mine, as they do not exploit any natural experiment-like setting, but rather construct a macroeconomic model and test it on actual data. Their findings suggest that the Swedish rent control system leads to higher vacancy rates in small municipalities, where rents are above market rents, and low vacancy rates in cities, where rents are well below market rents; their perspective is entirely demand-sided whereas the argumentation underlying my analysis bases on the assumption that the supply side is the limiting factor - their definition of vacancies seems to include only dwelling units that their owners would actually like to supply on the market. Using microlevel data from New York City, Rapaport (1992) does not find any effect of the rent regulation status of a housing unit on the probability of being vacant, whereby this paper's result is not supported by any pseudo experiment either.

Besides being - to my knowledge - the first empirical work explicitly on the link between rent control and vacancies that is based on a natural experiment, my analysis also contributes to the fraction of the related literature targeting European housing markets (which is by far outnumbered by US-American literature) and I have not found any empirical paper on the impacts of rental price regulation - let alone its impact on vacancies - that uses Austrian data.

My analysis is based on the introduction of a new tenancy law (the *Mietrechtsgesetz*) in Austria in 1982, which involved a rental price ceiling system that strikingly, only dwelling units in buildings constructed before 1945 were subject to, whereas all buildings would be generally free from rental price regulation before 1982 independently of their construction year (with some exceptions mostly depending on the quality of the dwelling unit, see Section 2 and 3). I exploit this policy break as a natural experiment and implement a difference-in-differences approach, identifying dwelling units in buildings constructed between the years 1919 and 1944 as the treatment group and those in buildings constructed from 1945 to 1960 as the control group. I am thus not interested in the effect on the construction of new buildings, but rather on the allocation of housing units: I expect that some landlords choose not to rent their dwelling units out after the rental price ceiling is imposed on their property, but prefer alternative uses such as using them as secondary residences. In other words, I hypothesize that

1. a unit's probability of being registered as a main residence should decrease after the introduction of the new tenancy law if this unit is in a building constructed before 1945, as compared to the reference probability of those in buildings constructed after 1945, and that additionally,

2. this effect should be strongest for dwelling units owned by single private persons, as I assume their owners have more alternatives to renting them out (and thus lower opportunity costs of not doing so) than e.g. municipality owned dwelling units'.

However, using data from the Austrian Buildings and Dwellings Census, I find no reliable evidence that would support these hypotheses - my estimates are insignificant, or even of the opposite sign than expected in some specifications. Although these findings are in accordance with those of Rapaport (1992), whose work is probably closest to mine, they contradict the predictions of economic theory as just described (assuming some positive price elasticity for the landlords) so I shed a little more light on the weaknesses of my approach in the end.

The rest of the paper is structured as follows: After sketching the historical development of Austrian tenancy law and the context in which the natural experiment I am exploiting takes place in Section 2, I describe my data and my empirical strategy in greater detail in Section 3 and introduce the econometric model and the estimation results in Section 4. In Section 5, I discuss the shortfalls and caveats of my investigation in greater detail before I recapitulate the major findings and provide the final conclusions of my investigation in Section 6.

2 Historical and Legal Context

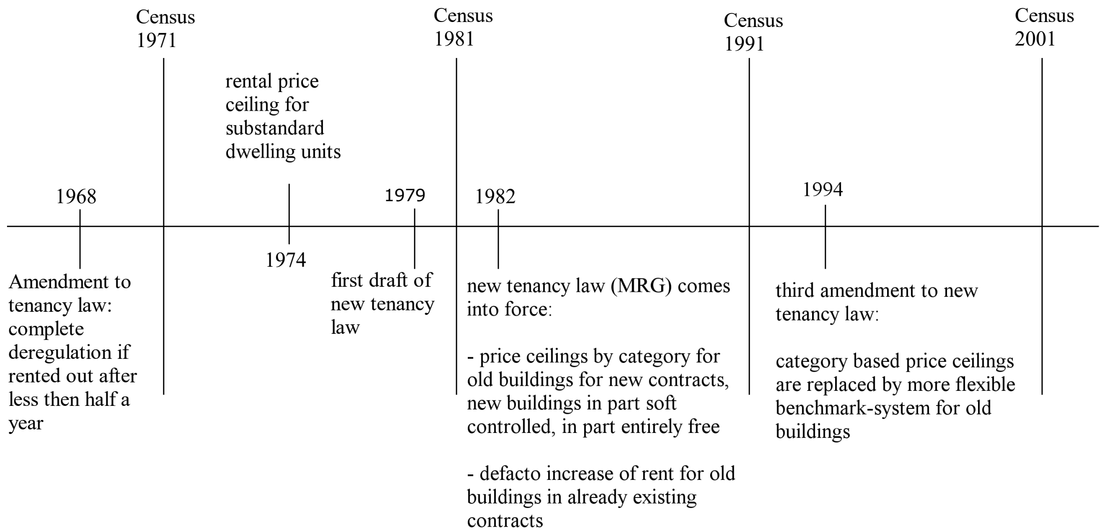
The following section aims at providing some historical and legal context by briefly summarizing the milestones of the development of the Austrian tenancy law and the general situation on the housing market, with a clear focus on rental price regulation and the 1982 policy break (*Mietrechtsgesetz*). If not declared otherwise, this overview is based on Würth (2012) and Langer (1995).

2.1 Until 1982

As I will describe in greater detail later in Section 3, the fourty-year period covered by my data starts in 1971. At this point, modern tenancy law had already had a turbulent history, starting in 1922 with the first separate codification of tenancy law in the *Mietengesetz* (BGBl 1922/872) which marked the beginning of legal institutions to protect the tenant (no unwarranted eviction, possibility to pass a contract on to his descendants etc.). In addition to a rental price ceiling for older buildings (in the form of a fixed ratio to the price charged in 1914 for the same dwelling unit, *Friedensmietzins*), attempts where made to decelerate the increase in rent for all dwelling units but also prices of other goods (e.g. the *Law regarding Price Regulation* BGBl 1949/166). The up and down of the degree of rental price regulation during the following decades finally resulted in a relative liberalisation in 1955, when rents would not be regulated anymore at all in contracts made after less than four months of vacancy (BGBl 1955/241), which was increased to six months in 1968 (BGBl 1967/281). This restriction would, of course, aim at setting incentives to avoid long vacancies. Notably, this liberalisation did not affect rental prices in old contracts, which would only be allowed to increase step by step.

Figure 1 sketches the further development in the form of a timeline. Remember that I retrieve my data from the Buildings and Dwellings Censuses of the years 1971, 1981, 1991 and 2001, which are marked in the graph. The liberal regime of 1968 would, unfortunately for my investigation, not last long. In 1974 already, the law maker decided to regulate rents in new contracts again for substandard dwelling units by means of a price ceiling of 4 Austrian Shilling per m^2 (BGBl 1974/409), whereby "substandard" refers to the *Law regarding Urban Renewal*

Figure 1: The History of Austrian Tenancy Law



(BGBl 1974/287) and means not having running water or a toilet inside the flat. After all these adjustments and punctual solutions, tenancy law had become so complex and fragmented into various separate special laws that plans were made for a fundamental reform. The first draft of the new tenancy law - the *Mietrechtsgesetz*, MRG - was sent out in the summer of 1979 (this might be important due to the possibility of an anticipation effect), and finally came into force on January 1st 1982. It brought considerable changes that are of importance for my purpose.

2.2 The 1982 Policy Break

The central novelty, which I will use as the treatment in my natural experiment, was the introduction of a system of three possible price regulation regimes, an object could be subject to depending on its characteristics. Remember that before, the only distinction for new contracts would be between a *ceiling price* (substandard objects) and *free market prices* (all others). After the break, there would instead be either 1) *free market prices*, an 2) *"adequate rental price"* or 3) *rental price ceilings by category.* The relevant reference value for the *adequate rental price* would be the usual market price for dwelling units comparable in size, type, quality of the flat and location, which implies that the price that a landlord was allowed to charge for these dwelling units was not below equilibrium price in an economic sense, and would thus not decrease supply under the scope of the classical microeconomic framework. I will therefore not distinguish between regimes 1) and 2) in my analysis, but rather assign all dwelling units subject to either of the two into the control group (no price regulation). The categorisation referred to by Regime 3) is basically oriented to the features of the flat, ranging from the top category A, which includes central heating and a fully equipped bathroom, B, which includes a bathroom, C, which includes running water and toilet, and D with either no running water or no toilet inside the apartment (so D corresponds to the substandard definition from the *Law regarding Urban Renewal*)[1].

Table 1 provides a juxtaposition of the situation before and the situation after 1982, including an overview over the types of dwelling units that would be subject to the different regimes. Note that "property flats" refers to a legal institution that allows a person to be proprietor of only one flat of a building without owning the whole building, which marks an exception from the rule *superficies solo cedit* meaning that by default, the legal status of the parcel of land decides the legal status of everything that is on it. As already explained, substandard dwelling units, i.e. those of the lowest category, would be regulated before the break since 1974. In contrast, the construction period would play a major role after the policy break. All buildings built after 1945 (from here on referred to as *new buildings*) would automatically be subject to one of the two liberal regimes. Strikingly, lowest category flats in new buildings or in buildings with less than three flats would thus be regulated (treated) before the break, but subject to free market prices (control) after the break, although I have no other possibility than to continuously assign them to the control group, as I will explain in greater detail in Section 3. I will discuss the degree to which this could rise doubts about the validity of my approach in Sections 3 and 5. Similarly, some of the flats in buildings built before 1945 (hence referred to as *old buildings*) would not be subject to the ceiling price by category, namely those which were either of outstanding quality in terms of category and

[1]Note that by law, there where additional requirements for the categories, e.g. bigger than 30 m^2 and at least a small kitchen for category A. However, I only have data for the criterion as described above for the years 1971 and 1981, so I am restricted to them whenever I use data on the categories.

Table 1: Juxtaposition of Regimes

	Before 1982	After 1982
Free market	Category A, B and C	”property flats” built after 1945 buildings with less than three flats all buildings built after 1953 without subsidies
Adequate rent		all others built after 1945 old flats rented out after less then 6 months if they are 1) category A and >90m^2 or 2) category B and >130m^2 or 3) category A, B or C if they were substantially renovated
Price Ceiling	Category D and E 4.00 S/m^2	the remaining old buildings, by category: A: 22.00 S/m^2 B: 16.50 S/m^2 C: 11.00 S/m^2 D: 5.50 S/m^2

Note: This table leaves out information that is irrelevant for my purpose. For the complete list of exceptions, see the full text of the law (BGBl 1981/520).

size or if the owner had put considerable effort into renovating them, and thereby increased their category to A, B or C (given the flat would not be vacant for more than half a year). This can be problematic in a twofold way: Firstly, it implies that not the whole treatment group was treated. In combination with the afore mentioned problem that some flats in new buildings would actually be subject to price regulation as they were substandard flats, this would lead to a downwards bias of my measurement of the treatment effect. Secondly, the exception that by renovating a flat the owner could escape the regulation means that treatment and control group are not perfectly fixed, instead, a systematic selection into the control group could potentially happen.

As can also be seen in Table 1, the price ceiling changed from 4 Shilling per m^2 for substandard flats to 5.5 Shilling for substandard old buildings after 1981, which I would consider comparable, so category D old buildings would in fact not face a change in their treatment status.

Talking only about the subset constituted by flats in buildings with more than two flats, I conclude that the actual treatment group in the sense of those dwelling units switching from no regulation to being subject to a price ceiling would consequently consist of buildings built before 1945 of category C and of category B or A if they are smaller then 130 and 90m^2 respectively (considering also dwelling units with one or two units would make things too complicated). The actual control group, on the other hand, i.e. those whose regulation status did not change at the break, would consist of old buildings of category D and new buildings of all categories other then D. Importantly, this implies that when I try to identify buildings as treated or not treated only by their construction year, as I already described quickly in the introduction, I will certainly fail to assign some of them to their actual group accurately. I will further elaborate why I came to the conclusion that this is still the best solution in Sections 3 and 5.

Yet another change that could possibly interfere with the new price regulation system was the de facto increase of the allowed rental price for those contracts that were still subject to the *Friedensmietzins*-regulation to a reasonable amount after being ridiculously low for a long period of time via the possibility of charging a maintenance fee (they would be adjusted the last time in 1952). This would consequently increase incentives for the landlord to keep up the corresponding contracts. Whereas new contracts and those buildings built around the threshold of 1945 would not be affected by this change, it could still disturb my results, as the dwelling units first rented out before 1914 (which was the reference point for the *Friedensmietzins*) would be substitutes to the old buildings in my treatment group in terms of which regime they are subject to for new contracts. As I see no way to explicitly include this simultaneous policy break in my model, this is also something to keep in mind.

In the period following the 1982 policy break, the most fundamental change was the replacement of the category system by a benchmark system (*Richtwertsystem*) for flats of categories A, B and C by the third amendment to the tenancy law (*3. Wohnrechtsänderungsgesetz*) in 1994 (BGBl 1993/800). Whereas category

D flats would still be subject to a nationwide universal price ceiling, there were different benchmark prices in the federal states of Austria for the upper three categories, whereby this benchmark would refer to a category A flat of average size and location. The actual price of a dwelling unit regulated this way would then depend on the deviations from this norm in terms of quality, size, location and extra features like e.g. balconies. In 1994, the published benchmark rental prices would range from 46.00 S/m^2 in the state of Burgenland over 50,40 S/m^2 in Vienna and up to 77.40 S/m^2 in Vorarlberg (BGBl. II Nr. 37/2005 (StF), announcement from 2005 but containing also old benchmark rental prices). Note that the ceilings from Table 1 would have been adjusted to inflation, reaching the prices of A: 29.60 S, B: 22.20 S, C: 14.80 S, D: 7,40 S in 1993, as announced in the very same law which also introduced the new benchmark system (BGBl 1993/800). Still, the jump between the systems remains remarkable, and albeit old buildings would still be regulated, the new flexible system would allow considerably higher rental prices and must thus be seen as a step towards more liberalisation. In Section 4, I address this relaxation of the treatment by restricting the data I use for the estimation to the years 1981 and 1991 only, so the 1994 amendment to the tenancy law is not covered anymore.

3 Data and Empirical Strategy

3.1 Data Provenance

My primary data source is the Buildings and Dwellings Census (*Gebäude- und Wohnungszählung*, hence abbreviated *GWZ*), which was carried out nationwide by Statistics Austria since 1951 in ten year-intervals before being replaced by a register-based method in 2011. Information had to be given not only at the building-level but also at the level of dwelling units, which I am interested in, particularly on the following characteristics: the construction period, whether the object is registered as a main residence, the category of the object, who uses the object (house owner, flat owner, renter, used as a service accommodation), the size of the flat and the proprietor of the object (a single private person, multiple private persons, the federal government, state government, municipality, non-profit construction associations), and the geographical location of the object.

Unfortunately, it was impossible to have a representative longitudinal micro-sample of this survey, and I am thus forced to work with aggregated data. Via the online-database of Statistics Austria, I retrieve data from the 1971, 1981, 1991 and 2001 GWZ on the numbers of main residence flats, non-main residence flats, flats of each of the categories and number of flats used by each of the above mentioned users. These numbers are aggregated by all construction period/owner/district combinations. The relevant construction year periods are "before 1919", "1919 to 1944", "1945 to 1960" and "1961 to 1980". I will explain below why in the end, I will only use the two periods that are just around the threshold of 1945, "1919 to 1945" and "1945 to 1960". The 125 political districts and statutory cities constitute the finest geographical level that data can be aggregated at in the online database. For illustration: The data retrieved in this format provides e.g. the number of dwelling units of category A in the district of Amstetten that were built between 1919 and 1944 and are owned by the municipality in the 1971.

As an additional source for demographic control variables, I use data from the censuses that were carried out simultaneously with the Buildings and Dwellings Censuses (*Volkszählung*, hence abbreviated *VZ*). On district level, I thus have the number of: total residents, residents by citizenship (Austrian, EU, Turkish, successor states of former Yugoslavia), residents by educational attainment (primary, secondary, tertiary, university), married residents, residents by professional status (self employed, clerk, not working), residents by age groups (children, young adults, adults, seniors) and the number of people commuting to this district. I had to drop the number of residents of Yugoslavian descent again, as this variable was not available anymore in the 2001 survey.

Note that as I can retrieve all of these variables at the district level only, I need to cluster standard errors at this level later in the regression models. Also, many, if not all of these variables, but also the GWZ variables, could potentially be *bad controls*, in the sense that they are themselves not independent of the output variable, i.e. there might be some mutual causation. E.g. I would expect the number of commuters to a district to decrease the vacancy rate, as some of these commuters would demand accommodation nearer to their work place, making this an interesting control variable. However, at the same time, a low vacancy rate could also be the sign of excess demand, forcing some people to commute as they do not find accommodation. This potential problem should be taken in mind when interpreting the results of the models that include the control variables.

3.2 Identification Strategy

In order to exploit the 1982 policy break in the form of a natural experiment, I will use the subset of those flats in buildings with more than two flats and identify those buildings built between 1919 and 1944 as the treatment group, and those built between 1945 and 1960 as the control group. This particular strategy has several reasons: First of all, I exclude all flats in buildings with more than two units from my data (i.e. I do not count them in my aggregated numbers), as units of categories A to C in these buildings would actually be entirely excepted from regulation before and after the break no matter when they were constructed, meaning they should be part of the control group. At the same time, those of category D would be subject to the price ceiling for substandard objects before the break, thus switching from being treated to not being treated (see Section 2). This would not only make them hard to handle in terms of properly assigning them to the groups, but it also implies some methodological issues, as the category (and also the substandard status) is not fixed, but can rather be altered by renovation by the landlord. I would consequently expect some systematic selection away from category D before the break, but not after. Apart from that, one can argue that buildings with one or two units differ significantly from bigger apartment buildings, they are less likely to be found in urban areas etc., so excluding them has the pleasant side effect of making my sample more homogeneous.

Similar problems also occur in the subsample of flats in buildings with more than two flats: Firstly, as stated in Section 2, new buildings of category D would be subject to price regulation before the break but would not be regulated afterwards. Table 2 shows that by 1981 5% of new buildings would in fact be classified as D (corresponding to the sum of the columns D and D')[2]. Also, high quality old buildings (category A and >90 m^2 and category B and >130 m^2) would be excepted from regulation, which was true for 3.5% of flats in old buildings with more than two flats in 1981. Thirdly, I had concluded in the last section, that low category old buildings would not change treatment status as the previous price ceiling for substandard objects was comparable to the price ceiling for category D after the break. Table 2 shows, that this excludes another 12.6% (in 1981) from the treatment group.

I thus end up identifying buildings as being part of the treatment and the control group respectively only by their year of construction, i.e. old vs. new buildings. Specifically, the two construction periods that I am going to use are those just around the threshold, namely built 1919 to 1944 and 1945 to 1960. I also made attempts using all old buildings and all new buildings, but I ended up deciding against doing so for two reasons: First of all, the construction period following 1945 to 1960 is already 1961 to 1980. Obviously, this means that, even if I were to run my model only on the 1981 and 1991 GWZ, I would observe some buildings in the control group right after their construction, not to mention if I use the 1971 GWZ as well. As I expect a dwelling units to be more likely to be vacant just after it was built, this means that the last construction period I

[2]Note: Categories as reported by statistics Austria. D and D' (together substandard dwelling units) are those categorized as D since 1981 but they are still reported separately by Statistics Austria to allow comparisons with the time before, when flats with only running water (column D) and flats with neither running water nor a toilet (column D') were counted separately.

Table 2: The Distribution of Categories over Time (in percent of row totals)

Year	Built	A	B	C	D	D'
1971	1919 - 1944	0.04	0.33	0.48	0.06	0.08
	1945 - 1960	0.07	0.76	0.09	0.03	0.05
1981	1919 - 1944	0.14	0.43	0.30	0.07	0.05
	1945 - 1960	0.23	0.65	0.07	0.03	0.02
1991	1919 - 1944	0.44	0.29	0.18	0.06	0.03
	1945 - 1960	0.59	0.32	0.05	0.02	0.02
2001	1919 - 1944	0.75	0.18	0.05	0.02	0.00
	1945 - 1960	0.80	0.18	0.01	0.01	0.00

Note: This table is only valid for the subset of flats in buildings with more than two flats.

can reasonably use is from 1945 to 1960. Secondly, the difference-in-differences approach is based on the concept of a pseudo *experiment*, where the assignment to the treatment is as good as random or at least not correlated to the supposed treatment effect to be tested, so groups need to be as comparable as possible to support this assumption. Of course, I assume buildings to be more similar if they were built closely together in time.

Table 3 shows a comparison of the percentage shares of my GWZ variables between the treatment and the control group in the 1981 GWZ.[3]. Note that these are actual percentages for the whole country, not only the mean of shares of all districts. N, the number of observations, refers to the actual number of flats, not independent observations (groups) in my data set. One can see, that even when using the two groups right next to each other, there are substantial and statistically significant differences, which means that even when only considering this subset of buildings in my investigation, the central assumption of the difference-in-differences approach, namely the pseudo random assignment to the treatment is not entirely met. On the other hand, this result is not so surprising after all,

[3]Note that I cannot provide a balance table-like comparison of the VZ variables, as I only have them at the district level, not by construction year

considering the historical background of the construction period 1919 to 1944 - World War II and the corresponding political and economic upheavals that is not comparable to the second construction period's. Also, although the groups are right next to each other, 42 years separate the earliest buildings in the first group and the latest buildings in the second group. Significant differences between the groups with regard to almost all measured characteristics are thus a caveat, in the light of which the results of this paper must be seen.

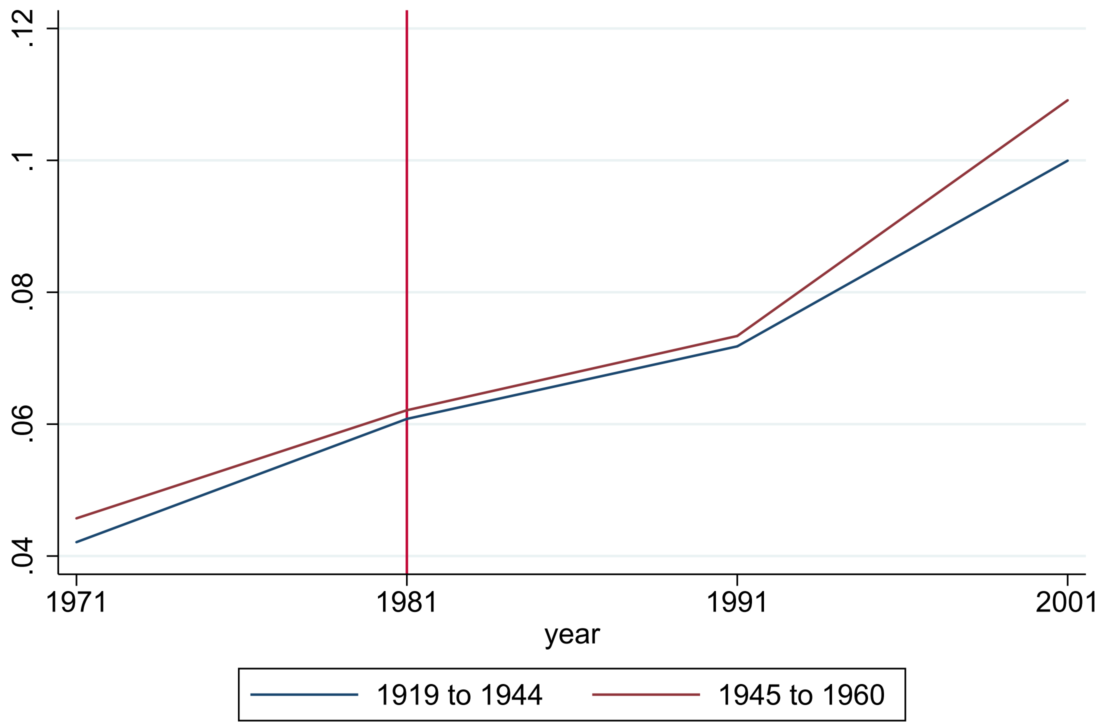
Table 3: Balance Table of GWZ Variables in 1981 (Shares in %)

	all	1919 to 1944	1945 to 1960	difference	p - value
vac	10.5	6.1	6.2	0.1	.005
A	37.5	14.1	23.2	9.1	.000
B	35.3	43.4	65.1	21.7	.000
C	9.7	29.9	7.2	22.7	.000
D	10.7	7.3	2.7	4.6	.000
D'	6.9	5.3	1.7	3.6	.000
flatowner	38	3.9	3.2	007	.320
houseowner	16.5	0.8	15.5	14.7	.000
rented	67.6	82.7	69.3	13.4	.355
service	5.6	6.1	6.3	0.2	.000
other	6.4	6.4	5.7	0.7	.756
selfused	20.3	4.8	18.7	13.9	.000
N	1,572,978	175,327	223,175		

Note: This table is only valid for the subset of flats in buildings with more than two flats. The p-value reported refers to the t-statistic of the coefficient of the group indicator in a linear regression of the variable at question on the group indicator, using frequency weights and clustering as in the main model introduced in Section 4.

My main variable of interest is the vacancy rate. However, it turns out that there is neither an official definition of when a dwelling unit is considered vacant, nor are there any official statistics of it for Austria - vacancy is also not a variable in the Buildings and Dwellings Census. The variable that I use to proxy vacancies, is whether a dwelling unit is registered as a main residence or not. Besides the

Figure 2: Trends of Vacancy Rates over Time



practical necessity for this step, the economic argument behind it is that theoretically, every dwelling unit not used as the main residence by its owner or renter would eventually be supplied to the housing market if rents were high enough. E.g.: Let us consider somebody who owns two flats and uses one of them as a vacation home, i.e. he has no urgent need for this second home, but it is also not vacant in the most narrow definition of the term (it is not entirely unused). If this person could only expect to be able to charge a high enough rent for one of this flats, he would eventually decide to rent it out. The only case in which I consider this argumentation as not valid are main residences, as it would be absurd that somebody would choose to live on the street because he can rent out his only flat profitably. Consequently, whenever I talk about the vacancy rate or a vacant dwelling unit referring to data from the Buildings and Dwellings Census, ”vacant” must always be interpreted as ”not registered as main residence”.

Figure 2 shows the development of the vacancy rates for the two construction year periods used as control and treatment group. Remember that the new tenancy law came into force on January 1st 1982, so the latest Buildings and Dwellings Census that I consider pre-treatment is from 1981, which is marked with a vertical red line in the graph. What I expected to see was, first of all, that vacancy rates would be more or less parallel between 1971 and 1981 and between 1991 and 2001, which would follow from the assumption that treatment and control group do not differ significantly statistically apart from the treatment. The graph shows that this expectation is more or less met; the trends converge slightly between the first two censuses and diverge a little more strongly between the last two censuses. However, one could argue that the four points in time I am restricted to are too few to reliably check whether the two time trends behave comparably, but they surely do not differ too strongly considering the steady common upwards trend that both share. However, as can be seen, the time trends are perfectly parallel between the 1981 and the 1991 Buildings and Dwellings Census. This is quite a surprise: In other words, this means that the category rental price ceilings for (most) dwelling units in old buildings since 1982 did not have any measurable effect on the nationwide vacancy rate - provided my assumptions are met, in particular the comparability of the groups in terms of common time trends and no systematic shocks on them other than the policy break and the validity of the interpretation of the term "vacant" as "non main residence". I will further check this preliminary result by applying a reduced form model on my data in the next section and elaborate on the shortcomings of the just described empirical strategy in Section 5.

4 Model and Estimation Results

4.1 The Model

As being vacant is a binary random variable, I want to apply a logistic regression, which requires that I transform the data. I thus duplicate every row in my data set (each representing the counts for my output and control variables for one group i.e. one survey year/district/construction year period/owner combination). I then create an indicator variable for vacancies, set it to 0 in the first of these duplicated rows and to 1 in the second, and consequently generate a *weight* variable equal to the number of non-vacant (aka main residence) dwelling units in this groups for the row where the vacant indicator is zero, and equal to the number of vacant dwelling units in the rows where the vacant indicator is one. Using *weights* as frequency weights, this data transformation enables me to run a logistic regression with the vacant indicator as the dependent variable, identical to when I would have actually multiplied each row in the original dataset with the number of individual dwelling units it represents (see Sribney).

The resulting (most expanded version of the) regression formula is then:

$$vac_{i,t} = \beta_0 + \beta_1 time_t \times old_i + \beta_2 time_{\mathrm{t}} + \beta_3 old_i + \beta_4 GWZ_{i,t} + \beta_5 VZ_{i,t} + \\ + \beta_6 year_t + \beta_7 group_i + \varepsilon_{\mathrm{i,t}} \qquad (1)$$

Where *time* is an indicator for post-policy break observations, i.e. from the 1991 and 2001 census and *old* is an indicator for old buildings, i.e. 1 if construction year period is 1919 to 1945, 0 if 1945 to 1960. The interaction of the two thus provides my measure of the treatment effect. *GWZ* is a matrix with the control variables from the Buildings and Dwellings Census, *VZ* contains the demographic control variables from the normal censuses. Note that I dropped the variables *used by house owner* and *used by flat owner* from the *GWZ* matrix, as these two measures depend highly on who the owner actually is: e.g. in the case of dwelling units owned by a legal person or the municipality, it is very unlikely that the flat is used by its owner, and if so, it would not be used as an accommodation. I also experimented with including the percentage change in number of inhabitants, as I would expect population growth to exercise some pressure on the housing market, but I then decided against doing so as this would cost me too much of my data (one fourth given only four separate survey years).

In order to guarantee comparability between the groups and the districts, I transformed all the variables into percentage shares of total buildings for GWZ variables and shares of total residents for VZ variables. As none of these variables are on the individual flat level, they must not be understood as actual controls for the characteristics of the single flat, but rather as controls for its environment in terms of the characteristics of other flats in the same group (GWZ variables) and of the demographic situation in the district of the building (VZ variables) respectively. *year* and *group* are year and group fixed effects. Note that due to only four realizations of the *year* variable, and the simultaneous inclusion of the *time* indicator into the formula, there are some collinearity issues, which do not bother me as long as I successfully control for year fixed effects.

I also attempted to run a simple OLS regression on the group level, converting not only the control variables to percentages as above, but also the dependent variable. Results did not differ substantially, and I decided to further focus on the weighted logistic regression model as it makes more sense intuitively, given the binary nature of the left-hand-side variable and the fact that districts differ remarkable in number of dwelling units, which I would not account for when I restrict myself to using percentage shares. The results of these estimations can be found in the appendix.

4.2 Estimation Results

The results of the different specifications I considered can be seen in Table 4. Note that all tables showing regression outputs report the raw coefficients. As these are logistic regressions, coefficients cannot be directly interpreted as marginal effects, but would rather need to be exponentiated in order to be interpretable as odds ratios. Raw coefficients nevertheless suffice to see the direction of the effect, as positive/negative exponents correspond to odds ratios greater/smaller than one. The first column shows a baseline model of the difference-in-difference approach in its most parsimonious form, including only the interaction term of interest and a constant; the variables *treat* and *time* control for differences between the two groups and between the two treatment periods. The effect is insignificant, which is not a surprise at all given the time trends in Figure 2. The next logical step is to include year fixed effects in column two, as the implicit assumption in the model in column 1 is that there are no time trends in the vacancy rates within the treatment periods, which I know is not true from Figure 2. Including year-fixed effects does not have so much of an impact, however, probably because there are only four years in total, and *time* controls for a good proportion of the variation already.

Table 4: Results: Frequency-Weighted Logistic Regression

	(1) total	(2) total	(3) total	(4) total	(5) total	(6) private	(7) public
treat×time	-0.020	-0.017	0.119	0.119^{*}	0.083	-0.020	0.048
	(0.035)	(0.035)	(0.069)	(0.058)	(0.051)	(0.051)	(0.073)
treat	-0.050	-0.049	0.200	0.063	-0.542^{**}	-0.750^{***}	-0.431
	(0.049)	(0.050)	(0.112)	(0.079)	(0.207)	(0.190)	(0.441)
time	0.568^{***}	0.941^{***}	0.687^{**}	1.080^{**}	-0.071	-0.231	0.542
	(0.035)	(0.047)	(0.217)	(0.384)	(0.538)	(0.475)	(1.079)
Year-FE	NO	YES	YES	YES	YES	YES	YES
GWZ-Controls	NO	NO	YES	YES	YES	YES	YES
VZ-Controls	NO	NO	NO	YES	YES	YES	YES
Group-FE	NO	NO	NO	NO	YES	YES	YES
Constant	-2.864^{***}	-3.054^{***}	0.712	-26.48	-17.56	30.332^{*}	-72.840^{**}
	(0.043)	(0.046)	(0.573)	(18.31)	(14.40)	(13.345)	(27.794)
N	1,614,684	1,614,684	1,614,684	1,590,444	1,590,397	566,470	641,495

Standard errors in parentheses. Standard errors were clustered at the group-level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Including the GWZ controls in column 3 leads to a positive coefficient of interest. Remember that the GWZ controls consist in the shares of dwelling units in the same district/construction year period group in each of the categories and by user type. On one hand, it intuitively makes sense that the quality of a dwelling unit is highly associated with its probability to be vacant, on the other hand, the already mentioned caveats must be kept in mind - categories are not stable, and there might even be some systematic selection into higher categories by landlords in the treatment group in order to escape the rental price regulation. But even assuming systematic selection is not a problem, reverse causality might still be an issue: one can argue that if the flat owner uses his flat himself, there are incentives to increase the category (the same is true if the tenant pressures the landlord to renovate the flat). As this is not the case for vacant flats, I expect this mechanism to bias the estimate downwards, as some of the variation caused by the interaction term is wrongly attributed to the share of low category dwelling units. It remains uncertain, to which degree this bad control problem matters, but it must be kept in mind.

Interestingly, including the VZ controls keeps the coefficient robust, but makes it significant at the 5% level (although the actual gain in precision is not substantial, as the standard error reveals). Note also that the number of observations remains fairly constant, which was a concern given that the VZ adds a lot of extra variables with some missing values, but this does not seem to be a big problem. However, including also group fixed effects in column 5 decreases the coefficient estimate and makes it insignificant again.

Columns 6 and 7 provide the results for privately (one or more private persons or a legal person) and publicly (the federal government, the state, the municipality or other public owners) owned buildings respectively. Apart from both coefficient estimates being statistically not significantly different from zero, the estimated treatment effect for privately owned dwelling units is negative and even lower than the effect on publicly owned dwelling units, which is insignificant but positive. This is rather counter-intuitive, as one of my hypotheses was that if there is an effect, it should be more prominent among privately owned dwelling units, as I consider private owners to have more alternative uses to renting a dwelling unit out than public owners. Results are thus not only statistically insignificant, but the point estimates for the two subsamples are also not of the expected direction.

Table 5 sheds further light on this issue, providing the regression results for the

Table 5: Results by Owner Type

	(1) one priv	(2) many priv	(3) char	(4) oth leg	(5) fed	(6) state	(7) muni
treat×time	-0.142**	0.070	0.134	0.320**	0.118	-0.497	0.055
	(0.052)	(0.071)	(0.082)	(0.110)	(0.194)	(0.355)	(0.092)
treat	-0.674**	-0.914***	0.804**	-2.245***	0.364	-3.635*	-0.743
	(0.216)	(0.262)	(0.251)	(0.430)	(0.674)	(1.617)	(0.432)
time	-0.719	0.161	-0.567	-0.136	0.914	3.160	0.549
	(0.629)	(0.354)	(1.201)	(1.050)	(2.103)	(4.639)	(1.210)
Version	FULL	FULL	FULL	FULL	FULL	FULL	FULL
Constant	37.851	33.080*	-19.839	3.028	-92.086	-260.910	-62.004*
	(19.437)	(15.835)	(27.994)	(48.740)	(76.498)	(168.583)	(31.534)
N	237,173	240,030	372,701	89,022	34,907	5,933	590,510

Standard errors in parentheses. Standard errors were clustered at the group-level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

full specification by owner type at an even lower level. As can be seen in the first row, strikingly, the coefficient of interest is significant at the 1% level but negative for dwelling units owned by single private persons, which is the exact opposite to what I would have expected. In fact, the whole story of a decision maker deciding whether to rent out a dwelling unit or not which basically constitutes the theoretical framework that underlies my investigation works best for this group. Single private persons owning other flats than the one they registered as their main residence actually are the owner type with the highest incentives to use the flat otherwise, if renting it out is not profitable enough. I imagine not only public owners, but also legal persons or even many private persons who jointly own a flat to be much less likely to find alternatives to renting out an object they own, thus lowering their opportunity costs of renting it out. However, column 4 in Table 5 shows that the estimated effect for dwelling units owned by other legal persons is significant and positive. Note that this owner type is called "other" legal persons because the owner type "charitable building association" also consists of legal persons, abbreviated ”char” in column 3. Columns 5 to 7 show that for government-owned dwelling units (federal government, state, municipality) the estimated coefficients are not significant, which is consistent with what I expected.

As a preliminary conclusion, I find that results are not in accordance with the expectations I held in the light of the underlying theoretical framework. To briefly summarize, the take-away from these first two regression outputs is that I do not find any reliable evidence that would support my hypothesis that the introduction of the new tenancy law in 1982 should lead to an increase in the probability of vacancy of old dwelling units. The coefficient of interest is significant and of the expected sign in only one of the specifications I tested, and insignificant in all others. In addition, the secondary hypothesis that dwelling units owned by private persons, especially single individuals, should show the strongest reaction, is also not in accordance with the empirical findings: admittedly, a significantly positive effect can be observed for dwelling units owned by non-charitable legal persons, but dwelling units owned by singly private persons even show a highly significant negative treatment effect.

As I mentioned above in Section 2 (see also Figure 1), there were some changes in tenancy law other than the new tenancy law of 1982, that might have affected buildings from before 1945 and buildings from after that threshold differently. In particular, this might be the case for the rental price ceiling for substandard dwelling units introduced in 1974, which implicitly had a stronger impact on the later treatment group as the percentage of substandard dwelling units was higher within old buildings, and the replacement of the category rental price system with a benchmark system in 1993, which was not only more flexible, but implemented considerably higher rental price ceilings than the category system did. Of course, I expect this relaxation of the regulatory mechanism to reduce any shift in the relative probability of vacancy between the groups that allegedly happened in 1982 due to the treatment, as the treatment becomes less restrictive and old and new dwelling units become more similar in terms of the rents that can potentially be charged for them. I take a closer look on these two ”secondary” policy breaks by framing each of them in a separate difference-in-differences model: As is reported in Table 6 in the first three columns, I run exactly the same specification as above again, but I redefine *time* to identify post-1971 observations, hereby basically estimating the relative change in probability between the groups in that time period, which I would attribute to the introduction of the rental price ceiling since 1974. For this purpose, I of course restricted the sample to only the Buildings and Dwellings Censuses of 1971 and 1981 in order to prevent potential divergences or convergences of the trends after 1981 to interfere with my measurement of the happenings between 1971 and 1981. Analogously, the last three columns assume the policy break to be between 1981 and 1991 and use only data from these two years, which is meant to capture the effect of the 1993 amendment to tenancy law. Columns 4 to 6 provide the estimates of the effect of the policy break between 1981 and 1991 just like the main models above, but using the data from 1981 and 1991 only. As can be seen, estimates are statistically not significant, apart from the coefficient for publicly owned dwelling units between 1981 and 1991. Whereas I do not have any plausible explanation for this single significantly positive coefficient, at least the coefficients for the "placebo breaks" between 1971 and 1981 and 1991 and 2001 are not significant. Had they in fact been significant, indicating a reaction to policy changes before and after the actual treatment (assuming that the trends of the groups' vacancy rates are comparable otherwise), it might have been the case that in the main regressions above, these other reactions would interfere with the effect of the actual treatment effect, the new tenancy law of 1982 and compensate it. In other words, the findings of Table 6 indicate that I do not have to worry about any issues of this kind. Apart from that, had I failed to measure the treatment effect of the new tenancy law in the main model due to the secondary policy breaks before and after, I should at least be able to measure it in the regression using only the 1981 and 1991 data, but also here, my results are not in accordance with the hypotheses.

At last, I must therefore conclude that my empirical findings do not support the theoretical predictions regarding a reduction of the supply. The rest of the thesis is dedicated to pointing out the potential shortfalls that might have prevented the difference-in-differences approach from working as it should and a number of caveats that I cannot refute when using the 1982 break in tenancy law as a natural experiment.

Table 6: Alterntive Breaks and Smaller Range

	1971			1981			1991		
	total	private	public	total	private	public	total	private	public
treat×time	0.025	-0.027	-0.002	0.010	0.047	0.258*	0.088	-0.032	0.045
	(0.062)	(0.058)	(0.140)	(0.053)	(0.065)	(0.110)	(0.066)	(0.059)	(0.112)
treat	-1.079**	-0.435	1.089	-0.726	-0.626	0.439	-0.825*	-0.929**	-1.294*
	(0.345)	(0.381)	(0.877)	(0.417)	(0.478)	(1.076)	(0.365)	(0.299)	(0.643)
time	0.285	-0.203	2.105**	-0.622	-0.878**	0.552	0.489	0.752*	-0.952
	(0.377)	(0.368)	(0.754)	(0.331)	(0.330)	(0.786)	(0.386)	(0.356)	(0.611)
Specification	FULL	FULL	FULL	FULL	FULL	FULL	FULL	FULL	FULL
Constant	7.202	-8.555	-8.910	-7.770	-6.587	-39.881	37.601	59.604**	31.357
	(10.067)	(11.173)	(18.957)	(9.760)	(9.111)	(20.407)	(20.805)	(18.454)	(35.918)
[1em] N	773,385	255,774	315,661	801,897	269,840	334,638	817,012	310,636	325,209

Standard errors in parentheses. Standard errors are clustered at the group-level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

5 Discussion of Shortfalls

5.1 Too Small Share of Dwelling Units Affected

One consideration which I entirely left out so far, is that my investigation refers solely to the rental price regulations for new tenancy agreements, whereas my data does not have any such focus: First of all, my data contains all dwelling units in Austria, irrespectively of whether they are currently rented out or have even ever been intended to be rented out by their owner. Dwelling units used by their owner (house owner or flat owner) make up 12.5% of the dwelling units in my cleansed final data set (i.e. only containing buildings with more than two units and only the two construction year periods 1919 to 1944 and 1945 to 1960), which is a much lower fraction than within all buildings (probably driven by single-family houses). Note further that I cannot simply exclude this subset of dwelling units that are not really affected by tenancy law, as not being rented out is the potential reaction of the treatment, and there is no way of distinguishing between "dwelling units which are not rented out anyway" and "dwelling units not rented out due to too low allowed rental price". But even among the dwelling units that would potentially be supplied on the housing market in the form of being rented out, I have the problem that the housing market is extremely slowly adapting, as rental agreements can potentially last for decades, especially considering corresponding laws preventing unwarranted eviction. E.g., in 2016, there were 1,608,000 active rental contracts, (684,000 with non-public landlords) as compared to approximately 183,000 new rental contracts were made in Austria (Tockner, 2017), which amounts to a turnover rate of just about 11.4%. Considering that this turnover rate is driven by a subset of the dwelling units I observe, namely those rented out via temporary rental contracts, the probability of actually being subject to a new contract and therefore the new legal situation further decreases. There might thus simply be a too small, not properly measurable *average* treatment effect, as only a small fraction of the dwelling units addressed by the category rental price system is actually in the situation where the treatment matters.

5.2 Flaws in the Natural Experiment

5.2.1 Inaccurate Identification of Treated Units

As explained in detail in Section 2 and Section 3, I knowingly fail to assign some dwelling units accurately to their actual treatment status: 12.6 % of dwelling units in buildings built between 1919 and 1944 would be classified as substandard, thus being subject to a rental price ceiling already before the policy break and assign them to the control group, as their treatment status does not change with the new tenancy law. Ceteris paribus, this would lead to an underestimation of the alleged treatment effect, as the part of old buildings that does not actually face any change in treatment status would bias the effect downwards. Similarly, 5% of new buildings would be classified as substandard, and would be subject to a rental price ceiling from 1974 to 1981 whereas being subject to market prices after 1982, meaning a transition from being treated to not being treated. This would be associated with an upwards bias of the treatment effect, since part of the control group shows a relaxation in the rental price regulation, causing their vacancy rate to decrease (given there is an actual treatment effect of the expected sign which I only fail to measure). Also, at least 3.5% of old buildings would not be subject to the category rental price system as the were of too high quality in terms of category and size, so they should actually be assigned to the control group. Analogously to the argumentation above, this would bias any treatment effect downwards.

5.2.2 Systematic Selection Into Higher Categories

Moreover, the exception of old dwelling units from the category rental price system under the condition that a major renovation was carried out that increased the category of the dwelling unit allowed landlords to escape the treatment. I can consequently not exclude systematic selection away from the treatment. Albeit this incentive to renovate their object was there for landlords who would have rented it out anyway as well as for owners who would have decided to use their dwelling unit otherwise (thus leave it vacant according to my definition), I expect this possibility of escaping the treatment to bias the estimated effect downwards: firstly, even assuming there is no systematic selection happening, but landlords rather renovate their dwelling units irrespectively of the legal consequences, falsely identifying units as treated although they really are not would decrease the average treatment effect as can be argued analogously to above. Even worse, assuming that systematically, those landlords who would have decided to leave their dwelling unit rather vacant than renting it out at a too low rental price in a counterfactual scenario (i.e. those who would have actually reacted to the treatment) are more likely to take advantage of this escape option, the treatment effect would be reduced even further. Of course, it is not possible to test this shortfall; it is clear from Table 2 in Section 3 that renovations happened, but I can only speculate on whether some of them were actually systematic in the way just described.

5.2.3 Story of Sharp Policy Break is not True

Another caveat that I already mentioned in Section 2 is that the policy break was not as sharp as would have been ideal for the difference-in-differences approach to work perfectly. The first draft for the new tenancy law was already published in summer 1979 - two and a half years before it actually came into force. Two and a half years might not be an eternity in the context of housing markets, but it might still have lead to anticipation effects. E.g., a landlord of an old building who's tenant just moved out in 1979 might have decided not to rent out the flat again, because he already expected that he would be better of using it himself after the already announced policy break in 1982. In my estimation, I would fail to attribute this reaction to the treatment, because it did not happen between 1981 and 1991 where I expect it. Indeed, Figure 2 in Section 3 shows that between 1971 and 1981, the vacancy rate of old buildings increases slightly relative to the vacancy rate of new buildings. However, the separate difference-in-differences-like estimation using the 1971 and 1981 data only in Section 4 in Table 6 does not confirm this observation. Should the anticipation of the treatment nevertheless lead to the described shift of the reaction in time, this would of course bias the treatment effect downwards, as I only measure the part of the landlord's reactions that happened after the policy break.

5.3 Non-Main Residence is a Doubtable Proxy for Vacancy

As already explained in Section 3, I am forced to use the information whether a dwelling unit is registered as a main residence or not as the proxy for vacancy. The story that I am thinking of, is the case of a landlord who owns more than one dwelling unit and has several options how to use them, e.g. as a secondary residence, as a vacation home and of course, renting it out. He would rent his dwelling unit out only if it is profitable (in which case it would most likely be registered as the main residence of the tenant), and for some landlords it becomes just not profitable anymore due to the regulations of the category rental price system, so they rather use it as their secondary residence or decide to keep it vacant to give it to their children in the future. Unfortunately, this ideal case is not the only reason why a person could decide to register a flat as a secondary residence: to think of just one possible story, over the 40 years I observe some people who can afford it might have developed the preference for having a main residence in the country side for weekends and a secondary residence near the city center to live in during the week. Assuming additionally that buildings on the country side are more likely to be built before 1945 and buildings in the city center are more likely to be built after 1945, this would ceteris paribus result in an increase in the relative rate of vacancy aka not-registered as main residence - rate for new buildings.

It is easy to make up similar stories that might have an effect on the fraction of dwelling units registered as main residences in the two groups, together with assumptions of some correlation of the construction year of a building and its location. I consequently see no possibilities to refute this caveat.

5.4 Changes in Total Number of Flats in Groups

A last, one very basic concern again refers to systematic selection out of the treatment group: One can argue that vacant buildings are more likely to be taken down. If now a policy leads to an increase in vacancies among old buildings, these vacant old buildings would simply be taken down with a higher probability than non-vacant old buildings, and the change in the probability of being vacant cannot be accurately measured anymore from the data.

However, empirically, I do not observe any decline in the number of dwelling units in my subsample of buildings containing more than two flats - in fact, I even observe a slight increase. The number of flats in buildings built between 1919 and 1945 increases from 175,895 in the 1971 Buildings and Dwellings Census to 177,971 in the 2001 census (+1.2%), the number of flats in buildings built between 1919 and 1944 increases from 221,865 to 232,508 in the same period (+4.8%) (See also Table 8 in the Appendix). This is a remarkable finding and seems rather counter-intuitive at first sight. However, it must be considered that these numbers count flats, not buildings, so if a new flat would be created in an already existing building, the construction year of building would be associated with this flat. Also, it is possible that large flats would be split up into two or more smaller ones, which not only by itself increase the number of flats, but would also allow some flats in buildings that before the split-up contained less than three flats would join my subsample. Yet, I do not consider this the main reason for the increase in numbers, as the average flats rather tended to become bigger (e.g. 16.4% of flats in old buildings and 12.9% in new buildings were below 35 m^2 in 1971 but only 7.6% and 8.0% respectively in 2001, whereas the share of flats of more than 130 m^2 would increase from 0.8% and 0.7% of old and new buildings to 1.5% and 1.4% respectively in the same period). Although special incentives to merge flats in old buildings were set by the new tenancy law, as this was considered a fundamental renovation and would thus allow the landlord to escape the rental price ceilings, most of the transition towards happened between 1971 and 1981 already, so it is doubtable to which degree it was driven by legal incentives that were only in force since 1982. See also Table 9 in the Appendix for all numbers regarding flat sizes.

The slight increase in the number of flats in both groups over the year must therefore most likely be attributed to the creation of new flats in already existing buildings, which, of course, again rises doubts regarding selection into the sample - one would assume that new flats are more likely to be generated when the future landlord can expect to rent it out profitably. It is furthermore not really possible to disentangle the different driving forces behind the changes in the number of flats in the groups and it remains uncertain, in which direction this could possible bias my estimation results.

6 Conclusion

In the current paper, I analysed the effect of a rental price ceiling on the supply of already existing dwelling units on the rented housing market, in other words, vacancies. To do so, I applied a difference-in-differences approach on a natural experiment in Austria, when the new tenancy law was introduced that involved a rental price ceiling system for dwelling units in buildings constructed before 1945, which therefore serve as the treatment group, whereas the rest of the dwelling units would be free of rental price regulation before and after, thus serve as the control group. Neoclassical theory yields that some potential landlords prefer alternative uses for their dwelling units when legally determined rents are below market equilibrium, and I thus expected to observe an increase in the probability of not being registered as a main residence (as a consequence of not rented out, i.e. vacant) of dwelling units in old buildings relative to the probability for new buildings. However, I did not find any convincing empirical evidence for this theoretical result, neither in the raw data nor in any of the econometric models I ran. Moreover, I expected privately owned dwelling units to show the clearest response, as I assume public owners not to act profit maximizing and having less alternative uses for the dwelling units they own. This expectation is also not confirmed by the data; the estimated treatment effect is insignificant for both publicly and privately owned dwellings and the effect estimated for dwelling units owned by single private persons is even estimated to be significantly negative in my preferred specification.

These rather unexpected results must be seen in the light of a number of caveats, most notably that the fraction of dwelling units actually treated is rather small as the treatment only applies to new tenancy contracts, that the assignment to the treatment and the control group is not perfectly accurate and even systematic selection might have occurred to an unknown degree, and strikingly, that there are other factors driving the share of non-main residences that might possibly be correlated with the buildings' construction period.

On the other hand, assuming that my results are indeed reliable, there are two possible explanations: firstly, the supply elasticity of rented housing for prices in the neighbourhood of the observed market rental prices and those determined by law in my natural experiment could be close to zero - a landlord's decision whether to supply a dwelling unit on the rented housing market or not would not be sensitive at all to governmental interventions in the form of a rental price ceiling. Secondly, the consideration that due to naturally occurring violations of the neoclassical model's assumptions, the housing market does not work as predicted, yields that prices in real-world unregulated markets could be above ideal markets' equilibrium prices. Even after real-world rental prices are reduced significantly by means of a rental price ceiling, they might thus still not be below those in the ideal market and efficiency is re-established rather than harmed. This would potentially even increase supply, which would be in line with the significantly negative (i.e. vacancy reducing) treatment effect for dwelling units owned by single private persons.

The policy implication I would then derive from my analysis would be consequently that the lawmaker needs not take into account any inefficiencies in terms of a reduction in the supply of already existing units on the rented housing market when imposing a rental price ceiling comparable to the category rental price system for distributional reasons or for reasons of social justice.

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Appendices

Table 7: Linear Model (Vacancy Rate in Percentages) - Results by Owner Type

	(1) onepriv	(2) manypriv	(3) char	(4) othleg	(5) fed	(6) state	(7) muni
treat×time	0.001	0.016	0.018	0.033*	0.004	-0.108	0.000
	(0.008)	(0.013)	(0.010)	(0.014)	(0.023)	(0.055)	(0.008)
Version	FULL	FULL	FULL	FULL	FULL	FULL	FULL
Constant	6.450*	5.893	-2.530	-9.725	-19.220	-41.021	-1.597
	(3.115)	(4.701)	(3.446)	(11.180)	(13.530)	(31.424)	(3.052)
N	894	693	704	810	545	204	813

Standard errors in parentheses. Standard errors were clustered at the group-level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8: The Number of Flats in Buildings Containing More Than Two Flats

	1971	1981	1991	2001
1919 to 1944	175,895	175,327	180,155	177,971
1945 to 1960	221,865	223,175	227,788	232,508

Table 9: Development of the Distribution of Flat Sizes Over the Years

Construction Period	Survey Year		below 35	35 to 45	45 to 60 (1971: 35 to 60)	60 to 90	90 to 130	above 130	row total
1919 to 1944	1971	sum	28,759	0	101,262	37,554	6,917	1,403	175,895
		share	0.16	0	0.58	0.21	0.04	0.01	1
	1981	sum	28,259	40,259	52,496	42,905	8,987	2,421	175,327
		share	0.16	0.23	0.3	0.24	0.05	0.01	1
	1991	sum	19,699	40,018	56,959	48,713	12,246	2,520	180,155
		share	0.11	0.22	0.32	0.27	0.07	0.01	1
	2001	sum	13,558	37,222	56,380	53,739	13,978	3,094	177,971
		share	0.08	0.21	0.32	0.3	0.08	0.02	1
1945 to 1960	1971	sum	28,556	0	107,208	75,388	9,197	1,516	221,865
		share	0.13	0	0.48	0.34	0.04	0.01	1
	1981	sum	30,197	29,028	72,987	77,294	11,035	2,634	223,175
		share	0.14	0.13	0.33	0.35	0.05	0.01	1
	1991	sum	22,532	29,169	78,237	81,410	13,837	2,603	227,788
		share	0.1	0.13	0.34	0.36	0.06	0.01	1
	2001	sum	18,534	28,599	79,297	86,530	16,291	3,257	232,508
		share	0.08	0.12	0.34	0.37	0.07	0.01	1