Unburden Renters by Making Landlords Pay the Commission?

Evaluating a Policy Reform in Germany

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Abstract

When renting an apartment, often a commission is payable by the renter. In a perfect market, standard economic reasoning predicts the rental price for an apartment with a commission payable by the renter to be lower than the price for the same apartment without commission. We test this hypothesis against an alternative hypothesis based on insights from behavioral economics about mental accounting, inattention, and cognitive limitations. We apply a difference-in-differences strategy exploiting a law reform in Germany in June 2015 determining that a landlord is no longer allowed to make a renter pay the commission for a real estate agent the landlord ordered. Using a panel dataset we thus compare price changes (pre–post reform) of apartments *without* a commission prior to the reform with price changes of apartments *with* a commission prior to the reform. Based on our results we reject the standard reasoning hypothesis of differing prices; moreover, we cannot reject the behavioral hypothesis of equal prices.

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1 Introduction

This paper investigates the question of whether behavioral bias affects prices of rental housing. Specifically, we ask the question whether subjects react to a lump-sum cost differently than to (the net present value of) a periodic cost. The lump-sum cost under consideration is a commission payable by renters of certain apartments at the time of concluding the rental housing contract; the periodic cost is the rent. When renting an apartment, in some but not all cases a commission is payable by the renter. Standard economic reasoning suggests that a renter's willingness to pay for the monthly rent should be lower if, in addition, she has to pay a commission at the time of signing the rental contract. This prediction is based on the assumption that renters are rational having a fixed willingness to pay for housing—including both the cost for a potential commission and the cost for the rent—and this willingness to pay does not depend on how the cost is divided between the lump-sum commission at the start of the contract and the net present value of the periodic rents. This means that the standard reasoning hypothesis assumes perfect transferability between the two sorts of costs. Based on behavioral insights, we doubt this perfect transferability. In this paper we test the null hypothesis of full transferability against the behavioral hypothesis of incomplete transferability. The latter hypothesis is based on three insights from the behavioral literature explained in the following.

The first behavioral concept is mental accounting, established by Richard Thaler (see Thaler, 1980, 1985, 1999). Empirical evidence suggests that people keep spending under control by forming multiple "mental accounts" (e.g., for food, housing, etc.) between which money is not (or not fully) transferable (see, e.g., Thaler, 1999). "Mental accounting violates the economics notion of fungibility" as "money in one mental account is not a perfect substitute for money in another account" (Thaler, 1999, p. 185). Mental accounting thus affects consumption choices. Almenberg and Karapetyan (2010) specifically show that mental accounting affects borrowing behavior in the housing selling market. Applied to the rental housing decision, we propose that people searching for an apartment to rent assign the cost for the commission to a different mental account (say for the account "moving expenses") than the cost for the monthly rent (say the account "regular rental expenses"). This is particularly likely as the two accounts have different time horizons: the first account encompasses one-time expenses for the time of moving, while the second account encompasses periodic (monthly) expenses. These different time horizons complicate translation between the accounts (Prelec and Loewenstein, 1998). If people do not transfer money between the two mental accounts, renters' willingness to pay for the rent is not lower in the case that the apartment is offered with a commission payable by the renter than in the case that the apartment is offered without a commission. If money is transferred partly but not fully between the two mental accounts, the willingness to pay for the rent of an apartment with a commission is lower than the willingness to pay for the rent of an apartment without

a commission but the reduction is smaller than predicted by the standard reasoning argument. The exact price reduction depends on the degree of transferability between the accounts.

The second insight that our behavioral hypothesis is based on is the different salience of different cost components and the associated level of attention paid by consumers. People looking for an apartment might focus on the rental price more than on side costs such as a commission payable at the time of contract conclusion. This means that the price for the commission is less salient than the rental price and apartment searchers thus pay more attention to the rental price than to a commission they are required to pay for some apartments but not for others.[1] When people look for an apartment, they often have some maximum rental price in mind they are willing to pay. But they rarely have two different maximal rental prices in mind, one that applies for apartments offered without a commission, and another (lower) maximum amount that applies for apartments offered with a commission payable by the renter. The phenomenon of different salience affecting consumer behavior has been found to affect online purchases, where consumers pay more attention to the item's direct price than to the shipping cost—even though the latter is directly related to the purchase (cf. Hossain and Morgan, 2006; Brown et al., 2010). Other work has shown that sales taxes can be less salient than an item's net price and that this importantly affects behavioral responses to taxation (Chetty et al., 2009). On the used car market buyers have been found to pay more attention to the first digit of the mileage of used vehicles than would be predicted by rational theory and this was also explained by the higher salience of the first digit compared to later digits (Lacetera et al., 2012; Busse et al., 2013). In the apartment rental decision, a lower salience of the commission compared to the rent would imply the following: if renters are completely inattentive to the commission, their willingness to pay for the rent for apartments with commission is the same as their willingness to pay for the rent for apartments without commission. If renters pay some but not full attention to the commission cost, the willingness to pay for apartments with commission would be lower than their willingness to pay for apartments without commission, but the reduction would be smaller than predicted by standard economic reasoning. The exact extent of the reduction would depend on the degree of attention paid to the commission cost (relative to the attention paid to rental prices).

A third behavioral argument why we hypothesize rents of apartments with commission not to be (much) lower than rents of apartments without commission is the cognitive load of translating between a lump-sum cost (the commission) and a regular cost (the monthly rent) and decision makers thus being prone to bounded rationality. Renters might be unable to calculate the monthly amount that is—given a certain expected rent duration—equivalent to the lump-

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[1]We do not argue that the amount of the commission is not transparent, it is fully transparent, but people might just pay less attention to it.

sum commission. Facing this cognitive load (some) renters might just neglect the commission cost in their apartment decision and the rental price difference between apartments with and without commission might be zero or at least smaller than predicted by standard reasoning.[2]

The strategy we apply to test the standard reasoning hypothesis (null hypothesis) against the behavioral hypothesis is explained in the following. Simply comparing the rental prices of apartments with commission to the rental prices of apartments without commission may lead to biased estimates because the commission status of an apartment is expected to be correlated with unobservable characteristics (quality) of the apartment. Apartments with commission are (mostly) advertised by real estate agents while most apartments without commission are advertised by private persons.[3] In order to estimate the true effect of a commission payable by renters on rental prices, we apply a difference-in-differences approach exploring a law reform in Germany that became effective in June 2015. The law reform consisted of introducing the principle "who orders pays" ("Bestellerprinzip") for services by real estate agents. It prescribes that the commission for real estate agents has to be paid by the person who ordered the agent—i.e., virtually always the landlord and never the renter.[4] Prior to the reform, when landlords ordered a real estate agent, in the vast majority of cases the commission was imposed on the renter and fixed to the maximal legal amount of 2.38 times the monthly rental price (i.e., twice the monthly rental price plus the VAT of 19%). After the reform, landlords that previously ordered real estate agents can either continue ordering agents and pay the commission themselves or stop ordering real estate agents.[5] Using panel data of advertised apartments before and after the reform, we compare changes in rental prices of apartments that were offered with a commission prior to the

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[2]Another, non-behavioral but rational reason for renters to account for commission costs differently than for the rental price could be employers paying for commission costs. This is relevant for the case of job-related moving when employers pay for moving costs. However, evaluating data from the representative household survey SOEP (German Socio-Economic Panel, data for years 1984-2013, version 30, SOEP, 2014, doi:10.5684/soep.v30, see also Wagner et al. (2007)), we find that only 15% of household moves in 2012/2013 were due to job-related reasons. Among these job-related moves, certainly not all employers pay for moving costs; and among those cases where employers pay for moving costs, many accord their employees fixed amounts independent of the actual costs. Hence, job-related moving is little reason for renters not to take into account the cost for commissions.

[3]Some apartments without commission payable by the renter are even advertised by real estate agents. In these cases, the landlords pay for the commission. However, these are exceptions. Further, some apartments with or without commission are offered by other commercial entities, such as housing construction enterprises. Even in these cases the fact of whether a commission is payable by renters is unlikely to be randomly distributed.

[4]Strictly speaking, the law reform determines that the real estate agent is allowed to rise money from the renter only in the case that the agent interfered with the landlord exclusively for the purpose of the contract with that single renter, cf. *Gesetz zur Dämpfung des Mietanstiegs auf angespannten Wohnungsmärkten und zur Stärkung des Bestellerprinzips bei der Wohnungsvermittlung (Mietrechtsnovellierungsgesetzt—MietNovG)*. This is a very unrealistic scenario. In fact, since the time of the reform, commissions have no longer been paid by renters (cf., e.g., Michaelis and von Wangenheim, 2016).

[5]Some people feared that after the reform real estate agents would try to raise money from renters by illegal ways like charging interested persons fees for viewing the apartment, for making the contract, or for administration in general. However, these practices are illegal and real estate agents risk losing their license and paying monetary fines. Furthermore, renters can claim the money back even three years later. Overall, the illegal fees seem to be rather low and exceptional, see, e.g., Kwasniewski (2016).

reform (treatment group) to changes in rental prices of apartments that were offered without a commission prior to the reform (control group). We test the hypothesis of the reform making the monthly rental prices of treated apartments increase by 1.98%. This price increase corresponds to what would be predicted by standard economic reasoning in the case that the expected rent duration is 10 years and the interest rate is zero (describing a very conservative scenario by assuming a long rent duration and a low interest rate).[6] In the presence of consumer myopia (i.e., discounting of future payments by a rate larger than zero—as we assume an interest rate of zero), the standard reasoning prediction would postulate an even higher price increase than 1.98%. We test the null hypothesis of an price increase of 1.98% (standard reasoning) against the alternative hypothesis predicting a lower or even zero price increase, i.e., the behavioral hypothesis.

Our results suggest that the null hypothesis can be rejected at all conventional levels. We find that mean expected rent duration would need to be at least 27 years (i.e., implausibly high) in order that the null hypothesis could not be rejected at the 5% level.

Previous evidence indicates that behavioral bias plays a role in the housing selling market: Genesove and Mayer (2001) found that loss aversion determines seller behavior in the housing market as owners subject to nominal losses set higher prices and incur a longer time on the market than other sellers. Bucchianeri and Minson (2013) report a positive relationship between listing prices and sale prices consistent with the literature on anchoring effects. Brunnermeier and Julliard (2008) found that money illusion explains a substantial part of the sharp run-ups and downturns in the housing market. Glaeser and Nathanson (2015) review rational and non-rational models that have been developed to explain housing bubbles. Salzman and Zwinkels (2013) give an overview how behavioral bias affect real estate finance and investment decisions. In contrast to the behavioral literature on housing selling markets, to the best of our knowledge, there is no attempt to investigate irrational behaviors of actors on the housing rental market. The

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[6]To the best of our knowledge, there is no credible statistic on the expected rent duration for Germany. The German Federal Statistical Office (Statistisches Bundesamt) does not even provide data on the mean rent duration—which, however, is a systematically different number: If, for example, mean rent duration was 10 years implying that there are renters with shorter durations and renters with longer durations, then the expected rent duration for an exposed offer is smaller than 10 years because short-duration renters go on the market more frequently than long-duration renters. According to a specialized survey about housing of the German Microcensus 2010 (Federal Statistical Office (Statistisches Bundesamt) 2012, Bauen und Wohnen Mikrozensus—Zusatzerhebung 2010, Bestand und Struktur der Wohneinheiten, Wohnsituation der Haushalte, Fachserie 5 Heft 1.), in the German western federal states, 19% of households live in their currently rented apartment for less than two years, more than 57% live in their currently rented apartment for less than eight years, and more than 79% live in their currently rented apartment for less than 20 years (cumulative probabilities). This suggests that there is considerable movement on the apartment rental market and since short-duration renters move into new apartments more frequently than long-duration renters, estimating the expected rent duration for a given apartment offer to 10 years is rather conservative. In the results section we provide evidence about the sensitivity of our results with respect to the mean expected rent duration.

previous literature in the field of housing rental markets focused on topics such as the development of rental prices over time or across regions (e.g., Himmelberg et al., 2005; Ambrose et al., 2015; Thomschke, 2015), the natural vacancy rate (e.g., Rosen and Smith, 1983), or the effect of government interventions like rent control (e.g., Arnott and Igarashia, 2000; Sims, 2007) and housing vouchers (e.g., Eriksen and Ross, 2015).

In many places, however, especially in large cities, the rental housing market plays an important role: In Germany, 48% of all households live in rented (instead of owned) homes. This rate is in the European Union with 30% and in the U.S. with 36% lower but still non-negligible.[7] Households renting an apartment in Germany spend on average around 28% of their available income on the housing rent. The share is much larger for low income households (close to 50% for households with a net income below 700 euros per month) than for high income households (around 10% for households with a net household income of 7,500 euros per month or above)[8] (Federal Statistical Office, 2016). In larger cities, these figures are likely to be even larger. Due to the large amount of money spent on housing rents, revealing behavioral biases in decisions of actors on the rental housing market is economically highly relevant.

To the best of our knowledge, there is no previous study investigating the effect of a commission payable by the renter on the rental price nor evaluating the real estate agent reform in Germany in general. In most countries, the real estate agent profession is regulated to some extent (CEPI, 2013). In the rental housing market of a number of countries the commission for real estate agents is paid by landlords (Belgium, U.K., Ireland, Netherlands, Norway, Switzerland), while in other countries, it is (partly) paid by renters (Austria, Finland, Denmark, France, Luxemburg, Italy, Sweden).The declared goal of the law reform in Germany was to implement the principle that the party ordering the agent has to pay for his service. The law reform thus intended to unburden renters, especially in large cities, where rents are high. If the willingness to pay of renters and thus the rental price would perfectly adjust as predicted by the standard economic reasoning, the law reform would not unburden renters and thus not reach the intended goal.

The paper is organized as follows: Section 2 describes the data and presents some descriptive analyses. Section 3 gives details about the empirical strategy: a difference-in-differences strategy with apartment-fixed-effects. Section 4 presents and discusses our results and robust-

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[7]Data from 2015 from Eurostat and U.S. Census Bureau, see http://www.tradingeconomics.com/european-union/home-ownership-rate.

[8]Rental costs here include net rents plus related costs such as waste water and garbage collection but do not include costs for heating and electricity. Data stem from Microzensus Zusatzerhebung 2010, for more details, see (Federal Statistical Office, 2016).

ness tests. Section 5 concludes.

2 Data and Descriptive Evidence

2.1 Data Description

The dataset used for our analysis was provided by the firm Immobilienscout24, which offers by far the largest online real estate marketplace for residential properties in Germany. The market share amounts to 63% with more than 11 million user visiting the webpage every month.[9] Our full dataset contains N=219,247 rental offers published on the Immobilienscout24 webpage between January 2012 and June 2016.[10] Certainly, the apartment offers contained in the dataset are not representative for Germany as there might be self-selection into using an electronic marketplace. Furthermore, due to cost and time constraints of immobilienscout24, the firm was willing to provide data only for two cities, which we chose to be Frankfurt and Stuttgart. We chose to focus on large cities instead of rural areas because more than 35% of the German population is currently living in cities (Eurostat, 2016). Also, renting instead of owning an apartment or house is much more common in cities than in rural areas. We specifically chose the cities of Frankfurt and Stuttgart because of the following reason: At the time when the law "who orders pays" became effective, another law affecting the rental housing market was decided on: the so-called "rent brake". However, the rent brake became effective only when local governments (governments of the federal states) decided to implement it. In the federal state of Hesse (where Frankfurt is located in), the rent brake became effective only on November 27, 2015, and in the federal state of Baden-Württemberg (where Stuttgart is located in), the rent brake became effective only on November 1, 2015. The time distance between the two reforms, the law "who

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[9]https://www.immobilienscout24.de/werbung/scout24media/plattformen/immobilienscout24.html

[10]This is the sample size we get after dropping some observations with extreme values that are most likely due to typos: We dropped observations with an apartment size of less than 10 sqm (128 cases; this is very careful as most of these observations have a 0 or 1 in the variable sqm; the 1-percentile of sqm is 20) or with an apartment size > 400 sqm (76 cases; again very careful as the 99-percentile is at 230). We further dropped observations with a price per sqm of less than 4 euros (another 2183 cases; the 1-percentile is at 4.11 euros) or more than 30 euros (another 551 cases; the 99-percentile is at 24 euros). We also dropped observations where the number of rooms was indicated to be smaller than 1 or larger than 15 (another 45 cases). Finally, we dropped observations where the average room size is less than 10 sqm (another 73 cases; these most likely contain typos in the number of rooms and/or sqm). Cleaning the data as described for some obvious typos or spammers is reasonable as most data fields are freely filled by the offerer and thus prone to typos and also because some spammers offer apartments with unrealistic features. Also, we dropped observations with missing values in our main variables rental price, post reform, commission, number of rooms, and square meters (another 1,665 cases) as these variables are used to construct the panel dataset (see Section 3.2) and to apply the diff-in-diffs strategy. The number of observations dropped due to cleaning and missing variables is very low compared to the overall sample (in total 4,721 out of originally 223,968 observations, i.e., around 2%) and it does not affect our results.

orders pays" and the rent brake, allows us to disentangle the two effects.[11]

The apartment price information included in the dataset is the monthly rental price excluding service charges. Monthly rental prices on immobilienscout24 are asking prices. We do not have explicit information about transaction prices; however, assuming that asking prices are equal (or very close) to transaction prices is plausible as negotiations about rents are very uncommon in Germany, especially in large cities. We further assume that renters are indifferent (apart from the financial burden) about whether a real estate is offered by a private person, by a real estate agent, or by any other commercial entity (e.g., a building company).

Table 1 reports summary statistics of all relevant variables for our dataset. Since most variables stem from non-mandatory fields, we face some item nonresponse, which can be seen from the last column of Table 1. The average rental price amounts to 997 euros. 95% of all observations are in the price range of 350 to 2,380 euros. Figure 1 (a) displays the distribution of monthly rental prices.[12] The histogram is bell-shaped and clearly skewed to the right. The average price per sqm amounts to 11.9 euros. 95% of all observations are in the price range of 6.67 to 18.49 euros per sqm. The distribution of monthly rental prices per sqm can be seen in Figure 1 (b). The distribution is nicely bell-shaped and close to being symmetric. 81.3% (18.7%) of the apartment offers were published before (after) the reform became effective at June 1, 2015. For more than half of all offers (54.7%) a commission had to be paid by renters; the percentage was 58.8% before the reform and 1.0% after the reform.[13] 18.6% of the apartments were offered privately, 56.8% by real estate agents, and 23.8% by other commercial entities (in particular, house constructing enterprises). 9.1% of all offers were potentially affected by the rent brake. The average apartment offered has 81.7 square meters and 2.67 rooms. 95% are in the range of 1 to 4.5 rooms and in the range of 30 to 158 sqm. The histograms in Figure 1 (c) and (d) display the distributions of the number of rooms and number of sqm. The distributions are bell-shaped and the means seem reasonable: The federal statistical office reports from its survey of 2011 on all apartments a mean number of rooms of 3.5 in Frankfurt and 3.8 in Stuttgart; the figures of the federal statistical office include the kitchen, while the numbers in our dataset exclude the

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[11]In other large cities in Germany, both reforms became effective simultaneously or shortly one after another: e.g., in Berlin the rent brake became effective already on June 1, in Hamburg on July 1, in Cologne (North Rhine-Westphalia) also on July 1, in Munich (Bavaria) on August 1. The law of the rent brake basically implies the following: In federal states where the rent brake is effective, rental prices in new contracts are not allowed to surmount 10% of the local average, which is calculated by local authorities. Exceptions are conceded to new buildings (built after 2014) and to some residential districts in Frankfurt (Berkersheim, Eckenheim, Harheim, and Unterliederbach). We construct a dummy variable capturing whether the rent brake affects the offer under consideration (i.e., this dummy variable depends on time of the offer, building year, and location) in order to control for the rent brake effect.
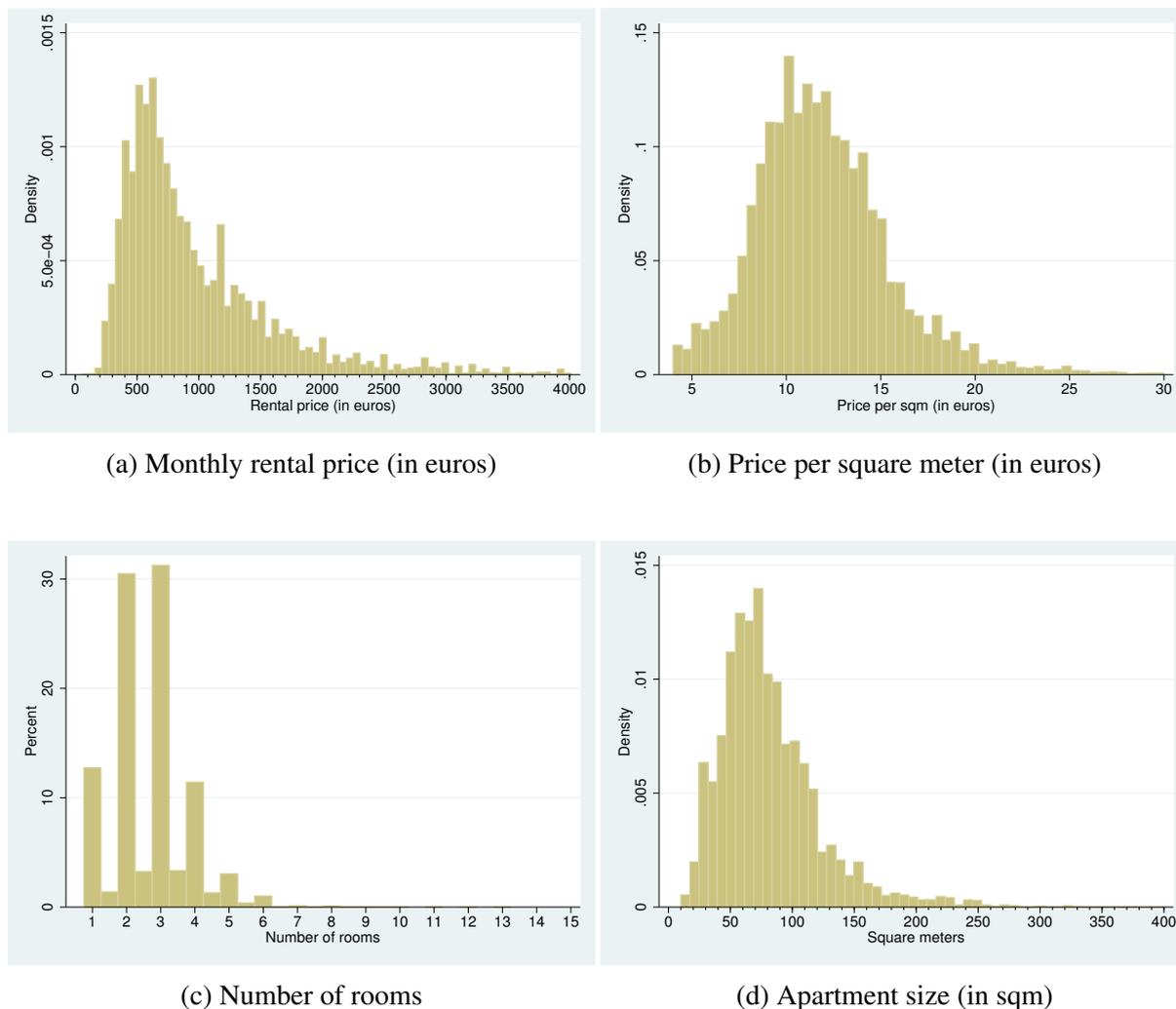
[12]For the figure, we cut at a price level of 4000.

[13]The 1% is either due to errors (e.g., people re-publishing an offer they already had published once before the reform and they now missed to delete the commission requirement) or due to attempts to ignore the reform.

Table 1: Summary statistics

Variable	Mean	SD	Min	Max	N
Main variables					
Rental price (in euros)	996.251	709.76	50	9000	219,247
Price per sqm (in euros)	11.903	3.586	4	30	219,247
Post reform	0.187	0.39	0	1	219,247
Commission	0.547	0.498	0	1	219,247
Number of rooms	2.671	1.099	1	13	219,247
Square meters	81.737	41.222	10	400	219,247
Further variables					
Frankfurt (vs Stuttgart)	0.687	0.464	0	1	219,247
Floor level	2.362	1.947	-1	24	176,418
Total number of floors in house	4.268	2.308	0	50	146,864
Balcony	0.683	0.465	0	1	199,132
Garden	0.173	0.378	0	1	171,310
Number of bathrooms	1.194	0.414	1	5	144,197
Guest bathroom	0.284	0.451	0	1	198,827
Elevator	0.368	0.482	0	1	189,346
Cellar	0.696	0.46	0	1	207,866
Built-in kitchen	0.711	0.453	0	1	194,016
Parking space	0.002	0.043	0	1	219,247
Inner-city	0.53	0.499	0	1	219,247
Rent brake	0.092	0.289	0	1	219,247
Private offer	0.187	0.39	0	1	214,580
Offer by REA	0.567	0.495	0	1	214,580
Offer by other commercial entity	0.238	0.426	0	1	219,247

kitchen. The mean apartment size has been found by the federal statistical office to be 72.9 in Frankfurt and 78.2 in Stuttgart (Federal Statistical Office, 2015).

(a) Monthly rental price (in euros)

(b) Price per square meter (in euros)

(c) Number of rooms

(d) Apartment size (in sqm)

Note: N = 219,247, data from Immobilienscout24, authors' calculations.

Figure 1: Distributions of apartment characteristics

2.2 Descriptive Analysis Over Time

Since the law "who orders pays" became effective nationwide at the same date (June 1, 2015), we illustrate the development on the rental market graphically over time. Figure 2 displays the development of rental prices over time. The day the law "who orders pays" became effective is indicated by the red vertical line; the two gray lines indicate the implementation of the rent

brake in Frankfurt (November 2015) and Stuttgart (December 2015).[14] Prices were continuously increasing prior to the reform and there is a peak at the exact time of the reform; however, the high level is not durable. Therefore, no clear price increase can be graphically inferred from the reform. At the end of the year 2015 there is a sharp decline in rental prices; this coincides with the implementation of the rent brake. However, after a strong decline, prices start to recover.
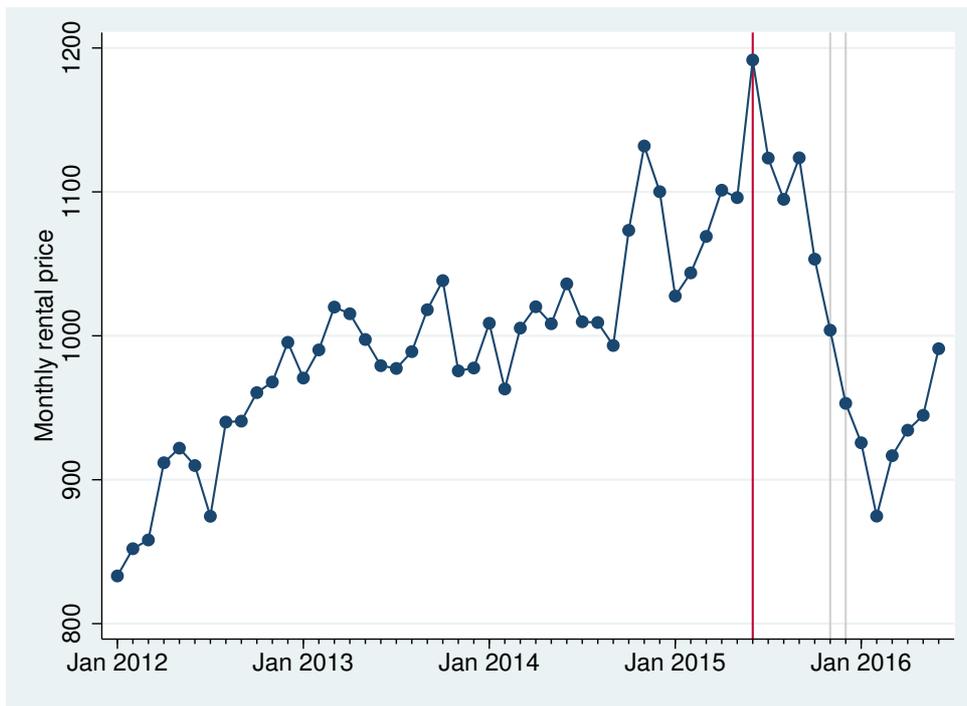
The fluctuation in rental prices could be due to fluctuating characteristics of the mean apartment over time. Hence, in order to reduce variation and increase precision, we further plot the residual of a regression of the natural logarithm of the rental price on a number of apartment characteristics[15] (Fig. 3). Again, it is hard to see from the graph whether the price increase at June 2015 reflects the overall time trend or is (additionally) due to the law "who orders pays". There is no clear discontinuity at the day the law became effective.

To estimate the effect of the reform we use a difference-in-differences approach comparing on the one hand apartments that had a commission payable by renters prior to the reform (treatment group) to apartments on the other hand that had no commission payable by renters prior to the reform (control group). Thus, our identifying assumption is parallel time trends with no shock affecting treatment and control groups differently. To investigate the plausibility of the assumption, we graphically plot the residual of a regression of ln(rental prices) on apartment characteristics (similar as in Fig. 3) prior to the reform separately for commission apartments and no-commission apartments (Fig. 4). The prices of offers without a commission are lower and more volatile than the prices of offers including a commission for renters. The different level of prices (residuals) reflect differences in unobservable characteristics. The higher volatility of offers without commission could be due to the fact that offers without a commission are mostly posted by private persons who are less experienced than commercial offerers (real estate agents or firms). Only a small share among the offers without a commission prior to the reform
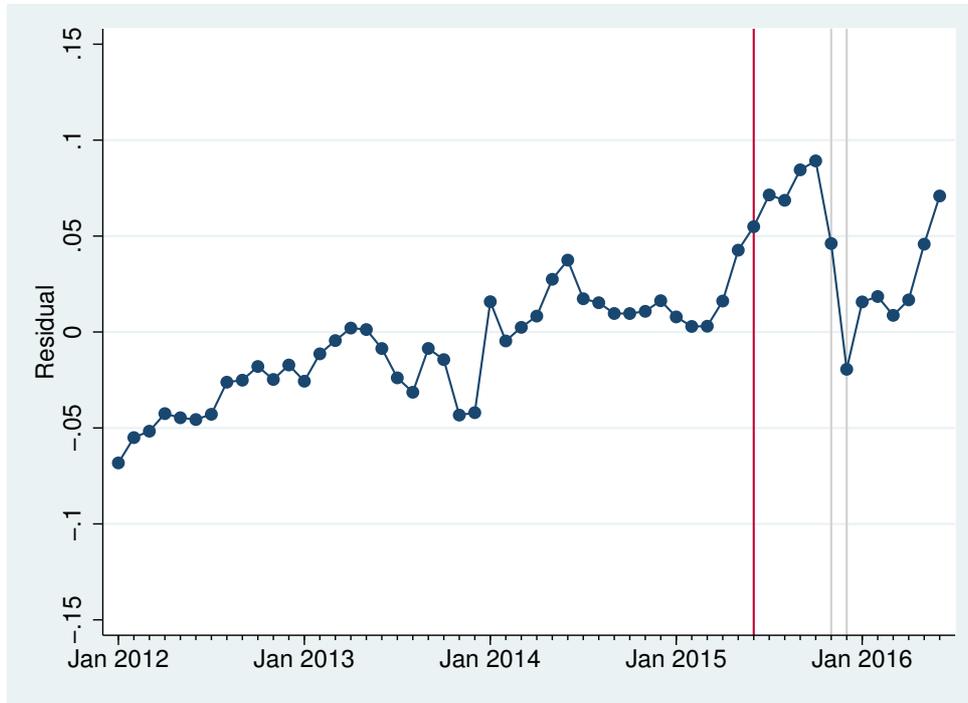
[14]Our data contains only the month and not the exact day each offer was published online. Therefore we round the rent brake start time to be December instead of November 27, assuming that this produces a measurement error of negligible size.

[15]The regressors include: city (dummy for Frankfurt vs Stuttgart), ln(square meters), number of rooms (dummy variables), floor level (six dummy variables for the floor levels between -1 and 4, one single dummy variable for floor levels between 5 and 9, a single dummy variable for floor levels of 10 or larger, and a dummy variable for missing information on floor level), total number of floors in house (six dummy variables for the number of floors between 0 (only ground floor) and 7, one single dummy variable for 8 or more floors, and a dummy variable for missing information on the number of floors), balcony (dummy variables for yes and no and missing information), garden (dummy variables for yes and no and missing information), interaction between balcony and garden, number of bathrooms (dummy variables for 1, 2, 3, and 4 or more, and a dummy variable for missing information), guest bathroom (dummy variables for yes and no and missing information), elevator (dummy variables for yes and no and missing information), cellar (dummy variables for yes and no and missing information), built-in kitchen (dummy variables for yes and no and missing information), parking space (dummy variables for yes and no and missing information).

Note: The red vertical line indicates the point in time when the law "who orders pays" became effective (June 2015), the two gray lines indicate the points in time the rent brake became effective in Frankfurt (December 2015) and Stuttgart (November 2015). N = 219,247, data from Immobilienscout24, authors' calculations.

Figure 2: Rental price

Note: Residuals from a regression of ln(rental price) on city (dummy for Frankfurt vs Stuttgart), ln(square meters), number of rooms (dummy variables), floor level (six dummy variables for the floor levels between -1 and 4, one single dummy variable for floor levels between 5 and 9, a single dummy variable for floor levels of 10 or larger, and a dummy variable for missing information on floor level), total number of floors in house (six dummy variables for the number of floors between 0 (only ground floor) and 7, one single dummy variable for 8 or more floors, and a dummy variable for missing information on the number of floors), balcony (dummy variables for yes and no and missing information), garden (dummy variables for yes and no and missing information), interaction between balcony and garden, number of bathrooms (dummy variables for 1, 2, 3, and 4 or more, and a dummy variable for missing information), guest bathroom (dummy variables for yes and no and missing information), elevator (dummy variables for yes and no and missing information), cellar (dummy variables for yes and no and missing information), built-in kitchen (dummy variables for yes and no and missing information), parking space (dummy variables for yes and no and missing information). The red vertical line indicates the point in time when the law "who orders pays" became effective (June 2015), the two gray lines indicate the points in time the rent brake became effective in Frankfurt (December 2015) and Stuttgart (November 2015). N = 219,247, data from Immobilienscout24, authors' calculations.
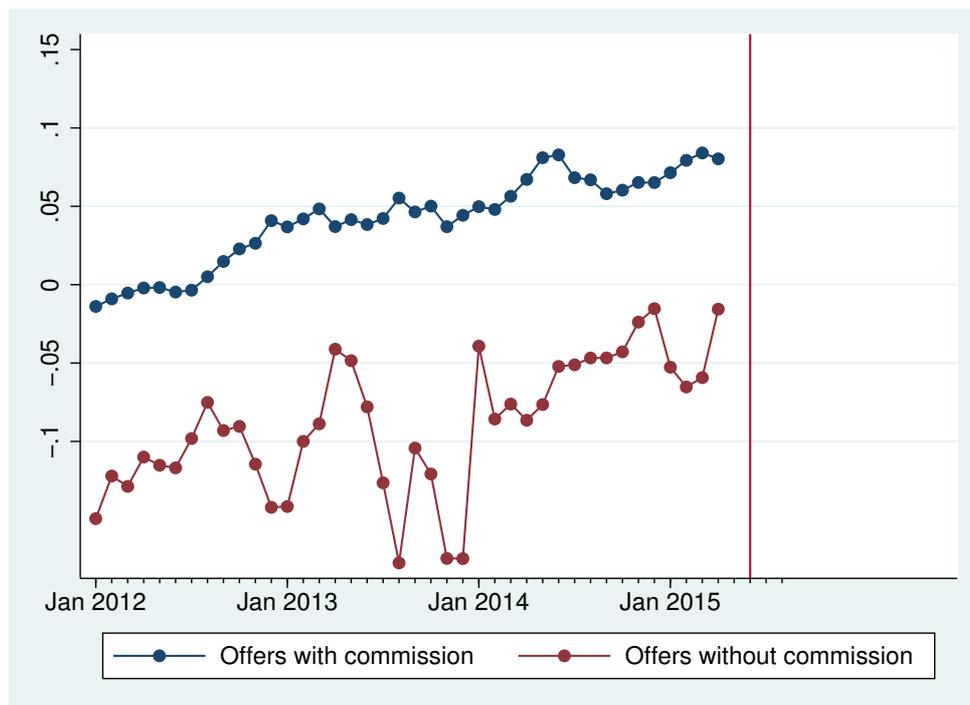
Figure 3: Residuals from a regression of ln(rental price)

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is posted by a commercial offerer, i.e., a firm or a real estate agent for which the landlord pays the commission. Offers with a commission are always posted by commercial offerers. Overall the price trends of offers with and without commission seem to be parallel. This provides evidence that the parallel trend assumption required for the diff-in-diffs identification is plausible for the present case.


Note: Residuals from a regression of ln(rental price) on city (dummy for Frankfurt vs Stuttgart), ln(square meters), number of rooms (dummy variables), floor level (six dummy variables for the floor levels between -1 and 4, one single dummy variable for floor levels between 5 and 9, a single dummy variable for floor levels of 10 or larger, and a dummy variable for missing information on floor level), total number of floors in house (six dummy variables for the number of floors between 0 (only ground floor) and 7, one single dummy variable for 8 or more floors, and a dummy variable for missing information on the number of floors), balcony (dummy variables for yes and no and missing information), garden (dummy variables for yes and no and missing information), interaction between balcony and garden, number of bathrooms (dummy variables for 1, 2, 3, and 4 or more, and a dummy variable for missing information), guest bathroom (dummy variables for yes and no and missing information), elevator (dummy variables for yes and no and missing information), cellar (dummy variables for yes and no and missing information), built-in kitchen (dummy variables for yes and no and missing information), parking space (dummy variables for yes and no and missing information). The red vertical line indicates the point in time when the law "who orders pays" became effective (June 2015). N = 174,347, data from Immobilienscout24, authors' calculations.

Figure 4: Residuals from a regression of ln(rental price) by commission status

If the standard reasoning prediction was fully true, we would expect landlords to continue engaging real estate agents, pay commissions themselves and raise rental prices accordingly. That is, real estate agents would be unaffected by the reform, the number of mandates for them would stay constant. If, on the other hand, the behavioral prediction was true and landlords

cannot (or to a smaller extent) increase rental prices at the moment when commissions drop for renters, we would expect landlords to engage less real estate agents than before the reform. After the reform, real estate agents should be engaged by only those landlords who value the real estate agent's service at least as high as the commission is. Considering the share of offers posted by real estate agents compared to private persons in Figure 5, we see that the share of agent offers has sharply decreased by about 10 percentage points at the time of the reform, while that of private offers has increased by about 10 percentage points. This pattern better fits the behavioral than the standard reasoning hypothesis.
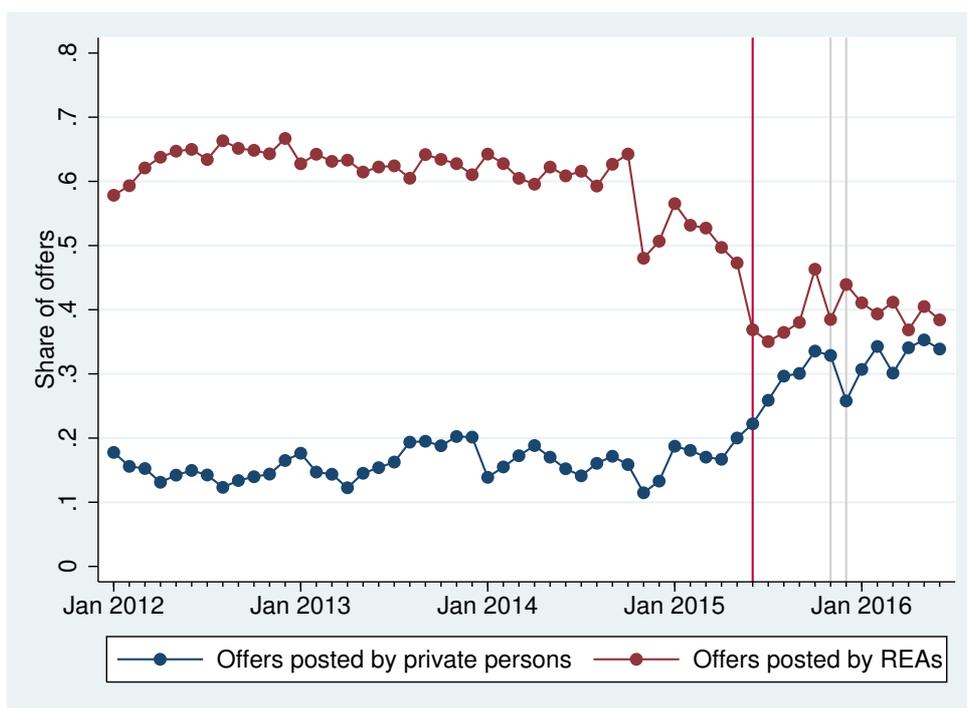
Note: The red vertical line indicates the point in time when the law "who orders pays" became effective (June 2015), the two gray lines indicate the points in time the rent brake became effective in Frankfurt (December 2015) and Stuttgart (November 2015). N = 219,247, data from Immobilienscout24, authors' calculations.

Figure 5: Share of offers posted by private persons and by real estate agents

However, this pattern alone does not prove the behavioral hypothesis because there is another reason why the number of real estate agents engaged could decline under the standard reasoning hypothesis. This is the case if the benefit for a landlord of engaging a real estate agent not only is the explicit service but also the "implicit service" brought by real estate agents making renters select into apartments with and without commission according to their intended rent duration: renters with a short intended rent duration are less willing to pay for a commission than renters with a long intended rent duration. This is because the average monthly financial burden of living (monthly rent + commission divided by the number of months) is

higher, the shorter the rent duration. The majority of landlords tend to prefer renters with long rent durations as every change in renters is typically associated with substantial costs for the landlords (e.g., times costs, service costs, maintenance costs). Since after the reform there is no way to make renters pay the commission and thus to separate renters with a long intended rent duration from renters with a short intended rent duration, i.e., this "implicit service" brought by real estate agents disappeared, landlords' willingness to pay for real estate agents decreased and thus less real estate agents are engaged.[16] The question of whether landlords previously benefited from real estate agents separating renters according to their intended rent durations, however, is unrelated to our hypothesis about the rental price change. The standard reasoning hypothesis (null hypothesis) states that landlords who previously ordered a real estate agent and made renters pay for the commission, should after the reform—as they can no longer make renters pay the commission—increase monthly rental prices accordingly. Whether they spend the money earned from the increased rent for a real estate agent or not is a different question. The behavioral hypothesis (alternative hypothesis), in contrast, states that monthly rental prices should not increase (or increase less) due to the reform because renters or landlords (or both) are unaware of the equivalence of the lump-sum commission and a certain increase in monthly rents—the unawareness being due to mental accounting, inattention, or bounded rationality due to cognitive load.

3 Method

3.1 Empirical Strategy

To identify the causal effect of the existence of a commission payable by the renter on rental prices, we apply a difference-in-differences approach. According to the standard reasoning argument, the law "who orders pays" affects only apartments for which renters had to pay a commission prior to the reform (treatment group). Apartments for which renters never had to pay a commission should not be affected and hence present the control group. We construct a panel dataset identifying apartments based on the exact address (street name and number as well as postal code), the floor level, the number of rooms and the size (in square meters). Each apartment identified based on these characteristics is assigned an individual apartment ID.[17] In

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[16]Theoretically, landlords could design front-loaded rent contracts, which are more attractive for long-duration renters than short-duration renters, in order to achieve the same sorting affect that a real agent commission payable by the renter would have. However, interestingly, this kind of rent contracts is absolutely uncommon in Germany's housing market; rent contracts display either constant (in the majority of cases) or increasing rents (in seldom cases).

[17]We are aware that this might not create a perfect panel as it could be possible that two or more apartments of the same size are located in the same floor of the same building. We assume that these apartments are very similar or, at least, that differences between these apartments are not correlated with the status of being offered with or

Figure 4 it is shown that the price (residual) level is different for treated and untreated apartments. This difference is most likely because of time invariant unobserved characteristics such as quality of the location as well as type and condition of the building. Due to the great advantage of the panel structure of our data we are able to control for all time invariant unobserved characteristics in our estimations by including apartment fixed effects.

For 60% of the apartments (remember, one apartment has several offers, i.e., several observations in the data), all offers prior to the reform include a commission payable by the renter. For 25% of the apartments, all offers prior to the reform are without commission payable by the renter. Only for 15% of the apartments, we find some variation, i.e., some offers prior to the reform include a commission payable by the renter and some offers do not. The occurrence of such "mixed" apartments can have two reasons: first, an apartment was offered once with and once without a commission payable by the renter; second we could observe offers from two or more distinct apartments with the same apartment ID (wrong matching) and some of these apartments were offered with commission, while others with the same apartment ID were offered without commission. Since even in the case of wrong matching the apartments within a single ID are likely to be very similar, we decided to keep them in the sample. A robustness test where we drop these cases is presented in Section 4.2; it shows that our results are robust in this respect. In order to keep the "mixed" apartments in the sample, we apply the following strategy: We classify apartments containing both, observations with commission and observations without commission, as partly treated. For example, assume that within one apartment ID we have two offers prior to the reform in the dataset, but these two offers relate to two different apartments (with the same size on the same floor etc.). Assume that one of these apartments is offered with and one without a commission and we observe both again after the reform. Because only one out of two observations is affected by the reform, we expect the average price change within this apartment ID to amount to only 50% of a comparable apartment which was fully treated. Hence, we estimate a difference-in-differences equation with the propensity to have a commission prior to the reform, i.e.:

$$ln(P_{ij}) = \beta_j + \gamma R_{ij} + \delta C_j R_{ij} + \lambda t_{ij} + \epsilon_{ij},$$

where P_i is the rental price of offer i (of apartment j), β_j are the apartment fixed effects, R_{ij} is a dummy variable taking on the value 1 if offer i is after the reform (June 2015) and 0 otherwise, t_{ij} captures a monthly time trend, C_j is the number of offers of apartment j prior to the reform for which a commission was payable by the renter divided by the number of all offers of apartment j prior to the reform; that is, C_j is the propensity to be treated, taking a value between 0 and 1. Hence, δ is the coefficient of main interest: the behavioral hypothesis

without a commission payable by the renter. Further below (Section 4.2) we conduct a number of robustness tests to check this assumption and find that it is not critical for our results.

suggests that $\delta < 0.0198$, in the extreme case, that $\delta = 0$. The standard reasoning prediction, in contrast, suggests that $\delta = 0.0198$ (null hypothesis). Rejecting the null hypothesis and finding that $\delta < 0.0198$ supports the behavioral hypothesis exposed in section 1.

Applying the outlined diff-in-diffs strategy ensures to disentangle between the reform effect and an (unknown) time effect at the exact date of the reform. Without a control group, accounting for the time trend in a linear way—or even including different polynomials—will never perfectly capture the time effect, which can have a ragged shape as is suggested by Figures 2 and 3. Estimating the reform effect merely as the price change at the time of the reform in June 2015 and misspecifying the overall time trend will strongly affect the estimated reform effect. In contrast, estimating δ by diff-in-diffs will account for any overall price change at the time of June 2015 that is not related to the reform. Our identifying assumption is that there is no other shock affecting rental prices of only those apartments that had a commission prior to the reform while not affecting apartments without a commission prior to the reform.

3.2 The Sample with Apartment Fixed Effects

Our original sample consists of 219,247 apartment offer observations. Restricting the sample to observations for which the apartment could be identified[18] reduces the sample size to 179,688 observations from 85,220 apartments. Among these, for 24,517 apartments (containing 111,159 observations) the sample contains at least two offers and at least one before the reform (this is necessary to determine the treatment status of the apartment). Since we only have data on years 2012 to 2016, this makes our sample even more restrictive with respect to mean rent durations: rent durations are shorter in our sample than in the overall population. Thus, we should detect an even higher price increase due to the reform if the standard reasoning prediction is true. Only those apartments which are observed at least once before *and* once after the reform (26,251 offers from 4,916 apartments) contribute to the estimation of our diff-in-diffs coefficient (δ). However, we do not restrict the sample to the 26,237 observations because all the 111,159 observations contribute to estimating the other coefficients and thus improve precision of the estimates.

Table A1 in the appendix reports summary statistics for the panel sample used for the diff-in-diffs estimation. Because apartments without observations prior to the reform are excluded, the percentage of offers after the reform has decreased to 9% (compared to 18.7% in the full sample). The same reason explains why the share of observations with commission has increased to 65.8% (compared to 54.7% in the original sample). All other apartment characteristics are

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[18]based on postal code, name of street, house number, floor level, square meters, number of rooms.

fairly similar between the panel sample used for the diff-in-diffs estimation (presented in Table A1 in the appendix) and the full sample (presented in Table 1).

4 Results

4.1 Main Results

Results based on the regression described in Section 3.1 are reported in Table 2. The estimation includes apartment fixed effects and we further control for the rent brake, which was implemented in November/December 2015. The coefficient of main interest is the interaction between the post reform dummy and the probability that an offer in this apartment had a commission prior to the reform, i.e., the diff-in-diffs estimator δ reported in the first row. We run a Wald test to test the null hypothesis H_0: $\delta \geq 0.0198$ (i.e., the standard reasoning hypothesis) and reject H_0 at any conventional significance level (see the next-to-last row in Table 2). This means that we reject the standard reasoning hypothesis stating that apartments with a commission payable by renters have 1.98% lower rental prices than apartments without a commission. This speaks in favor of the behavioral hypothesis stating that the price reduction is smaller or even inexistent. Given that the δ coefficient is not significantly different from zero and has even a negative sign, we cannot reject the extreme behavioral hypothesis of no price difference between apartments with and without commission payable by the renter. In the context of the law reform we conclude that we do not find indication that the law made prices of treated apartments increase.

Since the exact price increase predicted by the standard reasoning hypothesis strongly depends on the mean expected rent duration, we test the sensitivity of our results with respect to this parameter. We calculate the maximal mean expected rent duration for which we would still reject the null hypothesis at the 5% significance level. This turns out to be 326 months (see last row in Table 2), i.e., around 27 years. This means that only in the case that we assume a mean expected rent duration of more than 27 years, we cannot reject our null hypothesis. A duration of 27 years, however, is far longer than what the mean expected rent duration can plausibly be, in particular, given that renters with short durations affect the expected rent duration more than renters with long durations as these join the market more frequently (as explained in Section 1). This result illustrates that we must reject the standard reasoning hypothesis even when assuming very (unrealistically) long mean expected rent durations.

Table 2: Estimation of ln(monthly rental price)

	(1)
Post reform x commission	-0.0003
	(0.005)
Post reform	-0.0004
	(0.004)
Time trend t	0.001***
	(0.000)
Observations	111159
Groups	24517
P-value from testing H_0: $\delta \geq 0.0198$)	0.000006
Months significant at 5%-level	326

Note: The OLS estimation includes apartment fixed-effects and a dummy variable for the rent brake. *Post reform* is a dummy variable taking on the value 1 if the offer is from June 2015 or later. *Post reform x commission* is an interaction between the variable *Post reform* and the probability (value between 0 and 1) that an offer for this apartment had a commission prior to the reform. Standard errors in parentheses are clustered on the apartment level.

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

4.2 Robustness Tests

In this section, we present a number of tests to check the robustness of our results. As stated above, our identifying assumption for the diff-in-diffs strategy is that there is no price shock at the time of the reform affecting treated apartments differently than untreated apartments. A basic requirement for identifying the true reform effect is that the data is a true panel, i.e., apartment offers prior to the reform must be matched to offers from the same apartments (or at least similar apartments in both observed and unobserved characteristics) after the reform. If this is not the case and we mistakenly match different apartments (different quality) with the same address, floor level, and size to one single apartment ID and if at the same time we face selective attrition, the reform effect would be estimated with bias.

To illustrate this problem, assume that there are several apartments with the same apartment ID and that some of these apartments were offered with a commission payable by renters prior to the reform, while others were offered without a commission prior to the reform. Now assume that the apartments with commission do no longer appear in the sample after the reform—maybe because landlords of these apartments do no longer order real estate agents due to the reform and privately choose a different marketing channel such as social networks or newspapers. Assume that the apartments without commission prior to the reform, in contrast, do continue to appear in our data even after the reform (their landlords continue to choose the same marketing channel as they did before). Now, if, within this given apartment ID, apartments offered without commission prior to the reform are systematically different (e.g., lower quality) to apartments

offered with commission prior to the reform, the reform effect will be estimated with bias. To check whether this problem actually is relevant, we conduct three robustness tests.

First, we check whether average observed apartment characteristics—balcony, garden, cellar, built-in kitchen, guest bathroom—in treated apartments "change" more than average apartment characteristics in untreated apartments. If this was the case, we would fear a potential bias due to mismatching. We estimate the diff-in-diffs equation using as dependent variable indicators of the availability of a balcony, garden, cellar, and built-in kitchen, respectively (Table 3). The δ coefficient is not significantly different from zero in either of the five estimations. This suggest that observed apartment characteristics do not change pre-post reform more within the treatment group than within the control group; i.e., we are unlikely to facing the problem of systematically mismatching high quality apartments before the reform to low-quality apartments after the reform.

Table 3: Estimation of apartment characteristics

	(1) Balcony	(2) Garden	(3) Cellar	(4) Built-in kitchen	(5) Guest bathroom
Post reform x commission	0.016	-0.004	-0.016	-0.018	-0.005
	(0.031)	(0.007)	(0.034)	(0.010)	(0.012)
Post reform	-0.005	0.007	0.027	-0.023***	0.006
	(0.012)	(0.005)	(0.017)	(0.006)	(0.009)
Time trend t	0.000	-0.000	0.001**	0.001***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	97410	78312	103797	95310	98007
Groups	23303	20517	24147	22432	23696

Note: Estimations based on linear probability models. All estimations include apartment fixed-effects and a dummy variable for the rent brake. *Post reform* is a dummy variable taking on the value 1 if the offer is from June 2015 or later. *Post reform x commission* is an interaction between the variable *Post reform* and the probability (value between 0 and 1) that an offer for this apartment had a commission prior to the reform. Standard errors in parentheses are clustered on the apartment level.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In a second robustness test addressing the same concern, we reduce the sample in the following way: within the treatment group we include only those apartments that have post-reform offers posted by real estate agents and we exclude within the treatment group all apartments that are offered by private persons after the reform. As can be seen in Figure 5 above, not all landlords quit engaging real estate agents at the time of the reform. Those who continue engaging a real estate agent after the reform have to pay the commission themselves. The reason that this reduced sample can serve as a robustness test with respect to selective attrition is that for those apartments that are posted by real estate agents before *and* after the reform we do not expect

selective attrition because there is no reason for real estate agents to change their advertising channels at the time of the reform. Thus, systematically wrong matching of high-quality apartments before the reform to low-quality apartments after the reform is unlikely. The result of the estimation with the reduced sample is reported in column (1) of Table 4. Our result turns out to be robust: Even those landlords who decided to continue engaging real estate agents after the reform and since then have to pay the commission themselves do not increases rental prices accordingly; i.e., we reject the standard reasoning hypothesis, while not being able to reject the behavioral hypothesis of no price change compared to the control group.

Table 4: Estimation of ln(monthly rental price)—robustness specifications I

	(1) REA before and after reform	(2) Always or never commission
Post reform x commission	-0.002	0.002
	(0.005)	(0.005)
Post reform	-0.005	0.001
	(0.006)	(0.004)
Time trend t	0.002***	0.001***
	(0.000)	(0.000)
Observations	16252	98507
Groups	3193	22202
P-value from testing H_0: $\delta \geq 0.0198$	0.00001	0.00008
Months significant at 5%-level	355	238

Note: All OLS estimations include apartment fixed-effects and a dummy variable for the rent brake. *Post reform* is a dummy variable taking on the value 1 if the offer is from June 2015 or later. *Post reform x commission* is an interaction between the variable *Post reform* and the probability (value between 0 and 1) that an offer for this apartment had a commission prior to the reform. Standard errors in parentheses are clustered on the apartment level.

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

The latter robustness test is interesting also for a second reason: One could suspect professional real estate agents to have better knowledge about market prices compared to (private) landlords and this could be a reason why apartments with commission have higher (the maximally possible) rental prices. We call this the consultancy effect. The consultancy effect could be a reason for the price gap between commission- and no-commission-apartments shown in Figure 4. Landlords who stop ordering a real estate agent at the time of the reform might lack expert knowledge about market prices and thus eventually ask prices below the apartments' potential. However, this effect cannot drive results displayed in Table 4 column (1) because for the treatment group in this subsample, offers are always published by real estate agents, before and after the reform. Therefore, we do not expect the consultancy effect to decrease at the time

of the reform.

In a third robustness test addressing the same concern, we test whether our results are driven by the so-called "mixed" apartments (see Section 3.1). The "mixed" apartments are most likely among those that are potentially mismatched due to selective attrition. In a robustness test we therefore restrict our sample to only those apartments where offers prior to the reform were either always or never with a commission, i.e., apartments which are either fully treated ($C=1$) or not treated ($C=0$). The results are very similar to our main results, i.e., they seem not to be driven by "mixed" apartments (see Table 4, column (2)).

A number of potential concerns relate to time effects. Note that we find a strongly significant positive time trend in rental prices. In order to test the robustness of our results with respect to different specifications of the time trend, we stepwise add several polynomials of t in the regression; the results are reported in Table 5, columns (2) to (4). The results for δ are robust: it is not significantly different from zero and the null hypothesis H_0: $\delta \geq 0.0198$ is rejected at any conventional significance levels. Allowing for seasonal effects (by adding eleven dummy variables for each month of the year plus one omitted category) instead of the polynomials (column (5)) or in addition to the polynomials (column (6)) does not change our findings.

Next, we test the robustness of our result with respect to the concern that the reform effect did not kick in immediately but took some time to adjust. In alternative estimations, we therefore drop all observations in the first (column (1) of Table 6) and first two months (column (2)of Table 6) after the reform became effective. The results appear to be robust even when we allow for slow adjustment.

Table 5: Estimation of ln(monthly rental price)—with polynomial time trend and seasonal effects

	(1)	(2)	(3)	(4)	(5)	(6)
Post reform x commission	-0.0003	0.0003	0.0003	0.006	-0.0004	0.0005
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Post reform	-0.000	0.002	0.001	0.002	-0.000	0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Time trend t	0.001***	0.002***	0.002**	-0.000	0.001***	-0.000
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)
$t^2/1.000$		-0.008	-0.012	0.143		0.155
		(0.006)	(0.025)	(0.084)		(0.086)
$t^3/10.000$			0.001	-0.046		-0.048*
			(0.003)	(0.024)		(0.024)
$t^4/100.000$				0.004*		0.005*
				(0.002)		(0.002)
Seasonal effects					X	X
Observations	111159	111159	111159	111159	111159	111159
Groups	24517	24517	24517	24517	24517	24517
P-value from testing H_0: $\delta \geq 0.0198$	0.000006	0.00001	0.00001	0.00002	0.000005	0.00002
Months significant at 5%-level	326	297	297	286	330	290

Note: All OLS estimations include apartment fixed-effects and a dummy variable for the rent brake. *Post reform* is a dummy variable taking on the value 1 if the offer is from June 2015 or later. *Post reform x commission* is an interaction between the variable *Post reform* and the probability (value between 0 and 1) that an offer for this apartment had a commission prior to the reform. *Seasonal effects* include dummy variables for each month of the year. Standard errors in parentheses are clustered on the apartment level.

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

Table 6: Estimation of ln(monthly rental price)—robustness specifications II

	(1) Drop June 2015	(2) Drop June+July 2015	(3) Drop May 2015	(4) Drop April+May 2015	(5) Drop rental 2015	(6) Size ≤55 sqm	(7) Only inner-city	(8) Time trend cities
Post reform x commission	-0.002	0.001	-0.002	-0.003	0.002	-0.001	0.001	0.0003
	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.008)	(0.007)	(0.005)
Post reform	0.002	0.003	0.003	0.008	-0.001	0.006	-0.002	0.001
	(0.004)	(0.005)	(0.005)	(0.006)	(0.004)	(0.007)	(0.006)	(0.004)
Time trend t	0.001***	0.001***	0.001***	0.001***	0.001***	0.002***	0.001***	0.188
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.104)
Frankfurt x t								-0.000
								(0.000)
Observations	109218	107832	108827	106246	107592	26622	63739	111159
Groups	24517	24517	24454	24274	24517	7184	12410	24517
P-value from testing H_0: $\delta \geq 0.0198$)	0.00001	0.00031	0.00001	0.00004	0.00058	0.00808	0.00259	0.00001
Months significant at 5%-level	377	231	383	360	216	177	200	297

Note: All OLS estimations include apartment fixed-effects and a dummy variable for the rent brake. *Post Reform* is a dummy variable taking on the value 1 if the offer is from June 2015 or later. *Post reform x commission* is an interaction between the variable *Post reform* and the probability (value between 0 and 1) that an offer for this apartment had a commission prior to the reform. Standard errors in parentheses are clustered on the apartment level.

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

One could also be concerned that the reform was anticipated and its effect was discernible already before the law reform became actually effective. However, there is no clear theoretical reason why landlords should switch from letting renters pay the commission to paying the commission themselves even before the reform; also there is no rational reason why they should stop using real estate agent services before the reform law became effective. Nevertheless, to test our results for robustness against anticipation effects, we drop the first (column (3) of Table 6) and first two months (column (4) of Table 6) prior to the reform from the sample. The estimation results do not change compared to our main specification.

To further check whether the implementation of the rent brake law biases our results, we restrict the sample to observations prior to the implementation of the rent brake, i.e., we drop observations from October 2015 or later (column (5) of Table 6). Our results are again robust to the sample restriction.

Accordingly, we investigate the heterogeneity of apartments. Since small apartments usually have a higher turnover than large apartments, standard reasoning would predict the highest price increase for small apartments. In column (6) of Table 6 we restrict the sample to apartments which are smaller or equal to 55 square meters (smallest 25% of the sample). Not even for these apartments we find price increases due to the reform. The dynamics in the rental apartment markets are often argued to be strongest in inner-city districts. Consequently, the price effects of the reform should be most visible in these parts of a city. In column (7) of Table 6 we therefore restrict the sample to apartments located in inner-city districts and find no support for the standard reasoning hypothesis.

In column (8) of Table 6 we allow the time trend in rental prices to differ between the cities Frankfurt and Stuttgart. We find that the coefficient of the time trend interacted with the Frankfurt indicator is not significantly different from zero. Furthermore, estimations of all other coefficients remain almost the same.

5   Conclusion

In this paper we investigate the question of whether behavioral bias affects rental prices in the housing market. We examine whether renters react to a commission payable by renters at the time of contract conclusion differently than to the (net present value of) monthly rents. Standard economic reasoning would suggests that a renter's willingness to pay for the monthly rent is lower if, in addition, she has to pay a commission when signing the rental contract. Based on behavioral insights on mental accounting, inattention, and cognitive load we doubt this ef-

fect. Using data from the largest online real estate marketplace for residential properties in Germany we apply a difference-in-differences approach exploiting a law reform in June 2015 that inhibits landlords from making renters pay the real estate commission of agents ordered by the landlord. From the standard reasoning perspective it was argued that the law reform would not unburden renters as landlords would simply pass the burden of the commission through to renters by increasing the monthly rents accordingly. Exploiting the panel structure of our data and comparing price changes (pre–post reform) of apartments with a commission prior to the reform to price changes of apartments without a commission prior to the reform, we find that the changes in prices at the time of the reform are not different for the two groups. We can thus reject the standard reasoning prediction of an increase in rental prices. Our result could be explained by standard reasoning only if the expected rent duration was longer than 27 years, i.e., unrealistically long. We conduct a series of robustness tests confirming that the result cannot be explained by selective attrition nor the implementation of another law affecting the rental market (the law of the rent brake). Increases in rental prices are not even found for inner-city areas nor for small apartments, which usually have a higher turnover of renters and thus a higher potential price increase due to the reform. On the other hand, according to our results, the behavioral hypothesis of a zero effect of a commission payable by renters on rental prices cannot be rejected.

The limitation of this paper is that we cannot identify the exact behavioral mechanism at work; the three behavioral mechanisms discussed in the introduction are all compatible with our findings. We are able to empirically discriminate only between the standard reasoning hypothesis and the behavioral hypothesis in general.

Appendix

Table A1: Summary statistics—panel sample

Variable	Mean	SD	Min	Max	N
Main variables					
Rental price (in euros)	1106.371	784.081	130	9000	111159
Price per sqm (in euros)	12.393	3.669	4	30	111159
Post reform	0.09	0.286	0	1	111159
Commission	0.658	0.474	0	1	110845
Number of rooms	2.714	1.145	1	11	111159
Square meters	86.704	44.863	10	400	111159
Further variables					
Frankfurt (vs Stuttgart)	0.77	0.421	0	1	111159
Floor level	2.473	1.989	-1	24	95993
Total number of floors in house	4.494	2.337	0	50	77009
Balcony	0.724	0.447	0	1	97410
Garden	0.17	0.375	0	1	78312
Number of bathrooms	1.227	0.438	1	4	72507
Guest bathroom	0.332	0.471	0	1	98007
Elevator	0.465	0.499	0	1	92337
Cellar	0.698	0.459	0	1	103797
Built-in kitchen	0.771	0.42	0	1	95310
Parking space	0.001	0.032	0	1	111159
Inner-city	0.573	0.495	0	1	111159
Rent brake	0.032	0.176	0	1	111159
Private offer	0.1	0.3	0	1	108224
Offer by REA	0.64	0.48	0	1	108224
Offer by other commercial entity	0.253	0.435	0	1	111159

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