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Eva M. Berger* and Felix Schmidt†

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Abstract

We investigate the question of whether agents on the rental housing market are inattentive to sizeable side costs of renting, namely commissions payable by renters to real estate agents appointed by landlords. We exploit a natural experiment created by a policy reform in Germany that shifted the payment liability for commissions from renters to landlords. Based on panel data on offers for apartments to rent, we find evidence for substantial inattention. This has allocative as well as distributional consequences as it implies an inefficiently high demand for real estate agent services at the expense of renters.

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Keywords: Inattention, behavioral bias, rental housing, real estate agents, liability side equivalence, natural experiment

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1 Introduction

A central assumption in economics is that agents optimize fully taking into account all information. Specifically, consumers are assumed to equally take into account all cost components associated to a purchase. Contrary to this full optimization assumption, there is growing evidence about individuals being inattentive to part of the relevant information. Consumers are inattentive to product add-ons when buying a base good (Ellison, 2005; Gabaix and Laibson, 2006) and to side costs of a purchase such as shipping charges (Brown et al., 2010). Consumer inattention in these cases affects producer revenue and equilibrium shrouding behavior. There is also evidence that consumers are inattentive to some types of consumption taxes, depending on tax salience (Chetty et al., 2009; Finkelstein, 2009; Taubinsky and Rees-Jones, 2018). This is the case not only for small stakes consumption decision, but even for high stakes decisions like home purchases (Bradley, 2017). Inattention for consumption taxes has implications for the distorting effect of taxation and for optimal tax rates (see, e.g., Taubinsky and Rees-Jones, 2018).[1] The question arises whether consumers are inattentive also to sizeable service costs related to a purchase, as this can have distributional consequences as well as consequences for the efficiency for the service market.

The aim of this paper is to investigate inattention paid by market participants in the rental housing market, namely inattention to real estate agent (REA) commissions payable by renters. Inattention in this context is specifically relevant for two reasons: it affects (i) the efficiency of the market for REA services and (ii) the size of the burden borne by renters and landlords—who typically have a very different financial background.

In the consumption decision for an apartment to rent, the main cost the consumer (i.e., the renter) has to account for is the monthly rent. In Germany, for some apartments the renter in addition had to pay a lump-sum commission to an REA appointed by the landlord.[2] The REA commission used to be equal to 2.38 times the monthly rent and had to be paid at the time when the renting contract was concluded. This was made fully transparent to the renter[3] in

[1] See Gabaix (2019) for an overview of the literature on inattention.

[2] This is the case for roughly half of the apartment offers in our data.

[3] We use the word 'renter' here also for the person looking for an apartment to rent.

the apartment ad; and the renter in this case had no possibility to contact the landlord directly, but had to contact the REA instead. The REA commission constituted a side cost of renting as it was inevitably associated to the rent of some apartments and the renter did not receive any service from the REA. This is the case as the renter gets in contact with the REA only *after* having searched on her own for apartment ads on the online portal. The service brought by the REA consists of, in particular, organizing visits and preparing the rent contract, i.e., services otherwise provided by the landlord himself. The REA thus provides services to only the landlord, who is also the person that appointed the REA. In order for the renter to be perfectly attentive to this side cost and take it into account for the rental decision to the same extent as the rental price, the renter would have to translate the lump-sum REA commission into equivalent monthly amounts. Doing this would allow him to compare the total cost of renting an apartment for which an REA commission had to be paid by the renter to the total cost of renting an apartment for which *no* REA commission had to be paid by the renter. The necessity of translating the lump-sum commission into monthly amounts is a crucial reason for us to doubt full attention in this context. If renters compare rental prices without taking into account the monthly equivalent of the REA commission payable for some apartments but not for others, they are inattentive. The presence of different time dimensions of payments could be an explanation also for the findings of Bradley (2017), documenting irrational behavior in the large stake consumption decision of home purchases, namely property taxes (payable yearly) being imperfectly capitalized into home prices.

To investigate (in)attention to REA commissions, we test the standard economic hypothesis of full attention (null hypothesis) against the inattention hypothesis. According to standard economic reasoning with full attention, the rent of an apartment *with* an REA commission payable by the renter should be lower than the rent of a similar apartment *without* an REA commission payable by the renter. That way, the landlord, who consumes the REA service, always bears the cost of this service, either directly or indirectly; indirectly means making the renter pay the commission while reducing the monthly rent accordingly. Thus, the landlord is indifferent about whether to pay the commission directly or indirectly. If, in contrast, renters

are fully inattentive to REA commissions, apartment rents should be independent of whether there is an REA commission payable by the renter or not. Furthermore, in this case landlords appointing REAs might prefer passing the REA commission through to renters instead of paying it themselves. This allows landlords to make renters bear the cost of the REA service consumed by landlords. The two competing hypotheses are formalized in a simple model presented in Section 3. Using a sufficient statistic approach we empirically estimate the degree of inattention in this application, i.e., the inattention paramenter, following Chetty et al. (2009), DellaVigna (2009), Finkelstein (2009), and Lacetera et al. (2012).

In order to answer the question of whether inattention affects the rental housing market as described, the straightforward strategy would be to compare the rents of apartments for which a commission is payable by the renter to the rents of those apartments for which no commission is payable by the renter. However, apartment characteristics might differ between the two groups. A particularly well-suited context to study our question is therefore a recent reform in Germany called the "principle who orders pays" (*"Bestellerprinzip"*). The reform shifted the payment liability for REA commissions (claimed by REAs appointed by landlords) from renters to landlords. We exploit the reform as a natural experiment which allows us to estimate the causal effect of the existence of an REA commission payable by the renter on rental prices. According to the liability side equivalence reasoning taken from tax incidence analysis, a policy changing the liability side of any payment from one market side to the other should not affect the burden either market side bears. It should, in contrast, change the market price (here: the rental price) accordingly. This is the null hypothesis (in the following called the standard economic hypothesis) that we test against the alternative hypothesis, the inattention hypothesis, which predicts the rental price to increase less or not at all (case of full inattention).

We use a panel dataset of apartments offered for rent on the online real estate marketplace Immobilienscout24.de and apply a difference-in-differences approach: we compare the rental price development of apartments that were, prior to the reform, offered *with* a commission payable by the renter (treatment group, affected by the reform) to the rental price development of apartments that were, prior to the reform, offered *without* a commission payable by the renter

(control group, not affected by the reform). The panel structure of the data allows us to control for all time-invariant unobserved apartment characteristics, which potentially affect both the rental price and the marketing channel (i.e., REA versus no REA) and thus the selection into treatment and control group.

We find evidence against the standard economic hypothesis and in favor of the inattention hypothesis: We find no reform-induced increase in rental prices, not even for particularly susceptible groups of apartments nor even after an adaptation phase. The mean expected rent duration would need to be more than 41 years (i.e., implausibly high) in order that the null hypothesis could not be rejected at the 5% significance level. Relating to the inattention framework (Chetty et al., 2009; DellaVigna, 2009), we find the inattention parameter to be close to one (full inattention). This is consistent with estimates of consumer inattention in other fields such as non-transparent sales taxes ($\theta = 0.94$ based on a natural experiment by Chetty et al. (2009); $\theta = 0.65$ based on a field experiment by Chetty et al. (2009)) and imperfect capitalization of property taxes into home prices (close to full inattention, Bradley (2017)). The cost of inattention is borne by renters; it amounts to 2,38 times the monthly rent, i.e., in our sample more than EUR 2,000 for the average renter.

The doubt of full attention here primarily refers to renters. Yet, we do not exclude that landlords are as inattentive as renters meaning that landlords are inattentive to the fact that making renters pay the REA commission should be equivalent to increasing the rental price by a certain amount. Put differently, landlords are inattentive to the (standard economic reasoning) fact that they always bear the cost of the REA service they consume, either by paying it directly or by making renters pay the commission and reducing rents accordingly. They are unaware of the fact that shifting the economic burden of the commission to renters is actually possible only under renter inattention. In contrast, we do not claim that landlords are inattentive to REA commissions *payable by landlords*. In the case that landlords have to pay the commission themselves, the commission is not a side-cost of renting but a direct cost of lending by the landlord. Landlords can freely decide about whether to market the apartment on their own or to buy the service from an REA. They can easily compare the (lump-sum) cost to the (lump-sum)

benefit brought by an REA. There is no need for translation between lump-sum and monthly costs. This is why inattention is hypothesized to play a role only for REA commissions *payable by renters*. An alternative explanation for the findings could be that renters are inattentive, while landlords are not but have a correct belief about renters' inattention. In this case, there is also no need to reduce the rental price when shifting the REA commission to the renters. Hence, both explanations are consistent with our results. Since both possibilities have the same implications, the distinction is irrelevant in our context.

Our research relates to a growing body of empirical literature documenting inattention of consumers (see above).[4] Inattention in the rental housing market, however, is particularly interesting for both allocative and distributional reasons. From an allocative perspective, one particularity of the here studied context is that the fact of renters being inattentive to REA commissions does not lead to an increase in landlords' earnings directly—other than is the case for inattention to side costs such as shipping charges (Brown et al., 2010). Inattention of rental market participants (and the practice of letting renters pay the REA commission) leads to the situation that landlords do not fully internalize the cost of the REA service they consume. This raises the demand for REA services by landlords and results in a consumption level above the social optimum. The reform under consideration—though simply regulating the payment liability—corrects this market inefficiency. This conclusion is consistent with the data showing that the number of REAs appointed by landlords has dropped substantially at the time of the reform.

From a distributional perspective, inattention in the rental housing market is relevant because it affects the two market sides differentially, even in the case that participants on both market sides are equally inattentive. And individuals on the demand side of the market (renters) are typically less wealthy than individuals on the supply side of the market (landlords). The burden renters bear due to inattention is substantial as expenditure on housing rents—and thus on REA commissions payable by renters—is substantial. In Germany, households spend on average 28% of their available income on the monthly housing rent. The share is even larger

[4]There is also a growing body of theoretical literature modelling the implications of consumer inattention to the full variety of available goods (e.g., De Clippel et al., 2014; Hefti, 2018; Ho et al., 2017) and to product attributes (e.g., Bordalo et al., 2013, 2016).

for low-income households: close to 50% for households with a net income below EUR 700 per month.[5] Over the last decade, rental housing became increasingly expensive, especially in large cities. In Germany, rents have increased by 30% over the last ten years in cities and 21% in rural areas without agglomerations in West Germany.[6] In most developed countries, a significant share of households live in rented (rather than owned) homes. In Germany, this is the case for 48% of all households, overall in the European Union for 30%, and in the U.S. for 36%.[7] The shares are larger in cities than in rural areas. Given the significant number of renters, the high share of expenditure for rental housing, and increasing rental prices, policy makers have been looking for ways to unburden renters. The reform under consideration is one such policy measure that, according to our results, was successful in unburdening renters—even though this is contrary to standard economic reasoning.

To the best of our knowledge, there is no previous study evaluating the effect of the reform under consideration; nor is there a previous study investigating the effect of the existence of an REA commission payable by the renter on the rental price. In the rental housing market of a number of countries, such as Austria, Finland, Denmark, France, Luxemburg, Italy, and Sweden, the commission for REAs appointed by landlords is (partly) paid by renters. This is the case even though in most developed countries, the REA profession is regulated to some extent (CEPI, 2013). Hence, investigating the effect of changes in regulations such as the legal reform under consideration is relevant for a number of countries. Our findings show that a simple shift of the payment liability between market sides can effectively change the burden each side bears.

Yet, we are unaware of any previous study examining behavioral biases on the rental housing market—in spite of the economic relevance, given the large amount of money spent on housing rents. In contrast, there is some evidence of behavioral biases of actors on the housing *selling* market: Repetto and Solis (2017) document that apartment buyers are prone to the left-digit

[5]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).

[6]Federal Institute for Research on Building, Urban Affairs and Spatial Development (*Bundesinstitut für Bau-, Stadt- und Raumforschung—BBSR*), see Figure A1 in the Appendix.

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

bias. Genesove and Mayer (2001) found that loss aversion determines seller behavior 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.[8] Understanding behavior that deviates from standard economic assumptions is crucial for understanding the consequences of policy measures and for designing effective policies. Analyzing the allocative and distributional consequences of a policy measure regulating payment liabilities, we contribute to this understanding.

The paper is organized as follows: In Section 2 we explain the institutional background, giving details about the policy reform under consideration and the rental housing market in Germany. In Section 3 we provide a simple model to formally present our testable hypotheses. Section 4 describes the data source and variables. Section 5 presents a descriptive analysis over time. Section 6 gives details about the estimation strategy: a difference-in-differences model with apartment-fixed-effects. Section 7 presents and discusses the results and robustness tests. Section 8 concludes.

2 Institutional Background

Prior to the reform under consideration in this paper, when landlords appointed an REA to market their apartment, 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 rent plus the VAT of 19%).[9] This was the case even though the REA provided a service exclusively to the landlord (e.g., taking photos of the apartment, publishing the apartment ad, organizing visits with people interested in the apartment, preparing the contract). The REA did not provide any service to renters as renters got in contact with the REA only *after* having found

[8] Glaeser and Nathanson (2015) review rational and nonrational models that have been developed to explain housing bubbles. Salzman and Zwinkels (2013) give an overview how behavioral bias affects real estate finance and investment decisions.

[9] In our sample, 2.75% of the apartments offered prior to the reform claim a commission of less than 2.38 times the monthly rent.

the apartment ad by their own search effort. The REA commission was payable as a lump-sum at the time when the renting contract was concluded. This was made fully transparent to the renter already in the ad and obvious to the renter also due to the fact that the renter in this case was in contact only with the REA instead of with the landlord (e.g., when visiting the apartment). The REA commission thus constituted for renters a side cost of renting as it was inevitably associated to the rent of some apartments while the renter not receiving any service from the REA. Yet, not all landlords appointed REAs. In our sample (from the cities of Frankfurt and Stuttgart), prior to the reform, around half of the apartment offers included an REA commission payable by renters (see Section 4).

The law reform exploited as a natural experiment in this paper is called "principle who orders pays" (*"Bestellerprinzip"*). It has been effective in Germany since June 2015. The law prescribes that the commission for REAs acting on the rental housing market has to be paid by the person who appointed the agent, i.e., in virtually all cases the landlord.[10]

Some people feared that, after the reform, REAs would try to raise money from renters by illegal ways like charging potential renters some fees for viewing the apartment, for making the contract, or for administration in general. However, these practices are illegal and REAs 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).

The new law was subject to active public debate. A key ex-ante argument by some economists against the law was that it would not unburden renters (the law's declared political goal) as landlords who appoint an REA (and now have to pay the commission themselves) would simply increase monthly rents such that the total burden for each party remains constant. This is the standard reasoning hypothesis that we are testing in this paper.

[10]Strictly speaking, the reform determines that the REA is allowed to raise money from the renter only in the case that the agent gets in contact with the landlord only for the purpose of arranging a contract with that single renter, cf. *Gesetz zur Dämpfung des Mietanstiegs auf angespannten Wohnungsmärkten und zur Stärkung des Bestellerprinzips bei der Wohnungsvermittlung (Mietrechtsnovellierungsgesetzt—MietNovG)*. An REA being in contact with a landlord only for the purpose of the contract with one single renter is a very unrealistic scenario. In virtually all cases, REAs have a number of apartments in their "portfolio" as they have been mandated by the landlords to find a renter for these apartments. In fact, since the time of the reform, commissions have no longer been paid by renters (cf., e.g., Michaelis and von Wangenheim, 2016).

Since the reform "principle who orders pays" changes the liability side of the REA commission, it provides a setting in which to examine inattention to REA commissions payable by renters. If market participants were fully inattentive, rental prices had no reason to increase due to the reform. If market participants were perfectly attentive, rental prices would increase systematically.

The exact price increase predicted by the standard reasoning hypothesis depends on the mean expected rent duration. As a benchmark, in the case of a mean expected rent duration of ten years, i.e., 120 months, and an interest rate of zero (most conservatively), we should observe an increase in rental prices by 1.98% (= 2.38/120). This is our benchmark hypothesis to be tested. Below we test the sensitivity of our results with respect to the mean expected rent duration assumed (see Section 7). Because there is no suitable statistic in Germany on expected rent durations nor even on *completed* rent durations, we had to use a plausible value that is rather conservative regarding our hypothesis test in order to make sure not to over-reject. When choosing a value for the rent duration, one has to be aware of the fact that the mean *expected* rent duration for a specific apartment offered on the market is systematically shorter than the mean *completed* rent duration because short-duration renters move into new apartments more frequently than long-duration renters; hence, a landlord offering her apartment is more likely to encounter a short-duration renter than a long-duration renter, even in a situation where the numbers of short- and long-duration renters are equal. The only available statistic loosely related to what we need can be found in a specialized survey about housing of the German Microcensus 2010.[11] According to that, in the German western federal states, 19% of households have lived in their currently rented apartment for less than two years, more than 57% have lived in their currently rented apartment for less than eight years, and more than 79% have lived in their currently rented apartment for less than 20 years (cumulative probabilities).[12] These numbers suggest that there is considerable movement on the rental housing market and our arbitrary as-

[11] Federal Statistical Office (Statistisches Bundesamt) 2012, Bauen und Wohnen, Mikrozensus-Zusatzerhebung 2010, Bestand und Struktur der Wohneinheiten, Wohnsituation der Haushalte, Fachserie 5 Heft 1.

[12] The period of time living in the currently rented apartment has been surveyed by the German Federal Statistical Office only by brackets as presented here.

sumption of the mean expected rent duration being ten years seems to be rather conservative with respect to the hypothesis we test.

3 Formal Framework

In this section we provide a simple framework to structure thoughts and formalize our hypotheses. We do not provide a fully specified model of the market but use a sufficient statistic approach as proposed by Chetty (2009) and DellaVigna (2018). In the following, we first provide the framework under the null hypothesis, i.e., under standard economic reasoning. We then release the key assumption of full attention, introducing the parameter θ measuring the degree of inattention. We thus apply the framework of Chetty et al. (2009), DellaVigna (2009), Finkelstein (2009), and Lacetera et al. (2012) to the context of the rental housing market.

3.1 Framework Under Standard Economic Reasoning

Consider a competitive market where the overall equilibrium price for living in a certain apartment to rent is equal to the renter's willingness to pay.[13] In the case of no commission payable by the renter, this overall monthly price consists of solely the rent. In the case of a commission payable by the renter, the overall monthly price consists of the sum of the rent and the monthly equivalent of the REA commission. This means that $V = P + \frac{C}{D}$, where V is the renter's monthly willingness to pay, P is the monthly rent, C is the lump-sum commission payable by the renter (which is zero in the case of no commission payable), and D is the rent duration in months.[14] Rearranging yields

$$P = V - \frac{C}{D}. \tag{1}$$

[13] Certainly, in a search market with heterogeneous goods, the overall price might be below the willingness to pay. This, however, is not critical for our analysis as long as the reform under consideration does not change the key parameters of the market such as search costs occurring at the renter or the distribution of apartment characteristics. The distance between the final price and the willingness to pay is assumed to be unaffected by the reform.

[14] For simplicity, the interest rate is set to zero. See Section 7 for a discussion.

This implies that the rent of an apartment *without* REA commission payable by the renter is higher (i.e., $P = V$ since $C = 0$) than the rent of an apartment *with* REA commission payable by the renter (i.e., $P = V - \frac{C}{D}$). Consequently, abolishing commissions payable by renters (as the reform does) should increase rents of apartments that, prior to the reform, did have a commission payable by renters, by exactly the amount $\frac{C}{D}$. This is what the standard reasoning hypothesis suggests.

Furthermore, considering landlords' decision prior to the reform about whether to appoint an REA (with the commission payable by the renter) or not, we note the following: In the case of appointing an REA, the landlord's net earnings (in monthly terms), π, are equal to the monthly rent earned, i.e., $\pi_{REA} = P_{REA}$. Inserting P from (1) yields

$$\pi_{REA} = V - \frac{C}{D}.$$

In the case of appointing no REA and marketing the apartment on his own, the landlord's net earnings are equal to the monthly rent minus the one-time expenditure for marketing the apartment on his own, E, divided by the rent duration D, i.e., $\pi_{noREA} = P_{noREA} - \frac{E}{D}$. Inserting P from (1) with $C = 0$ yields

$$\pi_{noREA} = V - \frac{E}{D}.$$

This implies that, prior to the reform, a landlord appoints an REA if $E > C$.

After the reform, landlords are still free to appoint an REA, but, in the case they do, they have to pay C themselves to the REA. This means that, for deciding whether to appoint an REA or not, landlords directly compare the commission C, which they have to pay to the REA in the case of appointing one, with the cost E occurring in the case they do not appoint an REA but market the apartment on their own. Hence, the decision criterion for landlords to appoint or not an REA is equal before and after the reform. The only difference is that, before the

reform, landlords pay C indirectly through a lower monthly rent earned, while after the reform, landlords pay the commission directly.[15]

In the simple framework presented we make the following assumptions: First, we assume that renters do not derive any utility or disutility from the apartment being marketed by an REA versus being marketed by the landlord on his own. Second, we assume that the reform does not affect other parameters of the model, in particular V or D. Note that for the analysis of the effect of the reform on rental prices, only the rent duration *prior to the reform* is important because the rent duration *prior to the reform* determines how much the rental price should increase. The assumption of D being unaffected by the reform is therefore uncritical. Third, the crucial assumption in this framework under standard economic reasoning is the assumption about full attention paid to the monthly equivalent of the REA commission, i.e., equal attention paid to both cost components, the rent and the monthly equivalent of the REA commission. It suggests that the renter's demand depends solely on the *sum* of rent and monthly equivalent of the REA commission and is independent of the *distribution* between the two cost components. If, however, renters do not fully take into account the monthly equivalent of the REA commission but are (partly) inattentive to this cost component as argued in the introduction, the assumption is violated. In the following, we therefore provide a framework allowing for inattention for the monthly equivalent of the REA commission.

3.2 Framework with Inattention

To account for inattention paid to the monthly equivalent of the REA commission, we adapt our framework by introducing the parameter θ measuring the degree of inattention. Doing this we follow the models in Chetty et al. (2009), DellaVigna (2009), Finkelstein (2009), and Lacetera et al. (2012). Otherwise, we here make the same assumptions as in the framework under standard economic reasoning. A renter's monthly willingness to pay is now $V = P + (1 - \theta)\frac{C}{D}$. With $\theta \in (0, 1)$, the demand depends now to a smaller extent on the monthly equivalent

[15]Note, the reform does not change the service brought by the REA nor the value the landlord assigns to this service. Furthermore, as discussed in section 1, the REA does not provide any service to the renter; the renter gets in touch with the REA only after having found the apartment offer on his own search initiative.

of the commission ($\frac{C}{D}$) than on the rent (P). The parameter $\theta = 1$ would imply full inattention, while $\theta = 0$ would imply no inattention and the model would reduce to the standard economic model presented above. Since we now have

$$P = V - (1 - \theta)\frac{C}{D},$$

the reform is expected to raise P by $(1 - \theta)\frac{C}{D}$ (for apartments with a commission payable by the renter prior to the reform). Hence, this model predicts the reform-induced rent increase to be smaller than the standard reasoning model does.

In addition, in this framework, the reform changes landlords' decision criterion for appointing an REA. Before the reform, appointing an REA made landlords earn

$$\pi_{REA} = P_{REA} = V - (1 - \theta)\frac{C}{D},$$

while appointing no REA made them earn

$$\pi_{noREA} = P_{noREA} - \frac{E}{D} = V - \frac{E}{D}.$$

This implies that landlords used to appoint an REA if $E > (1 - \theta)C$. It means that landlords did not account for the full commission cost when deciding for or against an REA prior to the reform.

After the reform, as landlords now have to pay the commission directly, the decision criterion for appointing an REA is simply $E > C$. Hence, landlords with $(1 - \theta)C < E < C$ should have appointed an REA prior to the reform but will not do so after the reform. These cases constitute an inefficiently high demand for REA services prior to the reform as for these cases the cost of the REA service is larger than its benefit. The model predicts the reform to reduce this inefficiently high demand for REA services. Note, however, that the predicted decline in the number of appointed REAs is irrelevant for the prediction about the rent increase. The rent increase depends solely on the commission status prior to the reform. No matter what a landlord

spends the additional rent on after the reform (REA commission or marketing the apartment on his own), the model predicts the reform to raise P by exactly $(1-\theta)\frac{C}{D}$.

In the following sections we use a panel data set of offer rents to estimate the reform-induced rental price change and thereby estimate the parameter θ.

4 Data

4.1 Data Source and Panel Sample

The dataset used for our empirical analysis was provided by the firm Immobilienscout24, which offers one of the largest online real estate marketplaces for residential properties in Germany. Due to 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. Rent dynamics have been particularly strong in the selected cities (cf. Figure A1 in the Appendix) suggesting that demand is high. Moreover, Kholodilin (2012) provides evidence that rents in these cities tend to be underpriced compared to many other large cities in Germany and Europe. Thus, the supply side on the rental housing market is likely to be strong enough for landlords to be able to easily raise rents. If we can reject the standard reasoning hypotheses for these two cities, it is unlikely that we cannot do so in other regions in Germany.

Our dataset contains apartment offers for the time period between January 2012 to June 2016. For our difference-in-differences analysis we use a panel sample of apartments that have been advertised both before and after the implementation of the reform. Ads from the same apartment are identified based on the exact address (postal code, name of street, house number), floor level, size in square meters, and number of rooms.[16] In the key period after the reform

[16]We do allow missings in the variable 'floor level' because for apartments in one-floor buildings this variable has often kept blank by the advertiser. Overall, we are aware that our matching method might not create a perfect panel as it could be possible that two or more apartments of the same size and same number of rooms are located in the same floor of the same building. However, we assess this problem to be minor as most residential buildings

(June 2015 to June 2016) 22,931 different apartments have been advertised on the platform. Among these apartments we keep only those 4,554 apartments (19.9%) from which we also have an offer prior to the reform (i.e., between January 2012 and May 2015).[17] Only for those apartments, we know the treatment status, i.e., whether prior to the reform the apartments have been marketed with or without a commission payable by the renter. The panel of apartment ads allows us to analyze the rental price change from before to after the reform for both treatment and control group. Since we have to restrict our sample to apartments that have been advertised at least twice whithin a time span of five years, the sample is certainly not representative for all apartments in the two cities but selective: apartments with the tendency of having short rent durations are overrepresented. For the tested hypothesis this has the following consequences: standard economic reasoning predicts the reform-induced rental price increase to be stronger, the shorter the expected rent duration. Thus, the rental price increase should be even stronger and easier to detect in our sample than in a representative sample of apartments.

4.2 Variables and Summary Statistics

Table 1 reports summary statistics of all variables, both overall and by treatment status. Treated apartments are those that, prior to the reform, were offered with a commission payable by renters. These are 54% of the apartments in the sample. Control apartments are those that, prior to the reform, were offered without a commission payable by renters. The summary statistics in the table refer to the period prior to the reform, i.e., 01/2012–05/2015.

in Germany are not huge. Also, we can plausibly assume that in these cases, the two (or more) apartments are very similar or, at least, that differences between these apartments are not correlated with the status of being offered with or without a commission payable by the renter.

[17] For apartments offered more than once before or after the reform, we keep only the most recent offer.

Table 1: Summary statistics by treatment status

	Overall			Control			Treatment		
Variable	**Mean**	**Std. Dev.**	**N**	**Mean**	**Std. Dev.**	**N**	**Mean**	**Std. Dev.**	**N**
Main variables:									
Monthly rent (in euros)	845.437	537.849	4554	801.578	464.576	2114	883.436	591.541	2440
Monthly rent per sqm (in euros)	12.231	3.078	4554	11.852	3.025	2114	12.561	3.086	2440
Commission (dummy)	0.536	0.499	4554	0	0	2114	1	0	2440
Commission amount	1096.509	1458.236	4504	0	0	2114	2066.391	1415.32	2390
Commission amount imputed	1126.546	1470.229	4554	0	0	2114	2102.577	1407.867	2440
Apartment size (in sqm)	68.991	34.474	4554	67.663	30.606	2114	70.141	37.472	2440
Number of rooms	2.365	1.019	4554	2.380	0.972	2114	2.353	1.059	2440
Floor level	2.406	2.085	4320	2.401	2.204	2007	2.410	1.976	2313
Frankfurt (vs Stuttgart) (dummy)	0.728	0.445	4554	0.718	0.450	2114	0.737	0.440	2440
Apartment offer posted by:									
Private person	0.223	0.416	4501	0.463	0.499	2092	0.015	0.121	2409
Real estate agent	0.537	0.499	4501	0.177	0.382	2092	0.849	0.358	2409
Other commercial entity	0.240	0.427	4501	0.360	0.480	2092	0.136	0.343	2409
Further apartment characteristics:									
Number of bathrooms	1.340	0.604	3486	1.331	0.595	1531	1.347	0.612	1955
Balcony (dummy)	0.670	0.470	4090	0.693	0.462	1913	0.650	0.477	2177
Garden (dummy)	0.167	0.373	3518	0.177	0.382	1632	0.159	0.365	1886
Elevator (dummy)	0.393	0.489	3840	0.400	0.490	1744	0.388	0.487	2096
Cellar (dummy)	0.713	0.453	4447	0.713	0.452	2091	0.712	0.453	2356

Note: The summary statistics are reported for the period prior to the reform (i.e., 01/2012–05/2015). The abbreviation sqm stands for square meters. The variable 'commission amount' contains the commission indicated in the ad; the variable 'commission amount imputed' contains the commission amount calculated as 2.38 times the monthly rental price for apartments with commission. Data from Immobilienscout24, authors' calculations.

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. Comparing the asking prices in our dataset to official statistics about rents from the Federal Institute for Research on Building, Urban Affairs and Spatial Development (Bundesinstitut für Bau-, Stadt- und Raumforschung—BBSR) supports this view.[18] Overall, the average monthly rent in our data amounts to EUR 845, and 90% of all apartments are priced in the range of EUR 350 to EUR 1,870.

54% of the apartments claimed a commission from the renter prior to the reform (treatment group). The average commission claimed in the treatment group amounts to EUR 2,066. This is very close to the imputed commission of EUR 2,103 calculated as 2.38 times the average monthly rent.

Key apartment characteristics such as apartment size and number of rooms are consistent between our sample and official numbers: The Federal Statistical Office (Federal Statistical Office, 2015) 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 number in our dataset—2.4—excludes the kitchen. The mean apartment size in square meters has been found by the Federal Statistical Office to be 73 in Frankfurt and 78 in Stuttgart, in our dataset it is 69.

Apart from the information about apartment characteristics, our data also contains some information about the person or entity publishing the apartment ad. These data, however, are less precise as they have been saved in the course of the operating procedure of the firm Immobilienscout24: The firm assigned the ads to different staff members within the firm depending on the type of publisher. Some staff members took care of ads from private persons only (or foremost), others of ads from REAs, again others of ads from construction firms, etc. The information about the offerer in our dataset is based on this firm-intern assignment and thus

[18] In the official data the mean rent per sqm in Frankfurt in 2012 is EUR 10.8 (compared to EUR 11.4 in our data) and increases to EUR 12.8 in 2016 (compared to EUR 13.1 in our data). In Stuttgart it is EUR 9.5 in 2012 (compared to EUR 10.3 in our data) and increases to EUR 11.9 in 2016 (compared to EUR 12.6 in our data).

might—due to mis-assignment etc.—be of lower accuracy than other variables in the dataset. Nevertheless, we report summary statistics of these variables in Table 1. Overall, 54% of ads were posted by REAs, 85% among the treatment group and 18% among the control group. The occurrence in the control group could be explained by REAs paid by landlords as well as by measurement error in this variable. Overall, 22% of ads are posted by private persons, here the percentage is 1.5% in the treatment group (this number is certainly due to measurement error because private landlords were not allowed to raise a commission from renters) and 46% in the control group. A third group of advertisers constitutes 'other commercial entities'. These include foremost house constructing firms, but also firms in the financial sector. Commercial entities were allowed to claim commissions from renters. Overall, 24% of the ads are posted by other commercial entities, 14% among the treatment group and 36% among the control group.

Further apartment characteristics (number of bathrooms, availability of balcony, garden, elevator, and cellar) are also summarized in Table 1. They appear to be similar between treatment and control group.

5 Descriptive Analysis Over Time

5.1 Rental Prices

Figure 1 (a) illustrates the development of the mean rental price per square meter over time.[19] Overall, we see an increasing trend of rental prices from around EUR 11 per square meter in 2012 to around EUR 13 per square meter in 2016. At the time of the reform (indicated by the vertical line), if anything, rental prices are slightly attenuated. One reason for this decelerated trend could be related to the sample construction: Apartment offers prior to the reform have no restriction regarding prior observation, i.e., they include newly constructed apartments and those that have been extensively renovated. The sample of apartment offers after the reform, in contrast, cannot include newly constructed apartments as they are restricted to those apartments

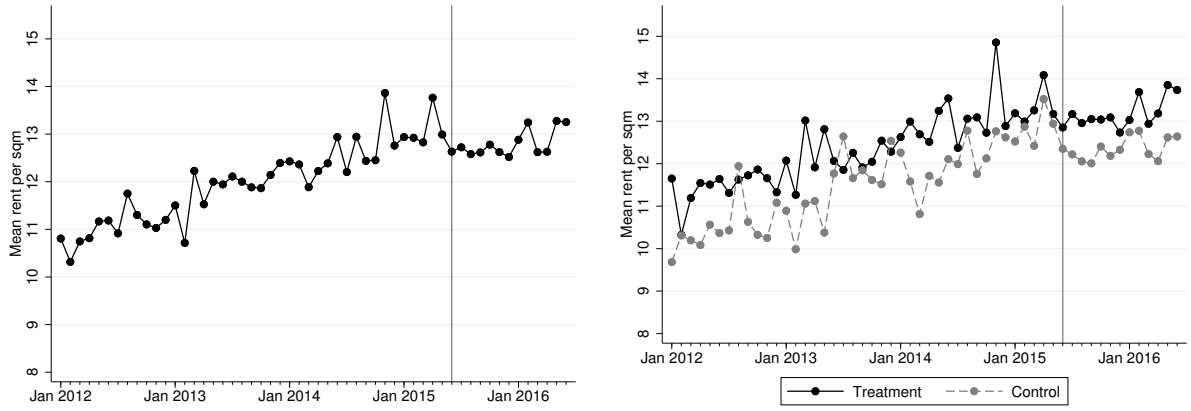
[19]The graph pools data from both cities, Frankfurt and Stuttgart, as the aim of this paper is not to discover differences between the two cities. The interested reader finds the graph plotted separately by city in Figures A2 and A3 in the Appendix.

that have been offered already between January 2012 and May 2015. Comparing the graph to the equivalent graph based on the original sample (i.e., before restricting to the smaller panel sample)—see Figure A4 in the Appendix—we do not see such a decelerated rental price trend at the time of the reform. Besides that, the overall trend in rents is not of primary importance because the identification of the reform effect (or better: the inattention effect) relies on the difference in the development of treatment and control group. This is considered in the following.

Figure 1 (b) shows the development of mean monthly rental prices by treatment status. Treated apartments are those that were, prior to the reform, offered with a commission payable by the renter. Control apartments were, prior to the reform, offered without a commission payable by the renter. After the reform, all apartments are forced to be offered without a commission payable by renters. Rental prices of control apartments appear to be always slightly lower than rental prices of treated apartments. This is opposite to the prediction by standard economic reasoning saying that apartments with a commission payable by renters should have a lower monthly rent. The observed price difference could, however, be due to different apartment characteristics, even those unobserved in the data, in particular quality of the particular location, but also condition of the building, room layout, daylight in the apartment, quality of apartment facilities such as bathroom facilities, etc.). Due to the panel structure of our data we are able to control in our estimations for all time-invariant unobserved characteristics by including apartment fixed effects (see Section 6).

Besides the level, the overall trend of both treatment and control group seems to be roughly similar over the whole time span. The relatively strong fluctuation in the graph is due to the fact that each point prior to the reform is based on a sample size of only around 20-100 observations. And apartments (both within the treatment group and within the control group) are heterogeneous in a number of characteristics, observable and unobservable.

Figure 1: Mean monthly rent per square meter over time



(a) Overall

(b) By treatment status

Note: Treated apartments are those that, prior to the reform, were offered with an REA commission payable by the renter. Control apartments are those that, prior to the reform, were offered without a commission payable by the renter. The vertical line marks the time when the law "principle who orders pays" became effective (June 2015). The sample includes 9,108 ads from 4,554 apartments. Data from Immobilienscout24, authors' calculations.

5.2 Volume

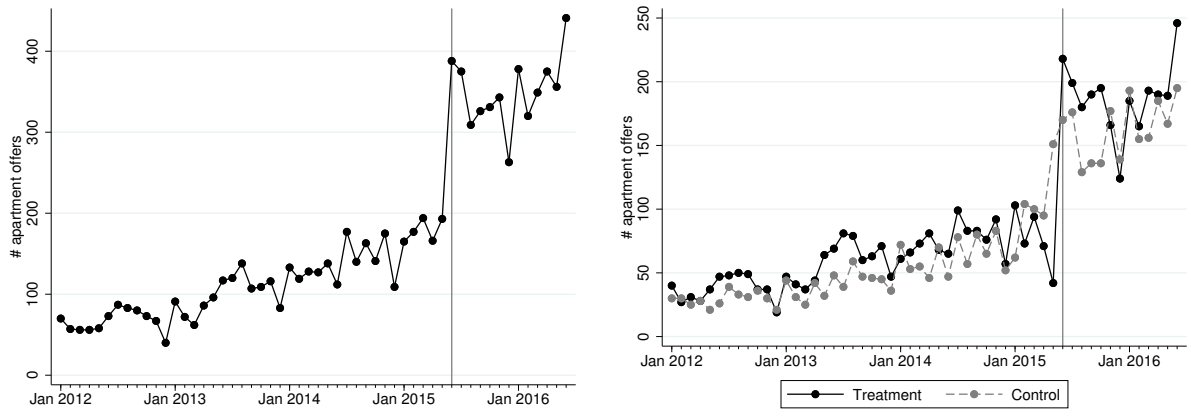
Figure 2 (a) plots the number of apartment ads per month.[20] The volume increasing sharply at the time of the reform is an artefact of the construction of the panel dataset: we keep in the dataset only those apartments from which we have an ad after the reform and match these to ads prior to the reform from the same apartment. This implies that the total number of ads after the reform—spread over the period of June 2015 to June 2016—is equal to the total number of ads prior to the reform—spread over the longer time period of January 2012 to May 2015. In the original sample (i.e., before restricting to the smaller panel sample) there is no such jump at the time of the reform (see Figure A7 in the Appendix). Figure 2 (b) plots the number of apartment ads separately by treatment and control group. In most months, the number of ads posted by treated apartments is slightly larger than the number of control apartments. Only a few months before the reform, the number of ads from treated apartments falls below that of control apartments. This could be due to an anticipation effect: Landlords and REA's posting an apartment ad a couple of months before the reform know that the rental contract with the

[20]The interested reader finds the graphs plotted separately by city can be found in the Appendix, Figures A5 and A6.

new renter is likely to be concluded after the reform; and if this is the case, REAs cannot raise money from the renter. Therefore, these apartments are posted without a commission from the renter. This implies that these apartments are assigned to the control group in our sample. This anticipation effect leads to a similar pattern after the reform, as we keep in the sample only those apartments posted again after the reform: those apartments posted shortly before the reform are unlikely to appear in the sample shortly after the reform, but more likely to appear in a later month. This is why, after the reform, the number of ads from the control group increase over time relative to the number of ads from the treatment group. The discussed effect has implications also for the mean rental price over time as the sample selection (into treatment and control group) changes shortly before the reform period. However, in our estimations these effects are controlled for as we include in the model apartment fixed effects.

If the reform affected the REA decision even before the exact time of the reform, this does not affect the prediction based on the standard reasoning hypothesis about the effect on rental prices. Also, robustness tests with respect to potential anticipation effects (see Section 7) suggest that our results are not sensitive to any kind of anticipation effect.

Figure 2: Number of apartment offers over time



(a) Overall

(b) By treatment status

Note: Treated apartments are those that, prior to the reform, were offered with an REA commission payable by the renter. Control apartments are those that, prior to the reform, were offered without a commission payable by the renter. The vertical line marks the time when the law "principle who orders pays" became effective (June 2015). The sample includes 9,108 ads from 4,554 apartments. Data from Immobilienscout24, authors' calculations.

5.3 Advertisers

As explained in Section 4.2 we have some information about the advertisers behind the apartment ads; an ad can be initiated by an REA, a private person, or some other commercial entity (house construction industry mostly). Standard economic reasoning would predict the share of ads coming from REAs versus private persons not to change at the time of the reform. It would predict apartments that, prior to the reform, were marketed by REAs claiming a commission from the renter, to continue to be marketed by REAs. The commission after the reform would be paid by the landlord but the rental price would increase accordingly. As landlords would be able to raise a higher rent from renters (exactly such that landlords can finance the REA commission), there is no reason to change the marketing strategy from REA to private marketing.[21] The inattention hypothesis, in contrast, predicts some landlords to stop appointing REAs, namely those landlords for whom $(1-\theta)C < E < C$ (see Section 3). To check our data regarding the consistency with either hypothesis, Figure 3 plots the share of ads posted by (a) REAs, (b) private persons, and (c) other commercial entities over time for treatment and control group separately.

From figure (a) we see that, prior to the reform date, the large majority of ads in the treatment group are posted by REA's, i.e., 80-90%. The percentage sharply decreases at the time of the reform to around 60%. Hence, a significant number of landlords that previously appointed REAs stop to do so at the time of the reform. This is consistent only with the inattention hypothesis but not with the standard reasoning hypothesis. Within the control group, the share of ads posted by REAs is much smaller, around 10-20%. These are ads from apartments that are marketed by REAs but the commission being paid by the landlord, even prior to the reform. The share does not change substantially at the time of the reform. This is consistent with our expectation as these apartments are not affected by the new law, they acted in accordance to the new law even prior to the new law becoming effective.

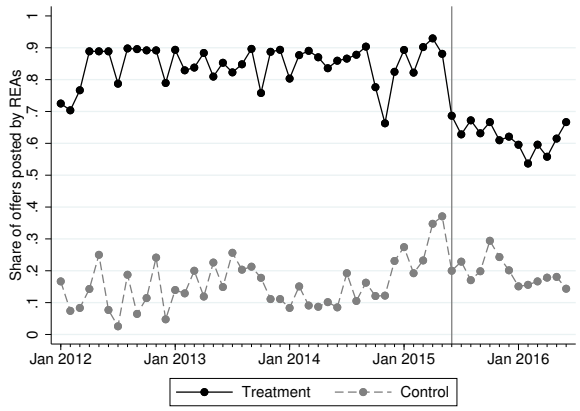
Figure 3 (b) shows the other side of the coin: Prior to the reform date, basically zero percent of ads in the treatment group are posted by private persons. This is consistent with the (old) law

[21] At least, this is true if landlords do not face stronger credit constraints than renters do—which seems a plausible assumption.

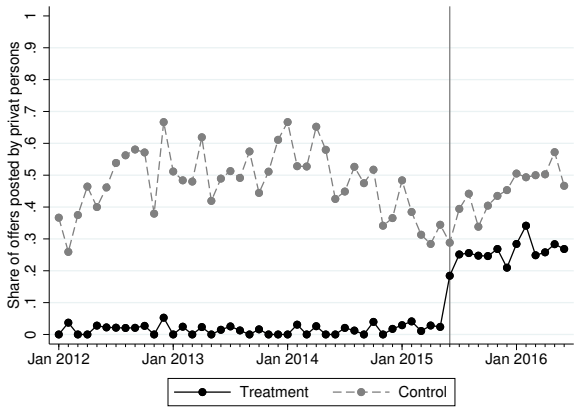
prohibiting private persons to claim a provision from the renter. At the time of the reform, the share jumps to around 20-30%. This means, that 20-30% of those apartments that claimed a commission from the renter prior to the reform (i.e., those apartments that were marketed by an REA or some other commercial entity), are marketed privately after the reform. A substantial share of landlords in the treatment group thus change their marketing strategy at the time of the reform. Landlords in the control group, in contrast, do not change their marketing strategy at the time of the reform. The same is true for other commercial entities as can be seen from Figure 3 (c): for both treatment and control group there is no significant change in the share of ads posted by other commercial entities. Hence, the data of advertisers is consistent with the inattention hypothesis rather than with the standard economic hypothesis.

Note that the standard economic hypothesis predicts the rental price of apartments in the treatment group to increase, irrespective of whether landlords change their marketing strategy after the reform. What determines the rental price increase is the commission paid by the renter prior to the reform. Said differently, the commission paid by the renter prior to the reform determined the amount that landlords had to reduce the rent earlier. The reform terminates these rent reductions as it stops renters paying REA commissions. The reform-induced rental price change is what we explore in the following estimation analysis.

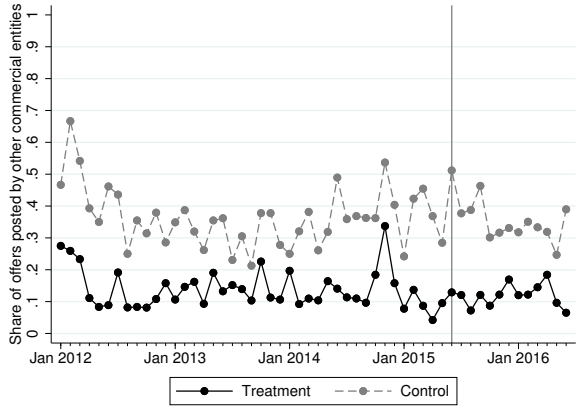
Figure 3: Share of apartment offers posted by REAs, private persons, and other commercial entities — over time



(a) REAs



(b) Private persons



(c) Other commercial entities

Note: The graph shows, separately for treatment and control group, for each month the share of apartment offers posted by REAs (a), private persons (b), and other commercial entities (c) among all apartment offers, respectively. Treated apartments are those that, prior to the reform, were offered with an REA commission payable by the renter. Control apartments are those that, prior to the reform, were offered without a commission payable by the renter. The vertical line marks the time when the law "principle who orders pays" became effective (June 2015). The sample includes 9,032 ads from 4,547 apartments. Data from Immobilienscout24, authors' calculations.

6 Estimation Strategy

To identify the causal effect of the legal reform "principle who orders pays" on rental prices of concerned apartments and thus estimate the degree of inattention, we apply a difference-in-differences approach. According to the standard reasoning argument, the reform affects only apartments for which renters had to pay a commission prior to the reform (treatment group). Apartments for which renters never paid a commission should not be affected and hence serve as control group. The equation we estimate is the following:

$$ln(P_{it}) = \beta_i + \gamma R_t + \delta T_i R_t + D_t + \varepsilon_{it}, \tag{2}$$

where P_{it} is the monthly rental price of apartment i offered at time (month) t and β_i is an intercept term that is allowed to vary between apartments (apartment fixed effects). R_t is a dummy variable taking on the value 1 if the offer is published after the reform (i.e., after June 2015) and 0 otherwise. T_i is the treatment indicator, which is included only in the interaction with R_t; the main effect of T_i is absorbed by the apartment fixed effects. D_t captures the time trend, in our main specification it is restricted to linearity. In robustness tests we relax the linearity restriction and find that the results are not sensitivity to this assumption (see Section 7). ε_{it} in equation 2 captures an idiosyncratic error term.

The coefficient of main interest in our estimation is δ. Standard economic reasoning (null hypothesis) predicts δ to be equal to 0.0198 (assuming the expected rent duration to be equal to ten years) and thus the inattention parameter θ from Section 3 to be equal to zero. Rejecting the null hypothesis and finding that $\delta < 0.0198$ (and thus $\theta > 0$) is evidence for the inattention hypothesis. In the extreme case of full inattention we should find $\delta = 0$, thus inferring $\theta = 1$.

Applying the outlined diff-in-diffs strategy ensures to disentangle between the reform effect and other time effects at the date of the reform. Without a control group, one could estimate the reform effect merely as the rental price change at the time of the reform for the average apartment. First, this would attenuate the results since there is no reform-induced rent increase predicted for untreated apartments. Second, accounting for the time trend in a linear way—

or even including different polynomials—would never perfectly capture the time effect, which can have a ragged shape. This could lead to distortion. In contrast, estimating δ as we do by equation 2 accounts for any overall time effect that is not specific to either treatment or control group. Our identifying assumption is thus that, at the time of the reform, there is no other shock affecting rental prices of treatment and control apartments differently.

7 Estimation Results

Our main results are reported in Table 2. The difference-in-differences estimator δ based on equation 2 appears in the first row. Column (1) contains the results for the full sample. Based on a Wald test we reject the null hypothesis H_0: $\delta \geq 0.0198$ (standard reasoning hypothesis when rent duration is ten years) at any conventional significance level. This means that we reject the hypothesis of the reform making rents of treated apartments increase by 1.98%. This speaks in favor of the inattention hypothesis stating that the rental price increase is smaller or even zero. Given that the δ coefficient is -0.0013, i.e., close to zero and not significantly different from zero, we cannot reject the hypothesis of the reform not having increased rental prices at all.

Table 2: Estimation of log monthly rent

	(1) Full sample	(2) Frankfurt only	(3) Stuttgart only	(4) Apartment size < median	(5) Inner-city districts only
Reform × commission	-0.0013	-0.0000	-0.0032	-0.0011	-0.0014
	(0.0037)	(0.0043)	(0.0072)	(0.0051)	(0.0062)
P-value from testing H_0: $\delta \geq 0.0198$	<0.001	<0.001	<0.001	<0.001	<0.001
# Offers	9108	6634	2474	4510	4622
# Apartments	4554	3317	1237	2255	2311

Note: Least squares estimations of the dependent variable ln(monthly rent). The first row contains the estimated coefficient related to the interaction between the reform dummy and the treatment indicator, i.e., the estimated δ from equation 2. All models further include apartment fixed effects, the reform dummy (taking on the value one if the offer is published in June 2015 or later and zero otherwise), and a linear time trend. Standard errors in parentheses are clustered at the apartment level. Data from Immobilienscout24 2012–2016, authors' calculations.

Translated to the θ parameter of the sufficient statistic introduced in section 3, we reject the null hypothesis $H_0 : \theta = 0$ and we cannot reject the hypothesis of full inattention paid to REA commissions payable by renters, $H_1 : \theta = 1$. Given that in our benchmark specification δ is

estimated to be -0.0013, setting $D = 120$, the point estimate for θ results to be 1.07.[22] The 95% lower confidence bound for θ is 0.76. This suggests a substantial degree of inattention.

Comparing to earlier studies about consumer inattention in other contexts, the size of our inattention parameter is consistent: Brown et al. (2010) cannot reject the hypothesis of full inattention to shrouded shipping charges on Ebay; Chetty et al. (2009) find nearly full inattention to nontransparent sales taxes based on a natural experiment on alcoholic beverages (θ=0.94) and an inattention parameter of 0.65 based on a field experiment on cosmetics. Certainly, inattention is likely to be context-dependent (as pointed out by Lacetera et al. (2012)) and should be compared with caution. Evidence about more high-stakes decisions and in an area related to that in our study is provided by Bradley (2017). He studies inattention to imperfect capitalization of property taxes into home prices. He finds an inattention parameter of close to one, resulting in an overpayment of roughly $10,000 equal to about 5% of the median sale price. In our rental market context, we find people being inattentive to the REA commission, which is equal to 2.38% times the monthly rental price, i.e., in our data at mean more than EUR 2,000.

The estimated degree of inattention crucially depends on the mean expected rent duration that we assume. Instead of setting $D = 120$ (10 years), setting $D = 60$ (5 years), the point estimate of θ results to be 1.03 with a 95% lower confidence bound at 0.88; setting $D = 180$ (15 years), the point estimate of θ results to 1.10 with a lower bound at 0.64. Figure 4 shows θ as a function of D, given our benchmark estimate for δ. It illustrates that varying the assumption about the mean expected rent duration within a plausible range always results in an inattention parameter of substantial size. Only when setting $D \geq 496$ months (41 years), we can no longer reject the null hypothesis of zero inattention. A duration of 41 years, however, is far longer than what the mean expected rent duration can plausibly be, especially given that in our sample short-term rented apartments are over-represented and given the fact that the expected rent duration is systematically shorter than the mean rent duration, as outlined in Section 2.

[22] This is the result from solving $-0.0013P = (1-\theta)\frac{C}{D}$, plugging in $C = 2.38P$ and $D = 120$.

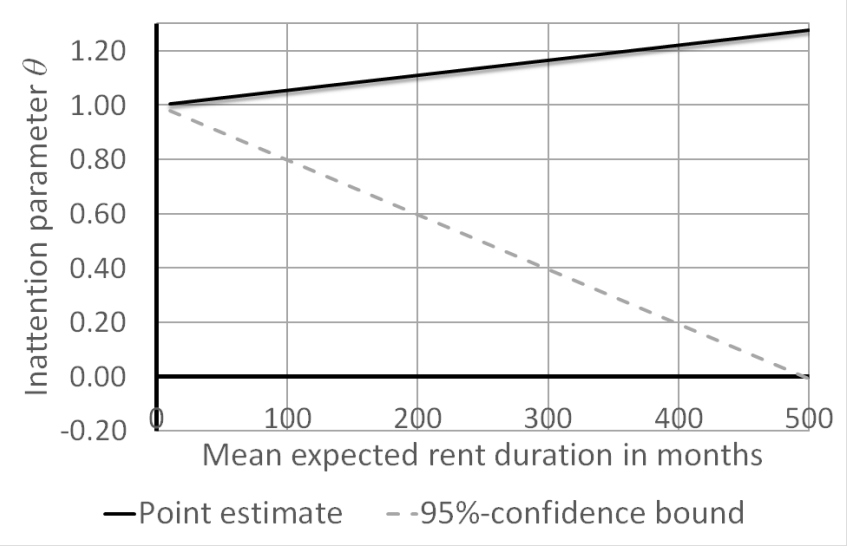


Figure 4: Estimated inattention parameter θ as a function of the mean expected rent duration D

Note: The figure illustrates the estimated inattention parameter based on the benchmark estimation reported in Table 2 column (1).

In the formulation of our benchmark null hypothesis we set the interest rate to zero. This is close to reality at that time. Nevertheless, if we release the zero-interest assumption and allow for a positive interest rate, the rent increase predicted by the standard reasoning hypothesis is even higher and the hypothesis is rejected even more "easily". A similar reasoning applies if we allow for consumer myopia, i.e., consumers discounting future payments by a rate larger than the actual interest rate.[23] In this case the standard reasoning prediction would postulate an even higher price increase than 1.98% and thus be more likely to be rejected. The same is true if renters have credit constraints reducing their ability to afford the lump sum REA commission at the beginning of the rent contract period.

In order to check whether our results differ between the two cities in our sample, we report results from separate estimations in columns (2) and (3) of Table 2. The δ coefficients result to be close to zero and insignificant and we can reject $H_0 : \delta \geq 0.0198$ at any conventional significance level for either city.

Locations and apartments are very heterogeneous. We were concerned about the possibility that outliers or certain groups of apartments due to irregular pricing trends prevent us from identifying the reform-induced rent increase. In a further step, we therefore focus our investigation on apartments for which standard reasoning predicts the reform-induced rent increase to be particularly strong.

First, these are small apartments because small apartments on average have a higher renter turnover than large apartments; this means, that mean expected rent duration (D) should be smaller and thus the reform effect should be stronger. In the estimation reported in column (4) of Table 2 the sample is restricted to apartments smaller than 63 square meters (smallest 50% of the sample). The result is very similar to that from the benchmark specification; not even for small apartments we find reform-induced rental price increases.

Second, another group of apartments that usually has a high renter turnover is apartments in inner-city districts. Also, rental price dynamics in inner-city districts are often argued to be particularly strong. Thus, increasing rents should be particularly easily feasible for apartments

[23] In the context of car purchases, Busse et al. (2013) find that consumers are not myopic about future spendings for fuel. Allcott and Wozny (2014), in contrast, do find evidence for consumer myopia.

in these areas. Results from estimations based on the sample restricted to apartments located in inner-city districts[24] are reported in column (5) of Table 2. Even here, we do not find a rent increase and we have to reject the standard reasoning hypothesis in favor of the inattention hypothesis.

We perform a series of further analyses to test the robustness of our results with respect to a number of concerns. The first concern is that professional REAs have better knowledge about market rents compared to (private) landlords. This implies that REAs can offer apartments at a price that comes closer to the maximally possible market rent for the relevant apartment. This could be another reason why apartments in the treatment group have higher rental prices than apartments in the control group. We call this effect the consultancy premium. Landlords who stop appointing an REA at the time of the reform lose the consultancy premium, i.e., after the reform these landlords suddenly lack expert knowledge about market prices and thus ask prices below the apartments' potential. This could be a reason for the reform-induced price increase to be smaller than otherwise expected. In column (1) of Table 3 we address this concern by reducing the sample in the following way: within the treatment group we include only those apartments that have post-reform offers posted by REAs. We thus exclude within the treatment group all apartments that are offered by private persons after the reform. This keeps the consultancy premium constant between before and after the reform. As can be seen in Figure 3 above, only a minor share of landlords in the treatment group quit appointing REAs at the time of the reform (26%). Those who continue appointing an REA after the reform have to pay the commission themselves. Our results based on the described restricted sample turns out to be robust: Even those landlords who decided to continue appointing REAs after the reform do not increase rental prices. This means that we reject the standard reasoning hypothesis, while not being able to reject the inattention hypothesis implying no rental price change compared to the control group.

[24]The assignment of districts to the inner-city area is done using the definition of the city administrations. According to this classification, 51% of the apartments in our sample are located in inner-city districts.

Table 3: Estimation of log monthly rent — robustness tests

	(1) Drop if treated switch to private	(2) Drop June 2015	(3) Drop June+ July 2015	(4) Drop March-May 2015	(5) Drop Dec 2014-May 2015	(6) Control for rent control	(7) Drop ≥ Oct 2015	(8) Interactions location–time
Reform × commission	0.0003	-0.0012	0.0002	-0.0030	-0.0045	-0.0012	-0.0081	0.0017
	(0.0039)	(0.0039)	(0.0042)	(0.0040)	(0.0043)	(0.0037)	(0.0061)	(0.0037)
P-value from testing H_0: $\delta \geq 0.0198$	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001
# Offers	7844	8332	7582	8002	7100	9108	2796	9108
# Apartments	3922	4166	3791	4001	3550	4554	1398	4554

Note: Least squares estimations of the dependent variable ln(monthly rent). The first row contains the estimated coefficient related to the interaction between the reform dummy and the treatment indicator, i.e., the estimated δ from equation 2. All estimation models further include apartment fixed effects, the reform dummy (taking on the value one if the offer is published in June 2015 or later and zero otherwise), and a linear time trend. Model (6) further includes a dummy variable for rent control. Model (8) further includes interactions between the time trend and indicators of location quality (in quartiles for each city). Standard errors in parentheses are clustered at the apartment level. Data from Immobilienscout24 2012–2016, authors' calculations.

Table 4: Estimation of log monthly rent — various specifications of the time trend

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reform × commission	-0.0013	-0.0013	-0.0011	-0.0012	-0.0018	-0.0032	-0.0023
	(0.0037)	(0.0037)	(0.0037)	(0.0037)	(0.0037)	(0.0036)	(0.0038)
Time trend t	0.0025***	0.0021***	0.0032***	0.0057**	0.0025***	0.0050*	
	(0.0002)	(0.0004)	(0.0012)	(0.0028)	(0.0002)	(0.0027)	
t^2		0.0077	-0.0345	-0.2243		-0.1477	
		(0.0078)	(0.0456)	(0.1908)		(0.1807)	
t^3			0.0047	0.0555		0.0322	
			(0.0050)	(0.0494)		(0.0459)	
t^4				-0.0044		-0.0025	
Seasonal dummies	No	No	No	No	Yes	Yes	No
Monthly dummies	No	No	No	No	No	No	Yes
P-value from testing H_0: $\delta \geq 0.0198$	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001	<0.001
# Offers	9108	9108	9108	9108	9108	9108	9108
# Apartments	4554	4554	4554	4554	4554	4554	4554

Note: Least squares estimations of the dependent variable ln(monthly rent). The first row contains the estimated coefficient related to the interaction between the reform dummy and the treatment indicator, i.e., the estimated δ from equation 2. All estimation models further include apartment fixed effects and the reform dummy (taking on the value one if the offer is published in June 2015 or later and zero otherwise). Standard errors in parentheses are clustered at the apartment level. Data from Immobilienscout24 2012–2016, authors' calculations.

A second concern could be that the reform effect did not step in immediately but took some time to adjust. In alternative estimations in Table 3, we therefore drop all observations of offers published in the first month (column (2)) or in the first two months (column (3)) after the date the reform became effective. The results appear to be very similar to our baseline results.

A third concern relates to the possibility of anticipation effects based on the announcement of the law reform "principle who orders pays". The law was passed in the parliament in March 2015. Theoretically, there is no economic reason (no standard economic argument) why the pure announcement of the reform should immediately affect rental prices. Also, there is no economic reason for landlords to stop appointing REAs at the time of the announcement instead of waiting until the time of the reform becoming actually effective. And even if some landlords did so, this would simply mean that their apartments are counted as control apartment in our analysis. Standard economic reasoning would still predict treated apartments to raise rental prices relative to control apartments at the time of the reform. Nevertheless, to test our results for robustness against anticipation effects, we drop all observations from the date of announcement of the reform to the date of the reform becoming effective, i.e., observations from the first three months prior to the reform (March to May 2015) from the sample (column (4) of Table 3). The

estimation results do not change compared to our benchmark specification. The same is true if we drop all observations in a 6-month range prior to the reform, i.e., Dec 2014 to May 2015 (column (5) of Table 3).

Fourth, we address a concern related to rent control. Raising rental prices for new contracts was possible in Germany without restrictions during the main period under consideration.[25] Regulations of rents in new contracts have been introduced only with the rent control law (*"Mietpreisbremse"*) that became effective in Frankfurt only for some districts[26] and only on November 27, 2015, and in Stuttgart only on November 1, 2015. In general the rent control law does not apply to apartments in new buildings (built after 2014). Empirical studies found that the effect of the rent control law on rents was rather moderate (Kholodilin et al., 2016; Mense et al., 2017; Thomschke, 2016). Nevertheless, we test the sensitivity of our results with respect to the introduction of the rent control law: First, we include a control variable for apartment offers that might be affected by the rent control (column (6) of Table 3).[27] Second, we drop from the sample all apartment ads published in the period affected by the rent control law (column (6) of Table 3). The results are very similar to our baseline results, the point estimates even remain negative. Thus, our results are unlikely to be driven by the rent control law.

A fifth concern relates to differences in apartment characteristics in treatment and control group. The main apartment characteristics included in the ads seem to be fairly similar between the groups (see Table 1). However, checking the quality (popularity) of locations we indeed find some differences: Deviding postal code areas into quartiles according to the average rent per square meter (separately by city), we find for example that 19% of treated apartments are located in the most valuable locations (highest quartile) of Frankfurt, while this is true for only 11% of control apartments. Differing characteristics in treatment and control group do not automatically endanger the identification of a treatment effect because we control for all time invariant apartment characteristics by including in our estimation model apartment fixed effects. Our identification strategy would be endangered, however, if the value of these characteristics

[25] In contrast, rent increases for existing contracts are regulated.

[26] Districts that were decided by the local government not to be subject to the rent control law are Berkersheim, Eckenheim, Harheim, and Unterliederbach.

[27] In the sample after the reform, 55% of the apartments were potentially affected by the rent control law.

evolved differently over time, specifically between before and after the reform. Given the pretty short time span considered in our data (4.5 years) this is unlikely to be a serious problem. Nevertheless, to test the robustness of our results we include in our model interaction terms between the quality of location and the time trend. The results remain stable: we still find a zero rental price increase and cannot reject the hypothesis of full inattention (column (8) of Table 3).

Finally, we check the robustness of our results with respect to the specification of the time trend. Note the clearly positive trend in rental prices that can be seen, e.g., in Figure 1. In our benchmark specification, we account for the time trend in a linear way. In order to test the robustness of our results with respect to the linear specification, we stepwise include the second, the third, and the fourth polynomial of t in the model (Table 4, columns (2) to (4); for convenience, column (1) repeats the benchmark specification that was also reported in column (1) of Table 2). We find the regression coefficients related to the polynomials of order 2 to 4 to be statistically insignificant (also jointly) and thus conclude that the linear specification is appropriate to capture the time trend. More importantly, the estimate of δ is robust: it is always close to zero and insignificant in either specification and the null hypothesis H_0: $\delta \geq 0.0198$ is again rejected at any conventional significance level. Allowing for seasonal effects (by adding twelve dummy variables for calender months) instead of the polynomials (column (5)) or in addition to the polynomials (column (6)) does not change the picture. Including dummy variables for each single month in the sample (column (7)), i.e., the most flexible specification of the time trend possible, does not either.

To sum up, we do not find evidence for a reform-induced rental price increase. For any expected rent duration of less than 41 years, we reject the hypothesis of a rental price increase predicted by standard economic reasoning. In contrast, we even cannot reject the hypothesis of a zero rent increase which would imply full inattention. The lack of landlords passing the burden of the commission back to renters via increased monthly rents is likely to be due to the fact that renters, prior to the reform, did not take into account the REA commission in their rental decision, i.e., they were inattentive to the amount of the REA commission when making

the rental decision. This inattention implies that landlords appointing an REA prior to the reform (and making renters pay the commission) did not bear the (full) cost of the REA service. And this in turn implies that, prior to the reform, the quantity of REA service consumed by landlords was higher than socially optimal. Certainly, some landlords will always have reasons for not appointing an REA even in the case that the cost was zero for them. One reason, for example, might be that some landlords might want to select their renters personally rather than transferring the (pre-)selection to an REA (that might have different incentives, e.g. with respect to rent durations). Finally, the reform not only unburdened renters but also improved market efficiency by reducing the inefficiently high level of REA service consumed.

Certainly, this conclusion only holds under the assumption that REAs do not improve market efficiency. This could happen, in particular, if REAs shortened the time of rental offers on the market and thus lowered the vacancy rate. However, in many big cities vacancy rates of rental housing are minuscule and thus there is little room for REAs to accelerate the marketing process. In the two cities considered in this paper, vacancy rates were around 1% or below both before and after the reform in 2015. According to data from the Federal Institute for Research on Building, Urban Affairs and Spatial Development[28] the vacancy rate of residential housing in Frankfurt was between 0 and 1% in the years 2014 to 2016; in Stuttgart, the rate was between 1 and 2% in 2014 and between 0 and 1% in the years 2015 and 2016. This is consistent with data from Empirica Regionaldatenbank[29] reporting vacancy rates for Frankfurt of 0.6% and 0.5% in the years 2014 and 2016, respectively; in Stuttgart the numbers are 1.0% and 0.7% for the years 2014 and 2016, respectively. These numbers suggest that the decrease in REA services consumed did not increase vacancy rates. Hence, the law under consideration is likely to have improved market efficiency.

[28] Bundesinstitut für Bau-, Stadt- und Raumforschung (BBSR).
[29] *CBRE-empirica-Leerstandsindex.*

8 Conclusion

We have shown that agents on the rental housing market are inattentive to real estate agent commissions payable by renters. Analyzing panel data of rental housing offers and exploiting a legal reform that shifted the payment liability of the REA commission from renters to landlords, we found the REA commission payable by renters not to affect rental prices, implying inattention. Based on a sufficient statistic approach we have estimated the inattention parameter to be close to one with the lower bound (95% CI) being 0.76 in our benchmark specification. Our findings are robust to a number of sensitivity checks with respect to mean expected rent duration, consultancy effects, adjustment effects, anticipation effects, and the specification of the time trend.

The contribution of this paper was to investigate whether inattention as described exists, to measure its degree, and to unveil its implications in the rental housing context. With that purpose, we follow Gabaix (2019) in using the term inattention to refer to the behavioral bias pinned down based on revealed choices (cf. "effective attention", Gabaix, 2019, p. 34). A limitation of this study is that we did not investigate the exact psychological mechanisms underlying inattention in this context, i.e., whether it is due, for example, to lacking salience or to mental accounting.[30] Though, major implications of our findings are unrelated to the exact mechanisms underlying inattention in this context.

Our findings have the following implications: First, without regulating the payment liability and with landlords making renters pay the REA commission, landlords do not (fully) internalize the cost of the REA service they consume. This leads to the quantity of REA services consumed to be higher than the socially optimal level. Simply regulating the payment liability of REA commissions thus can improve the market efficiency—unlike suggested by standard economic reasoning. Second, the consequences of inattention are asymmetric for the two market sides. While the supply side (landlords) benefits, the demand side (renters) suffers—even

[30]For seminal work on mental accounting see (Thaler, 1980, 1985, 1999). 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) transfered. If renters, for example, assign the monthly rents to the mental account, say, "housing expenditure" while assigning the REA commission to the mental account, say, "moving expenditure", they would not make the two cost types comparable and not take them equally into account during the rental housing decision.

in the case that both are equally inattentive. With landlords making renters pay the REA commission, landlords consume the REA service at the expense of renters. This is important to disclose, in particular in light of the fact that renters are typically less wealthy than landlords and expenditure for rental housing makes up an important share of renters' available income, especially for poor households.

The situation analyzed here is not unique to Germany but similar in a number of other countries. Hence, the reform would have similar effects in many countries. More generally, we conclude that a policy regulating payment liabilities can have economic effects, both for allocation and distribution—unlike stated by standard economic reasoning. It is thus important to take into account behavior that deviates from standard economic assumptions when designing and evaluating similar policy measures.

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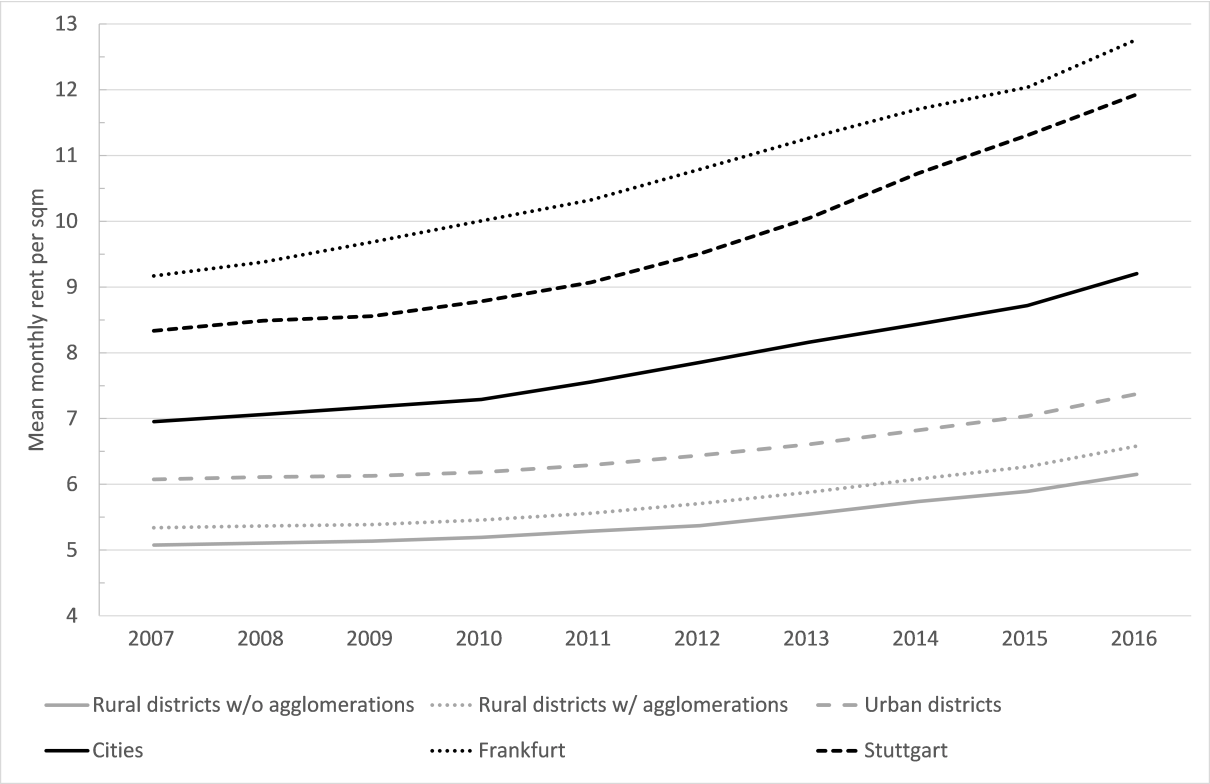
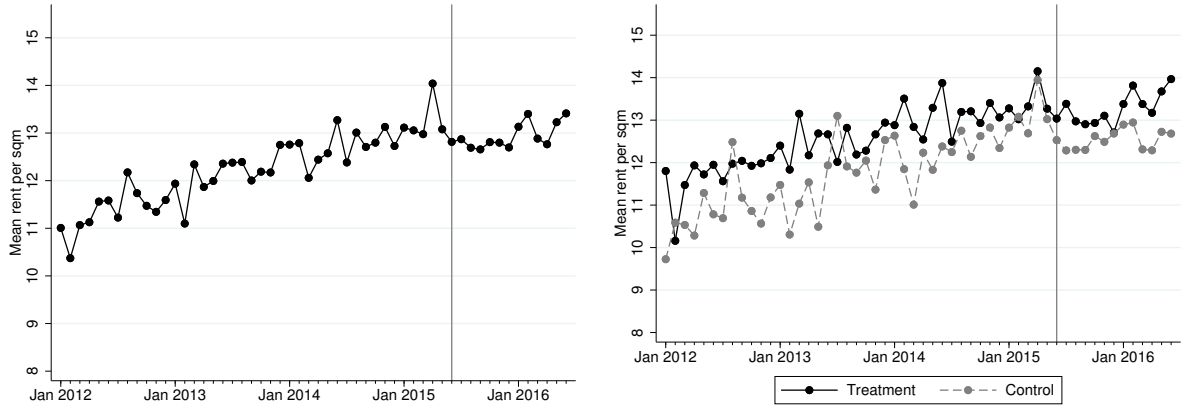
Appendix

Figure A1: Development of monthly rental prices per square meter (offer prices) by type of district, West Germany, 2007–2016



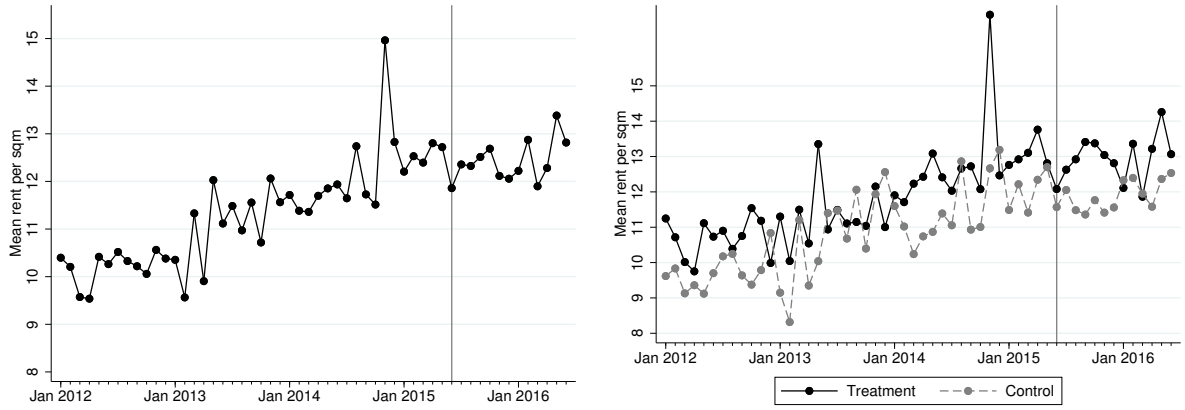
Note: Own graph based on data from the Federal Institute for Research on Building, Urban Affairs and Spatial Development (*Bundesinstitut für Bau-, Stadt- und Raumforschung—BBSR*). The types of district are administratively defined (*"Siedlungsstrukturelle Kreistypen"*).



(a) Overall (b) By treatment status

Figure A2: Mean monthly rent per square meter over time — Frankfurt

Note: Treated apartments are those that, prior to the reform, were offered with an REA commission payable by the renter. Control apartments are those that, prior to the reform, were offered without a commission payable by the renter. The vertical line marks the time when the law "principle who orders pays" became effective (June 2015). The Frankfurt sample includes 6,634 ads from 3,317 apartments, data from Immobilienscout24, authors' calculations.

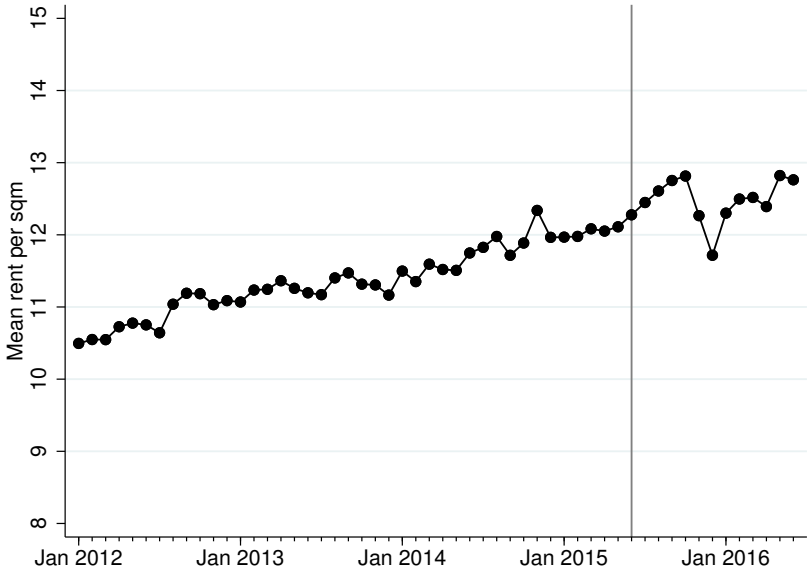


(a) Overall (b) By treatment status

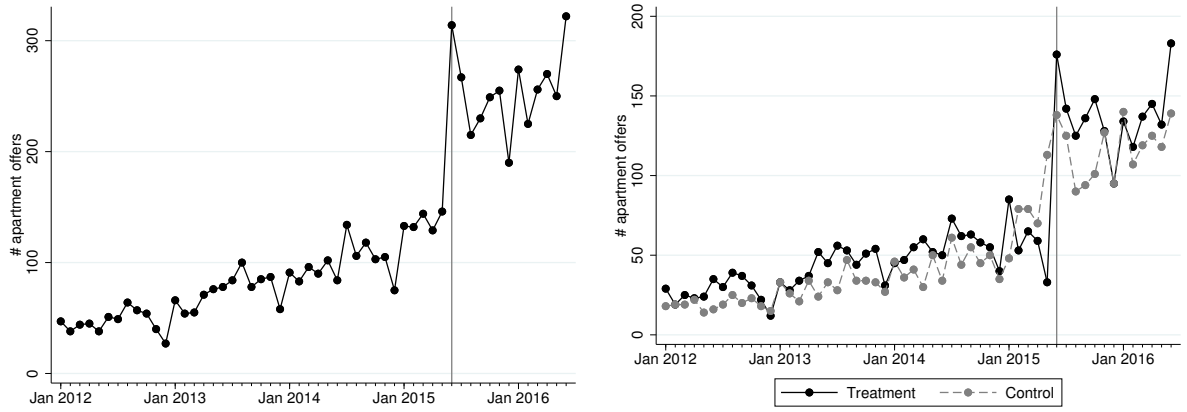
Figure A3: Mean monthly rent per square meter over time — Stuttgart

Note: Treated apartments are those that, prior to the reform, were offered with an REA commission payable by the renter. Control apartments are those that, prior to the reform, were offered without a commission payable by the renter. The vertical line marks the time when the law "principle who orders pays" became effective (June 2015). The Stuttgart sample includes 2,474 ads from 1,237 apartments, data from Immobilienscout24, authors' calculations.

Figure A4: Mean monthly rent per square meter over time — original sample (not restricted to panel sample)



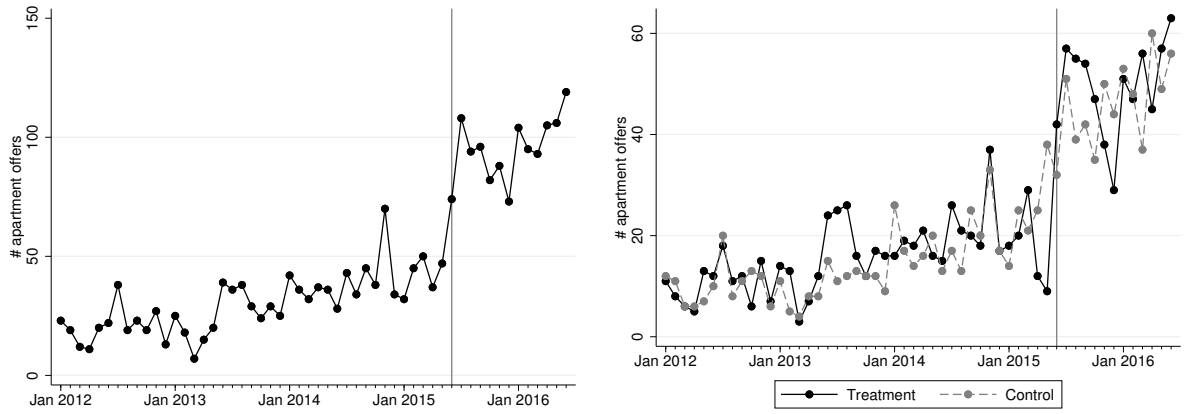
Note: The vertical line marks the time when the law "principle who orders pays" became effective (June 2015). 100,513 ads, data from Immobilienscout24, authors' calculations.



(a) Overall (b) By treatment status

Figure A5: Number of apartment offers over time — Frankfurt

Note: Treated apartments are those that, prior to the reform, were offered with an REA commission payable by the renter. Control apartments are those that, prior to the reform, were offered without a commission payable by the renter. The vertical line marks the time when the law "principle who orders pays" became effective (June 2015). The Frankfurt sample includes 6,634 ads from 3,317 apartments, data from Immobilienscout24, authors' calculations.



(a) Overall (b) By treatment status

Figure A6: Number of apartment offers over time — Stuttgart

Note: Treated apartments are those that, prior to the reform, were offered with an REA commission payable by the renter. Control apartments are those that, prior to the reform, were offered without a commission payable by the renter. The vertical line marks the time when the law "principle who orders pays" became effective (June 2015). The Stuttgart sample includes 2,474 ads from 1,237 apartments, data from Immobilienscout24, authors' calculations.

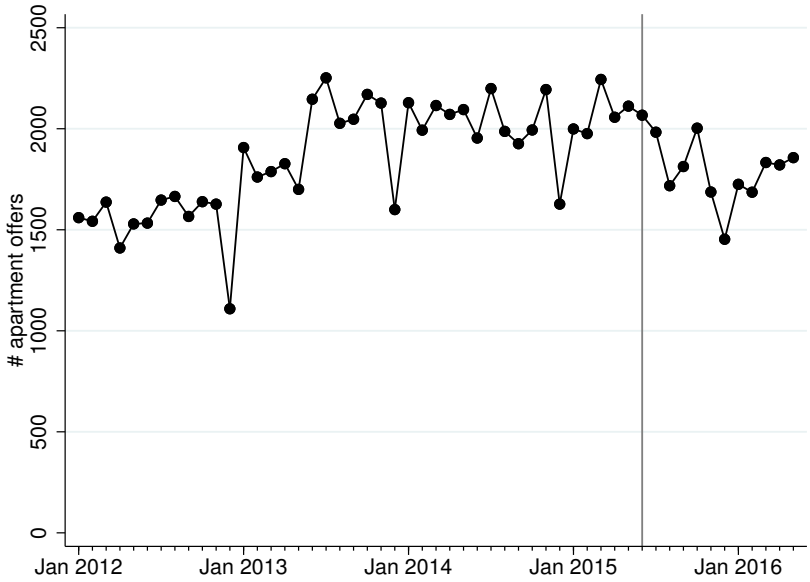


Figure A7: Number of apartment offers over time — original sample (not restricted to panel sample)

Note: The vertical line marks the time when the law "principle who orders pays" became effective (June 2015). Setbacks in the number of observations appear in every December. 100,079 ads, data from Immobilienscout24, authors' calculations.