Distributional price effects of rent controls in Berlin

When expectation meets reality

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Abstract

An enormous increase of initial rents in many German cities over the last decade has prompted the current grand coalition to implement a new rent control called "Mietpreisbremse" in 2015 (literally a brake on rental prices). This reform aims to stop exploding rents and to provide particularly more affordable rental housing in the lower and medium rental price segment. Since then, rental prices of re-lettings are capped at a local rental index in declared areas. As an exception, newly built flats or those that have been reconstructed extensively are not affected by the reform and landlords may always uphold the rent paid by previous tenant. I apply a classical difference-in-difference strategy and a new changes-in-changes model including covariates in order to analyze both average and distributional price effects of the intervention in Berlin. The basis for empirical results is data on newly offered rental prices from 2012 to 2016 that is also enhanced with the local rental index and previous rents. Thereby, I can define the range of effects one could reasonably expect beforehand. Results indicate that the reform indeed lowered initial rents temporarily. In contrast to the reforms intention, however, significant effects are found only in the upper price segment. The effects also fall short of anticipated expectation and fade out too fast. Meanwhile, newly offered rental prices even outrun the pre-reform level, which highlights the lack of enforcement supplementary. I will therefore argue that the reform so far has failed to meet the intended objective and is poorly targeted.

Keywords: rent control, quantile regression, counterfactual distribution, difference-in-difference, changes-in-changes

JEL classification: R21, R23, R31, R33, C1

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1 Introduction

In the last decade, many German cities experienced a strong in-migration from rural areas and abroad, which provoked enormous rent increases across urban areas. This led the German government to introduce a law called "Mietpreisbremse" in 2015 (literally a brake on rental prices). It enables every federal state government to declare tight housing markets in which re-lettings are upper-bounded at the level of a local rental index. In June 2015, the German capital Berlin was the first state to initiate the new rent control and since then, this law has come into force in several other cities as well. Most notably, according to the justification given by the federal government for the new law, the reform was designed to make initial rents affordable again, especially for low- and medium-income households.[1]

In order not to stifle new construction and maintenance of rental units, newly built flats or those that have been reconstructed extensively form an exception. In addition, landlords are always free to ask the rental price paid by the previous tenant. Despite those exemptions, one would expect a meaningful drop in newly offered rental prices beforehand, as both the local rental index and the previous rent are far lower than actual market prices in most cases. This stands in sharp contrast to previous regulations, where only the price setting for sitting tenants was limited but initial rental contracts were left untouched.

From a theoretical point of view, the analysis of rent control can be presented as one of the least controversial among housing economists when rents are effectively frozen over time. However, such rigid rent controls are certainly infrequent these days and have mostly been replaced by other forms of more flexible rent regulations (Lind, 2001). But even in this context, a package of negative effects – stemming from underproduction and misallocation – prevail in standard economic models. In a much-cited article, however, Arnott (1995) questions the generality of basic textbook arguments given a variety of flexible rent regulations. In a nutshell, he doubts models with a simple price ceiling to reasonably explain complex rent regulations. In particular, he argues that well-designed interventions could countervail against imperfections in housing markets. Meanwhile, more nuanced models seem to find their way into the literature (Arnott and Igarashi, 2000; Basu and Emerson, 2000; Basu and Emerson, 2003; Lind, 2007; Micheli and Schmidt, 2015), but many economists remain skeptical about artificial rental price ceilings. In fact, unintended effects on uncontrolled prices are likely to occur and researchers see no consistent benefits to tenants (Turner and Malpezzi, 2003; Early, 2000). Other reservations include a reduced building activity (Glaeser and Luttmer, 2003; Lind, 2003), the conversion of rental into owner-occupied units (Sims, 2007) or low maintenance of rental apartments (Arnott and Shevyakhova, 2014; Lind, 2015). Economists also cite inefficient allocation (Glaeser and Luttmer, 2003; Skak and Bloze, 2013), reduced mobility (Munch and Svarer, 2002) and segregation due to rent controls (Glaeser, 2003).

Having said that, Arnott (1995) advised to perform evaluations on a case-by-case basis because the institutional frameworks and the different types of rent control mechanisms vary to a great extent among countries and different types of flexible controls will have different impacts on the market. The findings of the authors mentioned should therefore not be generalized blindly. In the run-up to the new rent control in Germany, however, I could not find any advocates of the German rental brake in the economical literature. Given a conglomerate of potential side effects, it was rather expected to cause more harm than good (Benček and Klodt, 2014; Hiller and Schultewolter, 2014; Kholodilin and Ulbricht, 2014; Hiller and Gröbel, 2015;

[1] BT-printed matter 18/3121 from 11/10/2014.

Deschermeier et al., 2016).

With the German Mietpreisbremse having been in existence for one year (MPB, hereafter) it is clearly too soon to empirically evaluate its long-term consequences. But first, it is important to see that the MPB might provoke no price effects of any kind. Many tenants in Berlin are often desperate to even get a flat for rent and might not call for the admissible rental price. Besides, landlords do not have to worry about any consequences if they ignore the legal situation – aside from refunding the inadmissible rent from the date at which the tenant objects to the asking rent. As a result, there might be an incentive to charge market rents rather than the permissible rent since there is nothing to lose but much to gain. Landlords are therefore suspected of ignoring legitimate rental prices in many cases and renters might fail to demand the adequate rent.[2] A significant price effect is thus far from clear in practice and it remains doubtful how far low- or middle-income households benefit from the new rent control. That is why this paper is directed to establish the effectiveness of the MPB at different rental price segments, which are of special interest in order to determine the reforms achievements.

A policy intervention that includes some flats and leaves some unaffected opens up the setting for the well-known difference-in-difference (DID) analysis (e.g., Ashenfelter (1978) and Card and Krueger (1994), to quote only the most prominent applications). Researchers using a standard DID design compare changes over time in any outcome of interest between a control group and a treatment group, where the latter is affected by the intervention and the control group is not. Assuming parallel shifting of both outcomes in the absence of the intervention, the effect of the intervention is recovered by the difference in the differences between the treatment and control group over time. While the DID strategy is often applied in labor economics, it has hardly been used in the analysis of rent controls. Sims (2007) assesses the end of Massachusetts rent control applying a DID estimator. Finding a substantial drop in regulated rental prices due to the reform, his results indicate only little effects on the construction of new housing but he observes a lower quality of maintenance and owners converting units away from rental status. In contrast, non-controlled housing stock shows some but only small effects.

In a recent study, also Kholodilin et al. (2016) used a DID-strategy to identify the price effects of the MPB in Germany, where areas subject to the amendment act as treatment group and adjacent areas without any regulation serve as control group. Results indicate that there is no significant effect on newly offered rental prices in analyzed areas as a whole and rental prices continue to follow the previous growth path regardless of the new legal conditions.

Yet, in a standard DID only average treatment effects are identified, whereas researchers might be interested in distributional effects instead. This matters especially in the context of the following analysis since the MPB was primarily introduced in order to provide affordable housing in the lower and medium rental segment. To this end, further strategies are needed to identify distributional effects and some analysts broadened the idea of standard DID to quantiles (Meyer et al., 1995; Poterba et al., 1995). With the quantile-DID one compares units according to their quantile rather than to their outcome as in the DID. But this strategy has been proven by Athey and Imbens (2006) to be valid only under some additive separability restrictions, which are hard to justify in many empirical applications. Athey and Imbens (2006) show instead how to compare units across both outcome and quantiles in order to identify treatment effects along

[2]Meanwhile, there are only two known cases where a landlord has been convicted of charging too much by a final judgment. See also Immobilien Zeitung: "Mieterbund kritisiert Preisbremse als wirkungslos" from 09/12/2016.

the entire distribution. Unfortunately, their so-called changes-in-changes (CIC) model does not include a quick implementable estimator in the presence of covariates. This might be a potential reason for its little application as covariates may increase the precision of the estimates and often take the underlying common trend assumption to hold (Lechner, 2011; Angrist and Pischke, 2009). Recently, Melly and Santangelo (2015) developed an extension of the CIC model to the presence of covariates, which makes estimation of distributional treatment effects using the CIC more applicable.

I take the opportunity of the policy change to apply the new model by Melly and Santangelo (2015) using data on newly offered rental prices in Berlin from 2012 to 2016, whereas those flats unaffected by the amendment will act as the control group. To incorporate the exceptions of the reform, I will also match the data with the local rental index and I will simulate the unknown previous rents using counterfactual exercises. Thereby, three rental prices of interest are available and I will compare each of them to the rental price of the control group, using the DID as well as the CIC model with covariates.

This strategy allows me to distinguish between the actual and the expected effects. As the latter effects can serve as a benchmark for the minimal impact of the reform, they are essential to judge the reform's effectiveness and this breakdown has not been conducted in the context of the German MPB so far. It also helps to deepen the understanding of the reform's conception. Most notably, I will illustrate that rising rents some periods after implementation are not necessarily a contradiction to the reforms effectiveness due to the possibility of asking the previous rent. Additionally, the present study shall add to the existing literature on rent controls with a distributional strategy by applying the CIC model with covariates by Melly and Santangelo (2015) to a new rent control regime.

Results of the empirical analysis indicate that there was indeed a small price effect due to the reform shortly after its introduction. This decline, however, is found to be significant only in the upper part of the rental price distribution, indicating that the reform does not benefit apartment hunters in the lower and medium segment. Moreover, the actual effects are distinctly smaller compared to the minimal impact expected and this gap has not been closed to date. On these grounds, the reform is poorly targeted and cannot unfold the outcome originally intended by now.

The rest of this paper is organized as follows. In the next section 2, I will shortly describe the housing dynamics in Berlin during the last decades and the legal framework of the rental brake. Afterwards, the methodological strategy is outlined in section 3, both intuitively and formally. Then I present the data used including the strategy to construct the unknown previous rent in section 4. Subsequently, the empirical results will be discussed in section 5, followed by a brief conclusion.[3]

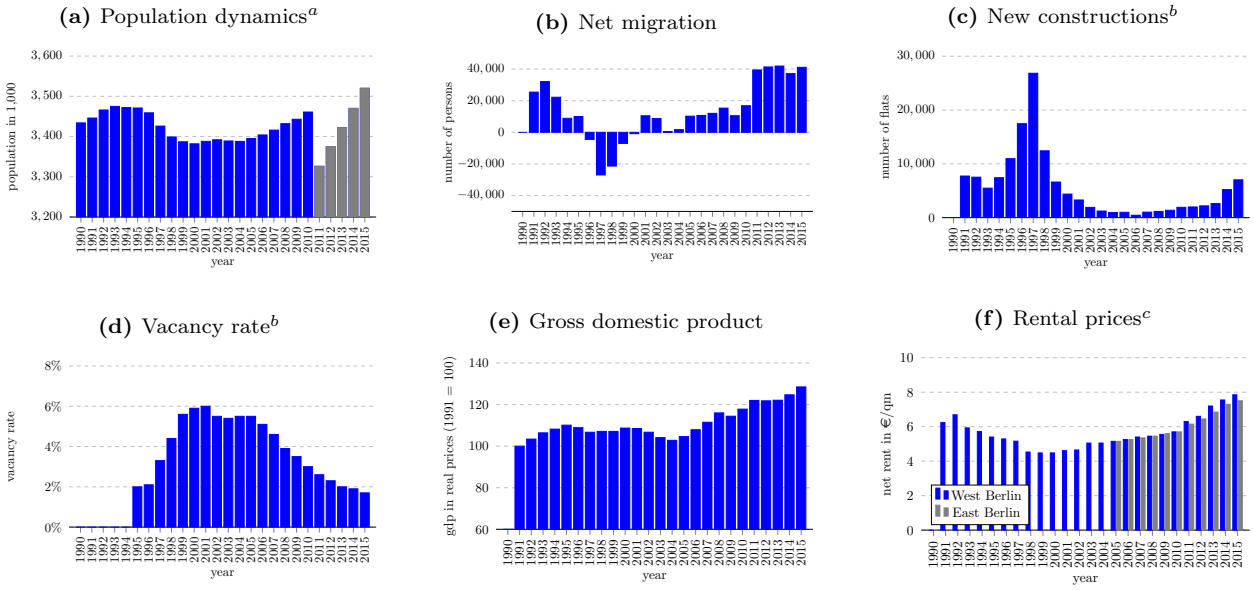
2 Institutional Background

Rental housing dynamics in Berlin

The decision to proclaim Berlin the new capital of unified Germany in 1990 raised enormous expectation for the city's growth and development. Following re-unification, an influx of people came up against short and

[3]To avoid confusion, my use of language requires some explanation. Unless otherwise stated, throughout this study any rental prices mentioned will refer to asking prices of re-lettings. It is important to note that there is typically a substantial gap between rental prices of re-lettings and existing rental agreements. For a long time, upratings for sitting tenants were censored by law in Germany, while re-letting prices were entirely at the landlord's discretion. The latter reacted therefore more dynamically and are higher by comparison in many German places.

Figure 1
Housing data Berlin



[a] Population relies on census 1987 until 2010 and on census 2011 afterwards. Thereby, the population level was adjusted.
[b] Flats in multi family houses (mfh).
[c] Initial rent of flats at any age in mfh, located in average residential areas with three rooms and around 70 sqm.

Source: Statistical office Berlin-Brandenburg, BBU (vacancy rate), IVD (rental prices).

outdated housing supply and rental prices jumped up momentarily. At the same time, new construction was stimulated by subsidies but the expected in-migration and economic activity failed to manifest itself in the mid-nineties (Figure 1) In the following decade, population shrank due to out-migration, building activity broke down and Berlin witnessed a downward trajectory in rental prices. Since then, equally the reverse situation can be observed.[4]

On balance, over the last ten years roughly 240 thousand people moved to Berlin. New constructions could not keep pace with growing demand and vacancy rates dropped significantly. Accordingly, rental prices have soared within this period and again, the rental situation in Berlin has turned from a relaxed to a strained tenant market. As a consequence, the bulk of residents is affected because the rental market plays a major role compared to the purchase market: According to the latest census in 2011, the share of rented flats is 85% in Berlin, making it one of the highest shares among big cities in Germany.

Rising rents and a shortage of rental flats available is a phenomenon not only observable in Berlin. Many German cities faced such a scenario due to increasing urbanization, strong migration patterns from abroad and only little housing construction within the last decade. Moreover, low interest rates trigger the flight into "concrete gold" which promises to safeguard assets and old-age prosperity. As a result, purchase prices jumped up in many cities within the last five years as well, which is likely to additionally fuel the upswing in rental prices.

[4] English market reports on recent developments are available by Berlin Hyp and CBRE (2016) and Investitionsbank Berlin (2016).

Berlin is the biggest metropolis in Germany, currently housing roughly 3.5 million people living in nearly two million households. Although the German capital has still not yet reached top rental levels, it is often characterized as one of the most dynamic housing market among German cities in the last decade. In contrast, average earnings in Berlin remain relatively low and lag behind local rental price dynamics. Therefore topics such as affordable rents, social housing supply or the transformation of rental apartments into owner-occupied apartments dominate day-to-day politics in Berlin, especially in the wake of state elections in 2016. Not surprisingly, the Senate Administration of Berlin was the first federal state pioneering the MPB in June 2015 to curb runaway rents in new leases.

Legal framework

While rent regulations were applied extensively in Germany during World War I and II, in the second half of the last century they were cut back steadily. During this period, rent regulations in West Germany basically comprised social housing, tenant protection and rent regulations for current tenants (Kholodilin, 2016). The latter were tightened in West Germany again in 1982 by the so-called capping limit, which restricted shifts in rental prices of existing contracts to a maximum of 30% within three years. In 2001 the capping limit has been lowered to 20% and since 2013, in tight regional housing markets – declared by the federal states – the latitude has been cut down on 15% within three years.[5] By contrast, rent controls on new leases have been consistently dismantled since the 1960s in almost all West German areas apart from West Berlin.

The rental market in divided Berlin was completely controlled in the eastern part and highly regulated in isolated western districts: in West Berlin roughly 50% of the entire housing stock was publicly funded with restrictions on rental prices. Additionally, for almost the entire remaining parts of the private sector, rent controls for ongoing tenancies and newly offered flats constructed before World War II continued until the early nineties (Hubert, 1998). These special regulations were primarily preserved in order to encourage relocation to isolated West Berlin. After re-unification, West German rent controls for current tenants were retained in unified Berlin, but regulations on new contracts were phased out. Accordingly, since the early nineties landlords have been free to impose any new rent the Berlin market will bear in the private sector.[6]

Consequently, the MPB constitutes a decisive event in this centurys German housing policy. The idea dates back to a strategy paper by the Social Democratic Party in the run-up to the elections in 2013 and was stipulated in the coalition agreement in December 2013.[7] It was not until April 2015 that the so-called Germanys tenancy law reform (MietNovG) passed the national parliament and since June 2015 every federal state is empowered to cap initial rents in designated areas with housing shortages. The corresponding ordinance had already been drawn up in Berlin and it was implemented at once in the entire city in June 2015. Principally, rents in new tenancies are permitted to exceed no more than 110% of the local rental index in the following five years.

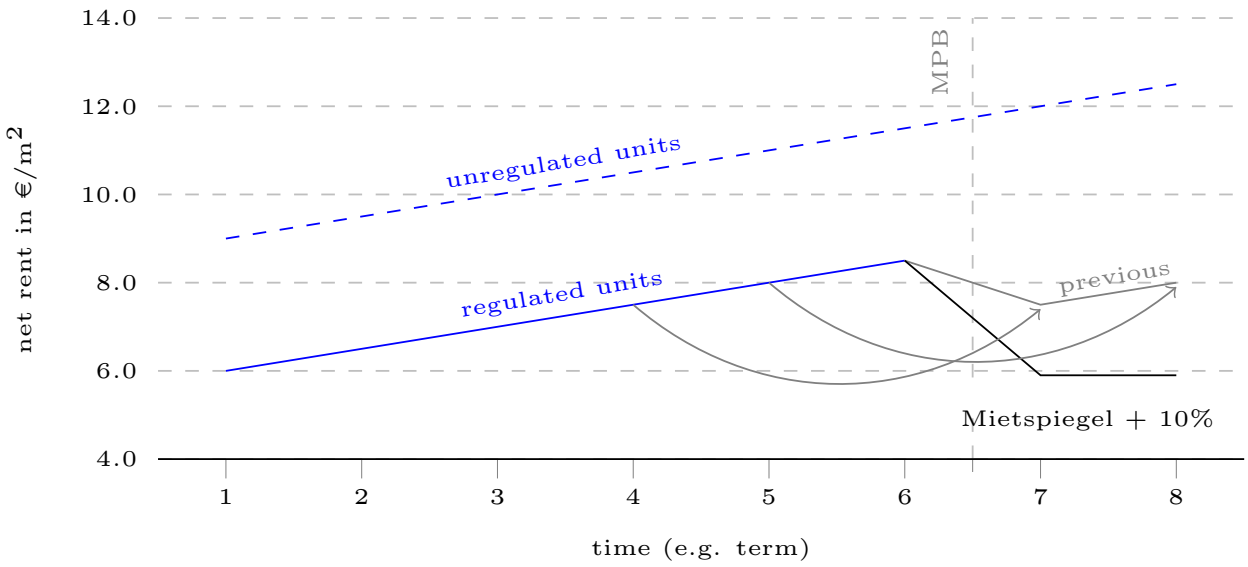
The local rental index ("Mietspiegel", hereafter) is a database to determine typical local market rents

[5]The current capping limit in Berlin is 15% within three years (§558 BGB (German Civil Code)). Note that it is possible to additionally allocate 11% of modernization costs to monthly rents (559 BGB). See also Fitzenberger and Fuchs (2016) for a empirical analysis of sitting tenant discounts in West-Germany.

[6]To be precise, usury rents are prohibited by §5 WiStG. However, this law is only applicable given a substantial housing shortage in a housing market as a whole (Federal Court, D.f. 01/28/2004, VIII ZR 190/03) and, in fact, the restriction is meaningless in practice.

[7]See a SPIEGEL report from 08/01/2013: www.spiegel.de/politik/deutschland/steinbrueck-vorstoss-spd-plant-drastische-mietpreisbremse-a-876443.html.

Figure 2
Stylized principles of MPB



Source: Own representation.

in the private sector, given comparable characteristics of flats and their location. It is based on all non-subsidized contracts between landlords and tenants in the four years preceding the Mietspiegels's reference date, including older contracts that have been updated within this period. Prior to the rental brake, the Mietspiegel only affected existing contracts: landlords can max out the limits given by the capping limit only if changes do not outrun the corresponding Mietspiegel rent. Since the rental brake was put into force, the Mietspiegel (plus 10%) is likewise instrumental to initial rents.[8]

The MPB, however, does not cover new buildings constructed after October 2014 or extensively modernized flats.[9] Flats for temporary use are also exempted. Moreover, in cases where the rent paid by the previous tenant already exceeded the Mietspiegel level by more than 10%, the landlord can ask a new tenant to pay the same rent, but is not allowed to increase it any further.

It is worth illustrating this somewhat complex system of the new MPB in a concise illustration, also to clarify the effects to be expected through the reform. I depicted its principles in Figure 2, which shall cover rental prices of new contracts only. I suppose a steady rise in rental prices of both regulated and unregulated apartments prior to the reform and the latter continue to rise even after the MPB was imposed. In contrast, prices of controlled units will be prevented to do so through a cap by the MPB. Unlike the name "rental brake" suggests, the MPB will not just lower the growth rates. Depending on the fluctuation rates, it will rather induce a short-term drop in rents and continuous growth rates in the medium-term. This is because market rents exceed the Mietspiegel level – which is adjusted every two years – in up to three-fourths of

[8]The typical rent could also specified by rents of three similar flats or expert reports. In spite of an ongoing debate about methodological shortcomings of the German Mietspiegel (Lerbs and Sebastian, 2015), the rental index in Berlin has recently been held sufficient to trace the local reference rent (Regional Court Berlin, D.f. 07/07/2016, AZ 67 S 72/16 and Regional Court Berlin, D.f. 07/09/2016, AZ 18 S 111/15).

[9]A modernization is considered extensive if its costs rise above a third of the cost to build a comparable flat. If a modernization is not extensive, it is still allowed to allocate 11% of modernization costs to monthly rents additionally (§559 BGB).

Berlin cases and the rents paid by the previous tenant might exceed this level in many cases.[10] In other words, there will be a significant proportion where not the Mietspiegel level is the admissible reference value but the previous rent.

Even though the previous rent remains a mystery due to a lack of suitable data, it is still possible to give an impression of its impact. In Figure 2, I assumed for the sake of simplicity that every flat was re-rented three periods ago, which could for instance correspond to a year and a half if each period refers to six month. This assumption is certainly not true for every single flat, but it is a reasonable assumption given that 61% of all Berlin households move within five years (Table A.2). Under this assumption, the previous rent would just follow the growth path from a year and a half ago after a one-time reduction in rental levels. So the effects one could expect beforehand will range anywhere between the previous rent and the Mietspiegel level – depending on the relocations in Berlin. That is, a drop of asking rents to the Mietspiegel level will be the maximal effect to be expected, while a reduction to the previous rent will be the minimal effect. The Mietspiegel level, though, will always be the minimal benchmark, irrespective of whether the previous rent undercuts this level.

This stylized example greatly simplifies reality, but it is crucial to assess the empirical results. According to many media reports, rental prices continue to relatively grow even after the MPB's implementation and these observations are often constituted as a failure of the MPB. In fact, increasing rents are principally not antithetical to its effect – provided a downward shift in rental levels took place before. Also note that previous rents in Figure 8 will not surmount the level in period six, but they will vary back and forth between period four and six levels in the future. In light of the above, the conception of the MPB prevents a final evaluation regarding its effectiveness. There is a great range to the length of time tenants stay in an apartment, which is likely to be scattered across the city and no appropriate data is available. The fluctuation, however, will have a great impact on the evolution of initial rents and it is of great importance to see that surging rental prices do not contradict the reform's effectiveness in the short-run.

It is also worth mentioning that the MPB affects new constructions and modernized flats through an indirect channel. Since the future Mietspiegel will also rely on regulated rents, its adjustment will be limited through the new regulation. This minimizes future rent increases through a lower capping limit for all current tenants, including even those living in flats not covered by the MPB.

Apart from new rent controls on initial contracts, the tenancy law reform (MietNovG) contains another fundamental change, the so-called *Bestellerprinzip*. It determines that whoever appoints a real-estate agent has to settle the agent's account. Unlike the MPB it applies to all of Germany since June 2015. Prior to that change, a tenant typically ended up paying the real-estate agent's fee, regardless of who hired the agent. It is argued by the government that in combination with the MPB, landlords can no longer add the service fee to the rent. But they can continue to do so when renting out flats for temporary use. This change proves to be important in order to detect unintended side effects and to comprehend the assignment of the control group in section 4.[11]

Given this rather complex system of rent controls in Berlin (and many other German areas), it is difficult to place German regulations on the continuum of rent controls. Since the MPB is in force, most new rental contracts are captured below market rent level and will remain so in the upcoming five years. Thus, the

[10] See Deschermeier et al. (2016) for a comparison of asking rents and Mietspiegel levels and also Figure 5a.
[11] The *Bestellerprinzip* is discussed by Michaelis and Wangenheim (2016) in more detail.

present regulations is certainly not a flexible or so-called second-generation system that Arnott (1995) had in mind. Such systems focus on ongoing tenancies or a modest mitigating of initial rent dynamics, whereas the MPB prevent rental prices to surpass pre-reform levels.

In a more differentiated classification, Lind (2001) distinguishes five types of rent controls. While category A and B cover regulations on sitting tenants, C to E also apply to new leases and sometimes even to new constructions. The purpose of category C is to prevent usury rents, rent regulation D and E aim to bring temporary peaks in rents back to the long-term equilibrium price. Type D allows the rent level to catch up with the market rent level rather quickly, whereas type E keeps rents below market levels in the long run. It is to some extent a question of how to define a 'quick' adjustment. But as the Mietspiegel level usually changes only little after adaption, I would assign the MPB rather to a rent control of type E.[12] While this brief overview on rent control classification can help to transfer other empirical results to the MPB, this paper is not directed to distinguish the MPB from other systems in detail. I will, instead, highlight the stylized example in Figure 8 with empirical data in order to identify short-term price effects of the MPB along the entire rental price distribution.

3 Empirical approach

3.1 Motivation

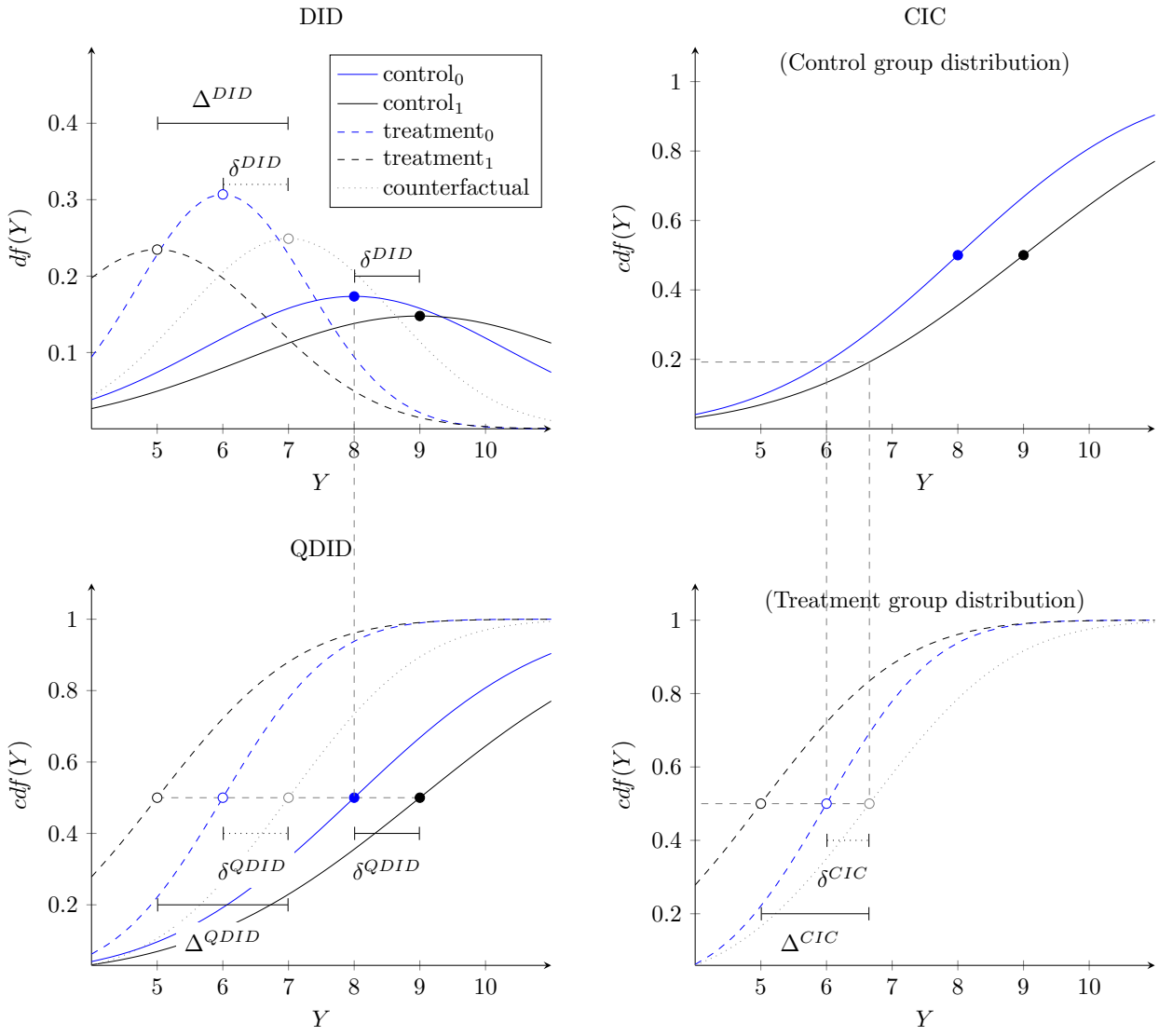
A simple example shall serve as illustration in order to introduce applied identification strategies intuitively and to distinguish the CIC model from other approaches.[13] In this example, only two groups and two periods are considered: The treatment group T receives a treatment in the second period 1 but not in the first period 0, whereas the control group G is never treated. The example consists therefore of four distributions and for the sake of simplicity I will use normally distributed random samples, each subsample with a different mean and variance, respectively. These four distributions are pictured in Figure 3 and will correspond to the unconditional outcome of interest, e.g. rental prices. Throughout the subfigures, each distribution of the control group is represented by solid lines, with a blue line in the first period and a black line in the second period. The equivalent distributions of the treatment group are in same colors but dashed lines. Every line also includes a single dot, which specifies the corresponding average outcome. In the expected style of Berlin rental market, the average outcome of the control group is higher compared to the treatment group and shifts over time, whereas the average outcome of the treatment group declines.

To determine the average treatment effect, we would like to know the average outcome between a world in which units receive the treatment and a world, in which they do not. Naturally, only one of both scenarios is ever observable in reality. It is therefore assumed that the average trends in the treatment ($G = 1$) and in the control group ($G = 0$) after the policy reform would have continued the same way as before in the absence of treatment. This standard DID strategy is plotted in the upper-left subfigure of Figure 3, where the average trend over time of either group is assumed to be identical in the absence of the intervention. The average shift over time in the control group δ^{DID} is then added to the observed mean of the treated

[12] See Deschermeier et al. (2016) for a comparison of asking rents and Mietspiegel levels. See also Haffner et al. (2008) for comparative analyses of rent control systems.

[13] Another distributional approach would be the *RIF*-setting by Firpo et al. (2009), which is not addressed here. See Havnes and Mogstad (2015) for an empirical application.

Figure 3
Identification strategies



Source: Own representation based on Athey and Imbens (2006).

in period zero, which brings us to the counterfactual outcome: the expected mean of the treated after the reform if they were not treated. Finally, the treatment effect Δ^{DID} is given by the gap between the average outcome of the treatment group after the reform and the counterfactual outcome.

If researchers are interested in other effects apart from the average treatment effect, distributional strategies such as the Quantile DID are needed. It is illustrated in the lower-left subfigure. Contrary to the Standard DID, the Quantile DID is applicable to any quantile τ and accordingly, the cdf is plotted for a better illustration. Here, the shift in outcome over time δ^{QDID} at the τth quantile of the control group is added to the initial outcome of the treatment group at the same τ. Again, this brings us to the counterfactual

quantile. Analogous to the standard DID estimator, the $QDID$ estimator is then given by Δ^{QDID}. As the example relies on normal distributions, the standard DID effect is just equal to the median $QDID$ effect, $\tau = 0.5$. To identify the quantile treatment effect others than the median effect, one simply have to move upwards or downwards the line that connects these five dots and one will find the quantile treatment effect Δ^{QDID} to change.

Finding all five dots along a horizontal line, it is also immediately apparent that the $QDID$ estimation compares units across both groups and time according to their quantile instead of to their (average) outcome, as in standard DID. In most empirical settings, though, it is not comprehensible why one should compare units across groups and time according to their quantile. Unless we observe identical units with differences in group and time. [14]

Instead, Athey and Imbens (2006) suggest comparing units across groups according to their distributional outcome and over time according to their quantile. Their strategy is depicted in the right-hand side of Figure 3 and works as follows. Starting point is the lower-right panel, e.g. at the median level of the treatment group before the reform was introduced. In contrast to the $QDID$, where one would search the corresponding outcome of the same quantile, in the CIC the corresponding quantile of the same outcome is searched for. Finding this quantile in the upper-right panel, the CIC calculates the shift over time in the control group δ^{CIC} at that very quantile and – back in the lower-right panel – adds this difference to the initial outcome to predict the counterfactual distribution. Finally, the counterfactual quantile outcome is subtracted from the actual quantile outcome in the treatment group after the reform to detect the quantile treatment effect Δ^{CIC}. In absolute numbers, in the CIC model it is assumed that the shift over time of flats at 6 EUR/sqm would be the same in the absence of the reform. On the contrary, in the $QDID$ model the evolution in the outcome of median flats is supposed to be identical across groups, regardless of its outcome. Visibly, the CIC strategy identifies a smaller effect at the median in the example, $\Delta^{CIC} < \Delta^{QDID}$.

To clarify the underlying assumption in each model, one could also ask what would happen if one of these exercises is conducted without any reform. If this assumption holds true then in the standard DID the average outcome of the treatment group after the reform would be equal to the average counterfactual outcome and the average treatment effect Δ^{DID} would be zero. Similarly, in the $QDID$ and CIC model the distribution of the treatment group after the reform would be equal to the predicted counterfactual distribution and one would find the corresponding treatment effects to be zero. Consequently, if more than two periods and at least two periods before any intervention are available, the underlying common trend assumptions are verifiable.

Before turning to the formal derivation of identification strategies, two aspects should be highlighted. First, it is important to see that only unconditional effects have been considered until now. While adding covariates to control for heterogeneity is straightforward in the DID setting, the CIC models would require different assumptions and further calculation procedures. Second, it is obvious to see that the CIC is only calculable in the range which is covered by either the control group or the treatment group in period zero. Outside of this range, the counterfactual distribution is not feasible.

[14] To be precise, Athey and Imbens (2006) show that the $QDID$ strategy is only valid under additive separability assumptions. In an empirical application of Havnes and Mogstad (2015), however, unconditional $QDID$- and CIC-results are quite similar.

3.2 Formal derivation

Formally, a unit $i = 1, ..., N$ belongs to group $G_i \in \{0, 1\}$ (with 1 being treated) and is observed in period $T_i \in \{0, 1\}$. Given a treatment indicator $I_i = G_i \times T_i$, the observational rule for any outcome of interest Y_i can be expressed by

$$Y_i = Y_i^N(1 - I_i) + I \cdot Y^I. \tag{1}$$

Here, Y_i^I denotes the output if an unit receives the treatment and Y_i^N if the same unit is not treated. In the absence of the intervention, the output also satisfies the relation

$$Y_i^N = \alpha + \beta \cdot T_i + \gamma \cdot G_i + \varepsilon_i, \tag{2}$$

where β specifies a time-effect and γ a group-specific time-invariant effect. The error term ε_i denotes unobservable characteristics and is assumed to be uncorrelated with (G_i, T_i).

To ease notation, I will drop the subscript i and introduce the shorthand notation by Melly and Santangelo (2015)

$$Y_{gt}^N \equiv \ Y^N |\ G = g, T = t;\ Y_{gt}^I \equiv \ Y^I |\ G = g, T = t;\ Y_{gt} \equiv \ Y |\ G = g, T = t, \tag{3}$$

with the corresponding distributions $F_{Y^N|gt}$, $F_{Y^I|gt}$ and $F_{Y|gt}$, respectively.

In the standard DID, the common trend assumption

$$E(Y_{11}^N) - E(Y_{10}^N) = E(Y_{01}^N) - E(Y_{00}^N) \tag{4}$$

removes biases associated with a trend unrelated to the intervention and includes the non-observable, counterfactual term $E(Y_{11}^N)$. Given this assumption holds true, the average treatment effect Δ^{DID} is equal to

$$\Delta^{DID} = E[Y_{11}^I] - E[Y_{11}^N] \rightarrow \ Y = \alpha + \beta \cdot T + \gamma \cdot G + \Delta^{DID} \cdot I + \varepsilon, \tag{5}$$

and calculable by standard OLS regression.[15]

It seems natural to replace $E[\cdot]$ by $F^{-1}[\cdot]$ in order to derive the quantile treatment effects (QDID) using quantile regression (Koenker and Bassett, 1978). As previously stated, Athey and Imbens (2006) show that this strategy is justified under additive separability restrictions only and not robust to monotone transformation. Alternatively, they suppose to modify the additive linear model in Equation 2 in such a way that the outcome in the absence of the intervention is given by the strictly increasing production function

$$Y^N = h(U, T), \tag{6}$$

where the random variable $U = u$ denotes unobservable characteristics of any unit. These unobservable characteristics may differ across groups, but are assumed to be time-invariant within groups, $U \perp T|G$.

[15] Lechner (2011) provides an extensive survey of difference-in-difference estimators including further estimation details. See also Imbens and Wooldridge (2009) and Bertrand et al. (2004) for comprehensive DID discussions.

In other words, the CIC assumes common growth in outcome between the treatment and control group distribution in the absence of the reform. This assumption is the equivalent to the common trend assumption in the standard DID and the crucial condition to identify a CIC estimator.[16]

The treatment effect along the distribution of Y is then represented by

$$\Delta^{CIC} = F^{-1}_{Y^I|11} - F^{-1}_{Y^N|11}, \tag{7}$$

where the first term on the right hand-side is given by the empirical data. The counterfactual second term is not observable but Athey and Imbens (2006) show that the full distribution is identified through

$$F^{-1}_{Y^N|11}(y) = F^{-1}_{Y|01}\left(F_{Y|00}\left(F^{-1}_{Y|10}(y)\right)\right). \tag{8}$$

That said, their strategy does not include a tractable estimator in the presence of covariates, which are often needed to increase the precision of estimates and to make the common trend assumption hold true. To overcome this limitation, Melly and Santangelo (2015) suggest to assign the CIC to the case with covariates $X = x$, for which they assume the production function in Equation 6 to be

$$Y^N = h(U, T, X). \tag{9}$$

This time, unobservable characteristics may differ across groups again, but are assumed to be time-invariant within conditional groups, $U \perp T|G, X$. This means, the trend over time needs to be identically conditional on X.[17] Just as in the CIC model without covariates, the key to derive treatment effects $\Delta^{CIC}(\cdot)$ including covariates is to identify the conditional counterfactual distribution, which is proven to be

$$F^{-1}_{Y^N|11x}(\tau) = F^{-1}_{Y|01x}\left(F_{Y|00x}\left(F^{-1}_{Y|10x}(\tau)\right)\right), \tag{10}$$

where $F_{Y|00x}\left(F^{-1}_{Y|10x}(\tau)\right)$ is the conditional rank function and $\tau \in (0, 1)$. To construct these terms, Melly and Santangelo (2015) use quantile regression in order to estimate the unobserved distribution

$$\widehat{F}^{-1}_{Y^N|11x}(\tau) = \widehat{F}^{-1}_{Y|01x}\left(\widehat{F}_{Y|00x}\left(\widehat{F}^{-1}_{Y|10x}(\tau)\right)\right) = x'\widehat{\beta}_{01}\left(\int_0^1 1\left(x'\widehat{\beta}_{00}(u) \leq x'\widehat{\beta}_{10}(\tau)\right) du\right). \tag{11}$$

Along with $\widehat{F}^{-1}_{Y^I|11x}(\tau) = x'\widehat{\beta}_{11}(\tau)$, which is again predictable using quantile regression, the conditional CIC-estimator is then given by

$$\widehat{\Delta}^{CIC}(\tau|x) = \widehat{F}^{-1}_{Y^I|11x} - \widehat{F}^{-1}_{Y^N|11x}. \tag{12}$$

Conditional distribution effects, however, might give only little understanding of the treatment since one cannot tell where a unit will be in the outcome distribution before and after a treatment. It is therefore more insightful to make statements about the distribution as a whole, which require unconditional effects

[16] Furthermore, strict monotonicity of the production function $h(U, T)$ is supposed and results only apply to the range in which unobservables of both groups overlap, $\mathbb{U}_1 \subseteq \mathbb{U}_0$.

[17] Again, strict monotonicity of $h(U, T, X)$ is required and the support assumption extends to $\mathbb{U}_{1x} \subseteq \mathbb{U}_{0x}\ \forall x \in \mathbb{X}$. See also Melly and Santangelo (2015) for technical details of the estimation process. I used the STATA command `cic` by Blaise Melly to estimate $\widehat{\Delta}^{CIC}(\tau|x)$.

based on conditional models. To derive them, Melly and Santangelo (2015) propose to rearrange

$$\widehat{F}_{Y^{N,I}|11x}(\tau) = \int_{\mathbb{X}} \widehat{F}_{Y^{N,I}|11x}(\tau) dF_{X|11}(x) \tag{13}$$

such that the unconditional treatment effect (based on the conditional model) is

$$\widehat{\Delta}^{CIC}(\cdot) = \widehat{F}^{-1}_{Y^{I}|11} - \widehat{F}^{-1}_{Y^{N}|11}. \tag{14}$$

Carried over on the setting of the MPB, the quantile treatment effect $\widehat{\Delta}^{CIC}(\cdot)$ denotes the distributional price impact on treated units due to the reform – assuming they would follow the same conditional path as unregulated units without any reform along the whole distribution.

Based on the inference theory provided by Chernozhukov et al. (2013), Melly and Santangelo (2015) also show how to estimate the covariance matrix of $\widehat{\Delta}^{CIC}(\cdot)$ in order to obtain consistent pointwise standard errors $\widehat{\Sigma}(\tau)$ using bootstrap methods. These standard errors are needed to construct pointwise and simultaneous confidence sets, which are essential to evaluate the results obtained. While a 95% pointwise confidence interval can easily be constructed by using a critical value of 1.96, simultaneous confidence require a different decision rule.[18] Again, inference results from Chernozhukov et al. (2013) are transferred to the CIC setting in order to estimate a critical value $\widehat{\sigma}_{1-\alpha}$ such that all parameters are available to construct valid simultaneous confidence bands:

$$\lim_{n\to\infty} \Pr\left\{\Delta^{CIC}(\tau|x) \in \left[\widehat{\Delta}^{CIC}(\tau|x) \pm \widehat{\sigma}_{1-\alpha} \cdot \widehat{\Sigma}(\tau)\right] \text{ for all } \tau\right\} = 1-\alpha. \tag{15}$$

Now also consider there are several periods during which none of the groups is treated, e.g. in period $t = 0$ and $t = -1$. Then, the traditional null hypothesis can also be used to test the time-invariance assumption. In that case, the simultaneous confidence set of the quantile treatment effect would not differ from zero based on the significance level α. Since there are several periods available, I will test the common trend assumption to hold prior to the reform.

4 Data and empirical model

Data

The basis of the empirical analysis is a random sample of asking rental prices that is based on a combination of different distribution channels. In addition to the leading Internet marketplaces, specialized internet sources and print media are incorporated into the database as well.[19] The data is researched continuously on a daily level and adjusted for duplicates, both in cross section (across all sources) and in longitudinal section (across time). It contains information about all housing features announced in the advertisement, including the monthly net rental prices excluding services, address, apartment size, number of rooms, year of construction and information about several housing features.

[18] The results by Chernozhukov et al. (2013) are also used to prove that their estimators are consistent and asymptotically normally distributed. See also Chernozhukov et al. (2013) and Melly and Santangelo (2015) for technical details of estimation procedure.

[19] The data is provided by empirica-systeme.

It must be noted, however, that there is a crucial limitation to the database. Rental prices offered do not reflect transaction rents and there are no studies analyzing the potential gaps between transaction and asking rents in Germany. While it is uncommon in Germany to renegotiate the rental prices offered, the deviation from transaction rents has not yet been proven to be a well behaved random error. This is because there is no German transaction rental data available that allows for detailed studies on a small regional level.[20] Still, the Internet is likely to reflect a high market activity as it is the most common and publicly available platform to offer flats for rent in Germany without any systematical access restriction. Besides, there is no alternative data on rental transactions available, so that the data I use is widespread in German housing research and highly respected as – at least – the best substitute for real transaction rents.[21]

I rely on data that covers the interval from June 2012 up to and including May 2016, making data available for three years prior to the reform and one year thereafter. Using the start date of advertisements, I split the data into eight periods, where each period corresponds to six months. Note that the latest two terms will always represent the time after the MPB's introduction. The analysis restricts itself to Berlin (separated by the current community code of the German Bureau of Statistics) and to rental apartments in multi-family houses, excluding vacation homes, shared apartments and publicly funded flats.

Treatment and control group

There are principally two main options to distinguish the treatment and the control group. For instance, Kholodilin et al. (2016) utilize neighboring regulated and unregulated areas, which means sub-urbanization areas are compared to outlying districts within cities. However, regulated areas are strained housing markets by definition, whereas unregulated municipalities are not and I would therefore expect both housing markets to change heterogeneously over time. Unfortunately, the current version of the study by Kholodilin et al. (2016) does not show the common trend assumption to hold true, which is why I remain skeptical about this strategy. Besides, in this framework central district areas within cities are excluded from the analysis because they do not border areas without any regulation. These central areas, however, are presumably hardest hit by the reform and their exclusion would lower the empirical results.

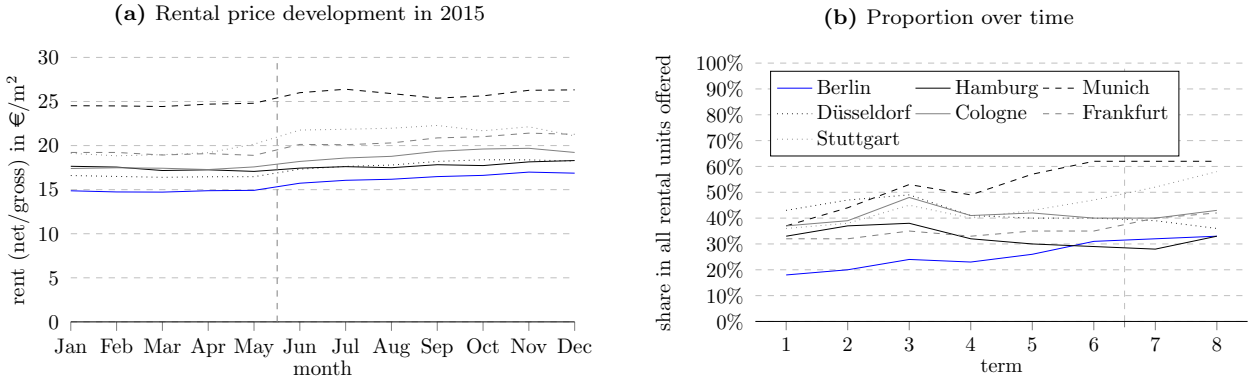
Another option is to contrast treated and non-treated units instead of areas. In the course of the MPB, newly built, modernized or temporary living apartments could serve as control group in the empirical analysis. The definition of the control group, though, becomes a moot point if units will be moved from regulated to unregulated status through the reform. In that case, the supply of controlled units would be diminished while non-controlled flats would be put on the market excessively. Then, the rental price in the regulated market cannot react to this shortage of supply due to rent ceilings, but one would expect a decline in unregulated asking rents given an oversupply in this sector. A comparison of controlled and non-controlled rental prices is then likely to be misleading.

Rental prices of temporary living apartments, in fact, have been found to have jumped up June 2015 – at exactly the date on which the *Bestellerprinzip* was imposed nationwide (Figure 4a). This observation is striking not only in Berlin but in all German metropolises, regardless of when the local MPB was intro-

[20] For example, cooperative flats are often placed on internal waiting lists. Anecdotal evidence also suggest that highly desired apartments are transferred "under the table" rather than put up on the Internet.

[21] In a recent call for entries to evaluate the MPB, the Federal Office for Agriculture and Food explicitly demanded to use the very same or similar data.

Figure 4
Temporary living apartments



duced.[22] Moreover, temporary living units are frequently offered as a total package at gross costs including all additional services, which might be error prone and misleading in terms of net rents.

In Berlin, there is also a steady increase within the last four years in the quantity of advertised temporary living apartments. A graphical examination of this trend in Figure 4b, however, points rather towards a general trend which is also visible in other metropolises, particularly in Munich, Frankfurt and Stuttgart. This progress has not been investigated empirically, but according to several media reports it is reducible to a changing, that is more flexible world of work – independent of the MPB.[23] While the shift has not been clarified empirically either in quantity or in asking prices of temporary living apartments, a great uncertainty regarding spillover effects remains. This is why I exclude these units from the analysis in order to compare prices of controlled units to a consistent time-series of prices in the non-controlled sector.

In addition, there has also been a moderate increase of newly built or modernized flats in Berlin for the last four years (Figure 5). But this trend more likely reflects a shift in building activity rather than an impact of the MPB in the short run. At least, I cannot see any conspicuous time trend of rental prices of those apartments. In the end, I decided to include only newly build or modernized flats from the unregulated market, also to use a control group, which might not be adjustable quantitatively at once. In detail, I use advertisements that state first occupancy after modernization or new construction as control group, whereas temporary living apartments will be dropped and all remaining flats belong to the treatment group. The observed rental price of treated flats will be denoted as y^o from here on, the rental price of flats out of the control group as y^c. Ultimately, the definition of the control group and the treatment group is also justified by a common trend in rental prices prior to the reform, as will be shown below.

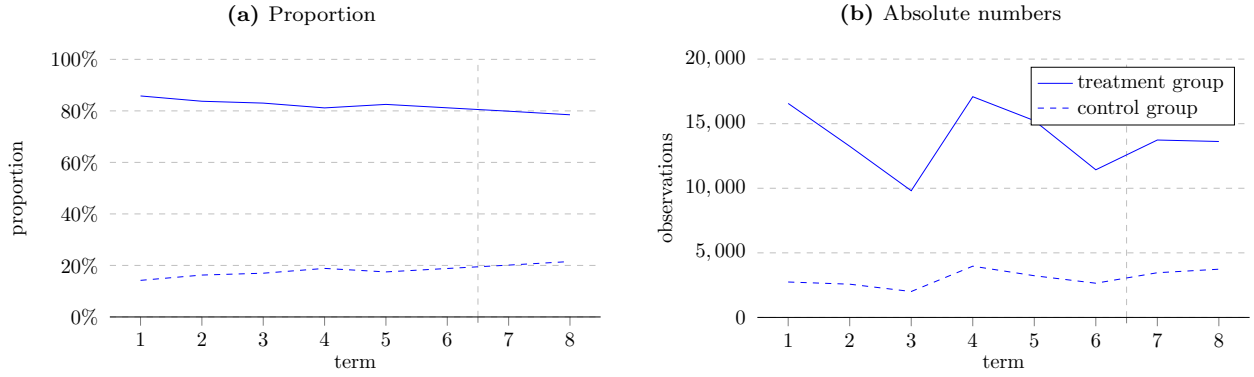
Empirical model

Throughout the analysis, the net rent excluding services is the dependent variable. To apply the CIC model with covariates, I use a basic log-linear model including twelve district dummies and those explaining

[22] In Hamburg it was imposed in July, Munich, Cologne and Düsseldorf followed in August and in Frankfurt and Stuttgart it started in November.

[23] See, e.g., an article in Wirtschaftswoche: "*Vermieter möblierter Wohnungen dürfen die Miete weiter frei festlegen*" (08/1/2016, p. 78).

Figure 5
Development of treatment and control group



variables given in Table 1.[24] After dropping all observation with missing values in covariates, the sample constitutes of 135,111 units, from which 82% belong to the treatment group.[25]

The decision to include both the year of construction and size of unit quadratically was made upon a graphical analysis, all other variables are included as dummies. Coefficients of OLS and median quantile regression in Table 2 are highly significant throughout and reveal an expectable structure of the rental market in Berlin. OLS residual diagnostics in Figure A.1 show no noticeable conspicuousness except a long tail distribution, which highlights the need for distributional studies.[26] Results also show the spatial rental price dimension with the highest prices in central districts (Friedrichshain-Kreuzberg, Mitte and Charlottenburg) and in well-off residential areas (Steglitz-Zehlendorf and Pankow). The districts Marzahn-Hellersdorf and Spandau are at the bottom of the list, both characterized by tower block complexes.

Mietspiegel rent

To attach the permissible rent to each relevant unit in the data, I begin with the current Mietspiegel. It is classified into 96 rental categories with respect to a flat's year of construction, size of unit and three location types (basic, regular, good). Additionally, for every category there is low, a medium and an upper level, depending on the flats amenities. Geocoding of the data and the official Mietspiegel location enables me to match the specific Mietspiegel location with each flat in the sample. Subsequently, the size of an apartment, the year of construction and the location type is considered to append the Mietspiegel level to each flat that is affected by the reform, where I use the upper Mietspiegel level if a flat is well equipped and the medium level if it is not. Admittedly, this procedure cannot claim to identify the exact Mietspiegel level of each flat precisely as the information given in the data does not coincide with those features required to derive the Mietspiegel level. It is therefore only a rough approximation, but note that this approximation tends to

[24] I specified several other models including far more variables, but could not find a meaningful change in the estimated effects. Therefore, I decided to keep the model simple in order to facilitate interpretation and to reduce the time of bootstrapping the results.

[25] I also have to drop offers with imprecise geocodes ($>$ 100 meter) because these units cannot matched with the official Mietspiegel location. Note also that the analysis does not include treated flats for which no Mietspiegel is available (e.g. furnished flats).

[26] There are some flats that are underestimated, indicated by low standardized residuals ($<$ 5). These flats are probably examples of misleading advertisements (e.g. 75m^2 and well equipped at the cost of 350€). Nevertheless, this conspicuousness does not justify their exclusion from the study.

Table 1
Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.
ln(rent/m^2)	2.112	0.271	0.765	3.135
Year of construction	1947.473	39.551	1806	2016
Year of construction^2	3,794,216.875	154,157.07	3,261,636	4,064,256
Living size	75.64	34.304	22.25	318
Living size^2	6,898.22	7,820.506	495.063	101,124
Building with $\geq$ 10 floors	0.036	0.186	0	1
Top floor	0.273	0.446	0	1
Lift	0.381	0.486	0	1
Balcony/terrace	0.717	0.45	0	1
Garden	0.184	0.388	0	1
Outdated endowment	0.025	0.156	0	1
Normal endowment (base)	0.456	0.498	0	1
High-quality/luxury endowment	0.519	0.5	0	1
Bad condition	0.022	0.148	0	1
Normal condition (base)	0.465	0.499	0	1
Good condition (renovated/redeveloped)	0.513	0.5	0	1
Charlottenburg-Wilmersdorf	0.116	0.32	0	1
Friedrichshain-Kreuzberg (base)	0.073	0.26	0	1
Lichtenberg	0.049	0.216	0	1
Marzahn-Hellersdorf	0.071	0.257	0	1
Mitte	0.128	0.334	0	1
Neukölln	0.068	0.252	0	1
Pankow	0.128	0.335	0	1
Reinickendorf	0.063	0.242	0	1
Spandau	0.065	0.246	0	1
Steglitz-Zehlendorf	0.084	0.277	0	1
Tempelhof-Schöneberg	0.075	0.264	0	1
Treptow-Köpenick	0.08	0.271	0	1
Treatment group	0.820	0.384	0	1
N		135,111		

be generous: each flat is at least assigned to the medium Mietspiegel level and half of the sample is even allocated to the upper level. Subsequently, I add the permissible premium of 10% to each Mietspiegel rent, to which I will refer below as y^m.

Previous rent

The main element of uncertainty in my analysis is the previous rent. There is no suitable panel data on flats in Germany available and the exact period of tenancy is unknown. The point however, is not to find the true previous rent (which is impossible), but to give an idea of the effects to be expected if every flat is priced at the previous rent. Therefore, I estimate the market rent of every flat assuming it was re-rented a year and a half or six quarters ago. According to a special tabulation by the Statistical Office Berlin-Brandenburg, at the end of 2015 the average period of tenancy in Berlin was 9.7 years, with the longest duration in the district Treptow-Köpenick (12.3) and the shortest in Mitte (7.9) (Table A.2). It is also known that 61% of all residents move within 5 years and the average duration of this share comes to 1.7 years in Berlin.[27] Accordingly, assuming a tenancy duration of 1.5 years does not reflect reality and clearly overestimates the fluctuation. The intention is rather to create an example for the minimal decline in rental prices to

[27]Note that residents also include home owners, which are likely to move less. Moreover, fluctuation rates based on status of registration are likely to be biased due to failed registrations. E.g. the latest microcensus in 2006 reports a fluctuation rate of roughly 6% for Berlin.

Table 2
Regression results

	OLS-regression		Median-regression	
Year of construction	-0.1972***	(0.0018)	-0.2089***	(0.0022)
Year of construction^2	0.0001***	(0.0000)	0.0001***	(0.0000)
Living size	-0.0036***	(0.0001)	-0.0042***	(0.0001)
Living size^2	0.0000***	(0.0000)	0.0000***	(0.0000)
Building with $\geq$ 10 floors	-0.0651***	(0.0030)	-0.0719***	(0.0035)
Top floor	0.0289***	(0.0012)	0.0261***	(0.0014)
Lift	0.0381***	(0.0013)	0.0354***	(0.0015)
Balcony/terrace	0.0097***	(0.0013)	0.0093***	(0.0015)
Garden	0.0508***	(0.0014)	0.0538***	(0.0016)
Outdated endowment	-0.0456***	(0.0033)	-0.0418***	(0.0039)
High-quality/luxury endowment	0.0353***	(0.0011)	0.0327***	(0.0013)
Bad condition	-0.0757***	(0.0035)	-0.0641***	(0.0041)
Good condition (renovated/redeveloped)	0.0695***	(0.0012)	0.0658***	(0.0014)
Charlottenburg-Wilmersdorf	0.0108***	(0.0025)	0.0025	(0.0029)
Lichtenberg	-0.1984***	(0.0031)	-0.2040***	(0.0036)
Marzahn-Hellersdorf	-0.3848***	(0.0029)	-0.3989***	(0.0034)
Mitte	-0.0321***	(0.0024)	-0.0482***	(0.0028)
Neukölln	-0.1378***	(0.0028)	-0.1536***	(0.0032)
Pankow	-0.0705***	(0.0024)	-0.0860***	(0.0028)
Reinickendorf	-0.2263***	(0.0029)	-0.2312***	(0.0034)
Spandau	-0.2803***	(0.0028)	-0.2849***	(0.0033)
Steglitz-Zehlendorf	-0.0672***	(0.0027)	-0.0697***	(0.0032)
Tempelhof-Schöneberg	-0.1052***	(0.0027)	-0.1111***	(0.0032)
Treptow-Köpenick	-0.2196***	(0.0027)	-0.2240***	(0.0031)
Treatment group	-0.1078***	(0.0015)	-0.1153***	(0.0018)
Term 2	0.0394***	(0.0020)	0.0398***	(0.0024)
Term 3	0.0726***	(0.0022)	0.0729***	(0.0026)
Term 4	0.0840***	(0.0019)	0.0860***	(0.0022)
Term 5	0.1124***	(0.0019)	0.1127***	(0.0023)
Term 6	0.1398***	(0.0021)	0.1394***	(0.0025)
Term 7	0.1543***	(0.0020)	0.1547***	(0.0023)
Term 8	0.1897***	(0.0020)	0.1937***	(0.0023)
Constant	194.4405***	(1.7907)	205.9111***	(2.1033)
R^2	0.5196			

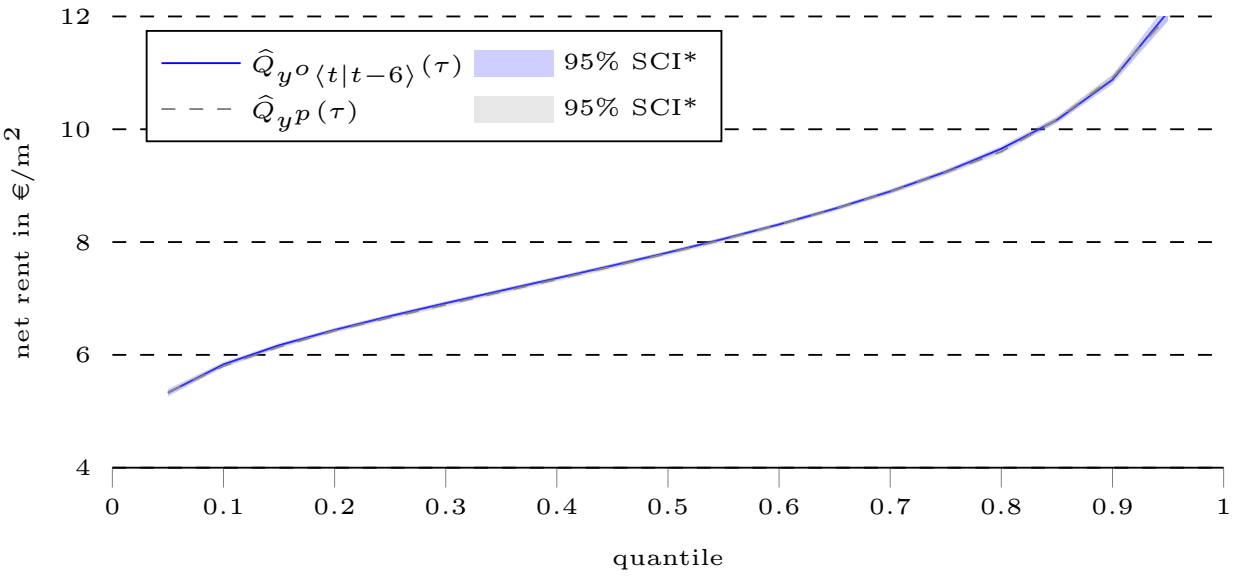
Dependent variable: ln(rent/m^2); Standard errors in parentheses; $N = 135,111$
* $p < 0.05$, ** $p < 0.01$, ***, $p < 0.001$;

be expected after the reform. This is why I decided to assume the shortest plausible duration.[28] As an aside, flats that have been rented out, for instance, fifteen, ten or even five years might be subject to a modernization in many cases before they are put on the market again.

The simplest approach would be to replace asking rents from today with asking rents from a year and half ago. In this setting, however, I would not allow the sample composition to change over time and would potentially face a bias. Alternatively, I construct the asking price that was asked previously for matchable flats using basic counterfactual exercises. Namely, for each flat covered by the MPB, I estimate the market rent that was asked for this very type of flat six quarters before. For each treated unit i and each quarter $t = 13, ..., 16$ after the reform, I predict the conditional rental price $\widehat{y}_i(\tau|x_i) = x_i'\widehat{\beta}_\tau$ for all quantiles $\tau \in \{0.05, ..., 0.95\}$ in t and in $t-6$ using quantile regression and the model described above. Then I store

[28]Note that new rental contracts are typically not allowed to be canceled within the first year.

Figure 6
Previous rent distribution



* Simultaneous confidence bands.

Note: The counterfactual distribution $\widehat{Q}_{Y_{\langle t|t-6\rangle}}(\tau)$ and the simultaneous confidence bands of $\widehat{Q}_{y^p}(\tau)$ has been estimated using the STATA command `cdeco` (with 100 bootstrap replications).

predicted values out of sample to estimate the relative change between current and former rental prices:

$$\widehat{\Delta}_i(\tau|t) = \frac{\widehat{y}_i(\tau|t-6)}{\widehat{y}_i(\tau|t)}. \tag{16}$$

Subsequently, assuming conditional rank invariance, I multiply the observed rental price from today y_i^o with the estimated relative change in predicted values:

$$\widehat{y}_i^p(\widehat{\tau}|t) = y_i^o(\widehat{\tau}|t) \cdot \widehat{\Delta}_i(\tau|t) \text{ if } \widehat{\tau} = \tau, \tag{17}$$

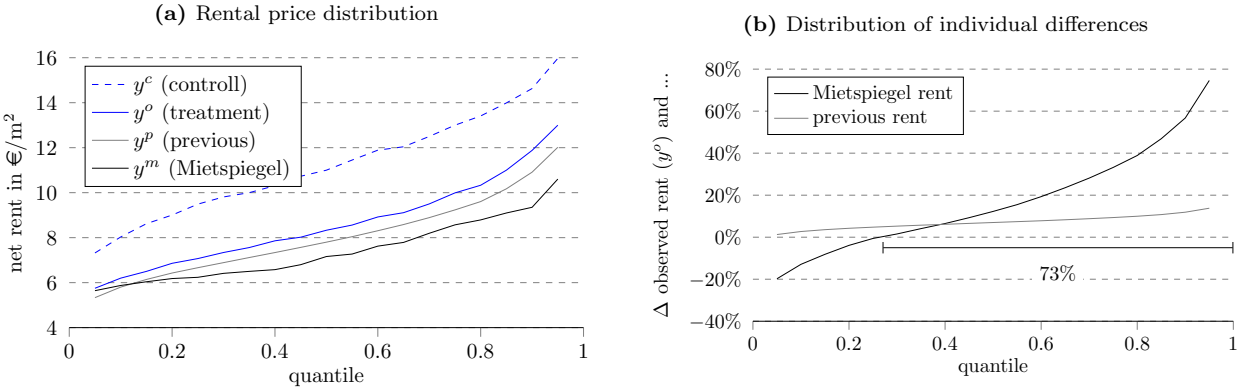
where $\widehat{\tau}$ corresponds to the sample quantile in period t. This so-called *common-coefficient-approach* (Abbring and Heckman, 2007) is one of the most basic approaches of counterfactual estimation, but it turns out to yield reliable results. In order to illustrate this, I compare the estimated joint distribution with a well-identified marginal counterfactual distribution.

The literature on counterfactual distribution provides an increasing list of approaches to construct the marginal counterfactual distribution. For ease of computation, I revert to a recent approach by Chernozhukov et al. (2013). In a nutshell, they suggest to integrate the former conditional distribution $\widehat{F}_{Y_{t-6}|X_{t-6}}$ over the range of current covariates $\widehat{F}_{X_t}(x)$ such that the distribution of interest is given by

$$\widehat{Q}_{Y_{\langle t|t-6\rangle}}(\tau) = \widehat{F}^{-1}_{Y_{\langle t|t-6\rangle}}(\tau) = \int \widehat{F}_{Y_{t-6}|X_{t-6}}(y|x) d\widehat{F}_{X_t}(x). \tag{18}$$

Here, the structure of previous conditional prices is mixed with current characteristics to obtain the uncon-

Figure 7
Unconditional distribution of rental prices (post-reform)



ditional marginal counterfactual distribution – based on the conditional model.[29]

In Figure 6, I plotted the estimated counterfactual distribution as described above, next to the one based on the strategy by Chernozhukov et al. (2013). There is virtually no numerical difference and the simultaneous confidence bands highlight the comparability. I will therefore use the joint counterfactual distribution to include the previous rent y^p into the empirical strategy.

Distribution and dynamics

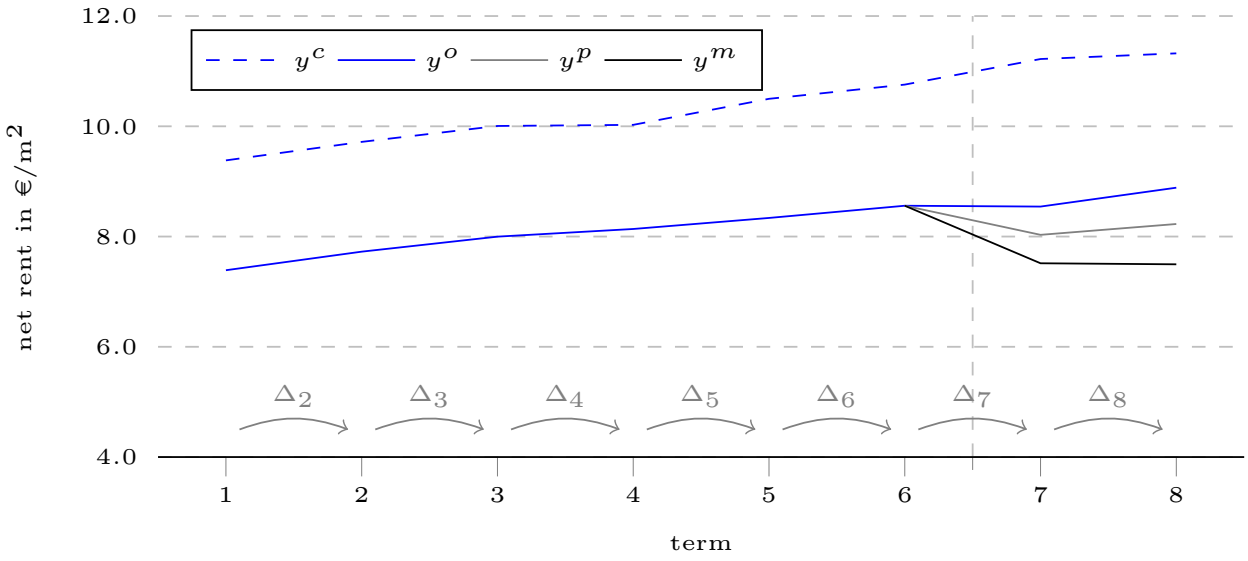
Next to the distribution of rental prices in the control group, the data construction presented permits to separate three different types of rental prices for each unit in the treatment group after the reform was implemented in May 2015. Their distributions are all figured Figure 7a. The control group shows, as expected, the highest rental price distribution after the reform, followed by the treatment group and the previous rent. The lowest rent distribution, not surprisingly, is given by the Mietspiegel level.

Given a steady increase in rental prices prior to the reform, the observed rent of the treatment group exceeds the previous rent along the whole distribution – but with the highest gaps in the upper rental price segment, as can be seen in Figure 7b. This is not true for the Mietspiegel rent, which rather undercuts the observed rental price in roughly one quarter of the cases. The most plausible explanation for this "underpricing" is a questionable location classification of the Mietspiegel (e.g. Kauermann and Windmann, 2016). While some outlying low-price areas are assigned to 'good'-location in the current version of the Mietspiegel, many vibrant areas (e.g. Kreuzberg or Neukölln) are still classified 'regular' or even 'basic'. As a result, in some alleged 'good' locations the corresponding Mietspiegel level is just not realizable with concern to demand, whereas market rents in alleged 'basic' or 'regular' locations often exceed Mietspiegel levels by up to 70%. Nevertheless, the current location classification has to be taken for granted. But it is important to keep in mind that 27% of all flat offered do not exhaust the permissible Mietspiegel level.[30]

[29] See Chernozhukov et al. (2013) for details and Thomschke (2015) for an application to Berlin rental prices. Fortin et al. (2011) provides a comprehensive comparison of other counterfactual approaches which have also been applied by McMillen (2008), Nicodemo and Raya (2012) or Qin et al. (2016) in the context of housing markets.

[30] Note that the distribution of differences in Figure 7b does not imply a higher Mietspiegel level in 27% of all cases in Figure 7a, because the distribution in Figure 7a corresponds to the rental level and the distribution in Figure 7b to the difference in rental levels. See also Figure A.2.

Figure 8
Unconditional rental price dynamics (mean)



During the three years preceding the introduction of the MPB, both the rental price in the treatment and the control group shifted quite parallel. In Figure 8 the unconditional mean evolution of all four relevant prices is depicted. The time frame reaches from term one to eight, with the MPB having been introduced after term six.[31] While the unconditional rental price of those flats treated (y^o) rose by 15.8% in three years from term one to six, the corresponding growth was 14.7% in the control group (y^c). Rental prices of flats unaffected by the reform continued to rise after the reform's introduction as well, whereas one can see a temporary and tiny slump of affected rental prices following the reform's start. This decline, however, is offset in term eight when rental prices of the treatment group start to rise again.

If every flat affected by the reform would be priced according to the previous rent, average rents would go down 6.2% to 8.03 €/m^2 right after the implementation of the MPB and then would continue to step up again afterwards. Even more apparent, they would drop 12.2% to 7.52 €/m^2 and stick to this level if every treated flat is tied to its particular Mietspiegel rent.[32]

These dynamics already prefigure an idea of the effects to be expected. But they cannot reveal the effectiveness of the MPB along the entire distribution and they do not consider conditional models. Therefore, I contrast changes over time in each rental price distribution of interest ($ln(y^o)$, $ln(y^p)$ and $ln(y^m)$) to changes in the outcome distribution of unregulated apartments ($ln(y^c)$). Based on the model described above, I use both a standard DID and the CIC model with covariates, where I estimate the CIC effects for all $\tau \in (0.1, ..., 0.9)$. I will apply this strategy seven times, each time I use a pair of terms and estimate the effects of interest from the first to the second term within this couple. This way I can identify average and

[31] Term one corresponds to June 2012 up to and including November 2012. Each of the following terms also corresponds to six month in a row.

[32] Since the Mietspiegel level does not change from term seven to eight, one would expect an constant gray line in Figure 8. But this will only happen if there is no change in the structure of flats offered.

distributional effects separately between all terms available (Δ_2 to Δ_8, see Figure 8). Note that each effect Δ_t includes only term t and $t-1$.

5 Results

Average effects

The average rental price dynamics of treated and non-treated flats changed almost equally over time prior to the reform. In Figure 9 the relevant interaction terms of the DID-estimation are depicted for all periods available, where Δ_2 to Δ_6 describe the relative changes before the MPB was implemented. In this time, I compared only the observed rental price of controlled to non-controlled flats as both the Mietspiegel and the previous rent was not relevant then. Note that effects refer to logarithmic changes in this chapter and can therefore be approximately interpreted as a percentage change. There are virtually no effects before legal changes were introduced, indicating that rental prices of the treatment and the control group followed the same relative path prior to the reform. Only from term two to three (Δ_3) regulated rental prices run slightly ahead of unregulated rental prices. This effect, however, is not significant at the 1% level (see Table A.1 for all DID-results). The common trend assumption is therefore highly confirmed three years preceding the reform, which is a necessary condition to get reliable results from DID-estimates after the reform was introduced.

After the MPB had taken effect, one finds a clear drop in observed rental prices on average. DID-effects of observed rental prices in Δ_7 amount to nearly -5% and are highly significant. This means that the MPB indeed caused initial rents to decrease shortly after its implementation. Now consider every flat subject to the MPB would be offered at the price given by the Mietspiegel rent. Recall that this price level is the maximal effect one could expect from the reform: there is no reason to fall below this level, except for insufficient demand. On average, newly offered rental prices would then collapse by roughly -15% from term six to seven (Δ_7). At the same time, the average price effect attributable to the reform would amount to a good -10% if every flat is priced according to the previous rent. The latter effect in turn specifies the minimal impact that could be achieved by the MPB. On these grounds, the relevant DID-effect in Δ_7 is not found to lie in the range of those effects one could suppose beforehand, indicating that the reform does not attain the desired price effects on average right after its implementation.

One year after the MPB was imposed, DID-results from term seven to eight (Δ_8) are structured differently. Compared to non-controlled prices, the observed rents start to rise again significantly at somewhat stronger rates. Next to it, since the Mietspiegel level remains basically unchanged between term seven and eight but rental prices in the control group continue to shift (Figure 8), the DID-effect of Mietspiegel rents in Δ_8 is still negative, but lower than in Δ_7. In contrast, DID-results with respect to the previous rent show no significant effect in Δ_8, which means that controlled rents would follow the same path as non-controlled prices if every treated flat would be charged based on the previous rent. The observable DID-effects are thus again out of the range which is determined by the minimal and maximal effects, but this time the discrepancy is less unique and needs some explanation.

I compare changes between term seven and eight (Δ_8) of relevant rents with corresponding changes in the control group in order to measure the follow-up effects of the MPB. As displayed at the example in

Figure 9
Difference-in-difference results

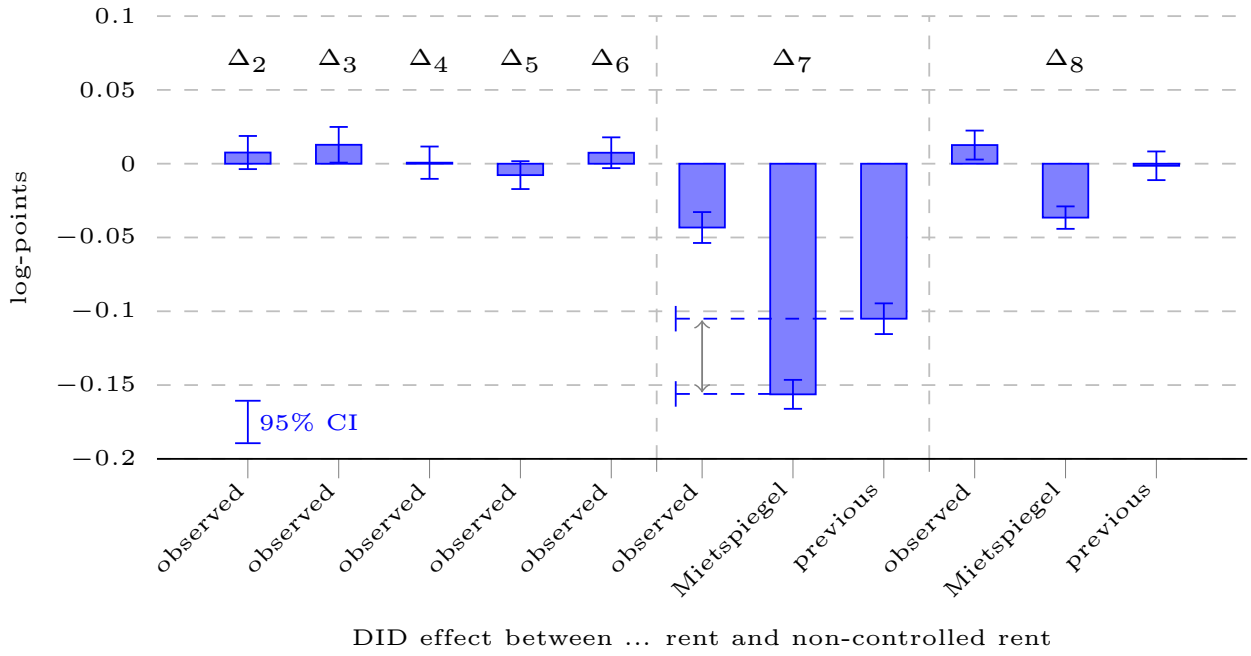
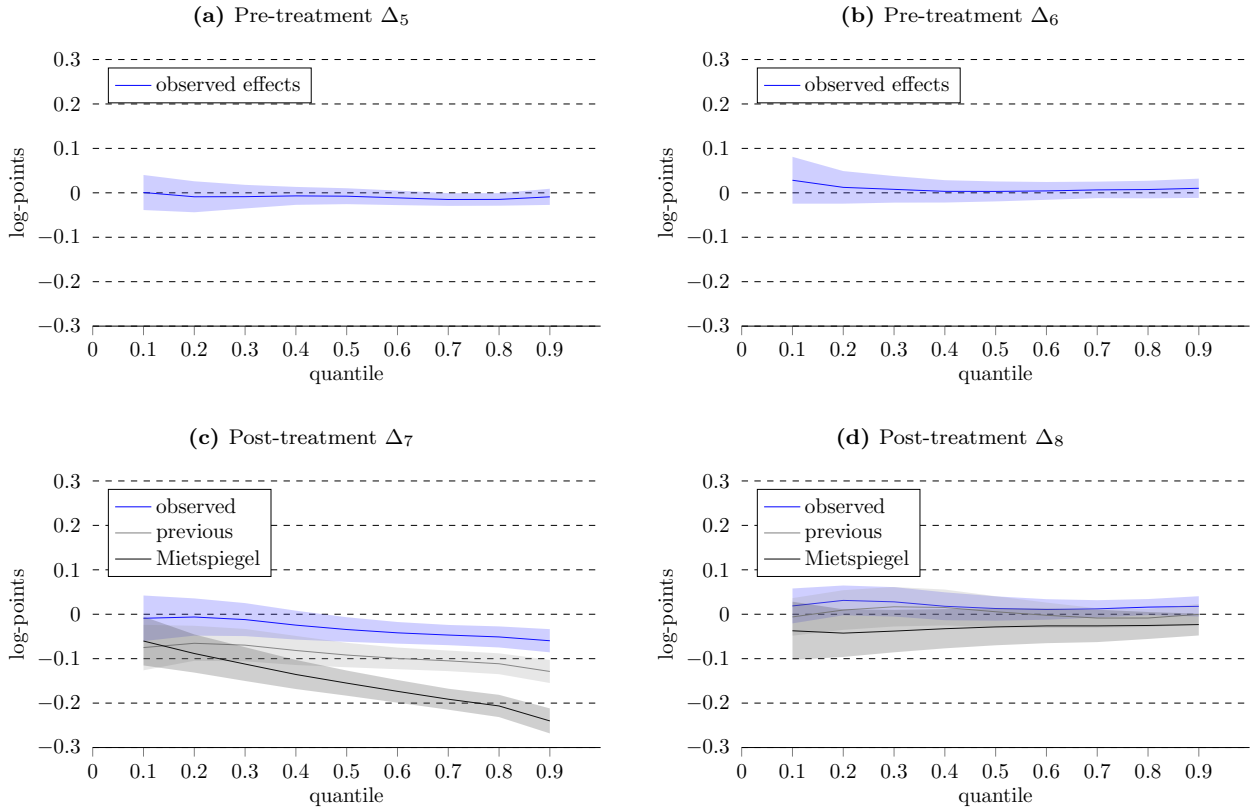


Figure 2, the trend of the treatment may indeed continue to grow from term seven to eight. This is due to the conception of the MPB and highly depends on the true fluctuation rates. Trends in controlled and non-controlled units can therefore embark on the same relative course and the *DID*-effect would be zero in that case. In Figure 9, this is apparent from the effect of previous rents in Δ_8. Since the observed and previous rent effects do not differ significantly (95% confidence intervals overlap), the follow-up impact on its own does not negate the effectiveness of the MPB. But given the considerable backlog from Δ_7, one rather would expect for newly offered rental prices to continue to relatively decrease than to rise in Δ_8. That said, while I can show the common trend assumption to be true prior to the reform, this does not necessarily imply that rents of unregulated flats continue to grow unfazed by the MPB. As discussed in section 2, the reform might also provoke unintended side-effects such as a quantitative shift from the regulated to the unregulated sector. In that scenario rental prices of the control group are likely to deviate from previous dynamics. This would in turn lower the estimated price damping effects. The observable and small positive *DID*-effect in Δ_8 should therefore not be over-interpreted, particularly given a slight slowdown in growth of unregulated rental prices (Figure 8). To sum the intermediate result, *DID*-results point towards a momentary drop in average rental prices of flats subject to the reform and this decline is attributable to the MPB. It does, however, not meet the average expectations and the discrepancy is not made up until now.

Figure 10
Changes-in-changes results



Note: Results have been estimated based on the conditional model in Table 1. Shaded bands represent 95% simultaneous confidence bands, estimated by bootstrapping with 100 replications. See Figure A.3 for Δ_2 to Δ_4

Distributional effects

The common trend assumption also holds along the entire distribution in the run-up of the MPB. For reasons of space, I only plotted the *CIC*-effects for Δ_5 to Δ_8 in Figure 10. In the two-year period immediately preceding the date of the MPB's introduction (Δ_5 and Δ_6), there was virtually no relative change in the rental price dynamics of regulated and unregulated flats. This is indicated by the blue shaded simultaneous confidence bands, which reveal no significant effect along the entire price distribution. This is also true for the remaining effects prior to the reform, depicted in Figure A.3. The time-invariance assumption – the equivalent to the common trend condition – is therefore also ensured in the distributional setting prior to the implementation of the MPB.

In the term proceeding the MPB's start, price dampening effects occur along the entire distribution, but they are only significant in the upper price segment (blue line in Figure 10c). In the range above the median, the blue shaded confidence bands are significantly different form zero and thus, manifest the negative price effects by the MPB for high-priced apartments. In other words, while I can find for half of the treated flats a significant drop in rental prices due to the reform, the other half of flats is not found to be affected significantly and this very half is the lower price segment.

Again, I repeated this exercise with the previous rental prices. The corresponding effects are depicted as a gray line in Figure 10c and show a similar structure as the observed effects: the higher previous rents are, the higher the effects of the reform. This is because the previous rent resembles the observed rent but at different levels, particularly in the upper segment (Figure 7b). Other than the observed effects, one finds the light-gray confidence bands to differ significantly from zero at any quantile. This means there would be a significant reduction of rental prices along the entire distribution if the asking price of flats would be the same price as the previous rent. In the lower price segment, however, confidence bands of previous price effects overlap with the confidence bands of price effects observed. It may therefore well be the case that the reform achieves the minimal effects to be expected in the lower price segment ($\tau < 0.5$). Compared with this, it can be established with high precision that the observed price effects are smaller compared to the minimal effects in the upper price segment, as blue and light-gray confidence bands do not overlap above the median.

The black line Figure 10c and its gray confidence bands give the maximal effects one would expect to find. This line reflects the price effects if every treated flat would be offered at the Mietspiegel rent. Similar to the minimal effects, there would be significant negative price effects along the whole distribution but with a steeper slope. This slope represents the displacement between observed rental prices and Mietspiegel rents: while there is a small proportion of flats that do not even reach the Mietspiegel level, most flats offered go beyond the Mietspiegel level by even up to 70% (Figure 7b). One finds therefore a significant drop in rental prices which is especially pronounced in the upper price segment. The gray confidence bands, though, are not set apart from the blue shaded confidence bands in the very low segment. It is therefore hard to state unambiguously on the reform's achievements in the lower part of the rental price distribution. In the upper price segment, by contrast, rental prices are found to decrease due to the MPB but the minimal effects to be expected dwarf this price dampening effect.

In term eight, the distributional effects of the reform show no visual differences between price segments. In Figure 10d one finds more or less three horizontal lines of the observed, the previous rent and the Mietspiegel effect. In addition, the confidence band of observed effects overlaps with the bands of previous and Mietspiegel effects, respectively. This indicates no significant discrepancy between all three effects. The extent of each effect is almost at the same level as the DID-effects. That is, there still would be a negative effect if every flat would be priced at the Mietspiegel level, the negative effect would disappear if the asking price of every flat would correspond to the previous price level and the observed effect is found to be slightly positive. But none of these three effects is significantly different from zero, which means there is actually no unique effect of any kind.

Just as in the DID-setting, the same interpretation holds for results in Figure 10d. Since Mietspiegel rents remain basically constant between term seven and eight along the distribution, there is a small negative but homogeneous effect. Slight differences at various quantiles only occur due to small changes in the sample composition. Furthermore, if every flat would be priced according to the previous rent, changes in rental prices would just be the same as in the control group and, in fact, effects of the previous rent in Figure 10d would therefore equalize. As emphasized before, the observed effects will be insignificant in the follow-up of the MPB if the previous rent is the new reference level and this is what we can observe in Figure 10d. Still, provided comparably small effects right after the MPB implementation, one should expect a distributional catch-up effect in Figure 10d rather than a renewed growth of rental prices in the controlled sector.

The German MPB was primarily introduced in order to provide more affordable rental units for low- and middle-income households. These households do not necessarily demand housing in the lower and middle price segment. However, assuming every household spends roughly the same share of income on rent, then they do. The consequence for controlled rental prices should therefore be a negative price effect in the lower and medium price segment. But findings to date suggest at best a significant drop in the upper price segment. In the lower and middle price segment, effects of the MPB rather fall flat according to the results. Naturally, a drop at any point of the rental price distribution widens the number of households that can afford these flats and thus, some middle-income households also gain from a decline in the upper price segment. However, this should not hide the fact that high-income households are still likely to benefit most from the MPB so far, given rental prices go down mainly in the upper segment. Loosely speaking, asking prices of relatively cheap flats remain unchanged due to the reform and comparably expensive flats become somewhat cheaper – but too little in comparison with the minimal effects to be expected.

Unfortunately, there are some limits to my estimates, especially in the lower price segment. As can be seen in Figure 3, using the CIC-strategy I compare units in the controlled and in the non-controlled sector according to their outcome at a corresponding quantile. For instance, what would be the shift in rental prices of a controlled unit at the cost of 7 €/m^2 in the non-controlled sector at the same cost? To answer this question, initially I have to find a flat in the non-controlled sector at the cost of 7 €/m^2. While there are sufficient comparative apartments in the upper price segment, they are rare in the lower price segment of non-controlled units (Figure 7a and Figure A.4). Shifts over time at the respective quantile of very low-price flats in the non-controlled sector should therefore be taken with caution and the CIC-results in the lower price segment as well. The procedure is nonetheless justifiable because all CIC-effects from Δ_2 to Δ_6 showed rental prices in the controlled and non-controlled sector to change equally over time prior to the reform, and this is also valid in the lower part of the price distribution. Another concern is a potential spillover effect that decrease the price in the non-controlled sector. Then all my estimates would be underrated. Given a quite steady rise in the non-controlled sector in post-treatment periods, though, I cannot find evidence for this bias at least in the short run.

It is also worth to become aware of the effects if everybody would stick to the new law. Then, we could observe a one-off drop in asking prices at rates of decline between the gray and light-gray bands in Figure 10c. One can immediately see that especially high-priced flats will become cheaper. So what does this mean for housing seekers? Berlin is a rental market in which demand exceeds supply, which is exactly the reason the MPB was introduced. Naturally, this is also true for other German cities and wherever it holds, there will be several applicants per apartment. It is then hard to see why low- or middle-income applicants of all seekers would be preferred on the waiting list. Even if the MPB takes effect, high-income households will be supported to pay less for a flat, which they would get anyway in most cases. It is therefor questionable whether the intervention can help vulnerable housing seekers to find affordable housing – regardless of the reform's effectiveness.

Following economic principles, it is essentially the interplay of supply and demand, which forces initial rents to go up if many people move to an area where they are confronted with little housing supply. An artificial rental price ceiling in the meaning of the MPB, however, will do little to rectify the situation in which demand outweighs supply. And what's more, the excess demand is even likely to be aggravated by the new rent control. Politicians are therefore well advised to remove the causes of rental shifts (a shortage

of sufficient and adequate housing supply) and not merely trying to combat the symptoms in a way that is not even enforced appropriately.

Taken all results together, after the one-year existence of the MPB the intended price effects have not been achieved. While there is indeed a small negative effect on average rental prices, it is too low compared to minimal expectations and fizzles out in the medium term. Similarly, this also holds for distributional effects, where a significant impact of the MPB could only detected in the upper price segment. Supplementary, while both DID and CIC-effects only take relative changes into account, unconditional absolute levels can also underpin (but not validate) the lack of enforcement. To see this, recall the stylized dynamics in Figure 2: No matter which relative effects are caused by the MPB, absolute levels of newly offered rental prices must never overrun the rental level of term six at any time after the MPB was imposed – provided the MPB takes effect. The current unconditional rental price distribution of controlled apartments is, however, clearly higher than the corresponding distribution at term six and this is true for any quantile (Figure A.4).

6 Conclusions

The new rent control law in Germany was intended to slow down the shift in newly offered rental prices. In this study, I analyzed the effectiveness of the intervention in Berlin. Using a DID and a CIC strategy, price effects on controlled rental units were identified at the mean and along the entire distribution. I also attached the Mietspiegel and the previous rent to the data in order to clarify the spread of effects one could expect beforehand. Observed effects are clearly out of this range, which indicates that the reform up to now has not been able to take its originally intended effect. While results suggest that there is indeed a small price effect provoked by the new reform, it applies particularly to high-priced apartments and dwindles too soon. The original goal of the reform – more affordable rental housing for low- and middle-income households – has therefore not been achieved after one year of the MPB. Evidently, too few involved actors in Berlin make an effort to comply with the law and it has also been designed without the possibility to verify its enforcement.

The presented paper is the first study to analyze the distributional price effects of rent controls on initial rents. I introduced a new method to the empirical literature on rent controls, which is a promising tool to evaluate rent controls in other regimes or cities as well. The evaluation of the German rent control, however, is still at an early stage and needs to be complemented by further studies. Results achieved here are no indication of effects in regions other than Berlin and accordingly, additional areas should be analyzed as well. To put the exemptions in more precise terms, further studies could disclose detailed fluctuation rates to narrow anticipated effects. Besides, more time is needed to empirically determine effects of the reform apart from the price effects. These might include long-term spillover effects on the non-controlled sector, a potential future reduction of building activity and maintenance or a transformation of rental to owner-occupied units.

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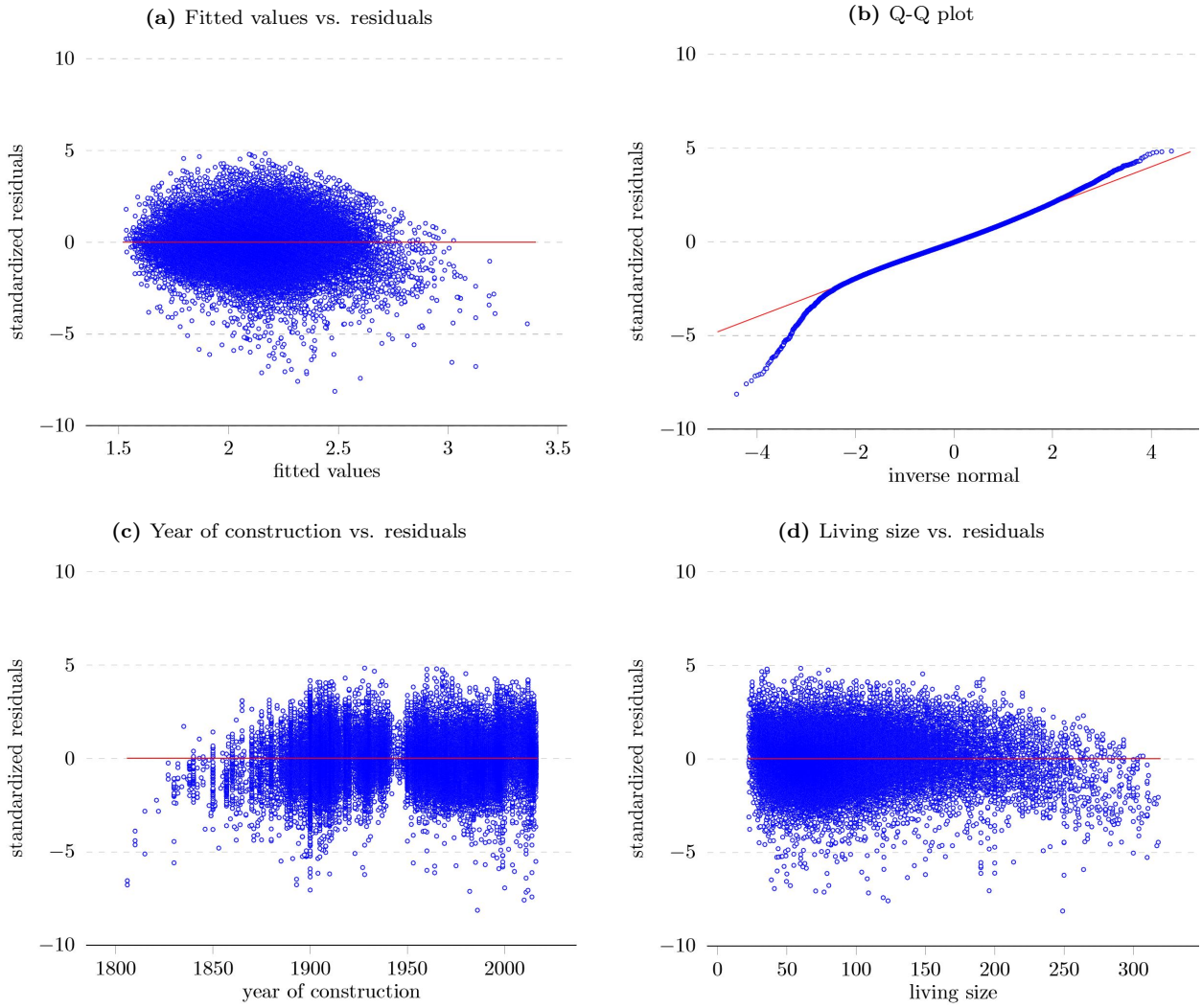
Appendix

Figure A.1
OLS diagnostics



Note: Residuals are taken from OLS-regression in Table 2.

Table A.1
Difference-in-difference results

	observed							Mietspiegel		previous	
	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(7)	(8)	(7)	(8)
Year of construction	-0.207***	-0.204***	-0.205***	-0.194***	-0.192***	-0.191***	-0.187***	-0.222***	-0.242***	-0.189***	-0.175***
Year of construction^2	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***
Living size	-0.004***	-0.003***	-0.003***	-0.003***	-0.003***	-0.004***	-0.004***	-0.003***	-0.002***	-0.004***	-0.004***
Living size^2	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***
Building with $\geq$ 10 floors	-0.068***	-0.079***	-0.073***	-0.076***	-0.074***	-0.055***	-0.049***	-0.049***	-0.041***	-0.054***	-0.065***
Top floor	0.031***	0.032***	0.030***	0.029***	0.027***	0.026***	0.028***	0.023***	0.019***	0.030***	0.030***
Lift	0.043***	0.046***	0.045***	0.036***	0.033***	0.036***	0.033***	0.015***	-0.006**	0.033***	0.029***
Balcony/terrace	0.007**	-0.004	0.006*	0.014***	0.012***	0.012***	0.013***	0.003	-0.006**	0.011***	0.013***
Garden	0.062***	0.066***	0.050***	0.043***	0.043***	0.051***	0.047***	0.040***	0.031***	0.045***	0.045***
Outdated endowment	-0.056***	-0.060***	-0.047***	-0.036***	-0.018*	-0.042***	-0.053***	-0.018**	-0.021***	-0.028***	-0.041***
High-quality/luxury	0.041***	0.029***	0.029***	0.034***	0.033***	0.032***	0.037***	0.103***	0.160***	0.038***	0.040***
Bad condition	-0.064***	-0.062***	-0.079***	-0.080***	-0.081***	-0.072***	-0.077***	-0.042***	-0.022***	-0.080***	-0.087***
renovated/redeveloped	0.072***	0.077***	0.067***	0.068***	0.068***	0.066***	0.072***	0.025***	-0.005**	0.064***	0.067***
Charlottenburg-Wilmersd.	0.038***	0.021***	0.017***	0.015***	0.001	-0.002	-0.013*	0.038***	0.062***	0.007	0.005
Lichtenberg	-0.197***	-0.208***	-0.200***	-0.190***	-0.195***	-0.204***	-0.203***	-0.143***	-0.095***	-0.183***	-0.168***
Marzahn-Hellersdorf	-0.369***	-0.395***	-0.388***	-0.376***	-0.394***	-0.401***	-0.390***	-0.302***	-0.209***	-0.394***	-0.378***
Mitte	-0.028***	-0.020***	-0.026***	-0.034***	-0.033***	-0.024***	-0.040***	-0.004	0.001	-0.021***	-0.023***
Neukölln	-0.144***	-0.148***	-0.145***	-0.144***	-0.146***	-0.130***	-0.125***	-0.070***	-0.015***	-0.120***	-0.105***
Pankow	-0.045***	-0.067***	-0.072***	-0.072***	-0.077***	-0.081***	-0.091***	-0.029***	0.005	-0.061***	-0.056***
Reinickendorf	-0.209***	-0.240***	-0.239***	-0.225***	-0.222***	-0.221***	-0.236***	-0.131***	-0.066***	-0.221***	-0.226***
Spandau	-0.272***	-0.293***	-0.283***	-0.281***	-0.285***	-0.273***	-0.284***	-0.167***	-0.088***	-0.274***	-0.280***
Steglitz-Zehlendorf	-0.038***	-0.049***	-0.058***	-0.067***	-0.085***	-0.084***	-0.091***	-0.016**	0.035***	-0.068***	-0.063***
Tempelhof-Schöneberg	-0.083***	-0.095***	-0.102***	-0.103***	-0.113***	-0.111***	-0.125***	-0.049***	-0.010*	-0.101***	-0.097***
Treptow-Köpenick	-0.193***	-0.214***	-0.219***	-0.222***	-0.235***	-0.228***	-0.231***	-0.128***	-0.055***	-0.219***	-0.216***
Treatment group	-0.107***	-0.096***	-0.092***	-0.094***	-0.103***	-0.097***	-0.138***	-0.119***	-0.289***	-0.098***	-0.203***
time$_2$	0.033***										
Δ_2 (time$_2$ × treatment)	0.008										
time$_3$		0.023***									
Δ_3		0.013*									
time$_4$			0.011*								
Δ_4			0.001								
time$_5$				0.035***							
Δ_5				-0.008							
time$_6$					0.021***						
Δ_6					0.007						
time$_7$						0.049***		0.043***		0.049***	
Δ_7						-0.043***		-0.156***		-0.105***	
time$_8$							0.024***		0.036***		0.024***
Δ_8							0.013*		-0.037***		-0.001
Constant	203.420***	200.912***	202.487***	191.648***	189.726***	188.459***	185.060***	217.770***	236.434***	186.615***	172.984***
N	35,138	27,646	32,875	39,524	32,547	31,276	34,551	31,276	34,551	31,276	34,551
R^2	0.485	0.508	0.500	0.491	0.498	0.510	0.503	0.570	0.696	0.541	0.550

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Note: Each regression (t) only includes term t and $t-1$

Figure A.2
Comparison of rental prices in the treatment group (post-reform)

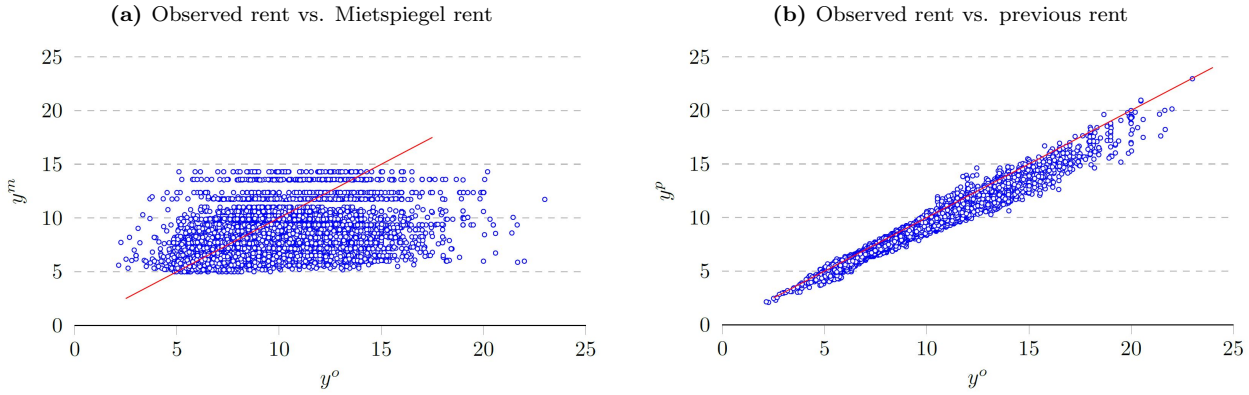
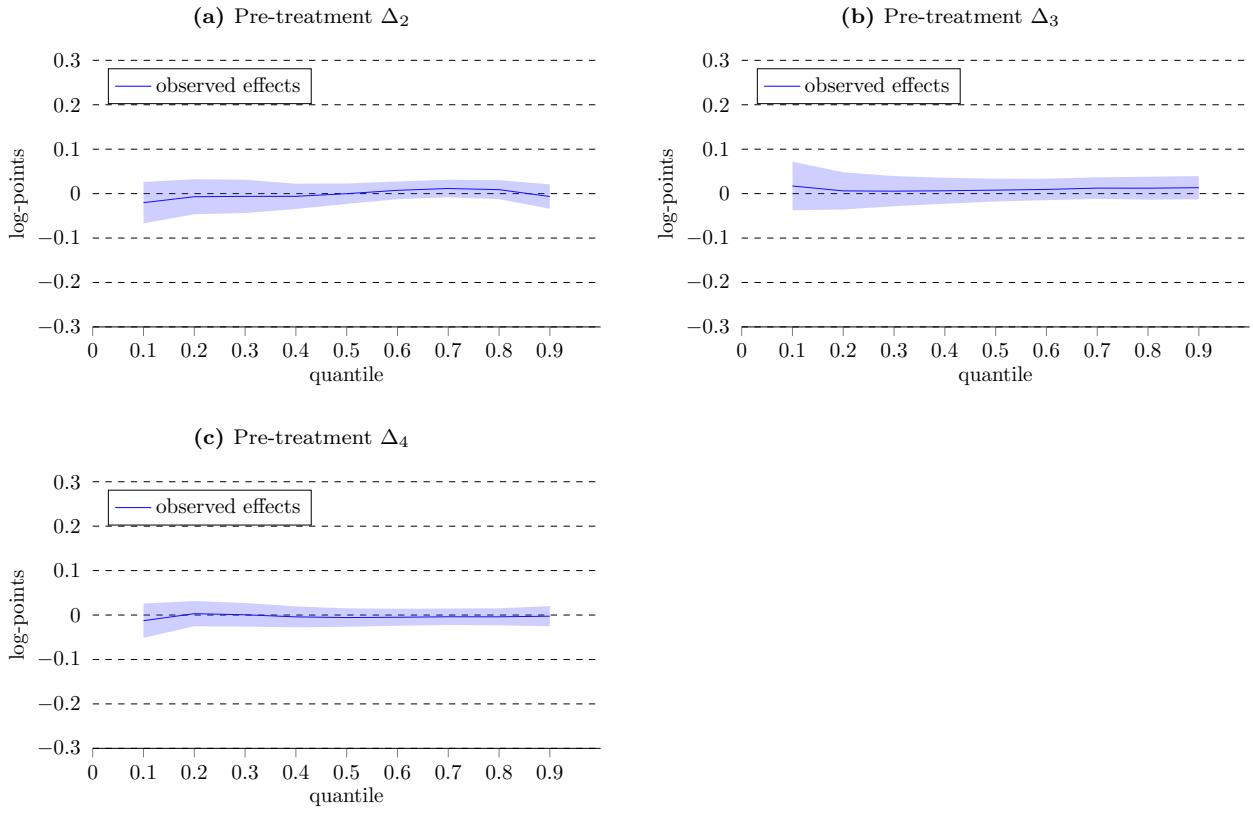


Figure A.3
Changes-in-changes results



Note: Results have been estimated based on the conditional model in Table 1. Shaded bands represent 95% simultaneous confidence bands, estimated by bootstrapping with 100 replications. See Figure 10 for Δ_5 to Δ_8

Figure A.4
Selected unconditional distributions

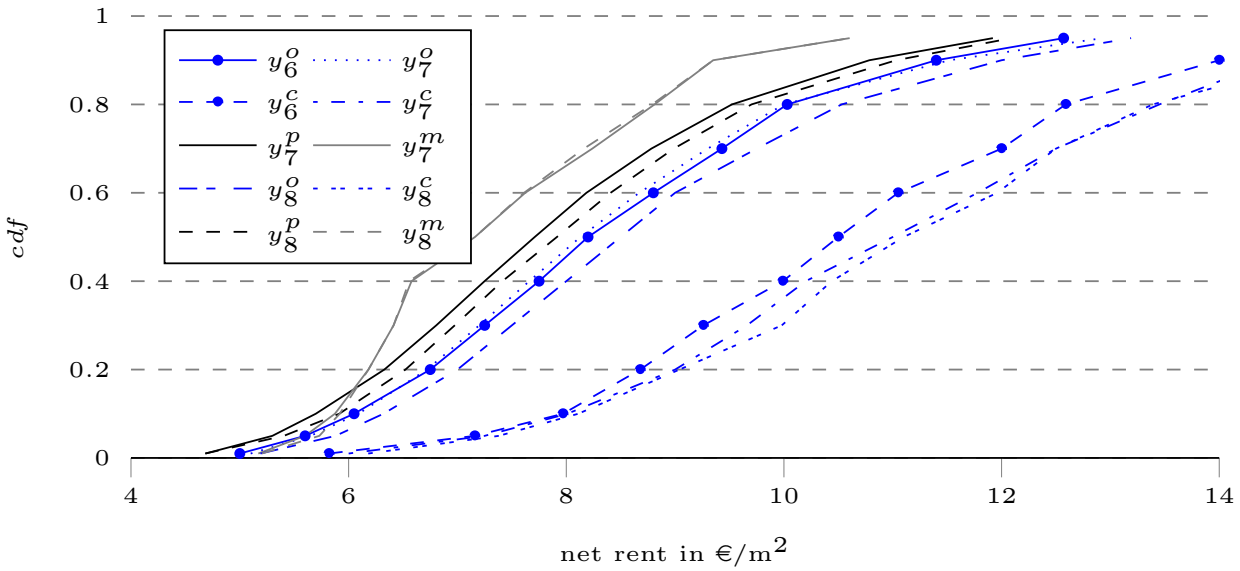


Table A.2
Average residence duration in Berlin, 2015

District	Total	Duration < 5 years	
	∅ Years	Share*	∅ Years
Mitte	7,9	64%	1,6
Friedrichshain-Kreuzberg	8,1	64%	1,6
Pankow	9,5	62%	2,3
Charlottenburg-Wilmersdorf	9,8	60%	1,6
Spandau	9,3	60%	1,7
Steglitz-Zehlendorf	10,1	60%	1,7
Tempelhof-Schöneberg	9,8	60%	1,7
Neukölln	8,9	62%	1,7
Treptow-Köpenick	12,3	60%	2,3
Marzahn-Hellersdorf	10,8	60%	2,4
Lichtenberg	10,6	61%	2,3
Reinickendorf	9,9	60%	1,8
Berlin	9,7	61%	1,7

* The share is only available for 2014.

Source: Statistical office Berlin-Brandenburg.