On the Effects of Homeownership Subsidies on the Spatial Distribution of Population, Housing, and Housing Prices within German Cities and Regions

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Abstract

This dissertation offers three contributions to the economic analysis of homeownership subsidies. It shows that the repeal of homeownership subsidies leads to residents living (i) closer to the center when considered within cities and (ii) less in the countryside when considered within regions. Furthermore, it shows that homeownership subsidies also have an affordability effect on the rental market: Subsidies to homeownership lead new homeowners to move away from the city center, resulting in a decrease in the rental demand and lower rents. In summary, homeownership subsidies encourage decentralized living and consumption of larger living spaces, which result in longer commutes and greater land sealing. Government housing policy, consequently, should be an important component in strategies to mitigate climate change.

Zusammenfassung

Diese Dissertation beinhaltet drei Beiträge zur ökonomischen Analyse der Subventionierung von Wohneigentum. Sie zeigt, dass die Abschaffung von Wohneigentumssubventionen dazu führt, dass die Bevölkerung (i) näher am Zentrum wohnt, wenn die Betrachtung innerhalb von Städten erfolgt und (ii) weniger stark am Land wohnt, wenn die Betrachtung innerhalb von Regionen erfolgt. Weiterhin zeigt sie, dass Wohneigentumssubventionen auch Preiseffekte auf dem Mietmarkt nach sich ziehen: die Subventionen führen dazu, dass neue Wohneigentümer vom Stadtzentrum wegziehen, was zu einem Nachfrage- und Preisrückgang auf dem Mietmarkt führt. Die Wohneigentumssubventionierung fördert dezentrales Wohnen und den Konsum größerer Wohnflächen, was längere Pendelwege und eine stärkere Flächenversiegelung nach sich zieht. Staatliche Wohnungspolitik sollte also einen wichtigen Baustein in Strategien zur Abschwächung des Klimawandels darstellen.

Acknowledgement

I feel deep gratitude to my advisors Kristof Dascher and Gabe Lee. Our stimulating conversations not only generated the ideas for large parts of this dissertation, but their advice also contributed enormously to my professional and personal development. A special thanks to Kristof, who co-authored a chapter of this dissertation and has been an instructive and dedicated mentor and friend to me over the past four years.

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Those to whom I owe the greatest gratitude are the people most important in my personal life: My family. I thank my parents for their constant support during my studies and especially during the four years I was writing my dissertation. This generous support cannot be taken for granted, and therefore I feel extraordinary thankfulness.

And I especially thank my dear wife Susanna. She carried me through this time, supported and motivated me, and lifted me up when I had self-doubt. And she was a good listener and discussion partner, even if the topics of this dissertation probably did not sweep her off her feet. Not infrequently, she has put her own wishes aside for me and my goal. Susanna, without your unconditional love and support, this journey would not have been possible. I promise that I will return the favor to you.

Contents

1 Introduction

"The horrible housing blunder – Why the obsession with home ownership is so harmful". This unambiguous title graced the cover of a 2020 issue of "The Economist" magazine. The editorial and a twelve-page special report focused on housing, homeownership and, in particular, governmental housing policies. In the big picture, the authors see homeownership (and the borrowing and thus indebtedness of households it entails) as one of the powerful amplifiers of economic recessions. In recessions with a strong involvement of the housing market and house prices collapsing across the board, triggering credit crunches, the economic impact has been stronger and more protracted than in crises in which the housing market was less involved.

But "The Economist" also has little good to say about owner-occupied housing at the individual level: To preserve the value of their properties, homeowners have an incentive to act as NIMBYs (Not In My Backyard), refusing new construction in their neighborhoods and thus tightening new housing supply. But at the societal level, the restriction of supply and the resulting higher housing prices limit the productivity of cities and regions by slowing the mobility and influx of workers. And also the debt incurred by many homeowners to realize the dream of homeownership has its risks. The currently low interest rates mean that more and more new homeowners are able to take on large debts while keeping their monthly repayments at a feasible level. In the event of economic recessions, rising interest rates, or unemployment, those repayments quickly become unsustainable. The risk posed by high home loan debt and defaults was impressively demonstrated in the 2008–2010 financial crisis, which was preceded by a collapse of the home mortgage markets in the United States ("The Horrible Housing Blunder" 2020).

Besides the media, the academic community is also concerned with the social and economic consequences of homeownership. On the one hand, homeownership has been *positively* linked to social and economic outcomes, such as the investment in social capital (DiPasquale and Glaeser 1999; Hilber 2010; Hoff and Sen 2005; Rossi and Weber 1996) or local public goods (Hilber and Mayer 2009), the raising of children (Green and White 1997), neighborhood stability (Rohe and Stewart 1996), or the control of local government (Fischel 2001). On the other hand, homeownership is under suspicion of increasing unemployment by impeding household mobility (Blanchflower and Oswald 2013; Munch et al. 2006; Oswald 1996; Ringo 2021), reducing entrepreneurship (Bracke et al. 2018; Harding and Rosenthal 2017), or restricting the supply of housing by owner-occupiers voting in favor of land-use regulations (Ahlfeldt and Maennig 2015; Fischel 2001; Hilber and Robert-Nicoud 2013; Ortalo-Magné and Prat 2014).

In summary, the literature on homeownership provides an ambiguous stance on its social usefulness. But the strand of the literature that deals specifically with analyzing government subsidies for homeownership takes a more clear-cut view: Implicit subsidies for homeownership, such as the mortgage interest deduction in the U.S. tax code, seem to result in the tax savings being used to build larger homes, i.e., to increase the intensive margin, rather than to encourage marginal households to take the step toward homeownership (Glaeser and Shapiro 2003; Hilber and Turner 2014; Sommer and Sullivan 2018).

The literature additionally shows that both types, indirect subsidies through, for example, tax breaks and direct subsidies through cash-transfers, tend to get capitalized in house prices, benefiting the seller of the home rather than the new owner-occupant (Carozzi et al. 2020; Davis 2019; Krolage 2020). Thus, even though there may be social benefits of homeownership, it seems that subsidizing it comes with high opportunity costs to society. Tax money could be used in welfare enhancing ways more efficiently elsewhere, offsetting the potential social benefits of increased homeownership as argued by, e.g., Bourassa and Grigsby (2000) and Follain and Ling (1991).

This dissertation, situated at the intersection of the urban, regional, and public economics literature, makes three contributions to the discussion of subsidies to homeownership: In two chapters, I highlight the effects of homeowner subsidies on suburbanization within cities and regions while another chapter focuses on the effects of homeownership subsidies on rents. Glaeser (2011), Gyourko and Voith (1997), Muth (1967), and Voith (1999) have argued that subsidizing homeownership encourages people to leave apartments in the city center to consume larger owner-occupied houses on the outskirts. In that sense, subsidies to homeownership simultaneously act as subsidies to suburbanization. Exploring the *repeal* of Germany's homeownership subsidy in 2005, I offer empirical evidence that *stopping* homeowner subsidies, indeed, also *stops* the outflow of people from (i) cities' centers to their outskirts at the *within-city* level and (ii) cities to neighboring counties at the *within-region* level.

We should be interested in suburbanization as it has, according to the European Environment Agency (2006), adverse consequences in at least three areas: It has consequences for the environment (such as increased pollution and energy consumption), the social structure (with increased segregation of the rich and poor), and the economy (as, e.g., higher per capita cost of providing infrastructure).

Understanding the impact of suburbanization on the environment is becoming increasingly important in times of climate change. People living more decentrally consume more living space per capita. And, with a tendency for building height to be reduced with increased distance from a city center, there will be larger houses on larger lots in the peripheries, which in addition must be provisioned with, e.g., road infrastructure, which is then only used by a few. These impacts imply higher per capita soil sealing. But, with increasing extreme weather events such as heavy rainfall, sealed soils will be unable to absorb and transport rainfall quickly enough, leading to higher flood risks in built-up areas (Abass et al. 2020; Du et al. 2015; Miller and Hutchins 2017).

Additionally, living in the periphery goes hand-in-hand with higher energy consumption, greater car dependency, longer commutes and thus, in sum, higher CO_2 emissions that accelerate global warming (Brownstone and Golob 2009; Cervero and Murakami 2010; Glaeser and Kahn 2010b).[1] In that sense, incentivizing peripheral living through homeownership subsidies also contributes to all of these negative environmental effects.

Shifting the focus from the spatial distribution of residents to the spatial distribution of housing prices, my third contribution shows that homeown-

[1] And, as a side note on environmental (and health) consequences, I will not conceal that there can also be negative effects of living in densely populated places: For air pollution in terms of particulate matter, which has a predominantly local effect rather than a global one like CO_2 emissions, Borck and Schrauth (2021) find that compact cities have higher concentrations of air pollutants than less densely populated places, implying adverse health effects for residents in more urbanized areas.

ership subsidies' incentives for residents to move to owner-occupied housing also has an effect on the market for rental housing. Exploiting the introduction of a new homeownership subsidy in Germany in 2018, I show that the exodus of new homeowners from their rental apartments translates into a negative demand shock in the rental market, lowering rents.

This finding is relevant because many cities around the world complain about high rents and the unaffordability of housing. However, policies such as rent control bring their own problems.[2] Diamond et al. (2019), studying the case of San Francisco, find that rent controls prevent renters' displacement in the short run but limit rental housing supply, likely driving up–rather than down–market rents in the long run. Mense et al. (2017) find that German rent control, where new rental housing units are exempt from the regulation, drives a "wedge" in the rental housing market: While the rent control decreases rents in the regulated sector, rents for new housing units (the unregulated sector) increase sharply.[3]

Since existing tenants receive no price signal about the scarcity of living space and thus no incentive to downsize their own living space, they do not move out of their apartments. Newcomers are thus forced into the unregulated–expensive–segment, which not only divides the housing market into regulated and unregulated units, but also divides the tenant population into new (unprotected) and existing (protected) tenants. And when rents in the unregulated segment become prohibitively high for newcomers, negative effects may also be seen in the productivity of a city's labor pool.

My contribution shows that subsidizing homeownership increases rental affordability without direct regulatory intervention in the price mechanism of the rental market, as it is the case with rent controls. This finding, which has not yet been documented in the empirical literature, adds to the–through the lens of rental affordability–positive societal effects of homeownership subsidies.[4]

[2] Most rent controls currently in effect are so-called "second-generation" rent controls that exempt parts of the housing market–usually newly built homes–from the regulation, in contrast to "first-generation" rent controls that have strict rent ceilings or even freeze rents for the entire market temporarily.

[3] Breidenbach et al. (2019) estimate the dampened effect on rent growth in the regulated sector to be only 2.5 % and thus very small.

[4] But of course, again, there could be more efficient ways of utilizing tax money to achieve the goal of rental affordability as by, e.g., directly paying subsidies to renters or subsidizing rental housing supply.

Thesis Structure

Chapter 2

Subsidizing homeownership makes cities decentralize, so Muth (1967) suggested over half a century ago, and so Glaeser (2011) and Voith (1999) have argued more recently. This chapter provides a first quasi-experimental test of "Muth's hypothesis". We analyze a homeownership subsidy's effects on urban form, by turning to Germany's 2005 subsidy *repeal*. Because housing in the city center was predominantly rental, prospective owner-occupiers needed to move to the city periphery. We are able to identify the subsidy's effect on decentralization because we capitalize on the subsidy's variation both in timing and design. We find that repealing the subsidy did contribute to recentralizing Germany's cities. This highlights the decentralizing role of the original homeownership subsidy. Inasmuch decentralization begets greater carbon dioxide emissions, encouraging homeownership is at cross-purposes with mitigating global warming.

This chapter was presented at the 11th Summer Conference in Regional Science of the German-speaking section (GfR) of the European Regional Science Association (online), the 2020 Virtual Meeting of the Urban Economics Association (by Kristof Dascher, online), the 16th Bavarian Micro Day (online), and the 28th Bavarian Graduate Program in Economics Research Workshop (Regensburg).

Chapter 3

This chapter documents effects of a homeownership subsidy's full repeal on the urban-rural residential location choice. First, I document the distribution of population across space for German labor market regions, using official NUTS-3 level population statistics. These labor market regions usually consist of a city (the urban core) and adjacent counties (the urban hinterlands) connected by commuter flows. Second, using IV-estimations in Difference-in-Differences and Triple-Differences frameworks, I exploit the 2005 repeal of Germany's lump-sum direct homeownership subsidy "Eigenheimzulage" on changes in this distribution across space. The results indicate that repealing subsidies to homeownership reverses subsidy-induced population flows to the periphery and thus makes regions re-urbanize. Cities' population gains derive in large parts from families with children and young residents of "building age", that are no longer able to become homeowners outside the city gates without the subsidy's support.

This chapter was presented at the 18th Bavarian Micro Day (online).

Chapter 4

This chapter analyzes the effects of German homeownership subsidies on the intra-city rent structure. Using a large-scale micro data set on German rent offerings, I first construct novel city rent indexes that include various rings around cities' centers. Using triple differences (TD) frameworks, I then estimate the introduction of the homeownership subsidies' effects on rent for the cities that received varying subsidy rates. The empirical results indicate that subsidies to homeownership lower central apartment rent premiums in those cities, where they give the "biggest bang for the buck". Consequently, I find that homeownership subsidies contribute to an increase in housing affordability through the price changes in the rental market: an increase in the subsidies leads potential homeowners to move away from the center, resulting in a decrease in the rental demand and lowering the rent.

This chapter was presented at the 60th Congress of the European Regional Science Association (online), the 31st Bavarian Graduate Program in Economics Research Workshop (online), and the 10th European Meeting of the Urban Economics Association (online).

Homeownership Subsidies and the Spatial Distribution of Residents within Cities

2

This chapter is based on Daminger & Dascher (2020) City Skew and Homeowner Subsidy Removal, BGPE Discussion Paper No. 195.

2.1 Introduction

Subsidizing homeownership makes cities decentralize, so Muth (1967) suggested over half a century ago, and so Glaeser (2011) and Voith (1999) have argued more recently. This paper provides a first quasi-experimental test of "Muth's hypothesis". We analyze a homeownership subsidy's effects on urban form, by turning to Germany's 2005 subsidy *repeal*. Because housing in the city center was predominantly rental, prospective owner-occupiers needed to move to the city periphery. To encourage homeownership inevitably meant to call for moving out. Repealing the homeownership subsidy revoked that call.

Germany's cities have recentralized conspicuously ever since the subsidy was repealed. Controlling for distance from the city center and for various fixed effects, population in every central ring (i.e., a ring among the third of rings closest to the center) grew by 5% between 2005 and 2017; while population in every peripheral ring (i.e. a ring among the two thirds of rings closer to the urban fringe) contracted by 2% over the same period. We label these asymmetric adjustments: *recentralization*, even as we understand that cities are open and that adjustments in rings are more than just mere rearrangements of the existing city population. It may be tempting to attribute recentralization to subsidy repeal. However, recentralization may also be driven by other forces.

To address the confounding influence of such other forces, we repeatedly make use of the fact that subsidy repeal affected but a subset of households. I.e., we exploit the variation in treatment resulting from subsidy repeal's timing and from the original subsidy's design. In terms of timing, the subsidy only benefited those old enough to have applied prior to repeal. In terms of design, the subsidy (essentially) only benefited those living in cities where real estate was not too expensive to begin with. Subsidy repeal undid either type of privilege. Repeal "treated" younger households (who then would never become eligible for the subsidy) and affordable city residents (dito), yet spared from treatment older households (who had long bought their home or had decided against it) and those in expensive places (who never could afford a home in the first place).

Timing-wise, we expect the decentralization of younger households to slow relative to that of older households–if not to reverse altogether. Design-wise, we expect the decentralization of affordable city residents to slow relative to that of residents in more expensive cities–if not to reverse altogether. Our empirical evidence bears out *both* these expectations (as explained shortly). We enter this evidence into the counterfactual scenario of how city peripheries would have evolved had the subsidy not been repealed. From the perspective of timing, our estimates suggest that younger households would have built app. 200,000 homes extra in city peripheries, had the subsidy not been repealed. From the design perspective, our estimates are nearly identical, implying that affordable city households would have added app. 200,000 homes to city peripheries had the subsidy not been repealed. These figures happen to agree on the estimated number of homes averted by subsidy repeal. More importantly, these figures inform us of the strong decentralizing effect *of the original subsidy* itself. They conform with, but also make more precise, "Muth's hypothesis".

We mean to contribute to the literature on the subsidy's impact on the spatial distribution of housing. While there is an extensive literature on the homeownership subsidy, much of this literature focuses on the merits or externalities of homeownership (e.g., DiPasquale and Glaeser (1999)) or on the subsidy's effects on homeownership attainment, welfare, house prices and rents (e.g., Hilber and Turner (2014) and Sommer and Sullivan (2018) and Kaas et al. (2021) more recently), rather than on urban form. There also is a vast literature on program evaluation, yet with the exception of Gruber et al. (2021)–who analyze Denmark's partial subsidy repeal but do not explicitly connect it to urban form–this literature does not address the homeownership subsidy. To the best of our knowledge, our paper is the first to occupy the two literatures' intersection; it is the first quasi-experimental analysis of the subsidy's effect on urban form.

Our paper is related to Gruber et al. (2021), however. These authors do not explicitly address urban form, but since they find that Denmark's repeal had no effect on homeownership attainment among high- and middle income households[1] (and since there was no repeal for low-income households), their results appear to imply that repeal had no effect on Danish cities' form. This seems at odds with our results. But note that Germany's repeal was for a lump sum subsidy targeted at individuals with a two-year maximum income of never more than €122,710 (and even less for most of its duration). That subsidy repeal mattered little to affluent individuals' decision on tenure appears perfectly consistent with a strong role of subsidy repeal for the tenure decision of individuals with much lower (or even low) incomes.[2]

Our fundamental measure of urban form is the distribution of population across city rings, i.e. the city's population "profile" or "shape" (Arnott and Stiglitz 1981). Changes in this distribution may take all sorts of form. Remarkably, we will see that changes in city shape over the period under investigation exhibit a particularly striking pattern. I.e., changes in ring populations' shares switch from all positive near the city center to all negative further out. It is in this sense that Germany's cities have actually become "more compact" (Dascher 2019). But we also track a more convenient summary measure of urban form, in estimating the city's "urban-suburban population gradient". Ring population first increases, then decreases in distance from the city center and so there is no unique population gradient on raw data. However, if we fit a spline to pre-reform ring population we may define a "population gradient" as the *extra* in population a peripheral ring enjoys over a central ring (conditional on the spline). Any subsequent growth (contraction) in this gradient (as might come about via unobservable shifts–or subsidy repeal) serves as an indication of growing (relenting) decentralization.

Various shocks may overlap with, and hence bias our understanding of, subsidy repeal. For example, larger cities' wage premia rose during the period under consideration (Dauth et al. 2018), surely pulling at least some

[1] For Denmark's three income brackets, repeal "raised net-of-tax interest rate by about 80 percent for the top group, by about 30 percent for the middle group, and left it roughly unchanged for the bottom group" (Gruber et al. 2021).

[2] We might add that the number of individuals with higher incomes is smaller than those with incomes below the eligibility threshold, and so behavioral changes induced in lower income brackets must matter more.

residents closer to the city center. Additional immigration came with the 2007/08 financial crisis and the subsequent crisis of the Euro, and with Syria's civil war around 2015/16. Many cities also expanded their child care facilities at their centers, enabling parents to re-enter the labor market earlier yet also drawing them closer to these facilities. To address these and many other (unobservable) changes, we allow for city and time fixed effects, and for interactions between the two. Ultimately, however, the desirable consistency of our estimates comes with our estimation design. This design provides for additional differencing, and hence further refines those who are treated and those who are not.

We difference our population data three times. Our basic, first, "difference" (D) is the city's urban-suburban population gradient. Our next difference, as a "diff-in-diff" (DD), is the shift in that gradient from before, to after, repeal. Such a shift in the population gradient may arise due to subsidy repeal, yet may also reflect an increase in central city amenities, rising female labor participation, international immigration into minority communities historically anchored to city centers, etc. To be sure to swipe out any such (observable or unobservable) urban-suburban shifter concomitant with subsidy repeal, we take yet another difference, across treated and untreated. This last difference, a "diff-in-diff-in-diff" (DDD, pioneered by Gruber (1994)), gives the extent to which population gradient shifts differ across age cohorts or city affordability. We expect the triple-diff estimator to provide a consistent estimate of subsidy repeal's impact.

Fig. 2.1 showcases our estimates of all three differences, as obtained further below in the paper's empirical section.[3] Estimated pre-repeal gradients ("D") for both our treatment scenarios (that is, "home accessibility" on the left and "home affordability" on the right) are found to either diagram's left, as the blue and red dots. Initial gradients are equal to, or at least close to, zero. Next, *changes of gradient* ("DD") can be read off the blue and red graphs' slopes. We note that gradient estimates for the treated–i.e. the young, as well as households in affordable cities–go down (graphs in red). Recalling the gradient as a peripheral ring's extra in population (vis-à-vis a central ring), we see that centers become stronger with the young, and in more affordable cities.

None of this, however, need be a convincing indication of subsidy repeal's effects. Possibly recentralization is similar, or even stronger, for the untreated?

[3]That is, in subsections 2.4.1 and 2.4.2. Rather than show a "difference-in-differences-in-differences" of *population*, Fig. 2.1 shows the equivalent "difference-in-differences" of the *population gradient*.

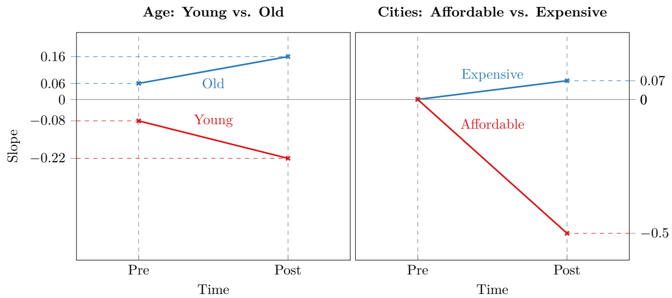


Fig. 2.1: Population Gradient Before and After Repeal, By Treatment

To address this concern, we turn from gradient changes to the *differences in changes of gradient* ("DDD"). In Fig. 2.1 these differences can easily be gauged from the differences in the blue and red graphs' respective ascent. Where estimated gradients for the treated went down (as just explained, see the graphs in red), the estimated gradients for the untreated went *up* (and certainly not down, graphs in blue), and this is true irrespective of type of treatment. I.e., far from also getting stronger, city centers become weaker, both with the old and in expensive places. *A fortiori* this implies that the gradient change for the treated (in red) is less than that for the untreated (in blue). Peripheries' population extra suffers more with the treated than with the untreated. No general shift in the balance between center and periphery is able to explain this realignment.

But subsidy repeal *is*. Our data are built from a large, finely graded sample of various urban demographics indexed by city, distance to the central business district (CBD), and year. We match official population statistics to city district level shape files (embodied in GIS information), then approximate various population strata for the full set of 1 km-wide rings around the city center. And while micro data are unavailable to us, we are able to inspect the impact of subsidy repeal on population strata particularly susceptible to the policy change, e.g., middle-aged vs. young individuals or households with vs. households without children. Available data cover 83 of the largest German cities and, for most cities, all years between 2002 and 2017. The ring data we obtain hence extend from 4 years before, to 12 years after, subsidy repeal. They permit us to trace in great detail the distribution of various demographics across city rings from before, to after, the reform.

There are three important ways in which Germany's repeal provides a suitable context for the analysis of a homeownership subsidy. First, repeal was for a federal, not for a local, subsidy. All cities saw their subsidy expire simultaneously. From any individual city's perspective, repeal was exogenous. It was certainly independent of how many of its households wanted to move out, and when. Second, the subsidy had been generous[4], and its repeal was full. Should repeal have the effects predicted above, they are more likely to manifest themselves under such a full, rather than a partial, repeal. Finally, repeal was independent of household income, rather than dependent on it, as would have been true for a repeal of the more common mortgage-interest-tax-deduction homeownership subsidy type. Every household was faced with the same nominal repeal, essentially reducing the number of dimensions of treatment variation down to both household age and real estate affordability.

Homeownership is often believed to benefit neighboring properties, both directly as well as via better local governance. But the spatial "side-effects" detailed above at least in part offset the benefits from subsidizing it. Decentralization matters to urban welfare, too. Various authors emphasize that urban form, one way or another, matters to residents' well-being. Brueckner (2000) emphasizes the benefits from decentralization, by pointing out how decentralization enables households to consume more housing; whereas Harari (2020) argues that cities "lose shape" when "growing out", and that such shape loss comes along with reduced urban connectivity. Harari (2020) identifies households' positive willingness-to-pay for living in more connected, i.e. less decentralized, cities, and hence points out the loss in urban welfare implied by decentralization.

In addition, Glaeser (2011) and Glaeser and Kahn (2010a,b) emphasize the global-warming related externalities associated with housing decentralization. Longer commutes, more spacious suburban homes, and larger and more cars per household all imply larger carbon dioxide emissions. In terms of climate change mitigation, recentralizing housing may contribute to reducing carbon dioxide emissions. In terms of climate change adaptation, recentralizing housing may seal less ground surfaces, and may thereby help attenuate those (often uninsured (Hennighausen and Suter 2020)) risks associated with river flooding, heavy precipitation and even landslides–risks considered increasingly relevant according to IPCC (2021, p. 3158).

[4]Federal government's aggregate yearly expenditures on homeownership subsidies had attained a staggering 11 billion Euro by 2004. By then expenditures on homeownership promotion had become the single largest subsidy in the federal budget. From a cumulative perspective, these expenditures summed to €106 billion over the 10 years the subsidy was in place.

The paper has six sections. Section 2.2 lays out the subsidy's design. Section 2.3 details the assembly of our geospatial city-ring-year panel, and presents some preliminary and coarse observations on urban structure. Section 2.4 sets out the much finer city ring population as a spline of distance to the CBD, and interacts changes in population profiles with cohort age (subsection 2.4.1) and housing affordability (subsection 2.4.2), to identify subsidy repeal's impact on urban form. Section 2.5 provides a discussion of our results, and also pursues the various counterfactuals made possible by them. These provide novel insights into the strength of the homeownership subsidy itself. Section 2.6 concludes.

2.2 Subsidy Timing and Design

Germany's homeownership subsidies start with the housing shortage following WW II. One can distinguish roughly four phases here. In a first phase (1949 to 1995), investment into owner-occupied property was income tax deductible, by way of a tax depreciation option. In the second phase (1996 to 2005), investment into owner-occupied property was subsidized lump-sum instead (*Eigenheimzulage* in German, and EZ for short). EZ was terminated by the end of 2005. In the following third phase, extending from 2006 up until 2017, the homeownership subsidy paused. Finally, and only as recently as 2018, federal government temporarily (for now) restored the homeownership subsidy, by introducing a variant of EZ for another three years (the fourth, and current, phase).[5] This paper exploits the transition from phase 2 to phase 3.

Table 2.1 provides an overview over essential features of the homeownership subsidy as they applied in phase 2. The subsidy in fact split into two separate prongs. Newly built homes were subsidized more than existing homes. Let q_3 (q_2) denote the price of a newly built (existing) home (where we reserve the price q_1 for the rental housing introduced further below). Then, for every year over a period of eight years altogether, subsidy payments amounted to $\min\{0.05 \cdot q_3, 2\,556\}$ Euro per newly built home, as opposed to only $\min\{0.025 \cdot q_2, 1\,278\}$ Euro for an existing home.[6] Com-

[5] This variant is the so-called *Baukindergeld*, or BK below. The state of Bavaria tops up BK by an extra 300 Euros.

[6] Our term "home" here applies to condos, apartments, detached or semi-detached housing alike, as long as they are owner-occupied. The distinction between newly built and existing homes was eventually lifted, in 2004. Then, and in year 2005, the subsidy was reduced to $\min\{0.01 \cdot p, 1250\}$ Euros, $p \in \{q_2, q_3\}$ for either type of property.

Tab. 2.1: EZ-Design: Prerequisites, Recipients, Payments, etc.

	1996–1999	2000–2003	2004–2005
Beneficiary			
Recipient		— Income tax liable individuals —	
Maximum 2-year taxable income	€122,710 (singles) €245,420 (couples)	€ 81,807 (singles) €163,614 (couples)	€ 70,000 (singles) €140,000 (couples)
Threshold increase per child	—	€ 30,678	€ 30,000
Object			
Subsidized Property		— Owner-occupied property (house or condo) —	
Subsidy			
Funding start		— Year of aquisition —	
Funding period		— 7 subsequent years —	
Child allowance	€767 per child	€767 per child	€800 per child
Yearly subsidy amount (baseline)			
New Construction (q_3)	min {5.0% of q_3, €2,556}	min {5.0% of q_3, €2,556}	min {1.0% of q_3, €1,250}
Existing Property (q_2)	min {2.5% of q_2, €1,278}	min {2.5% of q_2, €1,278}	min {1.0% of q_2, €1,250}

Note: This table represents the schematic structure of the subsidy. The subsidy can be divided into three time periods (second to fourth column): (i) 1996–1999, (ii) 2000–2003, and (iii) 2004–2005. The first change in 2000 applied to income thresholds only: these were reduced further, but could now also be increased in the presence of children. The second change in 2004 was more comprehensive: not only were the general income thresholds reduced even, also the distinction between the purchase of existing property and new construction was removed. From now on, both types of owner-occupied housing were subsidized equally. Over the entire period, the subsidy was paid out only upon moving into the owner-occupied property and then for a total period of eight years. Source: German Home Owners' Allowance Act (*Eigenheimzulagegesetz [EigZulG]*) with its amendments.

mon to all specifications for phase 2, households with children were always entitled to another €767 per child and year.[7]

Transition from phase 2 to phase 3 was gradual. Those who had applied for the subsidy by the end of 2005 remained entitled to receiving it up until eight years later.[8] As mentioned, subsidy payments were highly similar across cities. This was especially true if there were children. Take, as one not overly contrived example, a married couple with two children (and with combined 2-year taxable income of no more than €163,614) buying a new home in 2003 at the price of €200,000 (i.e. in an "expensive" city). This family would have received $2,556 + 2 \times 767$ a year, or a total €32,720 over all eight years. That same family would have received the identical total of €32,720 when buying a newly built home in an "affordable" city in which that same home cost only half as much.[9]

Terminating EZ meant terminating subsidies to *both*, existing and newly constructed homes. A minimum framework to sort out the net impact of this joint removal must allow for three types of housing: owner-occupied new housing and owner-occupied existing housing (the two subsidized types

[7] Subsidies applied to first homes, but couples were eligible for second homes, too.

[8] In fact, subsidy pay out period could be pushed back even further if, for example, applications for subsidy and building permission had been in by 2005 while construction was only completed by 2009.

[9] Generally, for any two homes costing more than the threshold €51,120 (a threshold rarely not passed) subsidy payments would have been the same.

of housing) and rental housing (the single non-subsidized type). The effect of simultaneously removing both of these subsidies (themselves of unequal size) is not obvious. We build on a multi-quality, Sweeney (1974)-type framework, and introduce three qualities of housing, with newly built owner-occupied homes (in the periphery) the best, existing owner-occupied homes (also in the periphery) the second best, and rental housing (in the city center) the lowest quality.[10] We assume fully elastic supply of peripheral new housing at construction cost $\overline{q}_3$, and we denote subsidies to existing and newly constructed housing as σ_2 and $\sigma_3 = 2\sigma_2$, respectively.

Twin subsidy removal then changes the structure of equilibrium prices. Appendix A.1 shows how joint subsidy removal implies $dq_1 > 0$. The rise in the equilibrium rental price has us conclude that if government removes its twin subsidy on new and existing owner-occupied housing, rental housing population (near the city center) goes up. Correspondingly, the two segments of owner-occupied housing recede, given the induced filtering inflow into central city rental housing. These observations underlie our subsequent strategy of discussing removal *as if* a single subsidy had been repealed.[11]

2.3 Data

Much as we would prefer to analyze a micro panel of EZ beneficiaries, this type of detailed information is not available, as noted above.[12] However, we are able to analyze strata of the urban population that are particularly (un-)susceptible to subsidy repeal (i.e. different age cohorts and households with vs. without children), and at the level of the very narrow ring. Let $2\pi r$ give the approximate area of the 1 km wide concentric ring around the CBD starting at distance r. If $D(r)$ is population density at distance r, then $g(r) = 2\pi r D(r)$ approximates the share of population inhabiting the 1-km-wide ring starting at r km away from the CBD. Let $\widetilde{r}$ denote the maximum distance from the CBD to the city's administrative boundary, i.e. "city size".

[10] Such a tenure-quality-hierarchy may be justified by appealing to informational asymmetries in housing (e.g., as in Arnold and Babl (2014)).

[11] These observations also indicate that subsidy removal has both quantity and price effects. Unfortunately, suitable rental data are not available for the years preceding subsidy repeal, and so we are not able to test our predictions on quantities and prices jointly. But see Daminger (2021b) for an analysis of implied changes in rents for a later round of the homeownership subsidy in Germany, from 2018 to 2020.

[12] Though a federal subsidy, EZ was not administered federally. Instead, local tax offices screened applications and supervised subsidy payout. According to the Federal Ministry of Finance, nowhere were data consolidated. This lack of centralized information may also help explain the dearth of studies on EZ.

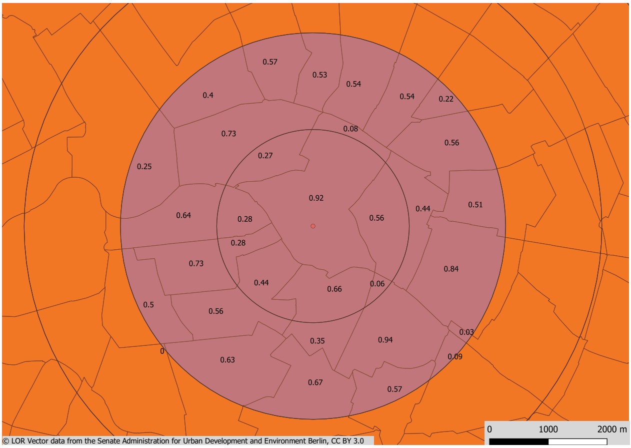


Fig. 2.2: Shares α_{11} and α_{12} for Berlin's first two rings

Note: The map illustrates how we intersect administrative districts with city concentric rings, using Berlin as an example. Polygons in the background show Berlin's administrative districts, while the purple circular area represents the first two rings around Berlin's historic city center (i.e. the city center, as the small red dot). Using GIS techniques, we intersect the area of each district with the ring partition. Any given ring's figures in black show the fraction of the district area falling into that ring. The district's population then is split between rings according to these area shares. Data: Authors' illustration using LOR vector data by Berlin's Administration for Urban Development and Environment.

Then as r ranges from 0 to $\widetilde{r}$, $g(r)$ captures the city's "population profile", or its "shape" (Arnott and Stiglitz 1981).

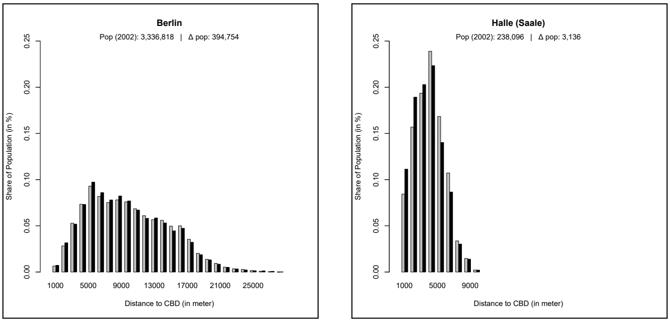
Data on g are not available for Germany, and so we infer them from available population data on cities' administrative subdivisions, resorting to standard geospatial techniques. Highly detailed subdivision data are provided by BBSR[13] and KOSTAT[14] for the largest German cities[15], and (in most cities) for all years 2002 through 2017. As the city's CBD we often (i.e. whenever possible) choose city hall.[16] Given the CBD's geo-coordinates, we next equate $\widetilde{r}$ with the maximum length of all rays extending out from the CBD. We partition the city into 1 km wide concentric rings around the CBD, and

[13] BBSR: Bundesinstitut für Bau-, Stadt- und Raumforschung.

[14] KOSTAT: KOSIS-Gemeinschaft Kommunalstatistik.

[15] We had to omit 21 among the 100 largest cities from this list, because for these cities shapefiles (see below) and/or data on population are missing. These cities are Osnabrück (48th in a list ordered by city size), Leverkusen (49th), Paderborn (56th), Heilbronn (62nd), Bottrop (66th), Bremerhaven (70th), Hildesheim (79th), Cottbus (80th), Kaiserslautern (81th), Gütersloh (82th), Hanau (84th), Ludwigsburg (87th), Esslingen am Neckar (88th), Iserlohn (89th), Düren (90th), Flensburg (93th), GieSSen (94th), Ratingen (95th), Lünen (96th), Marl (99th) and Worms (100th).

[16] When city hall no longer exists, we pick the central market square or some other significant building or square (a cathedral, for example) that could justifiably be considered part of the CBD.



Berlin, 2002-2017 Halle, 2002-2017

Fig. 2.3: Two Selected Population Profiles

Note: The panel on the left shows Berlin's population profile, while the panel on the right shows that for the city of Halle. The height of a bar depicts the share of the ring population in total city population at distance r from the city center. Gray bars show the ring population share in 2002, while bars in black show the corresponding share for 2017. In both cities, population shares near the city center (city fringe) are greater (smaller) in 2017 than in 2002. Data: Authors' calculations with KOSTAT data.

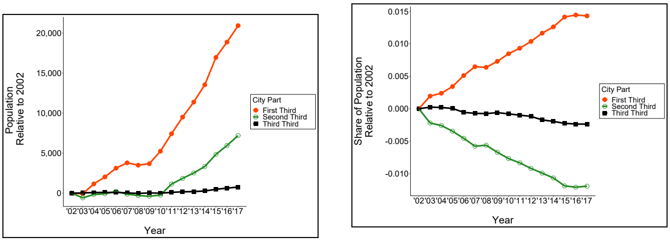
then intersect this partition with the city shapefile polygons.[17] Fig. 2.2 gives one example of the procedure, for Berlin's first two concentric rings around the historic city hall (itself shown as a small circle at the center of the map).

For each of city i's subdivisions $s = 1, \ldots, S_i$ we first use GIS to identify the area of the intersection of that subdivision with ring j, A_{sj}. Then $\alpha_{sj} = A_{sj}/A_s$ is the share of city ring j in subdivision s's area A_s. Of all n_s residents in subdivision s we next apportion $\alpha_{sj} n_s$ individuals to ring j.[18] Repeating this procedure for all subdivisions and summing over respective contributions, we estimate total population in city i's ring j at $n_{ij} = \sum_{s=1}^{S_i} (A_{sj}/A_s) n_s$. Repeating this procedure for every city in the sample yields the full set of population profiles, $\{g_i\}$. Fig. 2.2 highlights the procedure for Berlin's first two rings. For example, 92% of the centralmost subdivision's population are assigned to the first, while 8% are assigned to the second ring.

Fig. 2.3's two diagrams show the profiles g_i we obtain for Berlin and the (substantially smaller, more affordable) Eastern city of Halle. Central rings

[17] City shapefiles indicate subdivisions' polygonal boundaries. Where shapefiles are not publicly available we contacted municipal cadastral offices.

[18] This is an exact procedure only if residents are uniformly distributed across space–which of course they are not. We consider it a reasonable approximation.



2002–2017, Population

2002–2017, Population Share

Fig. 2.4: Recentralization in Germany's cities

Note: The figure's left-hand panel shows the average of population in city ring thirds between 2002–2017, while the right-hand panel traces the corresponding average of population shares. In absolute terms, the 1st and 2nd third of rings gain population, while the peripheral third of rings sees its population stagnate. In relative terms, the average share of a cities' population living in the centralmost third of rings rises, while the 2nd and 3rd thirds' shares both shrink. Data: Authors' calculations with KOSTAT data.

have gained weight in either city. This is the recentralization of population apparent from the raw data, and it represents a common trend present in almost all cities in the sample. Whenever possible we will make use of the full sample of 83 cities. Data are not always available for the full sixteen years 2002–2017, and this is why our (unbalanced) panel comes to somewhat less than the full number of observations. At best (i.e. for the analysis in subsection 2.4.2) our sample cities account for slightly over 22 million individuals (in 2002), and represent nearly one fourth of the country's population.

To provide some preliminary insight into recentralization, we aggregate every city's set of rings into consecutive subsets of thirds. We coarsely equate the 1st third of rings with the empirical counterpart of the previous section's rental housing (quality 1), the 2nd third with the counterpart of existing homes (quality 2), and the 3rd third with the remaining segment hosting newly built homes (quality 3). The first panel in Fig. 2.4 shows the change in the sample average of ring thirds' population over time. On average, the 1st third of rings (graph in red on screen) grows by over 20,000 residents between 2002 and 2017. Residents in the 2nd third of rings (green graph) on average also become more numerous, if only later and less so. Average population in the last third of rings (black graph) essentially stagnates.

Taking averages conceals cities' heterogeneity. For example, while 58% of Berlin's residents inhabit the 1st third of rings, and the share of those who populate the 2nd third is 40%, in the small city of Weimar the 1st and 2nd thirds of rings host very different shares of 73% and 25%, respectively.[19] So we alternatively cast our diagrams in terms of ring thirds' shares in city population (Fig. 2.4's second panel). Here we see that the 1st third's share on average grew by 1.5 percentage points almost; while the 2nd and 3rd thirds' shares both *shrank*. These observations starkly illustrate the extent to which Germany's larger cities underwent recentralization. Of course, these observations are based on mere sample averages for ring thirds, which themselves are coarse measures of city spatial structure. To estimate subsidy repeal's causal impact, we now turn to our full panel of finer profiles g.

2.4 Results

The standard monocentric city model (exhibiting $D'(r) < 0$) guides our choice of specification. Differentiating ring population $g(r) = 2\pi r D(r)$ gives

$$g'(r) \;=\; 2\pi D(r) \;+ 2\pi r D'(r). \tag{2.1}$$

The first term on the r.h.s. of eq. 2.1 is positive, while the second term is negative. Consider the marginal ring one mile further out. On the one hand, its population is greater because its ring area is (an "area effect"). On the other hand, its population is smaller because building height is (a "density effect"). Let us assume that $g''(r)$ is negative, i.e. that $2D'(r)+D''(r)r < 0$, so that population profile g is concave.[20] Concavity captures the hump-shape we observed earlier, in Fig. 2.3. Setting eq. 2.1 equal to zero and rearranging gives $1/r = -D'(r)/D(r)$, and this condition locates the r for which g is maximal, denoted r_0. For distances smaller than r_0 the "ring area effect" dominates; while for distances greater than r_0 the "density effect" does.

Baseline equation 2.2 "linearizes" $g(r)$ in piece-wise fashion, by explaining the conditional expectation of the population (or some stratum thereof further down) inhabiting city i, ring j and period t, y_{ijt}, with a simple spline. The spline's knot r_0 we set such that one third of rings are closer to, while two thirds of rings are further away from, the CBD. Further, μ_i is the city i fixed effect, PERI is a city periphery dummy and equal to 1 if ring j belongs to the last two thirds of rings (and zero else), while POST is the treatment period dummy and equal to 1 if year t dates to after 2005, the year of

[19] This also is why we consistently add city fixed effects later.
[20] This assumption holds as long as the density profile $D(r)$ is not too convex in r.

Tab. 2.2: Diff-in-Diff on Population

	(1)	(2)	(3)
Intercept	9.16^{***}	9.37^{***}	9.38^{***}
	(0.18)	(0.15)	(0.15)
Distance	0.39^{***}	0.23^{***}	0.23^{***}
	(0.08)	(0.04)	(0.04)
Peri × (Distance - $\tilde{r}/3$)	-0.80^{***}	-0.66^{***}	-0.66^{***}
	(0.09)	(0.06)	(0.06)
Peri	-0.35^{*}	0.02	0.02
	(0.20)	(0.15)	(0.15)
Post	0.03^{**}	0.07^{***}	0.05^{***}
	(0.02)	(0.02)	(0.02)
Peri × Post	-0.07^{***}	-0.07^{***}	-0.07^{***}
	(0.02)	(0.02)	(0.02)
City FE	No	Yes	Yes
City FE × Post	No	No	Yes
Adj. R^2	0.56	0.79	0.79
Num. obs.	14939	14939	14939
N Clusters	83	83	83

Note: OLS regressions with the logarithm of ring population as the response variable. Clustered standard errors (at city level) in parentheses. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

subsidy repeal (and 0 else). So our point of departure is the following diff-in-diff specification:

$$
\begin{aligned}
E(y_{ijt}|x_{ijt}) &= \alpha_0 + \mu_i + \alpha_1\,\text{DIST}_j + \alpha_2\left(\text{DIST}_j - \widetilde{r}_i/3\right) \times \text{PERI} \\
&+ \beta_1\,\text{PERI} + \beta_2\,\text{POST} + \beta_3\,\text{PERI} \times \text{POST}, \qquad (2.2)
\end{aligned}
$$

with x_{ijt} shorthand for the full list of covariates. The spline captures the city center's non-linear population attraction, as captured by coefficients α_1 and α_2. The coefficient of PERI, β_1, captures the "population gradient" obtained once we have controlled for the spline. The coefficient of POST, or β_2, assesses the change in population in the more central rings from before to after the reform. Most importantly, the coefficient of PERI × POST, or β_3, captures the extent to which the population gradient has adjusted from before to after reform. Absent the confounders that we discuss shortly, we expect $\beta_3 < 0$ (joint with $\alpha_2 < 0 < \alpha_1$). Given subsidy repeal, the population gradient should *fall*.

Table 2.2 shows our OLS coefficient estimates for eq. 2.2. Following column 1, the population gradient *did* decrease, by substantial and significant 7 percentage points. Following columns 2 and 3, this estimate remains unchanged, even as we control for city fixed effects (column 2) and city specific time effects (column 3). Going by the third column, before subsidy repeal each peripheral ring had an extra 2% of population over and above of what accounting for the spline would have us expect for the typical central ring; while after reform, it had 5% *less*. Alternatively put, the typical central ring grew by 5% from before to after reform, whereas the typical peripheral ring concomitantly shrank by 2%. I.e., controlling for city fixed effects, city specific time trends and distance to the CBD produces estimates of peripheral rings' population changes that are nearly twice as large as those gauged from the raw data shown in the panel on the right-hand side of Fig. 2.4.

These estimates give a flavor of the strength of the recentralization underway, and also are consistent with what we expect from subsidy repeal. To check if recentralization might even have started *prior* to subsidy repeal, we re-estimate eq. 2.2 on replacing POST with a full set of yearly dummies D_t, then estimating

$$\begin{aligned} E(y_{ijt}|x_{ijt}) &= \alpha_0 + \mu_i + \alpha_1\,\mathrm{DIST}_j + \alpha_2\left(\mathrm{DIST}_j - \widetilde{r}_i/3\right) \times \mathrm{PERI} \\ &+ \beta_1\,\mathrm{PERI} + \sum_{\substack{t=2002\\ t\neq 2005}}^{2017} \beta_t\,D_t + \sum_{\substack{t=2002\\ t\neq 2005}}^{2017} \gamma_t\,\mathrm{PERI} \times D_t \end{aligned} \quad (2.3)$$

Fig. 2.5 plots the estimated yearly shifts in the gradient relative to the 2005 gradient, $\widehat{\gamma}_t$, over time, joint with their confidence intervals. Pre-repeal, coefficient estimates essentially oscillate around zero; while post-repeal they are strictly negative always. This suggests that recentralization had not set in before the subsidy was removed (even if the pre-event time period on which we base this conclusion is admittedly short). Yet recentralization clearly did take off once the subsidy was repealed. Post-repeal, so we might add, coefficient estimates not just dropped; they dropped increasingly so. Intuitively, this reflects cohort after cohort of younger renters ceasing to move out, ultimately leading to a cumulative build-up in central rings' population advantage.

And still, while nothing appears to have driven city center and city periphery apart *before* repeal, we cannot rule out the possibility of some confounding effect setting in *joint with* repeal. For example, the 7%-decrease of the population gradient according to Table 2.2 might partly also be due to some concomitant "improvement in living centrally", rather than to the subsidy repeal itself. This concern motivates our "diff-in-diff-in-diff" approach (DDD) (see Gruber (1994)) over the following two subsections next. We consider two variations on this triple-diff perspective.

First we compare the change in population gradient (itself a "difference-in-differences") for the young with that for the old. As long as it affects both

Fig. 2.5: Additions to Population Gradient between 2002 and 2017

Note: This figure shows the estimated coefficients of γ_t from Equation 2.3. For this regression we restrict our sample to the 57 cities for which we have gap-free data from 2002–2017 (see Table A.2 in the Appendix). Data: Authors' calculations using BBSR and KOSTAT data.

young and old uniformly, any urban-suburban shifter such as a "general improvement in living centrally" will drop out from the difference between these gradient changes; while subsidy repeal, in affecting the young but not the old, will not (subsection 2.4.1). Likewise, we then compare the change in population gradient (a "difference-in-differences") taking place in affordable cities with that occurring in non-affordable ones. And again any "general improvement in living centrally" must drop out from the difference in these changes; whereas subsidy repeal, in affecting only those in affordable cities, must not (subsection 2.4.2).

2.4.1 Treatment by Accessibility

Repealing the homeowner subsidy meant repealing it for those too young in 2005 to have bought a home, for lack of income. It did not mean repealing it for those old enough to have bought a home, and applied for the subsidy, by then, though. We define as "young" in any given year those who are between 15 and 29 years, as "old" all middle-aged individuals in brackets 30–44, and as "very old" those who are 45 through 59. For the course of the 15 years that followed the year 2002, the young turned old as the old

Tab. 2.3: Old vs. Young Individuals

	OLS				2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	7.78***	7.84***	7.78***	7.83***	7.77***	7.88***
	(0.15)	(0.15)	(0.17)	(0.15)	(0.16)	(0.16)
Distance	0.21***	0.21***	0.21***	0.21***	0.21***	0.21***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Peri × (Distance - $\tilde{r}/3$)	−0.62***	−0.62***	−0.61***	−0.62***	−0.61***	−0.61***
	(0.06)	(0.06)	(0.07)	(0.07)	(0.07)	(0.07)
Post	−0.07**	−0.12***	−0.08***	−0.12**	−0.08**	−0.19**
	(0.03)	(0.03)	(0.03)	(0.05)	(0.03)	(0.07)
Young	−0.85***	−0.85***	−0.86***	−0.86***	−0.86***	−0.86***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Peri	0.06	0.06	0.05	0.04	0.05	0.05
	(0.15)	(0.15)	(0.15)	(0.15)	(0.16)	(0.16)
Peri × Young	−0.14***	−0.14***	−0.14***	−0.14***	−0.14***	−0.14***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Post × Young	0.96***	0.96***	0.97***	0.97***	0.97***	0.97***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Post × Peri	0.10**	0.10**	1.43**	2.89***	−0.63	−0.38
	(0.05)	(0.05)	(0.54)	(0.95)	(1.35)	(2.51)
Post × Young × Peri	−0.24***	−0.24***	−0.24***	−0.24***	−0.24***	−0.24***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Peri × Post × Female Labor			−2.47**	−5.22***	1.39	0.93
			(1.01)	(1.77)	(2.51)	(4.69)
City FE	Yes	Yes	Yes	Yes	Yes	Yes
City FE × Post	No	Yes	No	Yes	No	Yes
Adj. R^2	0.82	0.82	0.82	0.82	0.81	0.81
Num. obs.	4658	4658	4350	4350	4350	4350
N Clusters	50	50	46	46	46	46

Note: OLS regressions with the logarithm of the population count (in age strata) as the response variable. We match up age cohorts of years 2002/2003 (pre-subsidy repeal) and years 2016/2017 (post-subsidy repeal). For years 2002/2003, dummy Young equals 1 (zero) for residents aged 15–29 (30–44). For years 2016/2017, dummy Young equals 1 (zero) for residents aged 30–44 (45–59). Clustered standard errors (at city level) in parentheses. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

turned very old. We reasonably expect the initially old to move out into the home they had bought just in time prior to subsidy repeal, and the initially young to stay put. Empirically, we match up age cohorts in our data set by essentially setting up the 2002 number of young (old) against the 2017 figure of old (very old).

Let dummy YOUNG equal 0 (one) if the stratum in city i's ring j is 30 to below 45 (15–29) in 2002 and 45 to below 60 (30–44) in 2017. Our baseline equation is the following diff-in-diff-in-diff specification (DDD):

$$\begin{aligned} E(y_{ijt}|x_{ijt}) &= \alpha_0 + \mu_i + \alpha_1 \,\text{DIST}_j + \alpha_2 \left(\text{DIST}_j - \widetilde{r}_i/3\right) \times \text{PERI} \\ &+ \beta_1 \,\text{POST} + \beta_2 \,\text{YOUNG} + \beta_3 \,\text{PERI} \\ &+ \gamma_1 \,\text{POST} \times \text{YOUNG} + \gamma_2 \,\text{POST} \times \text{PERI} + \gamma_3 \,\text{YOUNG} \times \text{PERI} \\ &+ \delta \;\text{POST} \times \text{PERI} \times \text{YOUNG}. \end{aligned} \tag{2.4}$$

In eq. 2.4, it is coefficient δ that identifies the extent to which the population gradient for the young shifts differently from the gradient for the old, over the 15 years under scrutiny. We expect $\delta < 0$, i.e. that whatever change in gradient the young undergo to fall short of (be smaller than) the change

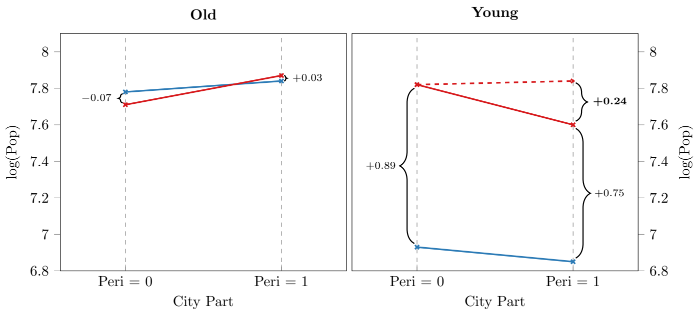


Fig. 2.6: Population Gradients for the Old and Young

Note: This figure illustrates the estimates from Table 2.3. The blue graphs' slopes show population gradients pre-subsidy repeal, while the (solid) red graphs' slopes indicate population gradients post-repeal. Since the logarithm of the old (the control group, left-hand panel) decreases by 7 log points in any central ring, while it increases by 3 log points in any peripheral ring, the population gradient for the old increases by 10 log points. Next, because the logarithm of the young (the treatment group, right-hand panel) increases both in central and peripheral rings, by 89 and 75 log points, respectively, the population gradient for the young falls by 14 log points. Combining these results, the dashed red graph's slope (also in the right-hand panel) gives the counterfactual gradient for where the subsidy has not been repealed. Then the population gradient for the young would be positive, rather than negative, and the log number of young in the periphery would be 24 log points higher than it actually is. Data: Authors' calculations using BBSR data.

in gradient undergone by the old. Now, from the first column of Table 2.3, our DDD-estimate is -0.24. Subsequent specifications allow for city specific time effects (second column), control for female labor market participation (which may have had a stronger effect on the city center than on its periphery), and also instrument for it, using sectoral employment shares in the hospitality, financial and public service sector as instruments (columns 5 and 6). Across these specifications, the DDD-estimate remains at -0.24, and highly significant.

It is instructive to decompose this estimate. On the one hand, the population gradient for the old increases, by 10 percentage points (see the diagram on the left of Fig. 2.3).[21] On the other hand, the gradient for the young decreases, by 14 percentage points (see the diagram on the right). The difference between these two changes, or 24 percentage points, reflects the extent to which the changes in the two gradients diverge.

2.4.2 Treatment by Affordability

We next address treatment by housing affordability. We partition our cities into "affordable" vs. "less-affordable/expensive". Dummy AFF equals 1 if

[21] Fig. 2.1's panel on the left, in the introduction, provides an alternative, yet equivalent, picture

Tab. 2.4: Ring Households with Children

	OLS				2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	6.84***	6.53***	6.72***	6.84***	6.72***	6.84***
	(0.15)	(0.13)	(0.17)	(0.14)	(0.17)	(0.14)
Distance	0.25***	0.25***	0.26***	0.26***	0.26***	0.26***
	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
Peri × (Distance - $\tilde{r}/3$)	−0.66***	−0.66***	−0.67***	−0.67***	−0.67***	−0.67***
	(0.10)	(0.09)	(0.10)	(0.10)	(0.10)	(0.10)
Peri × Post	0.07	0.10	0.95**	1.12**	0.48	0.66
	(0.12)	(0.18)	(0.40)	(0.44)	(1.20)	(1.34)
Peri × Post × Aff	−0.57**	−0.75**	−0.53**	−0.70**	−0.56**	−0.73**
	(0.16)	(0.22)	(0.16)	(0.22)	(0.17)	(0.23)
Peri × Post × Female Labor			−1.80**	−2.05**	−0.81	−1.10
			(0.86)	(0.91)	(2.57)	(2.86)
City FE	Yes	Yes	Yes	Yes	Yes	Yes
City FE × Post	No	Yes	No	Yes	No	Yes
Adj. R^2	0.79	0.79	0.78	0.79	0.78	0.78
Num. obs.	7125	7125	6804	6804	6804	6804
N Clusters	46	46	44	44	44	44

Note: OLS regressions (col 1–4) with the log of ring households with children as the response variable. 2SLS-Regression (col 5 & 6) with Peri × Post × Female Labor instrumented by sectoral employment shares in the hospitality, financial and public service sector. Clustered standard errors (at city level) in parentheses. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

the ring belongs to the most affordable 15 percent of cities in 2000 (where cities are ranked by their average price of land in year 2000), and 0 else. Guided by our discussion of the subsidy's design (section 2.2 and Table 2.1), we now choose a specification flexible enough to allow the population gradient in affordable cities to undergo an experience different from that in less-affordable ones, by interacting PERI × POST with AFF.

We expect the change in the "population gradient" in less-affordable cities, as the coefficient of PERI × POST, to exceed that in affordable ones. That is, we expect the coefficient of PERI × POST × AFF to be negative. Table 2.4 shows our results from estimating the accordingly modified model on families with dependent children; this, after all, is the stratum that should, in affordable cities, respond strongest to repeal–given the subsidy's per child bonus (section 2.2). Column 1 indicates that the coefficient of PERI × POST × AFF *is* significantly negative, and large in absolute value. Affordable cities see their population gradient drop by 50 percentage points, while expensive cities witness an increase in their gradient, of 7 percentage points. Here our DDD-estimate is 57 percentage points. In affordable cities, suburban population–and suburban housing stocks–would have been 57 log points higher, had the subsidy not been repealed. Fig. 2.7 illustrates these estimates.[22] Initial gradients can be read off the blue graphs' slopes, while post-reform gradients are given by the red graphs' ascent. From a triple-diff perspective, affordable cities' gradient experiences a 57 percent-

[22] Again, Fig. 2.1's panel on the right, in the introduction, provides an alternative, yet equivalent, picture.

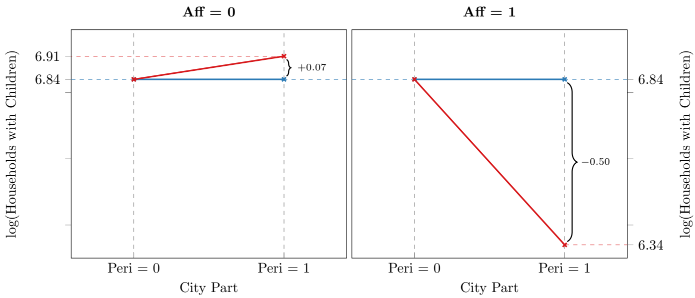


Fig. 2.7: Population Gradients for Affordable and Expensive Cities

Note: This figure shows the results from Table 2.4, indicating "population gradients" for affordable and less-affordable/expensive cities. Blue lines show these gradients for the period before the subsidy was repealed, red lines show the gradients for after repeal. The logarithm of the number of households with children in expensive cities (control group, left panel) increased by 7 log points in peripheral rings. In contrast, the logarithm of the number of households with children in affordable cities (treatment group, right panel) *decreased* by 50 log points in peripheral rings. Without subsidy repeal, affordable cities would have undergone the same development as expensive ones, meaning households with children would have suburbanized more, rather than less. Data: Authors' calculations using BBSR data.

age points reduction relative to the change in population gradient in unaffordable ones.

Column 2 also allows for interactions of POST and city fixed effects. Including these extra controls increases our estimate of the extra drop in affordable cities' population gradient even further, to 75%. Allowing for city specific time trends strengthens our result. Columns 3 and 4 control for female labor market participation interacted with POST and PERI. We expect female labor market participation to contribute to central rings' population. In the city center, commutes are shorter (allowing for more time spent with one's children), and population density is greater (permitting families to share child minding cost more easily). Accounting for this channel, and also allowing for the participation rate to be endogenous in columns 5 and 6, does not substantially alter our key result.[23] The coefficient estimate on PERI $\times$ POST $\times$ AFF remains negative, significant, and substantial, throughout.

Table 2.5 finally reports our results for estimating the affordability "premium" on *all* households in the sample, rather than just households with children. We find that our estimates of the coefficient of PERI $\times$ POST $\times$ AFF parallel those from Table 2.4.

[23] We omit the first stage regression here for brevity.

Tab. 2.5: All Residents

	OLS				2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	9.47^{***}	9.48^{***}	9.32^{***}	9.30^{***}	9.30^{***}	9.30^{***}
	(0.14)	(0.15)	(0.15)	(0.14)	(0.15)	(0.14)
Distance	0.21^{***}	0.21^{***}	0.21^{***}	0.22^{***}	0.21^{***}	0.22^{***}
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Peri × (Distance - $\tilde{r}/3$)	-0.64^{***}	-0.64^{***}	-0.63^{***}	-0.64^{***}	-0.63^{***}	-0.64^{***}
	(0.07)	(0.07)	(0.08)	(0.08)	(0.08)	(0.08)
Peri × Post	0.09	0.09	0.78^{**}	0.95^{**}	0.11	0.25
	(0.06)	(0.11)	(0.30)	(0.37)	(0.94)	(1.25)
Peri × Post × Aff	-0.58^{***}	-0.81^{***}	-0.44^{**}	-0.62^{**}	-0.53^{**}	-0.71^{**}
	(0.14)	(0.20)	(0.18)	(0.25)	(0.21)	(0.28)
Peri × Post × Female Labor			-1.50^{**}	-1.85^{**}	-0.08	-0.36
			(0.61)	(0.75)	(2.01)	(2.68)
City FE	Yes	Yes	Yes	Yes	Yes	Yes
City FE × Post	No	Yes	No	Yes	No	Yes
Adj. R^2	0.78	0.78	0.78	0.78	0.78	0.78
Num. obs.	13933	13933	11705	11705	11705	11705
N Clusters	77	77	62	62	62	62

Note: OLS regressions (col 1–4) with the log of ring population as the response variable. 2SLS-Regression (col 5 & 6) with Peri × Post × Female Labor instrumented by sectoral employment shares in the hospitality, financial and public service sector. Clustered standard errors (at city level) in parentheses. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Appendix A.3 offers various robustness checks. First, Table A.3 revisits the extra change in population gradient for affordable cities, by replacing "households with children" with the even finer strata of "households with 1 child", "households with 2 children", and "households with 3 or more children". Since the subsidy is strictly increasing in the number of children (essentially granting an additional 800 Euros per child-year, recall Table 2.1), we should also expect the subsidy repeal's impact on affordable cities to get stronger as the number of eligible children goes up. And so it does. We find that the coefficient on PERI × POST × AFF is monotonically decreasing in the number of children, from -0.63 down to -0.85 in columns 1 to 3, respectively. Accounting for female labor market participation endogeneity in columns 4 through 6, too, does not alter this picture.

We also replaced dummy AFF with a continuous price variable capturing affordability, $\overline{\text{PRICE}} - \text{PRICE}$ (where $\overline{\text{PRICE}}$ is the highest average real estate price among all cities in the sample and in the year predating our analysis (i.e. in Munich)). Tables A.4 to A.6 show that the coefficient on the three-way interaction variable $\overline{\text{PRICE}} - \text{PRICE} \times \text{PERI} \times \text{POST}$ retains its negative sign throughout. And third, we also vary the position of the spline's knot. Tables A.7 and A.8 show that results are essentially unchanged if the single knot becomes such that one fourth of rings is closer, while three fourths of rings are further away from, the CBD. Finally, we note that we have also estimated our equations by Poisson MLE, an alternative estimator that accounts for the count data nature of our ring resident figures. Typically these estimates are highly similar to those shown in the paper, and they hence are suppressed.

2.5 Discussion

Less Homeownership in Central Rings We have suggested that first-time buyers need to move out of the city center, for lack of central owner-occupied housing on the market. In that sense, our empirical results should be read as not refuting the combined hypothesis of (i) the subsidy encouraging tenure *and* (ii) first-time buyers having to move out. But we should also point to the additional available evidence emphasizing the spatial asymmetry in tenure. For a subset of our sample's cities, we are able to document the spatial distribution of building types. Here we see that the share of multi-family buildings decreases, while the share of detached and semi-detached buildings increases, monotonically in distance to the CBD. Multi-family housing is susceptible to externalities and hidden costs that make homeownership less attractive (Glaeser 2011), and so the spatial distribution of building types coincides well with the anecdotal evidence on the prevalence of renters (owner-occupiers) in the city center (suburbs).[24]

Stable Unit Treatment In a closed city, population is given, and changes in one ring's population are necessarily reflected in offsetting changes in some other ring's (or rings') population. In the closed city context, we would not be able to maintain that whatever happens to those who are not treated (the old, or to those in expensive cities) is independent of (the general equilibrium effects of) what happens to those who are (the young, and those in affordable cities). While subsidy repeal is a national policy event (and hence may appear to mandate a closed city perspective), Germany's cities are still open with respect to any location outside Germany yet within the European Union. For this reason, cities in the sample should be considered open. In the standard open monocentric city (Brueckner 1987), any city ring's demographic is exclusively determined by the level of welfare it offers relative to welfare obtainable to that demographic elsewhere.

Cohort-Specific Shifts One may wonder if our strong result in subsection 2.4.1 could also be due to unobservable differences in cohort-specific trends, e.g. millennials' preferential shifts. E.g., a small but growing literature asserts gentrification, and even a degree of city center renaissance, for certain population strata in US metro areas' urban core (Baum-Snow and Hartley

[24] Ahlfeldt and Maennig (2015) suggest that close to 80% of one- and two-family houses are owner-occupied, whereas more than 80% of dwellings with three families or more are inhabited by renters.

2020; Couture and Handbury 2020; Owens III et al. 2020). Such trend differences, however, are not an issue here, as we argue next by contradiction. Note first that more affordable cities on average also tend to be older. Now suppose it is age-specific shifts, wholly unrelated to subsidy repeal, that underlie the differential recentralization experiences of young and old. These same shifts then must also have more affordable cities (i.e. with their older populations) recentralize *less* (than expensive cities). But this contradicts what we just learned in subsection 2.4.2 on more affordable cities recentralizing *more*. Subsidy repeal, in contrast, is well able to explain stronger recentralization both of the younger and in more affordable places.

Counterfactual Analysis Consider our "accessibility" estimates from subsection 2.4.1. Let us assume that the gradient for the young would have moved in tandem with that for the old; this is an assumption of "parallel trends in slopes". Then, had the subsidy not been repealed, no 0.24 log points would have been shaved off the number of young in each ring. The dashed red graph in the right-hand panel of Fig. 2.1 indicates this counterfactual change in slope; while that of Fig. 2.3 shows the identical counterfactual change in the population of young.

Let $\hat{y}_{ij\bar{t}}$ denote the predicted value from estimating the expected (log) number of young in city i, peripheral ring j and post-reform (now simply indexed $\bar{t}$ in eq. 2.2).[25] Then

$$e^{\widehat{y_{ij\bar{t}}}} \; - \; e^{(\widehat{y_{ij\bar{t}}}-0.24)} \tag{2.5}$$

is the number of young individuals who, post-reform, never bought the home in city i and peripheral ring j they else would have bought. Summing over all cities' peripheral rings gives a total of 401,365 young individuals who never turned owner-occupier. On assuming that it is always two young individuals who buy a house jointly, the number of home purchases "averted" by subsidy repeal is 200,683. These app. 200,000 purchases would have translated into the additional construction of 200,000 actual homes in city peripheries, had no homes been vacant in city peripheries.[26]

[25] I.e., $\hat{y}_{ij\bar{t}} = \hat{\alpha}_0 + \hat{\mu}_i + \hat{\alpha}_1 \, \text{DIST}_j + \hat{\alpha}_2 \, (\text{DIST}_j - \widetilde{r_i}/3) + \hat{\beta}_1 + \hat{\beta}_2 + \hat{\beta}_3$.

[26] That the vacancy rate in peripheries was zero, is certainly not true. (These vacant (older) homes were of lower quality than the homes first-time buyers are observed to buy.) In any case, we do not, unfortunately, have ring-specific data on vacant housing.

Alternatively, consider our "affordability" estimates from subsection 2.4.2. Had the subsidy not been repealed, now no 0.57 log points would have been taken off the number of households with children in affordable cities' rings (Table 2.5). Again, dashed red lines in Fig. 2.1 and 2.3 indicate counterfactuals. In Fig. 2.1's panel on the right, the dashed line shows the counterfactual change in the gradient for households with children; while in Fig. 2.3's right-hand panel, the red dashed line shows the counterfactual change in households themselves. Corresponding figures for overall population are even higher. Using these latter estimates, and proceeding along the analogue of expression 2.5, an additional 401,937 first-time buyers would now have owner-occupied their home, too. Repealing the subsidy made these purchases not happen. Again on assuming a household size of 2 (and on presuming vacant housing largely irrelevant), an extra 200,969 homes would have been built in city peripheries had the subsidy not been scrapped. Coincidentally, either treatment scenario has subsidy repeal avert app. 200,000 homes in city peripheries.

Rents Building on a filtering logic (as laid out in this chapter's appendix A), the subsidy (its repeal) should not just benefit (hurt) those taking up the subsidy. Also, the subsidy (its repeal) should also benefit (hurt) those moving (not moving) into the rental housing left behind (not left behind).[27] From this perspective, repealing the homeowner subsidy can also help contribute to an explanation of the more recent surge in rents. Daminger (2021b) is able to combine detailed information on rent (by city rings) with the latest phase of Germany's homeownership subsidy, introduced in 2018,[28] and finds that BK indeed alleviated pressure on rents in cities that were affordable to begin with (yet only those).

Complementary Evidence In yet another companion paper to ours, Daminger (2021a) traces population changes in cities relative to changes of population in cities' hinterlands (rather than population changes in city centers relative to city peripheries). Based on an analysis of Germany's commuting zones, and employing a triple-diff analysis akin to this paper's analysis, Daminger (2021a) finds that city hinterlands' population premium (gradient) fell by more for the young than for the old. We conclude that it was not just that

[27] Recall that, in the Appendix's model, the change in the equilibrium rental price is shown to be strictly positive, $dq_1 > 0$. Conversely, introducing the subsidy is easily shown to drive rent down, $dq_1 < 0$.

[28] . . . termed *Baukindergeld* (BK), and endowed with features similar to those of the *Eigenheimzulage* (EZ), see section 2.2.

cities recentralized; entire *regions* did, too. The finer intra-urban adjustments under scrutiny in this paper mirror the larger intra-regional shifts identified in Daminger (2021a).

2.6 Conclusions

On a large sample of city rings, this paper shows how Germany's repealing a lump-sum subsidy towards low- and middle-income households encouraged the *re*-centralization of its population. We document how the young (never eligible for the subsidy) recentralized, while the old (often effectively having cashed in on it) decentralized. Likewise, we find that households who lived in cities that were affordable to begin with recentralized, while households in expensive cities decentralized. To put it briefly: the treated recentralized, whereas the untreated did not. *A fortiori*, the treated recentralized more than the non-treated. It is precisely this latter empirical observation that the economics of subsidy repeal had us expect.

Our estimates are for diff-in-diff and triple-diff specifications, each augmented by various fixed effects. These specifications appear well-versed in removing the bias in coefficient estimates which many potential confounders would otherwise introduce. Our specifications allow for city specific fixed effects pre-repeal (e.g. city idiosyncracies), for time fixed effects post-repeal and specific to each city (e.g., being located in Germany's East vs. West), and (in the case of triple-diff) also for a summary fixed effect common to each city periphery post-repeal (e.g. a sudden country-wide distaste for peripheral living, or a general improvement in central city amenities or employment). Throughout our analysis we juxtapose central ring with peripheral rings; but general equilibrium effects are not likely to have these rings interact. Urban hinterlands (outside our analysis) ensure that rings follow what happens there, not what happens in neighboring rings or demographics.

Our results derive from analyzing repeal; but our conclusions are meant to apply to the subsidy itself. If repeal makes cities (and regions) recentralize, then we now know the subsidy to make cities (and regions) *de*-centralize. Homeownership subsidies are near-to-ubiquitous. We expect homeownership subsidies to drive decentralization in many of these countries–even if, as comparing our results with Gruber et al. (2021), for example, indicates–we should be careful to observe the institutional details of the subsidy. These may strongly differ from one country to the next. What may be true for a lump-sum subsidy with tight income thresholds relative to subsidy eligibility may quite look different for a mortgage-interest deductible by everyone.

Encouraging homeownership (among low- and middle-income households at least) not just has the desired effect of encouraging homeownership, e.g. producing the positive externalities prominent in some of the literature (or subsidizing a governing party's clientele). As long as central housing is not conducive to owning one's home, encouraging homeownership may also have the spatial effects highlighted in this paper. These spatial effects likely will be more important (i.e. receive greater weight in our welfare function) in the future than they have been in the past. This is especially true when coupled with the stylized facts that decentralized housing and sprawling suburbs not just induce the decentralization of central city functions (such as retail, Dascher (2019)), too, but also produce greater carbon emissions. Then encouraging homeownership is at cross-purposes with the costly efforts to mitigate global warming, hopefully underway soon.

3 Homeownership Subsidies and the Spatial Distribution of Residents within Regions

This chapter is based on Daminger (2021) Homeowner Subsidies and Suburban Living: Empirical Evidence from a Subsidy Repeal, BGPE Discussion Paper No. 211.

3.1 Introduction

There are plenty of reasons why homeownership might be preferable to renting–and just as many against it. The advantages and disadvantages of homeownership are therefore subject of a large number of academic papers. But undeniably, many countries choose to support and actively promote owner-occupied housing. To do so, they can pick from a variety of policy instruments that can–quite broadly–be divided into two categories: First, there is the preferential treatment of homeownership through tax legislation, such as the mortgage interest tax deduction. Second, there are government policies and programs that support households in forming homeownership through, for example, equity loans, mortgage guarantees or direct subsidies.

This paper is interested in analyzing the effects of the latter, direct subsidies to homeownership, on the spatial distribution of population. Glaeser (2011) pointed out that subsidies to homeownership simultaneously act as subsidies to suburbanization if homeownership is formed more easily in rural than in urban areas. And Muth (1967) and Voith (1999) also argued that governmental programs, such as the federal income tax advantage for homeowners, may have played an important part in the US' experience of urban decentralization since the 1950s. This paper empirically tests the hypothesis of homeownership subsidies fostering suburbanization.

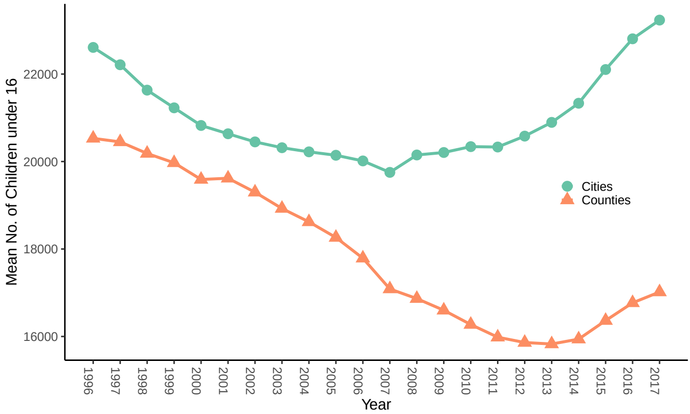


Fig. 3.1: Average number of children under 16 in cities and counties over time

Note: This graph plots the average number of children in cities and counties over time. Cities are shown as green dots while counties are symbolized by orange triangels. Around the time of subsidy removal, these lines diverge. The number of children rises in cities while it is (further) shrinking in counties. Data: Federal and state statistical offices.

Germany also had a long-standing tradition in flanking the formation of homeownership through subsidy programs or preferential tax treatments. But in 2005, it unexpectedly repealed its most important homeownership subsidy program, the *Eigenheimzulage* without replacement, providing an opportunity to empirically exploit this quasi-experiment. Figure 3.1 plots the average number of children under the age of 16 in German cities (green dots) and counties (orange dots) over time. Children usually live with their parents rather than by themselves, so the number of children can convincingly serve as a proxy for the number of families.[1] The figure shows that, around the time of subsidy repeal in 2005, trends in the number of children diverge for cities and counties. Counties continue to lose children (and thus families) while cities see an *increase* in the number of children.

In several difference-in-differences (DD) and triple differences (TD) approaches, I identify this subsidy's removal as one key component in Germany's "renaissance of urban living" in the decade that followed. I show that the demographic decline in rural areas–and simultanously the thriving of cities–was partly driven by the absence of young people and families with children who, without the subsidy tailored to them, were no longer able to form

[1] Official population statistics in Germany do not report figures for households or families.

Fig. 3.2: Share of housing units in houses vs. homeownership rate

Note: This graph plots the 2011 share of housing units in detached and semi-detached houses against the 2011 share of owner-occupied housing units in all housing units. Cities are shown as green dots while counties are symbolized by orange triangels. The figure shows that homeowners predominately occupy single- and two family homes, and these are found in counties, not in cities. Data: 2011 German Census.

homeownership outside the urban fringe and stayed in cities as renters. In contrast, the middle-aged, who had built up sufficient savings and were able to benefit from the subsidy before it was repealed, stayed put in their homes outside the city gates.

Although this lump-sum subsidy was paid regardless of properties' locations, cities and rural areas experienced different intensities of treatment: first, homeownership was formed most easily in rural areas, and this is also where the real subsidy rate, expressed as the share of the subsidy in the purchase cost of the property, was highest. In addition, for the longest part of the program's duration, the subsidy amount for new construction was twice that for existing properties, further increasing incentives to build in the hinterlands, where housing supply can react more elastically to increases in demand. And second, only certain strata of the population benefited from the subsidy (and were thus affected by its removal), opening up numerous opportunities to divide population into treatment and control groups.

Figure 3.2 plots the share of housing units in single and two-family houses in all housing units on the horizontal axis against the share of owner-occupied housing units in all housing units, i.e. the homeownership rate, on the vertical axis. Germany's 107 independent cites (*Kreisfreie Staedte*) are plotted

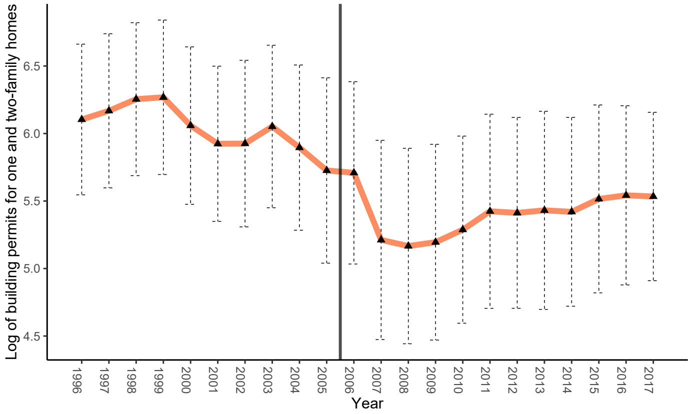


Fig. 3.3: Slump in rural building permits, post subsidy repeal

Note: The graph shows the development of the number of building permits for one- and two-family houses in all German counties. Following the repeal of the subsidy, building permits fall sharply. Triangles depict the mean value, while error bars represent the standard deviation of the data at each point in time. Data: Federal and state statistical offices.

as green dots, while its 294 counties (*Landkreise*) are represented by orange triangles. Two stylized facts can be inferred: First, there is a strong positive correlation between the the share of single and two-family homes and the homeownership rate.[2] And second, both the homeownership rate and the share of single and two-family homes are greater in counties than in cities.[3]

Presumably, homeownership subsidies lead a large proportion of recipients to build or buy an owner-occupied house outside the urban fringe, where housing supply tends to be more elastic than in the city (Baum-Snow and Han 2021; Cosman et al. 2018; Saiz 2010). Their change in tenure goes hand in hand with a move out of their previously rented apartment–and rental apartments are mainly found in multifamily buildings and thus in cities. Simultaneously, repealing a homeownership subsidy removes households' incentives (and probably often also the financial capability) to change tenure from renting to owning and puts an end to the subsidized outflow

[2] This link has also been shown by Hilber (2005, 2007), who finds that a detached house is substantially more likely to be owner-occupied than an apartment in a multi-family building and by Ahlfeldt and Maennig (2015), who documents that close to 80% of one- and two-family houses are owner-occupied.

[3] And, one should add that, e.g., Linneman (1985) and Coulson and Fisher (2014) provide two–albeit different–theoretical mechanisms for disentangling the causality issue, suggesting that the building type causally affects optimal tenure choice.

of residents from cities to peripheries. Figure 3.3 has some suggestive evidence that this indeed might have been the case with Germany's subsidy repeal. It shows the development of building permits for single- and two-family houses in German counties. It is clearly visible that substantially fewer building permits for houses (and thus owner-occupied housing) were issued. Thus fewer houses were built in rural areas in close temporal correlation with the subsidy's repeal in 2005, suggesting that fewer people were able to build their own home outside the urban fringe without government's helping financial hand.

For the empirical analysis, I use the grouping of cities and counties into labor market regions. Typically, becoming a homeowner does not involve moving to a completely other city and thus, e.g., changing jobs. Rather, former city renters move into their own homes in the hinterlands surrounding the city to continue having easy access to jobs and city amenities. Germany's labor market regions mirror that fact by being defined on the basis of their common labor market and commuter links between the city and the hinterlands. In each labor market region, I define all counties as the urban hinterlands (the treatment group) while the independent city (or in some rare cases: cities) forms the center (the control group). In my DD-analyses, I find that the repealing the subsidy impacted residential location choices predominantely in the age strata of people truly affected by the subsidy's repeal: Compared with the urban cores, significantly fewer people of "building age" and children (who are likely to be part of a family household) were living in the urban hinterlands post subsidy repeal.

This paper contributes to the strand of literature dealing with the causes and consequences of suburbanization or "sprawl": Brueckner (2000), Burchfield et al. (2006) or Baum-Snow (2007), among others, have identified a variety of different drivers of suburbanization. And one, in particular, is governmental policies promoting owner-occupied housing. Subsidization through the tax code by, e.g., mortgage interest tax deduction opportunities tends to be capitalized in house prices while simultanously rather increasing the intensive margin, i.e. subsidy recipients consuming more living space, than the extensive margin, i.e. steering them to forming homeownership in the first place (Carozzi et al. 2020; Davis 2019; Hilber and Turner 2014; Sommer and Sullivan 2018). This amplifying effect on house prices also seems to be the case with lump-sum cash homeownership subsidies as those in the German case (Krolage 2020). But not only prices for owner-occupied homes are affected. Daminger (2021b) exploits how subsidy recipients must have previously been renters and finds that fostering homeownership through cash transfers lowers demand for rental housing and thus rents.

The issue of homeownership-induced suburbanization is studied by, e.g., Daminger and Dascher (2020), Glaeser (2011), Gyourko and Voith (1997), and Voith (1999), who shed light on the connection between homeownership, its subsidization and homeowners' residential location choice. And this is exactly where this paper fits in best. While Glaeser (2011) and Voith (1999) raised the link between homeownership subsidies and suburbanization early, their contributions lack a detailed and rigorous empirical analysis of the causal relationship. Daminger and Dascher (2020) analyze population shifts *within cities* and propose a causal link between the removal of a homeownership subsidy and an increase in city centers' populations. This paper picks up there and analyzes population shifts *within regions* (and thus between cities and their hinterlands) in response to a homeowner subsidies' repeal.

But since the policy measure under study explicitly promoted homeownership, my study also contributes to the large literature on the consequences of homeownership: Numerous authors discuss the link between homeownership and (i) investment in social capital (e.g., DiPasquale and Glaeser (1999) or Hilber (2010)), (ii) investments in public goods (e.g., Hilber and Mayer (2009) on public schools), (iii) the effects on labor markets and employment (e.g., Blanchflower and Oswald (2013) or Ringo (2021) on unemployment rates or Harding and Rosenthal (2017) on self-employment) or (iv) voting behavior and the political economy of land use regulations (e.g., Ahlfeldt and Maennig (2015), Fischel (2001), Hilber and Robert-Nicoud (2013), and Ortalo-Magné and Prat (2014)).

This paper has five sections. Section 3.2 introduces the institutional background. Section 3.3 describes the empirical data and explains the spatial setup using Germany's labor market regions. Section 3.4 turns to the empirical analysis, which identifies the subsidy repeal's effects on the spatial distribution of populations within labor market regions. Section 3.5 concludes and provides some policy implications.

3.2 Institutional Background

Five phases in subsidizing homeownership can be distinguished in relation to Germany post World War II: In a first phase (1949–1995), investments in owner-occupied housing were tax-deductible. Phase 2 (1996–2005) consisted of a direct, lump-sum subsidy for purchasing or constructing owner-occupied housing (*Eigenheimzulage* or EZ for short). The EZ was repealed at the end of 2005 without replacement. In the following twelve years (2006–2017), the third phase, there was no distinct federal policy to promote homeownership. In 2018, a new support program (*Baukindergeld* or BK for short) was introduced, which pays out subsidies to households with children for the construction and purchase of owner-occupied housing. This fourth phase of subsidies to homeownership ended mid 2021 with the repeal of the BK. At present, in phase 5, Germany again has no governmental program explicitly subsidizing homeownership.

This paper addresses the 2005 repeal of the EZ and thus analyzes the transition from phase 2 to phase 3. This transition has features that make it particularly viable for empirical analysis. First, the subsidy accounted for a substantial portion of the public budget and was thus not negligible. The public sector disbursed a subsidy volume of more than €10 billion/year until 2005, making the EZ the single largest item in the federal subsidy budget. Over the entire period, the program totaled subsidies of €106 billion, financed by federal, state and local governments, and supported more than 4.5 million people in becoming homeowners.

Second, the announcement of the repeal could not be foreseen by any individual or municipality, and the repeal itself was closely linked in time, making strategic exploitation unlikely: on November 11, 2005, the governing parties announced for the first time that the EZ would be repealed at the end of the year, leaving only 1.5 months for interested applicants to conclude the purchase contract, file necessary building documents, and apply for the subsidy.

And third, although the subsidy was nominally location-independent, applicants outside cities had higher real subsidy rates. Thus, the elimination was bound to hit prospective owner-occupiers in the periphery harder than those in cities, where real estate and land prices were too high for the subsidy to make a real contribution in financing. Additionally, only those of "building-age" could benefit from the subsidy before it was repealed while those too young at the moment of repeal missed the opportunity to move into subsidized homeownership.

Table 3.1 provides an overview of the details of the subsidy. Both the purchase and the construction of houses and apartments for owner-occupancy were subsidized. For new buildings, the basic subsidy amount per year was

Tab. 3.1: Details on the subsidy scheme

	1996–1999	2000–2003	2004–2005
Beneficiary			
Recipient		— Income tax liable individuals —	
Maximum 2-year taxable income	€122,710 (singles) €245,420 (couples)	€ 81,807 (singles) €163,614 (couples)	€ 70,000 (singles) €140,000 (couples)
Threshold increase per child	—	€ 30,678	€ 30,000
Object			
Subsidized Property		— Owner-occupied property (house or condo) —	
Subsidy			
Funding start		— Year of aquisition —	
Funding period		— 7 subsequent years —	
Child allowance	€767 per child	€767 per child	€800 per child
Yearly subsidy amount (baseline)			
New Construction (q_1)	min(5.0% of q_1, €2,556)	min(5.0% of q_1, €2,556)	min(1.0% of q_1, €1,250)
Existing Property (q_2)	min(2.5% of q_2, €1,278)	min(2.5% of q_2, €1,278)	min(1.0% of q_2, €1,250)

Note: This table represents the schematic structure of the subsidy. The subsidy can be divided into three time periods (second to fourth columns): (i) 1996–1999, (ii) 2000–2003, and (iii) 2004–2005. The first change in 2000 covered only the income thresholds: these were generally reduced, but they could now be increased by the presence of children. The second change in 2004 was more comprehensive: not only were the general income thresholds further reduced but the distinction between the purchase of existing property and new construction was eliminated. From then on, both types of owner-occupied housing were subsidized equally. Over the entire period, the subsidy was paid out only upon moving into the owner-occupied property and then for a total period of eight years. Source: German Home Owners' Allowance Act (*Eigenheimzulagegesetz [EigZulG]*) with its amendments.

5% of the property's construction costs (including land acquisition costs - maximum €2,556). The purchase of existing real estate was subsidized with an annual basic subsidy amount of 2.5% of the purchase costs (maximum €1,278). Although the basic subsidy for new construction was higher than that for the purchase of existing property, the child allowance in both cases amounted to €767 per child per year.[4] The total annual subsidy (basic subsidy and child allowance) was paid out over a period of 8 consecutive years.

The subsidy was discontinued at the end of 2005, but applicants who began construction and buyers who signed the notarized purchase agreement before January 1, 2006, were still entitled to the EZ for the entire subsidy period of eight years. In the case of construction, the subsidy period began in the year of completion, and in the case of purchase, it began in the year of purchase; but note that the subsidy was only paid out on condition that the property was actually occupied by the owner.[5]

[4] The different treatment of new and existing houses/condos was abandoned by a change in the law in 2004: For the remaining two years of the subsidy, the annual basic subsidy amount was 1% of the purchase or construction cost (maximum €1,250). The child allowance was also raised slightly (to €800).

[5] Since building permits in Germany are valid for at least three years, it is also possible that construction work only began (and was completed) years after the building application was submitted, but that this building was nevertheless subsidized for the full period of eight years upon completion because the building application was submitted before the EZ was repealed. According to a report by the German government, the last (but only minor) payments for the repealed subsidy were made in 2017.

As stated earlier, the subsidy was granted irrespective of location, such that nominal subsidy rates in cities were just as high as in the hinterlands. But of course, real estate and especially residential land prices are much higher in cities than in the surrounding areas.[6] Take the example of a family with two children who was eligible for receiving the EZ and built its own home in 2003. In a city, the cost of land and construction might have been €400,000, while a comparable property could have been built in a nearby county for half the cost. The family received–no matter where it built–€2,556 per year (basic subsidy) + 2 × €767 (child allowance), i.e. €32,720, over the entire subsidy period.[7] However, the share of the subsidy in the financing of the home–the real subsidy rate–was only just over 8% in the city while it was twice as high in the hinterlands. So not only was it easier to build in the urban hinterlands (since there was simply more open space available for new construction) but also the real benefit of the subsidy was higher outside the urban fringe.

3.3 Data and Methods

Although the EZ was a federal subsidy, it was not administered centrally. Rather, local tax offices were responsible for its administration. I do not have microdata on subsidy recipients that would allow me to examine household characteristics and location decisions.[8] And yet, using aggregate population data, I can examine those segments of the population that were particularly affected by the subsidys repeal: Young residents of "building age" and children.

Using labor market regions, defined by the Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR), enables me to examine suburbanization trends in the wake of the policy change in a narrowly defined area. The change from renting to owner-occupancy is usually not accompanied by a complete move to a new city (and thus a change of job and the like). Rather, renters decide to settle permanently in their current place of residence and move into owner-occupancy to the hinterlands of the same city to continue to have easy transportation to their workplace or city amenities.

[6] See Braun and Lee (2021) for an assessment of detailed residential land prices in Germany.

[7] And it is easy to calculate that, for all properties more expensive than €51,120 (i.e. in virtually all cases), the subsidy amount was the same.

[8] According to the Federal Ministry of Finance, no nationwide consolidated data set actually exists. This lack of data may also explain why there are hardly any studies on the EZ.

Tab. 3.2: Descriptive statistics of population data

		N	Mean	SD	Min	Max
Cities	Population	1,892	200,811	268,777	33,944	1,830,584
	Building-Age (30–50y)	1,892	60,998	86,252	7,431	581,716
	Children (0–16y)	1,892	17,794	24,282	2,168	179,130
Counties	Population	2,420	205,940	122,143	55,624	662,712
	Building-Age (30–50y)	2,420	60,967	36,517	12,930	204,531
	Children (0–16y)	2,420	19,999	12,295	4,625	73,467

Data: Federal and state statistical offices

The BBSR constructs labor market regions on the basis of the share of commuters between cities (where most of workplaces are located) and counties, imposing five constraints on their construction: Labor market regions (i) use NUTS-3 level boundaries of cities and counties and do not cross state boundaries, (ii) provide jobs for more than 65% of the working population living in them, (iii) have more than 65% of their jobs occupied by citizens living in Germany, (iv) do not overlap and (v) have a maximum within-region one-way commute to the workplace of 45 minutes (Eckey et al. 2006; Kosfeld and Werner 2012). In principle, there are 258 labor market regions in Germany. I clean this data set from labor market regions that consist of only counties or only cities, or for which not all years of population data are available. My final data set has 72 labor market regions, depicted in Figure 3.4.

For the empirical analysis of the subsidy's repeal on suburbanization, I use official population statistics from the federal and state statistical offices. Table 3.2 has some descriptives on this data. In detail, my panel data covering 1996–2017 (i.e. starting with subsidy introduction (1996), enveloping subsidy repeal (end of 2005) and ending prior to the introduction of EZs successor (2018)) consists of annual population counts subdivided into age groups for each city (n=86) and for each adjacent county (n=110) of my labor market regions for ten years prior to twelve years after the EZs removal.

For the analysis of their spatial distribution, I choose age groups based on a study by Färber (2003), who analyzed early microdata for the first five years of the subsidy (1996–2000).[9] She found that nearly 70% of subsidy recipients were between 30 and 50 years old at the time of application. Individuals in this age group, who (i) were of family formation age and

[9] There is also an official government report analyzing these early years data (BBR 2002). Unfortunately, I am not aware of any study or data covering the second half (2001–2005), or the entire subsidy period.

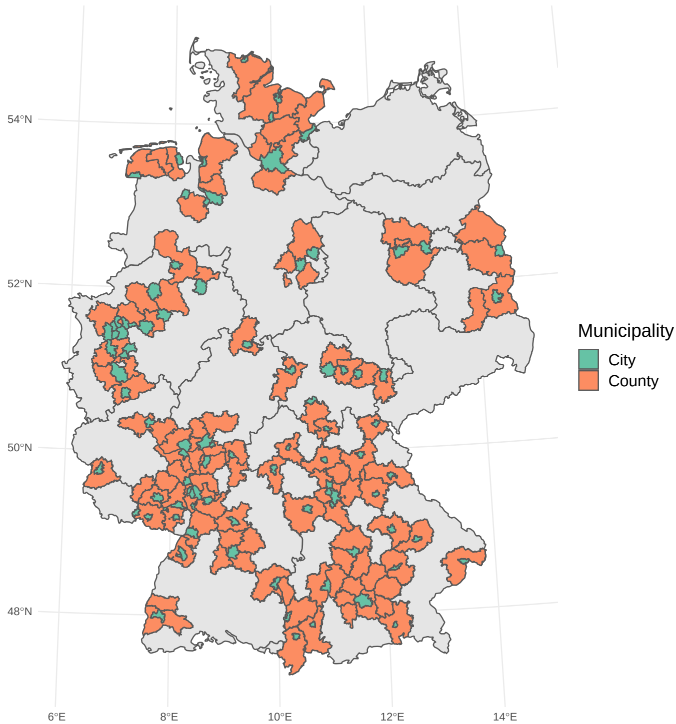


Fig. 3.4: Map of Germany's labor market regions

Note: The figure shows my data set of 72 labor market regions, consisting of (at least) one city (green) and adjacent counties (orange). Data: Federal Agency for Carthography and Geodesy.

therefore often need more living space and at the same time (ii) had already accumulated sufficient equity to purchase a property, were most adversely affected by the repeal of the subsidy.

In addition, Rohrbach (2003) notes that at least one child was living in over 60 percent of EZ-receiving households and that, in 80 percent of the cases, those children were under 16 years of age. If parents no longer move into their own home in the urban hinterlands after the repeal of the EZ, their underage children will also remain in the city, shrinking the size of this age cohort in the years after the repeal.[10]

3.4 Empirical Framework and Results

I motivate the empirical analysis with the following simple propositions: Assume that there are only two housing types: Rental housing in the city and both new and existing one- and two-family housing (owner-occupancy) in the urban hinterlands (recall this stylized fact from Figure 3.2). The repeal of the homeownership subsidy, which implicitly promoted housing in the hinterlands, resulted in fewer residents moving from rental into owner-occupied housing. Consequently, we observe reurbanization, i.e. a population advantage of the city over the hinterland, in the period following the repeal.[11]

To identify the causal effect of the subsidy repeal on the spatial distribution of the population, I present two approaches in this section: In the first approach, I use a Difference-in-Differences (DD) framework to show that the subsidy repeal caused a shrinkage of the affected age cohort in rural areas. In the second approach, comparing constant age cohorts using a Triple Differences (TD) framework, I show that those who were of "building-age" before the repeal of the subsidy live in rural areas fifteen years later while those who were too young to have benefited from the subsidy at the time of its repeal do not.

3.4.1 Urban vs. Rural Living

Identification The DD method allows me to identify a treatment effect (on, e.g., the number of children) by differentiating across space (cities vs. their

[10] Of course it does not have to follow that the "successors" in age groups of those having benefited from the subsidy must also be the ones that suffer from its removal. But I believe there are convincing arguments that this is indeed the case: For example, the age range in which people normally move into their own homes is relatively narrow.

[11] One could also formalize this, as did Daminger and Dascher (2020). They use a multiple-qualities filtering model adapted for the housing market (Sweeney 1974) and rank housing qualities by tenure. The repeal of a homeownership subsidy, which implicitly incentivized living in existing and newly built single- and two-family homes, leads to a higher demand in the rental housing market and a decline in demand in the owner-occupancy market through filtering processes. The strong link between tenure and residential location suggests that the repeal of this type of homeownership subsidy leads to population growth in central locations and population decline in peripheral locations.

hinterlands) and time (before and after the subsidy's repeal). The conditional expectation of the log of population count (in various age subgroups) y inhabiting municipality (city/county) i in labor market region j at year t is dependent on all covariates on the right side, in short x_{ij}^t:

$$E(\mathrm{y}_{ij}^t|\mathrm{x}_{ij}^t) = \alpha + \mu_j + \beta_1\,\mathrm{PERI}_{ij} + \beta_2\,\mathrm{POST}^t + \beta_3\,\mathrm{PERI}_{ij} \times \mathrm{POST}^t, \tag{3.1}$$

μ_j is a region fixed effect controlling for unobserved time-invariant factors that affect regions (cities and their hinterland) individually. PERI is a region periphery dummy turning 1 if municipality i is a county and thus belongs to the hinterlands and zero if i is a city. POST is a treatment period dummy turning 1 if year t dates to after 2005, the year of subsidy removal, and zero otherwise.

I briefly describe the interpretation of the coefficients and later discuss the actual estimated coefficients. β_1 indicates the difference in log population in counties compared to cities before the repeal of the EZ (difference in space). β_2, meanwhile, indicates the extra in log population after repealing the EZ compared to before (difference in time). The coefficient most important for interpretation, β_3, indicates how the difference between the log population of counties and cities changes in the wake of subsidy repeal (difference-in-differences). This DD-coefficient can be equated with the effect of subsidy removal on treated units, i.e. counties, over and above all other trends that might have influenced peoples' residential location choices.

Parallel trends In any Difference-in-Differences setup, the interaction term can only be interpreted causally if the common pre-trends assumption is not violated. This means that, in the absence of treatment, treatment and control group shall not trend differently (Lechner 2011). In my setup, I identify the number of children (as a proxy for families) as the strata of population strongest affected by subsidy removal. The parallel trends assumption thus demands that, in the period before the subsidy's repeal, the number of children in cities and urban hinterlands evolves similiarly.

Figure 3.5 allows a visual inspection of the trends in the number of children in cities and the urban hinterlands in the pre-treatment period. There is no evidence of a violation of the parallel trends assumption. To further elaborate the comparability in the number of children in cities and counties

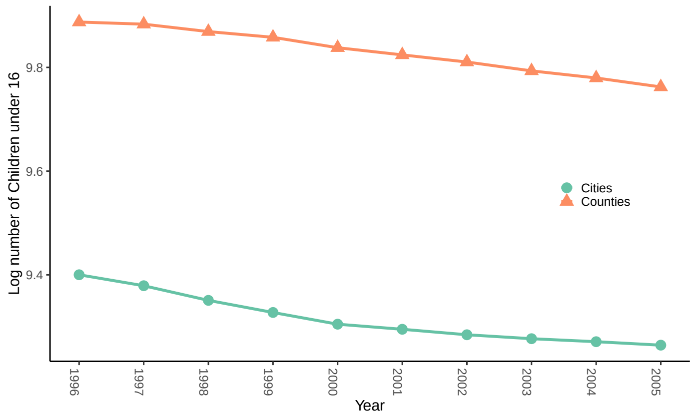


Fig. 3.5: Parallel trends in the pre-treatment period

Note: This graph plots the log number of children in cities and counties in the pre-treatment period. Cities are shown in green while counties are shown in orange. Clearly, both lines do follow a parallel trend before the subsidy repeal. Data: Federal and state statistical offices.

in the pre-treatment period, I additionally perform placebo interventions with a regression as Equation 3.1 and a restricted sample with years 1996–2005. Table 3.3 has the results with a placebo intervention in 1999 (column 1), 2001 (column 2), and 2003 (column 3). The estimated coefficient on the DD interaction is statistically not significant from zero, suggesting that cities and urban hinterlands did not exhibit differential trends in the pre-treatment period.

Results Table 3.4 has OLS and 2SLS results for the estimation of Equation 3.1 for children (up to age 16) in columns 1, 4, and 7, for building age population (aged between 30 and 50) in columns 2, 5, and 8, and for the total residential population in columns 3, 6, and 9.[12] I expect from the subsidy's design that its removal had the largest impact on the number of children (i.e. families) and the size of the population of building age.

[12]Since the dependent variable is of count nature, maximum likelihood estimation (Poisson) might also be appropriate. I also conducted an ML estimation, which led to the same conclusions as the OLS results.

Tab. 3.3: Testing the parallel trends assumption

	Log number of children under 16		
	(1)	(2)	(3)
Intercept	9.38***	9.35***	9.33***
	(0.10)	(0.10)	(0.10)
Placebo	−0.09***	−0.07***	−0.06***
	(0.01)	(0.01)	(0.01)
Peri	0.50***	0.51***	0.52***
	(0.08)	(0.08)	(0.08)
Placebo × Peri	0.02	0.00	−0.01
	(0.01)	(0.01)	(0.01)
Placebo Intervention Year	1999	2001	2003
Adj. R^2	0.11	0.11	0.11
Num. obs.	1960	1960	1960
N Clusters	72	72	72

Note: OLS regression with the log of children under 16 as the response variable. Placebo interventions are in years 1999 (column 1), 2001 (column 2), and 2003 (column 3). Observations are from 1996–2005. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Data: Population data are from federal and state statistical offices.

Tab. 3.4: Reurbanization of young residents post subsidy repeal

	OLS						2SLS		
	(1) Ln Children	(2) Ln "Building-age"	(3) Ln Pop	(4) Ln Children	(5) Ln "Building-age"	(6) Ln Pop	(7) Ln Children	(8) Ln "Building-age"	(9) Ln Pop
DD coefficients									
Peri	0.43^{***}	0.30^{**}	0.29^{**}	0.39^{***}	0.26^{*}	0.25^{*}	0.45^{***}	0.34^{**}	0.32^{**}
	(0.09)	(0.10)	(0.09)	(0.10)	(0.11)	(0.11)	(0.11)	(0.11)	(0.10)
Post	-0.06^{***}	-0.10^{***}	0.00	-0.00	-0.01	0.04	-0.26^{***}	-0.25^{**}	-0.20^{**}
	(0.02)	(0.01)	(0.01)	(0.04)	(0.04)	(0.04)	(0.07)	(0.08)	(0.07)
Peri × Post	-0.11^{***}	-0.03^{*}	-0.00	-0.13^{***}	-0.04^{*}	-0.03	-0.22^{***}	-0.13^{**}	-0.11^{**}
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)	(0.04)	(0.03)
Controls									
Ln Household Income				-0.26	-0.31	0.00	1.85^{**}	1.41	1.76^{**}
				(0.30)	(0.31)	(0.29)	(0.62)	(0.69)	(0.61)
Female employment rate				-2.52^{*}	-2.33	-2.36	-2.70	-2.29	-2.32
				(1.20)	(1.19)	(1.17)	(1.79)	(1.74)	(1.70)
Weak instruments (Ln avg. household income)							57.61^{***}	57.61^{***}	57.61^{***}
Weak instruments (Female employm. rate)							73.62^{***}	73.62^{***}	73.62^{***}
Wu-Hausman							13.33^{***}	10.47^{***}	11.12^{***}
Overidentifying							294.19^{***}	310.47^{***}	293.29^{***}
Labor Market Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Labor Market Region FE × Post	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.68	0.65	0.65	0.72	0.69	0.68	0.68	0.65	0.65
Num. obs.	4312	4312	4312	4152	4152	4152	3474	3474	3474
N Clusters	72	72	72	72	72	72	72	72	72

Note: OLS regressions (col 1–6) with the log of population in age strata as the response variable. Building-age residents are aged between 30 and 50 years; children are aged between 0 and 16 years. 2SLS regressions (col 7–9) with Ln Household Income and Female employment rate instrumented by sectoral employment shares in the hospitality, financial and public sector, the unemployment rate, and business tax revenue (see Appendix B.2 for description). Clustered standard errors (at region level) in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Data: Population data and data on household income and sectoral shares are from federal and state statistical offices. Data on the female employment rate, the unemployment rate, and municipalities' business tax revenues are from the INKAR database.

I start by discussing the OLS results of the plain DD-framework (columns 13) as outlined in Equation 3.1: For the log of the number of children as the response variable, the estimated DD-coefficient $\widehat{\beta_3}$, indicating the effect of subsidy removal on the urban hinterlands, is 0.11 in column 1 and statistically highly significant. For the log of the number of "building-age" population in column 2, this estimated coefficient remains negative and statistically significant. It is only for the total population in column (3), that both, $\widehat{\beta_2}$ and $\widehat{\beta_3}$, render statistically insignificant, with the point estimate actually being zero, suggesting no significant change in total resident population post subsidy repeal in either, city or periphery.

This plain DD-specification might suffer from omitted variable bias, as there are surely time-varying changes on the municipality level steering residents' location choices that I have not adequately controlled for. Therefore, in columns 4–9, I add Region $\times$ Post fixed effects to my specification and control for changes in the average household income and in womens' participation in the labor market. My choice of these two additional time-varying covariates is motivated by the results of Dauth et al. (2018) and Brenke (2015), who find that, in Germany's large cities (i) the urban wage premium and (ii) the female employment rate increased strongly. On the one hand, higher wages and thus household incomes increase the propensity of forming homeownership–and thus moving out–in the first place (Barakova et al. 2003; Di and Liu 2007; Goodman and Mayer 2018; Haurin et al. 1987). On the other hand, the–predominantly non-rural–presence of childcare facilities enables mothers to re-enter the labor force and might thus steer them (and their families) to cities to have short commutes and more time to spend with their children (Farré et al. 2020; Morrissey 2017; Neuberger et al. 2020).

In order not to confound my results by these effects, I control for the female employment rate and the average household income at the municipality level in addition to time-invariant effects at the region level in the pre- and post-treatment periods. I report OLS results of this specification in columns 4–6: Still, the estimated coefficient on the DD-coefficient $\widehat{\beta_3}$ is negative and statistically significant for the population strata of children under 16 (column 4) and the "building-age" population (column 5), while it renders statistically insignificant for the total residental population (column 6).

Both my additional control variables might suffer from endogeneity; hence the distribution of population across space might reversely influence, e.g., employment opportunities and thus household incomes as well as the female employment rate. Columns 7–9 thus report Two-Stage least squares (2SLS) results with the female employment rate and the average household income instrumented by sectoral employment shares in the hospitality, financial, and public sector as well as municipalities unemployment rates and business tax revenues. The diagnostics show that I chose strong instruments for both instrumented variables, and a test for overidentifying restrictions rejects the null that the model is overidentified. Further, the significant endogeneity test (Wu-Hausman test) provides evidence for variables being endogenous and thus OLS estimation not being equally consistent.

The 2SLS results suggest that it was indeed children (and their parents) that suffered most from subsidy repeal: After repeal, roughly 38% ($\widehat{\beta_2} + \widehat{\beta_3}$) fewer children, 32% fewer residents in "building age", and 27% lower total population lived in the urban hinterlands. As expected by its design, the subsidy played a major role in the residential location choice among young families.

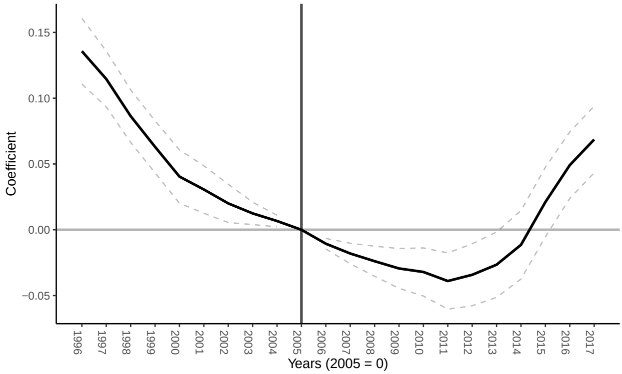
Next, I provide insight into the dynamics of population growth over time to show the precise coincidence of the EZ repeal with changes in population growth. I modify Equation 3.1 such that the 2005 repeal of the subsidy does not split my study period into two periods (pre- and post-treatment) but instead reveals the dynamics of population developments in cities and their neighboring counties using year fixed effects.

Let the conditional expectation of the log of children under age 16 y, inhabiting municipality i in labor market region j at year t depend on all variables on the right side, in short x_{ij}^t:

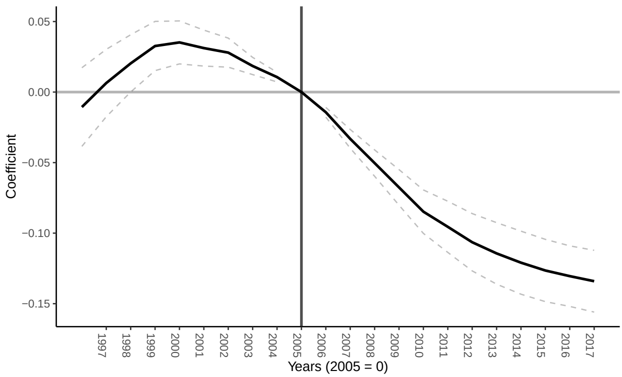
$$E\left(\mathrm{y}_{ij}^t | \mathrm{x}_{ij}^t\right) = \alpha + \mu_i + \beta_1 \,\mathrm{PERI}_{ij} + \sum_{t=2}^{T} \delta^t \,\mathrm{YEAR}^t + \sum_{t=2}^{T} \lambda_i^t \,\mathrm{PERI}_{ij} \times \mathrm{YEAR}^t \tag{3.2}$$

Adding a full set of year dummy-variables in this type of regression model would result in multicollinearity (dummy-variable trap). Therefore, one left-out year dummy serves as the reference, and I choose to leave out the dummy for 2005, the last year of the EZ being in effect. All changes in y indicated by the estimated coefficients $\widehat{\delta^t}$ and $\widehat{\lambda_i^t}$ are thus relative to 2005.

Figure 3.6 shows estimated coefficients of the Year dummies in the top panel (a) while bottom panel (b) shows the estimated coefficients of Peri × Year dummies. Panel 3.6a can be interpreted as the population trend of cities



(a) Year dummies ($\widehat{\delta^t}$)



(b) Peri $\times$ Year dummies ($\widehat{\lambda_i^t}$)

Fig. 3.6: Central and peripheral population growth dynamics

Note: This figure plots estimated coefficients along with 95% confidence intervals of the estimated coefficients $\widehat{\delta^t}$ and $\widehat{\lambda_i^t}$ from equation 3.2. Standard errors are clustered at the region level. With the subsidy still in place, the hinterland's lead over the city grows (figure 3.6b). After its repeal, this lead reverses into a lag.

(centers) while 3.6b represents the extra in population of the urban hinterlands (peripheries) compared to cites over time. I interpret this extra of the hinterlands over cities as the EZ effect, analogous to the DD approach from earlier. During the presence of the homeownership subsidy, the hinterlands' number of children increased *more* than that of the urban population with a peak in this extra around 2000/2001. Thereafter, this extra persists but becomes smaller. With the repeal of the homeownership subsidy in 2005, this dynamic reverses and the hinterlands' number of children grows *weaker* than the urban population.

Interesting is why the lead of the peripheries over the centers becomes smaller well before EZ's repeal. One reason could simply be that many homeowners-to-be took advantage of the first years of the EZ to switch to owner-occupancy, with the flow declining thereafter. And also recall the changes in the subsidy's rules regarding income limits, presented in Section 3.2. A pull-forward effect prior to the 2000 change is likely, with a reduction in income limits of this magnitude (by nearly 1/3) in sight. And lastly, it should not be neglected that the serious changes in the subsidy amount in 2004, which also occurred due to consolidation constraints of public budgets, were already being discussed years earlier. In sum, households probably pulled forward their homeownership decision as a result of this discussion. Thus, the temporal dynamics of the evolution of this population growth lead can still be convincingly linked to the EZ's temporal dynamics, because only after its repeal did the sign of the lead turn negative, reversing it into a lag.

3.4.2 Old vs. Young

Identification In my second approach, rather than comparing different individuals in age cohorts at two points in time, I try to follow the same individuals in age cohorts over time and exploit the effects of individuals' differential treatment by age. I restrict my data sample to 1996–2002 (before subsidy repeal) and 2011–2017 (after subsidy repeal). In the period before subsidy repeal, I set the age cohort of 30–44 year olds ("olds") who were able to benefit from the subsidy due to their "building age", against the age cohort of 15–29 year olds ("youngs"), who were too young to own a home at the time of and before subsidy repeal. Over the omitted period, the "young" become the "old" and the "old" become the "very old". Consequently, for the period after subsidy repeal, I set the 45–59 year olds ("very old") against the

30–45 year olds (now “old”, formerly “young”).[13] I expect the initial old to have built and started residing in their own homes in the urban hinterland in time before the subsidy was repealed. In contrast, the initial young could no longer benefit from the subsidy and remain in place.

Let dummy YOUNG be 1 (zero) if the age cohort in municipality i and labor market region j is between 15- and 29-years-old (30–44) in 1996–2002 and between 30- and 44-years-old (45–59) in 2011–2017. In the following triple differences model, the conditional expectation of the logarithm of population y_{ij} depends on all covariates on the right-hand side of the equation, x_{ij} for short:

$$
\begin{aligned}
E(\mathrm{y}_{ij}^t|\mathrm{x}_{ij}^t) = \alpha + \mu_i + \beta_1\,\mathrm{POST}^t + \beta_2\,\mathrm{YOUNG}_{ij}^t + \beta_3\,\mathrm{PERI}_{ij} \\
+ \gamma_1\,\mathrm{POST}^t \times \mathrm{YOUNG}_{ij}^t + \gamma_2\,\mathrm{POST}^t \times \mathrm{PERI}_{ij} + \gamma_3\,\mathrm{YOUNG}_{ij}^t \times \mathrm{PERI}_{ij} \\
+ \delta\,\mathrm{POST}^t \times \mathrm{YOUNG}_{ij}^t \times \mathrm{PERI}_{ij}\,. \qquad (3.3)
\end{aligned}
$$

Coefficient δ indicates the extent to which the young suburbanized *less* (or more, if the estimated coefficient is positive) than the old over the left-out fifteen-year period. This effect, which I interpret as the causal effect of subsidy removal on the residential choice of the young and old, is unbiased by effects that affect both age groups and that I do not explicitly control for, e.g., specific observable or unobservable growing urban amenities.

Results Columns 1 and 2 of Table 3.5 have OLS results without and with Region × Post fixed effects (in addition to Region fixed effects) and with the female employment rate and the logarithm of the average household income as covariates. Column 3 has, again, 2SLS estimates with the female employment rate and the logarithm of average household income instrumented by sectoral employment shares in the hospitality, financial, and public sector, the unemployment rate and the business tax revenue.

Figure 3.7 captures regression estimates in terms of “population gradients” using estimated coefficients from column 1. The gradients for the old (the control group) are shown on the left, while gradients for the young (the treatment group) are shown on the right. Solid blue lines in each panel

[13] Of course, with my aggregate data, I in fact *do not* observe individuals. I cannot account for “moving out of my sample”, i.e. that individuals forming an age cohort at one point are not the same individuals forming this (now older) age cohort later. It nevertheless seems highly implausible that the aggregate numbers consist of completely different individuals.

Tab. 3.5: Old vs. Young Residents

	OLS		2SLS
	(1)	(2)	(3)
TD coefficients			
Post	-0.09^{***}	0.03	-0.42^{*}
	(0.01)	(0.09)	(0.15)
Young	-0.28^{***}	-0.28^{***}	-0.30^{***}
	(0.01)	(0.01)	(0.01)
Peri	0.30^{**}	0.24^{*}	0.33^{**}
	(0.09)	(0.11)	(0.11)
Post $\times$ Young	0.15^{***}	0.15^{***}	0.17^{***}
	(0.02)	(0.02)	(0.02)
Post $\times$ Peri	0.11^{***}	0.10^{**}	-0.06
	(0.01)	(0.04)	(0.07)
Young $\times$ Peri	-0.09^{***}	-0.09^{***}	-0.11^{***}
	(0.01)	(0.01)	(0.01)
Post $\times$ Young $\times$ Peri	-0.11^{***}	-0.11^{***}	-0.09^{***}
	(0.01)	(0.02)	(0.02)
Controls			
Ln Household Income		-0.22	2.20^{*}
		(0.42)	(0.95)
Female Employment Rate		-2.68^{*}	-2.75
		(1.21)	(1.96)
Weak Instruments (Ln Household Income)			46.10^{***}
Weak Instruments (Female employm. rate)			48.31^{***}
Wu-Hausman			16.04^{***}
Overidentifying			414.36^{***}
Region FE	Yes	Yes	Yes
Region FE $\times$ Post	No	Yes	Yes
Adj. R^2	0.66	0.69	0.65
Num. obs.	5488	5200	3860
N Clusters	72	72	72

Note: OLS and 2SLS regressions with the log of population count as the response variable. Long panel with years 1996–2002 (before repeal) and 2011–2017 (post repeal). Young = 1 in years before (post) repeal, in age stratum 15–29 (30–44) and zero in years before (post) repeal, in age stratum 30–44 (45–59). 2SLS regression with female employment rate and Ln Household Income instrumented by sectoral employment shares in the hospitality, financial, and public sector, the unemployment rate, and the business tax revenue (see Appendix B.2 for description). Clustered standard errors (at region level) in parentheses. $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. Data: Population data and data on household income and sectoral shares are from federal and state statistical offices. Data on the female employment rate, the unemployment rate, and municipalities' business tax revenue are from the INKAR database.

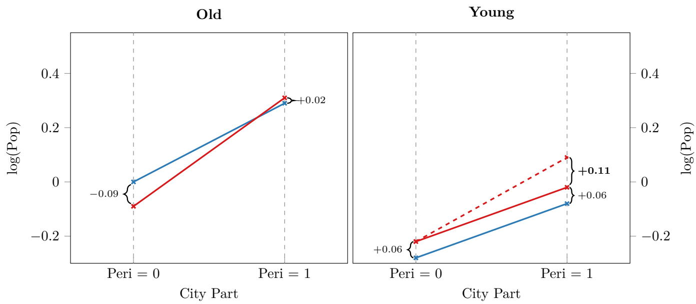


Fig. 3.7: Population Gradients of Old and Young

Note: This figure shows the results from Table 3.5 using "population gradients" of the old and young. Blue solid lines show these gradients for the period before the subsidy was repealed; red solid lines show the gradients for post repeal. The logarithm of the number of "old" population (control group, left panel) decreased by 9 log points in cities while it increased by 2 log point in the urban hinterlands. For the old, the difference between these two differences is 11 log points. The logarithm of the population count of "young" population (treatment group, right panel) increased symmetrically in both cities and urban hinterlands by 6 log points. For the young, the difference between these two differences is in fact zero. The red dashed line in the right panel shows the counterfactual situation if the gradient of the treatment group (the "young") had evolved the same as the control group (the "old"), i.e., if the subsidy had not been repealed: the slope of the "young"'s population gradient would increaseeven stronger and the log number of young population in the periphery would be 11 log points higher than it actually is (difference-in-differences). Data: Author's calculations using NUTS-3 population data from federal and state statistical offices.

depict the population gradient for the period pre-subsidy repeal while solid red lines depict it for the period post-repeal. The initial old who could have benefitted from the subsidy have indeed suburbanized. In cities, the incidence of this age cohort decreases by 9 log points while it increases by 2 log points in the urban hinterlands.

The young, on the other hand, who no longer benefited from the subsidy, grew evenly, by 6 log points in both urban and rural areas. The red dashed line in the right panel now shows how the population gradient of the young would have developed if the subsidy had not been repealed, i.e. if the young still had benefitted from the subsidy and had followed the same trend as the old: they would have suburbanized strongly, by 11 log points more than they actually did. The repeal of the subsidy prevented the young from moving to the urban hinterlands.

3.5 Conclusion

Germany's "new love with urban living" is not just a story of changing residential preferences, increasing urban wage premiums, stronger immigration, or higher female participation rates due to improved childcare options in cities. Rather, and often unnoticed, German homeownership policies played an important role in families' residential location decisions.

Owner-occupied housing in Germany is predominantly found in single- and two-family homes, and these are easiest built in the urban hinterland rather than in the city. The repeal of the largest homeownership subsidy program in 2005 deprived potential home builders–typically young families–of the incentive and financial means to build their own home. They stayed living in the city as renters and did not suburbanize.

Using multiple approaches with varying difference-in-difference frameworks, this paper establishes a causal link between the repeal of the homeownership subsidy and Germany's reurbanization. The central result is that children (and their parents) are less likely to live in the urban hinterland after the repeal. Moreover, I find that the age cohort that was still able to benefit from the subsidy before it was repealed now resides in the suburbs. Those who were in their teens before the subsidy was repealed did not create homeownership without it and stayed put at their initial place of residence.

These findings complement policy advice on issues of homeownership and suburbanization: First, if a policymaker's goal is fostering reurbanization, then repealing homeownership subsidies seems to be a promising tool to steer residential choice in favor of cities. Second, if living in cities leads to smaller commutes and living spaces–and thus fewer greenhouse gas emissions–then repealing homeownership subsidies might also be an important tool in the fight against climate change.

However, as Daminger (2021b) shows, homeownership subsidies also appear to function as a "price valve" for the urban rental market. Thus, third, the repeal-induced stop of population flows to the periphery, triggering a positive demand shock in rental housing markets, will likely lead to an increase in cities' rents.

Homeownership Subsidies and the Spatial Distribution of Rents within Cities

4

This chapter is based on Daminger (2021) Subsidies to Homeownership and Central City Rent, BGPE Discussion Paper No. 210.

4.1 Introduction

Surging rents and house prices in German cities have urged politicians to think about political countermeasures. Among calls for controlling rent or regulating housing companies, the focus also shifted to promoting owner-occupied housing. With its previous homeownership subsidy ("Eigenheimzulage") repealed in 2006, Germany revived its subsidies to homeownership in 2018. First-time homeowners–the targeted group of this homeownership subsidy–change their tenure from renting to owning, inducing a negative demand shock in rental housing markets. Tenure is closely linked to location, with rental housing almost exclusively located in apartments in multi-family houses, and thus near the city center. This paper provides an empirical test to the hypothesis that homeownership subsidies reduce demand for rental housing in central areas and thus reduce central apartment rents.

More specifically, I contribute to the literature in two ways. First, to the best of my knowledge, this paper is the first to analyze the effects of a lump sum homeownership subsidy on rents. Second, I construct a data set of the spatial structure of intra-city rents using a large micro-data set on apartment advertisements in 105 major German cities. I identify the subsidys effects on cities spatial rent structure by exploiting its design, which neither differentiated across cities nor across size nor across characteristics of the property. Although nominally solely depending on the number of children of the subsidy receiver, in real terms, it benefited prospective owner-occupiers in more affordable cities most, drawing them from central rental to peripheral owner-occupied housing. The results of several triple difference (TD) comparisons, exploiting variation in timing, intra-city apartment location, and inter-city levels of affordability, indicate that central city rent premium significantly fell with subsidy introduction.

Homeowners predominantly occupy single and two-family houses, and those tend to be outside a city center.[1] Subsides to homeownership thus induce population flows from (central) rental to (peripheral) owner-occupied housing, as documented in Daminger and Dascher (2020).[2] Their paper analyzes intra-city population flows in the wake of a similar subsidy's removal and finds that the removal steers people to live in the city center, contributing to a "central living renaissance". This paper follows up the analysis and documents changes in the spatial rent structure in the wake of a homeownership subsidy introduction. As new owner-occupiers at the peripheries have been city-center renters before, their move into homeownership corresponds to a negative demand shock in central rental markets.

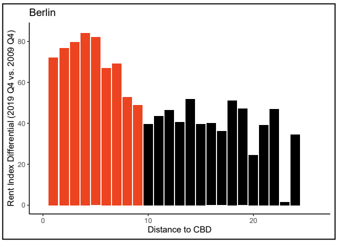
Baum-Snow and Han (2021) find that the housing supply elasticity is lowest near the city center and increases monotonically with distance to the center, meaning that in positive demand shocks, housing in central areas cannot be added swiftly enough and results in price rather than quantity changes. In negative demand shocks however housing can, at least in the short term, not simply be demolished, resulting in a supply curve that is "kinked downwards" (Glaeser and Gyourko 2005). Consequently, negative demand shocks–which I suspect the homeownership subsidy to trigger in central rental markets–will result in higher vacancies and lower (housing service) prices, i.e. rents.[3]

Figure 4.1 visualizes rent development in city rings over time for two selected cities, Berlin on the left and the (substantially smaller, more affordable) city of Halle in the German state of Saxony-Anhalt on the right. Each bar represents the rent index differential between 2009 Q4 and 2019 Q4 (and thus enveloping policy introduction in 2018 Q3) for a concentric [j, j+1)-km ring around the respective citys CBD. Bars in red belong to central

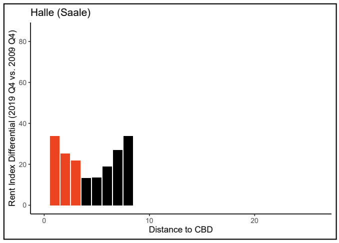
[1] Hilber (2007) finds that a detached house is substantially more likely to be owner-occupied than an apartment in a multi-family building. Ahlfeldt and Maennig (2015) document that close to 80% of one- and two-family houses are owner-occupied.

[2] And this is also true for the specific subsidy under review in this paper: According to an interim status report, until May 2020 (i.e. after roughly 4/5 of the planned program duration) close to 85% of all subsidy applications were for the purchase or construction of houses with the small remainder being applications for apartments.

[3] As only prospective owner-occupiers without prior real estate ownership in Germany are eligible for the subsidy, they truly must have been renters before.



Berlin, 2009-2019



Halle, 2009-2019

Fig. 4.1: Rent Index Differential in Rings

Note: This figure has the ring rent index differential between Q3-2009 and Q3-2019 for the city of Berlin on the left and the city of Halle an der Saale on the right. Every bar has the rent differential (y-axis) as a function of the distance to the city center (x-axis). While central rings (orange) apartment rent growth overshadowed that of decentral rings in Berlin, this was not the case for, the substantially smaller and more affordable, city of Halle an der Saale. Source: Author's calculations.

while those in black belong to peripheral rings.[4] The figure not only reveals that, compared to each city ring's respective baseline rent in 2009 Q4, Berlin experienced significantly larger increases in rents. It also shows that, while the biggest rent increases in expensive Berlin had to be endured by new tenants in central apartments, more affordable Halle saw this high rent increases not exclusively in the citys center. While renters wanting to live in Berlin's center rather than at its outskirts clearly had to pay an increasing premium for that location choice, this does not apply to the same extent in Halle. This advantage (and its development) of central rents over peripheral rents can be paraphrased as "central rent premium" (development), and exploring it in the course of subsidy introduction is the main subject of this paper.

The differing central rent premium development by city affordability is not restricted only to these two example cities in Figure 4.1 but can be identified as a pattern in the whole sample of cities. Subsidy amount is tied to the number of children and is thus fixed in nominal terms, but of course not in real terms. Families in affordable cities benefit more than their counterparts in expensive cities, where the subsidy in real terms is just a "drop in the bucket". Affordable cities' families use the subsidy to move into their owner-occupied home at the outskirts, lowering demand for (predominantly central) rental apartments and thus this segment's rents. What this paper now

[4]Here, central rings are defined as rings within the first third of all of a citys rings.

argues is that, had federal government not introduced the homeownership subsidy, affordable cities central rent premium developments would have followed the same pattern as that of their expensive peers. Halles central apartment rent increase thus would have greatly outpaced its peripheral rent increases, similar to Berlins development. In my regression analyses, controlling for many other (partly unobservable) factors that drive rental markets, I find that the central rent premium of affordable cities, compared with their expensive peers, rises *less* strongly with subsidy introduction. In that sense, subsidy introduction *dampened* central rent premium surge in affordable cities while expensive cities have not experienced a similar relief.

This paper contributes to two distinct strands of the literature: Previous work that deals with the effects of housing subsidies on prices by e.g. Sommer and Sullivan (2018), Davis (2019) or Hilber and Turner (2014) shows that subsidizing homeownership through the tax code might result in rising house prices, with Carozzi et al. (2020) or Krolage (2020) finding the same for direct homeownership subsidies. Effects of rental subsidies on apartment rents are studied by Eerola and Lyytikäinen (2021), Gibbons and Manning (2006) or Eriksen and Ross (2015). They find substantial subsidy capitalization in rents. Braakmann and McDonald (2020) study the effect of rental subsidies on house prices with the same result of subsidy capitalization in prices. This paper attempts to fill a gap in this literature by analyzing the impact of lump sum *homeownership* subsidies on *rents*. But it also adds to the literature dealing with homeownership subsidies spatial implications, such as Muth (1967), Glaeser (2011) or Daminger and Dascher (2020). If, as this strand argues, subsidies to homeownership contribute to suburban living, these population ("goods") shifts should also translate into shifts in associated (relative) market prices.

Additionally, this contribution seeks to complement the literature that deals with housing supply's price and rent implications. Mense (2021) shows that total new housing supply, i.e. housing for both tenure types, reduces rents throughout the rent distribution. Germany's subsidy provided a link between the two tenures by incentivizing moves from rental to owner-occupied housing. And although both new and existing owner-occupied housing units have been subsidized, a substantial portion of the subsidy has been used to build new housing units. Through this filtering mechanism from rental to owner-occupied housing, the subsidy has relieved pressure on rental markets by encouraging a move to owner-occupied homes.

The paper has six sections. Section 4.2 introduces the subsidy design. Section 4.3 describes the empirical data, explains the construction of the panel of city-ring-quarter/year rent indexes and presents some graphic representations of intra-city rent development. Section 4.4 turns to the empirical analysis, which identifies the subsidy introductions effects on the spatial structure of city rents by exploiting different treatment intensities by city affordability. Section 4.5 presents the results, finding that subsidy introduction dampened affordable cities' central rent premium growth. Section 4.6 concludes and provides some policy implications.

4.2 Context and Program Description

Four phases in subsidizing homeownership can be distinguished in Germany post World War II: In a first phase (1949–1995), investments in owner-occupied housing were tax-deductible. Phase 2 (1996–2005) consisted of a direct subsidy for purchasing or constructing owner-occupied housing (*Eigenheimzulage* or EZ for short). The EZ was repealed at the end of 2005 without replacement. In the following twelve years (2006–2017), the third phase, there was no distinct federal policy to promote homeownership. In 2018, a new support program (*Baukindergeld*, or BK for short) was introduced, which pays out subsidies to households with children for the construction and purchase of owner-occupied housing.

This paper examines the transition from phase three to phase four, the introduction of BK in 2018. Table 4.1 shows the main characteristics of the subsidy. All households with at least one child in custody are eligible. The taxable income of households with one child may not exceed €90,000, with this threshold being increased by €15,000 with each additional child. The subsidy promotes the purchase of owner-occupied condos and houses, both newly built and existing.

On June 26, 2018, the coalition committee of the Federal Government agreed that the BK should be granted retroactively as of January 1, 2018 and until December 31, 2020. This meant that only properties for which the purchase agreement had been signed by December 31, 2020 or, in the case of new buildings, for which planning permission had been granted by this date, were eligible.[5] On July 5, 2018, the German Bundestag passed

[5] With the coronavirus outbreak delaying many building permit processes, this deadline has been extended to March 31, 2021.

the corresponding legislative resolution. Since September 18, 2018, applications can be submitted to *Kreditanstalt für Wiederaufbau (KfW)*, a German state-owned development bank. According to the KfW, applications can be submitted until December 31, 2023 at the latest. The BK represented a huge part of the federal subsidy budget: A total of just under € 10 billion is earmarked for the three-year program, while only around € 5 billion were planned for social housing construction in the entire legislative period (four years).

The individual subsidy amount is based solely on the number of children, with a (program total) amount of € 12,000 being paid for each child in the household. The subsidy is not a one-time payment but is spread evenly over a period of ten years. The BK is, in nominal terms, equally granted, i.e. irrespective of the location of the property. To illustrate the importance of the BK in financing homeownership, I take the example of a family with two children who is eligible for support. This family receives a subsidy of € 24,000 for both, either the purchase of an existing property or the construction of a new home. If it purchases a property in an expensive city for e.g. € 200,000, the share of the subsidy in investment costs is 12%. If, on the other hand, it buys a comparable property in an affordable city for half the price, the share of the subsidy is doubled, to 24%. This illustrates that, although the subsidy is the same in nominal terms everywhere, the real subsidy rate varies greatly with property price and hence a city's real estate affordability.[6]

It should be emphasized that the application for BK was downstream and was only possible after the owner-occupied home was actually occupied. Thus, in the case of the purchase of existing housing, the move must have already taken place, while in the case of new construction, both construction and move-in must have taken place. While new construction naturally takes longer than simply moving into an existing home, both result in a negative demand shock to the rental housing market. Thus, the expectation is that effects on the rental market in the short run–for the first quarters–will arise from moves into existing housing, while in the longer run the effects of new construction add on as well.[7]

[6] As much as I would like to infer the spatial distribution of subsidy take-up, and thus the intensity of treatment, directly from micro data, these are not available to me.

[7] And this is also what early descriptive statistics from the regional building society (*Landesbausparkasse*) about the uptake confirm: For the few months in 2018, the share of subsidy applications for new owner-occupied homes in all applications of 2018 was 14%, while it rose to 27% in 2019 and to 32% for the first months of 2020.

Tab. 4.1: Design of Baukindergeld (BK)

Beneficiary	Entitled beneficiary	Households with children
	Maximum taxable hh. income	€ 90,000
	Threshold increase per child	€ 15,000
Object	Subsidized property	Owner-occupied housing (house & condo / new & existing)
Subsidy	Funding start	Year of move-in
	Funding period	9 subsequent years
	Assessment basis	Number of children
	Yearly subsidy amount (per child)	€ 1,200

Note: This table has the features of Germany's homeownership subsidy called "Baukindergeld". The rather unique aspect of the subsidy is that it is solely tied to the number of children living in the household, handing out a total of € 12,000 per child over the course of ten years. Applications (and payments) were tied to actual occupancy of the owner-occupied housing, making it possible to submit the application only after moving in. Source: Kreditanstalt für Wiederaufbau (KfW).

4.3 Data and Rent Index Construction

4.3.1 Data

I use data from several sources to analyze the intra-city development of rents in all German urban municipalities (*Kreisfreie Städte*). Rental advertisement data is from the Ruhr Research Data Center at the RWI - Leibniz Institute for Economic Research.[8] It contains all rental apartment advertisements and their characteristics from Germany's largest real estate platform immobilienscout24.de from 2008 Q1–2020 Q1.[9]

Usually, a property's geographic coordinates would be used to spatially locate it in a city area. Unfortunately, exact coordinates of the advertised properties are missing in the data. For the 2011 census however, the surface of Germany was overlaid virtually with a 1x1-km grid to enable data

[8] In Germany, rents are not officially registered or consolidated. I use the asking rent at the end of an advertisement's term (creators can adjust the asking rent during the ad's time on the platform), as I assume that to be very close on the actual contractually agreed rent. Additionally note that negotiating rents is uncommon in Germany.

[9] The market for rental houses is hardly existent in Germany. Though I also have data on that market, this partial data set has too few observations to estimate effects on city ring level consistently. Additonally note that the latest rental observations in my data set are from February 2020 and thus predate the coronavirus outbreak in Germany.

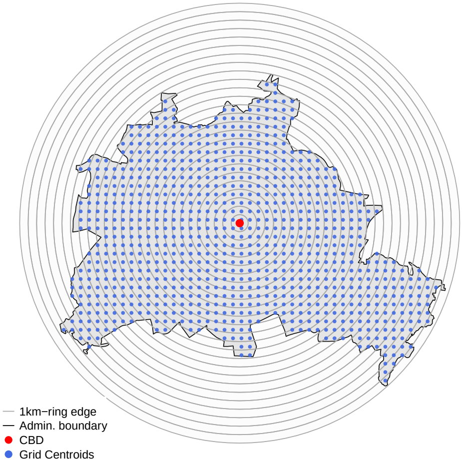


Fig. 4.2: Assignment of census grid cells to distance rings

Note: This figure shows the assignment of apartments to city rings in Berlin. The black solid line marks Berlin's administrative boundary, grey solid lines mark boundaries of 1km-wide rings, blue dots are the centroids of 1x1km grid-cells, and the red dot marks the CBD. Apartments in a specific grid are assigned to the distance ring the grid's centroid lies in.
Source: Author's illustration using shapefiles from the Federal Agency for Carthography and Geodesy.

analyses independent of administrative boundaries. Real estate ad data includes information on the assignment of properties to census grid cells.[10] Geographic information system (GIS) steps proceed as follows: Whenever possible, I define city hall as the Central Business District (CBD).[11] Using GIS-techniques, I determine the centroid of each grid cell located within an administrative city boundary. Next, I determine the linear distance between each cell centroid and the CBD, and group the grid cells into 1-km wide rings around the CBD.

Figure 4.2 visualizes the assignment of grid cells to these distance rings using Berlin as an example. It shows the CBD (*Rotes Rathaus*, red dot) within the administrative borders of Berlin. The blue dots are all centroids of 1x1km grid cells within Berlin's administrative boundaries. Starting from the CBD, cell centroids are aggregated into $j = \{1, \ldots, J\}$ 1km-wide con-

[10] GIS shapefiles describing the administrative boundaries and the census grid come from the Federal and State Statistical Offices.

[11] Some cities do not have a (historical) city hall. In these cases, I choose the historical marketplace or a building or square that can reasonably be considered part of the city's nucleus. Holian (2019) confirms for the US that city hall is a relatively accurate measure of CBD location.

Tab. 4.2: Descriptive Statistics of Rent Data

Property-related Variables			City-related variables		
Variable	Mean	Std. Dev.	No. of ...	Mean	Std. Dev.
Net Rent [in €]	571.37	416.26	Obs. per City	87,840	182,071
Living Space [in sqm]	70.69	29.97	Obs. per Quarter	195,400	33,214
Number of Rooms	2.52	0.96	Obs. per Ring	354,615	510,733
Year of Construction	1968	30.86	Obs. per City per Quarter	1,531	464
Distance to CBD [in km]	4.87	3.43	Obs. per City per Ring	8,473	19,040
Balcony [D]	0.60	0.49	Obs. per Quarter per Ring	7,586	10,589
Garden [D]	0.16	0.37	Obs. per City per Quarter per Ring	196.55	444.49
Guest Bath [D]	0.19	0.32			
Fitted Kitchen [D]	0.35	0.48			
Cellar [D]	0.54	0.50			
First Occupancy [D]	0.09	0.29			
Number of Cities: 109					
Number of Rental Advertisements: 9,574,605					
Number of Quarters: 49					

Note: This table has descriptives on the rent data. Dummy variables are indicated by [D]. Data: Author's calculation using RWI's dataset "RWI-GEO-RED: Real Estate Data".

centric rings. Each real estate ad is assigned to the distance ring in which its grid cell (centroid) is located.

Table 4.2 shows descriptive statistics for the data set. In total, roughly 9.5 million observations are used to calculate ring rent indexes. The average apartment has a monthly net rent of €571 and about 70 square meters of living space spread over 2.5 rooms. More than half of the apartments have a balcony or access to a cellar, while only 16% have access to a garden. Confidence in the representativeness of the sample is strengthened by the average year of construction (1968) and the fact that more than 90% of apartments are not first-time occupied; the sample is therefore not crowded by recently built and renovated apartments that might experience a separate rent premium and could distort rent indexes. The right panel of Table 4.2 shows city or, more precisely, index-related descriptives. It dispels concerns that data scope is not sufficient for index construction: On average, each citys rings have roughly 200 rental observations per quarter that can be used for the estimation.[12]

[12] Although it has to be noted that the standard deviation is rather large - central rings in Berlin of course have significantly more apartment offerings each quarter than peripheral rings of small Schweinfurt.

4.3.2 Hedonic Rent Index Construction

Next, I estimate hedonic rent regressions on the city level and separately for all cities in the sample.[13] The aim of the ring rent indexes computed from these regressions is to measure rent development over successive periods, controlling for quality characteristics of the apartment. Hence, the computed indexes use constant property characteristics and show the pure price changes over time. I construct rent indexes P_j^t using the Hedonic Dummy (HD) approach, which directly shows the marginal change in quality-adjusted price with respect to time t and ring j. Using a log-linear model, the estimated coefficients of the time and ring fixed effects refer to the marginal percentage change in rents in period t^t and ring j_j relative to period t^0 and ring j_1. Thus, a transformation of the estimated coefficients directly yields ring rent index P_j^t.[14]

The hedonic regression is given as

$$E\left(\mathrm{y}_{hj}^t|\mathrm{x}_{hj}^t\right) = \alpha+\sum_{k=1}^{K}\beta_k\,\mathrm{z}_{hjk}^t+\sum_{j=2}^{J}\gamma_j\,\mathrm{RING}_{hj}+\sum_{t=1}^{T}\theta^t\,\mathrm{TIME}_h^t+\sum_{j=2,t=1}^{J,T}\lambda_j^t\,\mathrm{RING}_{hj}\times\mathrm{TIME}_h^t\,, \tag{4.1}$$

where the dependent variable y is the log net rent of real estate ad h in ring j at time t. K characteristics of property h are contained in vector z[15]. $\mathrm{RING_j}$ is a dummy variable for the location of h, turning 1 if h is located in distance ring j and zero else. Its coefficient γ_j captures time invariant price differences between rings. $\mathrm{TIME^t}$ is a dummy variable turning 1 if rental advertisement h ended in t, indicating a successful lease, and zero elsewise. Its coefficient θ^t captures city-wide developments over time that affect all city rings. Finally, $\mathrm{RING_j}\times\mathrm{TIME^t}$ is an interaction term with λ_j^t capturing ring specific rent developments over time.

[13] A joint hedonic regression of all cities with all rings at all points in time would require enormous computing power. It is reasonable to consider each city as a closed real estate market.

[14] Compared to other approaches such as the hedonic characteristics approach, there is no need to define a "mean", "median" or "representative" dwelling. On the downside, the HD approach implicitly restricts quality characteristics of properties to be constant over time. In my robustness checks, I also calculate a Laspeyres-type double imputation index, which overcomes both shortcomings. Further, for excellent summaries of computation, strengths and weaknesses of the various approaches to estimating price indexes see Hill (2011), Haan (2010), Diewert et al. (2008) or Silver (2016).

[15] The exact specification of vector z is explained in detail in the appendix.

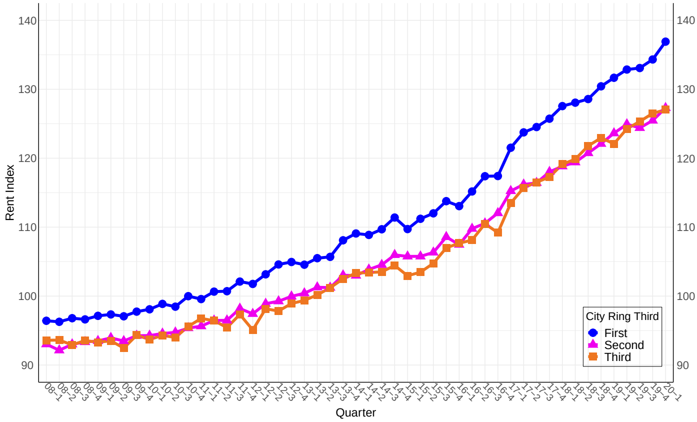


Fig. 4.3: Apartment Rent Growth in City Ring Thirds

Note: This figure plots the means in apartment rents across cities in the first third (blue), the second third (magenta) and the third third (orange) over time. In the overall sample, there are no striking changes in relative rents over the period under consideration. Data: Author's calculations.

Coefficients γ_j, θ^t and λ_j^t are of particular interest, as by correctly evaluating them, rent indexes for every ring j at time t, P_j^t, can be obtained. The omitted dummy variables of RING and TIME are reference categories. These omitted dummies are the most central ring $j = 1$ and the first quarter in the data sample $t = 0 = 2008$ Q1, respectively. In a log-specification, coefficients γ_j, θ^t and λ_j^t express the log-change to the reference, so one obtains index values by simply exponentiating the relevant estimated coefficients and multiplying by 100. The index value P_j^t for the reference, P_1^0, therefore is $\exp(0) \cdot 100 = 100$, while the index values of $j = 1$ for all the other points in time are obtained by evaluating θ^t as $\exp(\theta^t) \cdot 100$. Index values for $j \neq 1$ in t $= 0$ are, in a similar vein, calculated by $\exp(\gamma_j) \cdot 100$. Calculation of index values for $j \neq 1$ in t $\neq 0$ needs to consider the "city ring fixed effect" γ_j, the "time fixed effect" θ^t as well as the time-specific development of city rings, coefficient λ_j^t. The index values P_j^t for city ring $j = 2, \ldots,$ J at time $t \neq 0$ are therefore calculated as $\left[\exp\left(\gamma_j + \theta^t + \lambda_j^t\right)\right] \times 100$.[16]

Initially, to make cities' different spatial extents more comparable, I aggregate every city's set of rings into consecutive subsets of thirds. I equate the

[16] I also correct for a bias in P_j^t arising from the nonlinear transformation of a random variable, see appendix for details on index calculation.

1st third of rings with central city rental apartments while the 2nd and 3rd thirds denote peripheral apartments. Figure 4.3 shows the average time development of rent indexes for all three city thirds. Rental apartments in the city center (red) are the most expensive over the entire period, while apartments in the 2nd (green) and 3rd thirds (blue) are hardly cheaper. The figure also reveals that the anecdotally strong rent increases in German cities over the last ten years are not exclusively driven by rents spiking in central locations, although they seem to have enlarged their rent premium further.[17] Take as a brief numeric comparison two points in time: The first quarter in the data, 2008 Q1, and the last, 2020 Q1. Rent index differential between the 1st (central) third of rings and the 3rd (peripheral) third increased from 2.38 to 10.36. The cities' central rent growth over the period of these twelve years greatly outpaced that of the periphery. Since judging from a graph always contains imponderables, it is unwise to infer subsidy introduction effects directly from the average rent development in Figure 4.3. Here, effects are masked by averaging in the presence of great heterogeneity across cities in factors that affect housing prices, e.g. the centralization of amenities, the spatial distribution of employment, transportation infrastructure or natural features and building regulations that affect housing supply.[18]

Figure 4.4 returns to ring rent indexes and visualizes them as the "spatial intra-city rent structure" for three quarters in the data (in columns), the first two (2009 Q4 and 2014 Q4) predating subsidy introduction while the last (2019 Q4) was substantially after subsidy introduction in 2008 Q3. Munich, Stuttgart, and Heidelberg (first three rows) belong to the least affordable decile of cities, while the bottom three cities (Rostock, Weimar, and Cottbus) are among the most affordable decile of cities in the sample. The postulated differing central rent premium development by city affordability is almost visible to the naked eye: While expensive cities' apartments in close distance the CBD increased their rent advantage over their more peripheral peers pretty much unperturbed, affordable cities' centers saw their lead over the periphery dampened with subsidy introduction. In the following section, I examine this central hypothesis of the paper: the subsidy *dampened* the growth of the central rent premium, i.e. the rent differential between central and peripheral city parts, in those cities in which it could be used most effectively.

[17] A possible explanation for the rise in peripheral rents is that rent controls were introduced in many German cities around 2015. As a result of limiting the increase in existing rents (in the city center), one can expect rents for new construction (on the outskirts) to rise more sharply.

[18] And this is why the following regression analyses consequently include city fixed effects.

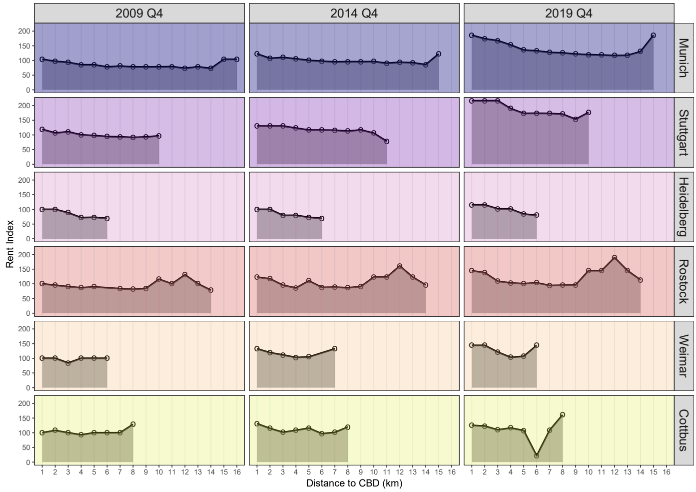


Fig. 4.4: Intra-city rent structure for three points in time

Note: This figure has the intra-city rent structure for six cities (rows) and three points in time (columns). The top three rows show three of the most expensive cities in the sample, while the bottom three rows show three of the most affordable cities. The central rent premium grew in all six cities, going from 2009 (first column) to 2014 (second column). But going from 2014 to 2019 (third column), enveloping subsidy introduction, affordable cities' central rent premiums did not grow as strongly as their expensive cities' peers. Data: Author's calculations.

4.4 Empirical Framework

4.4.1 Identification

To isolate causal effects of introducing BK on intra city rent structure, I set up a suitable regression model to study the subsidy's effects by exploiting variation in intra-city locations and inter-city variation in real estate affordability, and thus conditional on the true effectiveness of BK. Cities with high real estate price levels can be expected to be less affected by subsidy introduction, as argued before. In cities where the subsidy encourages the switch in tenure from rent to owner-occupancy, one expects population to live less centrally. Corresponding price effects should therefore lower central rents in affordable cities more sharply than in expensive cities. This link enables the use of a Triple Difference style approach, an extension to double differences first introduced to the economic literature by Gruber (1994), in which differences in treatment intensity (prior-introduction real estate price levels and spatial intra-city ring location) across cities give the cross-sectional variation needed to identify the effects of the treatment.

In any DD(D)-framework, the coefficient of the interaction term has a causal interpretation under three assumptions: First, the assumption that no other policy interventions or events coincided with the intervention and affected treatment and control groups unequally. Second, the assumption that there are no spillover effects between treatment and control groups. And third and most important, the common trend assumption, which states that in the absence of treatment, outcomes in treatment and control groups would have developed similarly (Lechner 2011).

As a first difference, I compare ring rents in the time period before the subsidy was introduced with the time period after. Although an introduction of a homeownership subsidy for families was already discussed during the federal election campaign before the October 2017 election, I consider anticipation effects unlikely. It was not until July 2018 that government announced the introduction of the Baukindergeld in its present form, and unexpectedly also retroactively for owner-occupied homes that were bought or granted a building permit from January 1, 2018. So before July 2018, it was not clear from which date purchases would be subsidized by the Baukindergeld, which marks the third quarter of 2018 as the start of the unanticipated intervention.

As argued, rental housing is predominantly found near the city center, so comparing central rents (i.e. rental housing with high treatment intensity) with peripheral rents (i.e. rental housing with lower treatment intensity) gives an opportunity to control for all other factors that affect the rental housing market[19] As a second difference, I thus compare central rings' rents with rents of peripheral rings.

However, the link between building type and tenure is not absolute. Rather, it depends on the relative costs of tenure types, which can be expressed, for example, via the price-to-rent ratio. If the homeownership subsidy increases the demand for owner-occupied housing and the supply does not respond perfectly elastic, prices for owner-occupied housing will also rise, and so will the price-to-rent ratio. Owners of apartments then have an incentive to

[19] "Treated" in this context does not mean that central rental housing is in any way directly targeted with the homeownership subsidy. But still it is treated indirectly, as first-time homeowners are renters that switch from rental housing to owner-occupied housing. The subsidization of their owner-occupied housing units ("directly treated" by the subsidy) does have an immediate effect on rental units ("indirectly treated" by the subsidy).

convert their rental apartments into condominiums. Thus, not only would the subsidy increase the demand for owner-occupied housing in the periphery while simultaneously decreasing the demand for rental housing in the center, but the conversion from rental apartments to condominiums would also decrease the supply of rental housing in the center. It is unclear, which of the effects would predominate and what the net effect would be.

In the case of Germany, however, this channel is very unlikely: provisions in the Civil Code give special protection to current tenants in the event of conversion from rental to owner-occupied apartments. When an occupied rental apartment is converted, the current tenant has a right of first refusal. If the tenant waives his or her right of first refusal, there is nevertheless a blocking period of at least 3 years before the lease can be terminated. In cities with "tight housing markets" (determined by the state government), this blocking period can be further extended from three to up to ten years. In 40 of my 105 cities, such an extension is in force.[20] So for landlords, it does not seem viable to exploit price increases in the owner-occupied market through conversion of rental apartments to condominiums.

I further argue that the introduction of the subsidy only played a role in the decision to switch from renting to owning in affordable cities. The subsidy was based on the number of children in the household and not on local housing price levels. In a more expensive city, the subsidy had too small a share of the total investment amount to really financially incentivize people to change tenure. Thus, the more affordable the city, the greater the (indirect) treatment intensity of the subsidy on apartment rents. In my main specification, I introduce groups of affordable cities' rings and expensive cities' rings to distinguish between the treatment intensity by real estate affordability, but also provide results for a continously varying treatment intenstity by city affordability.

One option with my data would be to estimate a Difference-in-Differences model with the difference in time and the difference in ring location separately for both affordability groups of cities, and to identify the subsidy's effect through the difference in the parameters of the interaction terms. The drawback of this approach is that (i) there is no t-statistic to assess whether differences between affordability groups have statistical significance and (ii) two parallel trend assumptions, one for each group, have to hold. The better suited approach thus is the estimation of a triple difference (TD) model that

[20] In detail, 16 cities have an extended blocking period of five years, 4 prohibit lease determination for eight years while 20 cities have a blocking period of ten years.

accounts for these group differences by including an additional treatment (group) indicator in the regression.

Common trends Olden and Møen (2020) show that the parameter on the triple interaction term is mathematically equivalent to the difference between the two separate group-specific difference-in-difference estimators, but (i) allows for statistical testing of the difference between groups while on the other hand (ii) does not require two parallel trend assumptions to hold in order to have a causal interpretation. The one parallel trend assumption that has to hold is that the relative outcome between treatment and control group in one group trends in the same way as the relative outcome of treatment and control group in the other group, in the absence of treatment. So in my setup, the differential (the "gap") in rents between central and peripheral rings in affordable cities does have to trend similarly to the differential in central and peripheral rents in expensive cities. To test for parallel trends in the pre-treatment period, I conduct a placebo intervention in 2015 Q3 and run separate DD-estimations for each group. Results indicate that central and peripheral rents do indeed not trend the same way when looking on affordable and expensive cities in DD-setups separately. But using this same placebo intervention, results in Table 4.3 show that the coefficient on the triple interaction is not statistically significant, indicating that the TD assumption of common trends in differentials between groups is not rejected.

Coinciding policies My identification strategy is based on the assumption that the introduction of the homeownership subsidy is not correlated with any other events or policies that affect central and peripheral rents differently. A variety of possible influences can be projected out through the third "difference" in city affordability. However, policies that dissimilarly affect central and peripheral rents and in addition do so dissimilarly in affordable and expensive cities may pose a threat to the identification strategy.

One policy measure that meets these conditions is the "Mietpreisbremse" (translated literally "rent brake"), a second-generation-type rent control introduced in some German cities with tight housing markets starting in 2015. The key feature of the "Mietpreisbremse" is that it prohibits rents in tight housing markets from being raised above the rent index rent when apartments are re-letted. There is an exception in the case of re-letting, if the rent of the previous tenant was already above the rent index. In this case,

Tab. 4.3: Testing the parallel trends assumption

Intercept	93.87***
	(1.92)
Center	6.92***
	(1.35)
Placebo	15.59***
	(1.17)
Center × Placebo	3.58***
	(0.68)
Aff × Placebo	−1.40
	(1.44)
Aff × Center × Placebo	−1.06
	(0.88)
Adj. R^2	0.22
Num. obs.	40431
N Clusters	105

OLS regressions with quarterly ring rent index as the response variable. Clustered standard errors (at city level) in parentheses. Placebo is a dummy for a placebo intervention turning 1 (zero), if the quarter is equal or post 2015 Q3. Observations used are from 2008 Q1–2018 Q2.
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

the landlord does not have to reduce the rent in the new tenancy to the level of the rent index, but rather enjoys protection of the status quo. The main exemption from the "Mietpreisbremse" applies for first-time leases of new properties or first-time leases after extensive renovation; in these cases, the brake does not apply and the rent level may be freely negotiated.[21]

While landlords did not have to formally prove that an exception to the "Mietpreisbremse" existed due to a higher rent paid by the previous tenant, this changed in January 2019. From then on, landlords had to inform tenants in writing and unsolicited prior to signing the lease, whether an exception to the "Mietpreisbremse" exists. While the regulations of the rent control itself thus did not change, they became more transparent for tenants and thus easier to enforce. Although I do not suspect that the mere increase in transparency had much impact on asking or agreed rents, it was nevertheless introduced after the introduction of the homeownership subsidy, and thus within the treatment period, possibly leading to biased results. Additionally, the "Mietpreisbremse" itself probably fulfills the requirements of a coinciding policy posing a risk to my identification strategy, as I suspect it to

[21] The effects of rent brakes are well documented in the literature, see for example Glaeser and Luttmer (2003) and Diamond et al. (2019), or specifically for the German case Mense et al. (2017, 2019) or Breidenbach et al. (2019).

be more relevant in expensive cities, and within these cities probably rather binding in central than peripheral locations. As I can not rule out that the new transparency rules of the rent control in 2019 or delayed effects of its introduction in 2015 dissimilarly affect rents in central and peripheral locations in cities of different affordability levels, I will–as a robustness check–exclude all cities from my sample that at any point between 2008–2020 introduced the “Mietpreisbremse” and were thus affected by its regulations and changes in it.

SUTVA In any causal study, the stable unit treatment value assumption (SUTVA) requires that the (i) composition of treatment and control groups is stable over time and that there are no (ii) spillover effects from treatment to control groups (Rubin 1977). Two spillover effects could occur in the specific setup of this paper: In response to the homeownership subsidy, households change their tenure. Since tenure is closely related to location, spillover effects might occur (i) *between* cities, from expensive cities (control) to affordable cities (treatment), and (ii) *within* cities, from central (treatment) areas to peripheral (control) areas. Since moving between cities involves substantial relocation costs, e.g. changing jobs, a severe spillover effect between cities as response to the subsidy seems unlikely.

The link between tenure and residential location this paper advocates consists of households moving from central to peripheral areas; if these peripheral locations households move to are indeed *within* the same city, estimates would be biased. I cannot completely rule out that the SUTVA assumption is violated and thus the possibility that the estimates for the average treatment effect are biased. However, this need not be the case: First, new single-family housing developments often occur just outside the city gates in the urban hinterland. But then, new owners moving from rental housing to owner-occupied housing causing the negative demand shock, do not move into the locations that comprise the control group.[22] And second, my treatment and control groups are *rents* in central and peripheral locations. However, when subsidy recipients become homeowners, they leave the rental market, hence, their move reduces the demand for central rental housing in apartments but does not directly alter the demand for peripheral rental housing in apartments. So although *population* spillover effects

[22] And this is what anecdotal evidence, e.g. for the case of Berlin’s renters moving into owner-occupied homes in counties in Brandenburg, the urban hinterlands outside of Berlin, seems to confirm

between treatment and control locations might occur, there is no (direct) spillover on peripheral rents.

4.4.2 Estimating Equations

The following triple difference model forms the core of my analysis:

$$
\begin{aligned}
E(\mathrm{P}^t_{ij}|\mathrm{x}^t_{ij}) = \alpha + \mu_i + \beta_1\,\mathrm{CENTER}_{ij} + \beta_2\,\mathrm{BK}^t \\
+ \beta_3\,\mathrm{CENTER}_{ij} \times \mathrm{BK}^t + \beta_4\,\mathrm{AFF}_i \times \mathrm{BK}^t + \beta_5\,\mathrm{AFF}_i \times \mathrm{CENTER}_{ij} \\
+ \beta_6\,\mathrm{AFF}_i \times \mathrm{CENTER}_{ij} \times \mathrm{BK}^t\,. \qquad (4.2)
\end{aligned}
$$

Here, the conditional expectation of rent index P in city i, ring j at time t is dependent on all variables on the right side, in short x^t_{ij}. μ_i is a city fixed effect, CENTER is a city center dummy (which is 1 if ring j is located in the first third of city rings), BK is a treatment period dummy (1 if time t is at or after the introduction of BK in 2018 Q3), and AFF is a city affordability dummy turning 1, if ring j belongs to a city that is more affordable than the median city in my sample. Cities are ranked by their average price per square meter of developable land in 2007, and thus predating the time period under observation to avoid endogeneity.[23]

It might be helpful to disentangle effects on city rent structure from Equation 4.2. First, consider the case of all dummy variables set to 0. This leaves us with the intercept α (and city fixed effect μ_i), representing the average rent index of expensive cities' peripheral thirds of rings before the subsidy's introduction.[24] Next, consider dummy CENTER and BK separately set to 1. In the first case, β_1 shows the rent premium in the central third of rings of expensive cities prior to BK while in the second case, β_2 shows the rise in average rent after subsidy introduction but also only in expensive cities'

[23] The construction and material costs of real estate hardly vary between cities. What makes up a large part of real estate prices in many cities and thus determines city affordability, however, is the price of land (see Braun and Lee (2021).

[24] Note that, in this setup with city fixed effects, affordable as well as expensive cities are modeled to have the same peripheral development prior to BK's introduction. I do not make use of a fully saturated model in the classic sense, i.e. a full set of dummy variables and their interactions, as the set of fixed effects of affordable cities is collinear with treatment dummy AFF. However, the influence of AFF is now captured in the city fixed effects and so this DDD-setup with fixed effects neither alters the identification strategy of subsidy introduction nor renders the DD/DDD-coefficients.

peripheries. Switching both dummies to 1, β_3 represents the rent premium increase of expensive cities centers after subsidy introduction.

Now, let only treatment period dummy BK stay 1 and additionally set the affordable cities' dummy AFF to 1. β_4 now shows the rent increase in affordable cities' peripheries in the wake of subsidy introduction, over that of expensive cities. For the last case, let all three dummies, AFF, CENTER and BK be 1. While β_5 represents the extra in rent affordable cities' centers had over their expensive counterparts prior to BK, β_6 shows how this differential changed after BK was introduced. And exactly this coefficient is what identifies the role of the subsidy introduction: The change in affordable cities' central rent premium prior and post treatment, in comparison to all other developments in other city parts and in less-treated (expensive) cities.

As a second model, I also estimate a variation of Equation 4.2 using a continuous variable to measure affordability rather than relying on dummy variable AFF. Let $\overline{\mathrm{PRICE}}$ be the price of the most expensive city in the sample, Munich.[25] Substract now the price of city i from this maximum, such that $\left(\overline{\mathrm{PRICE}} - \mathrm{PRICE}_i\right)$ gives a kind of reversed price rank, with the most expensive city having the lowest value (Munich, 0) and the most affordable city having the highest (Suhl, 950).[26]

The regression equation transforms to

$$\begin{aligned} E(\mathrm{P}_{ij}^t|\mathrm{x}_{ij}^t) = \alpha &+ \mu_i + \beta_1\,\mathrm{CENTER}_{ij} + \beta_2\,\mathrm{BK}^t \\ &+ \beta_3\,\mathrm{CENTER}_{ij} \times \mathrm{BK}^t + \beta_4\,\left(\overline{\mathrm{PRICE}} - \mathrm{PRICE}_i\right) \times \mathrm{BK}^t \\ &+ \beta_5\,\left(\overline{\mathrm{PRICE}} - \mathrm{PRICE}_i\right) \times \mathrm{CENTER}_{ij} \\ &+ \beta_6\,\left(\overline{\mathrm{PRICE}} - \mathrm{PRICE}_i\right) \times \mathrm{CENTER}_{ij} \times \mathrm{BK}^t\,. \end{aligned} \qquad (4.3)$$

Using $\left(\overline{\mathrm{PRICE}} - \mathrm{PRICE}_i\right)$ instead of treatment dummy AFF has the advantage, that the particular definition as affordable or expensive city does not play a role. Rather, results indicate whether the findings are robust throughout the distribution of cities' land price affordability in the sample.

[25] Munich's average price per square meter of land ready for development in 2007 already exceeded €1,000 (2018: 2,638 Euros/sqm).

[26] In the regressions, this price is divided by 1000 for better readability of the coefficients.

Tab. 4.4: Rent Index and Affordability

	All Cities				Only West	No "Bremse"
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	147.09***	161.44***	172.99***	172.62***	178.45***	165.85***
	(0.69)	(3.94)	(4.02)	(3.97)	(3.83)	(3.90)
Center	6.87***	6.83***	6.84***	6.85***	6.53***	0.71
	(1.35)	(1.35)	(1.34)	(1.34)	(1.44)	(2.34)
BK	22.98***	17.61***	13.59***	26.61***	26.59***	28.53***
	(1.45)	(1.45)	(1.34)	(0.93)	(1.03)	(1.12)
Center × BK	5.87***	5.81***	4.89***	4.67***	4.23***	3.42***
	(0.93)	(0.97)	(0.81)	(1.00)	(0.98)	(0.85)
Aff × BK	−2.88	−4.82**	−4.58**			
	(1.85)	(1.87)	(1.85)			
Aff × Center	−4.78***	−4.77***	−5.19***	−5.16***	−5.14***	−0.42
	(1.79)	(1.81)	(1.82)	(1.81)	(1.93)	(2.59)
Aff × Center × BK	−4.79***	−4.66***	−3.50***	−4.38***	−4.51***	−2.93**
	(1.18)	(1.22)	(1.29)	(1.45)	(1.66)	(1.34)
Log(New Houses)		−6.66***	−5.53***	−5.58***	−5.10***	−4.38***
		(1.05)	(0.85)	(0.85)	(0.91)	(0.93)
Log(New Apartments)		9.03***	6.44***	6.53***	6.32***	5.18***
		(0.83)	(0.65)	(0.65)	(0.74)	(0.69)
Vacancy Rate			−721.06***	−737.25***	−1115.60***	−503.64***
			(149.26)	(142.96)	(168.22)	(85.74)
City FE	Yes	Yes	Yes	Yes	Yes	Yes
City FE × BK	No	No	No	Yes	Yes	Yes
Adj. R^2	0.45	0.50	0.48	0.49	0.52	0.41
Num. obs.	47030	45289	40915	40915	33786	21610
N Clusters	105	105	103	103	86	61

OLS regressions with quarterly ring rent index as the response variable. Clustered standard errors (at city level) in parentheses.
Col (5) excludes all cities on territory of former GDR, Col (6) excludes all cities that (at any point in time) introduced a rent control.
Data on building completions is from the federal and state statistical offices, data on the vacancy rate from empirica AG.
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

4.5 Results

Table 4.4 has the OLS estimates of Equation 4.2 for all cities in the sample (column (1)–(4)), while column (5) excludes all cities located on territory of former German Democratic Republic to rule out the possibility that east German cities and their special rental market circumstances influence results.[27] Column (6) finally excludes all cities from the sample in which, at any time in the period of observation 2008–2020, the "Mietpreisbremse" applied. The estimated coefficient of interest, showing the effects of the subsidy introduction on central rent premium, $\widehat{\beta_6}$, equals -4.79 in column (1) and is statistically highly significant. Gradually adding additional controls for the number of house and apartment building completions and the vacancy rate (col (2) and (3)) does not alter this DDD-coefficient significantly. Even in models in columns (4)–(6), where I include City × BK fixed effects to control for unobserved effects in the treatment period, the estimated coefficient of β_6 retains its sign and high statistical significance. While central rent premiums in both, expensive and affordable cities' centers, continue to rise after the introduction of BK, it is the affordable cities premium that

[27] During the 2000's a lot of East Germany's (rental) housing was demolished, see for example Dascher (2014), Deilmann et al. (2009)) or Radzimski (2016).

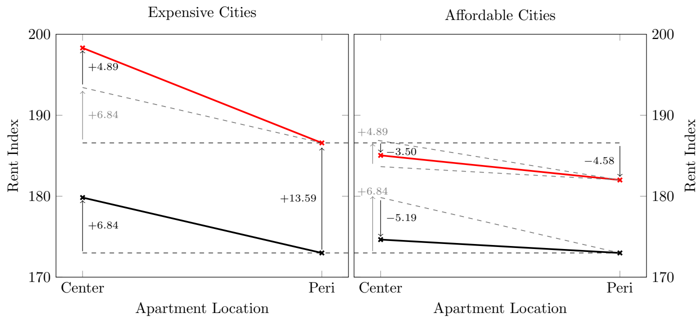


Fig. 4.5: Estimated Effect of Homeowner Subsidy's Introduction

Note: This figure shows "rent gradients" for expensive cities (left panel) and affordable cities (right panel) using estimated coefficients from Table 4.4. Black solid lines depict these gradients pre-treatment while red solid lines show them post-treatment. While in both city groups, central rents have risen, affordable cities' central rents did so by 3.5 index points less. Data: Author's calculations.

does so *less* sharply. The homeownership subsidy thus dampens central rent surge in those cities where it can be effectively used - the affordable ones.

Figure 4.5 graphically depicts the unequal effects of the subsidy introduction on expensive and affordable cities, using estimated coefficients from col (3). The black line in both panels depicts city rent structure before introduction of BK, while the red line shows this structure post subsidy introduction. Expensive cities (left panel) central rents were 6.84 index points ($\widehat{\beta_1}$) higher than its peripheral rents, pre subsidy introduction. In affordable cities (right panel), this pre-subsidy central rent premium accounted for only 1.65 index points ($\widehat{\beta_1} + \widehat{\beta_5}$). With subsidy introduction, expensive cities peripheral rental housing experienced a rent increase of 13.59 index points ($\widehat{\beta_2}$) while this increase in affordable cities amounts to 4.58 index points ($\widehat{\beta_4}$) less and thus to 9.01 index points in total. I now focus on apartment rents in central locations: With subsidy introduction, central rent premium on average grows in both, expensive and affordable cities. But it does so by 4.89 index points ($\widehat{\beta_3}$) in expensive cities and only by 1.39 index points in affordable ones, with the difference represented by $\widehat{\beta_6}$ equal to -3.50. This is what can be identified as, and equated with, the dampening effect of subsidy introduction.

I will also briefly comment on the estimated coefficients of the additional controls, the log number of "home completions" i.e. the number of newly built apartments and one- and two-family houses, and the vacancy rate. All are statistically highly significant and imply that (i) a one log-point increase in the number of new apartments results in an increase of ring rent index of 5.18–9.03 index points, (ii) a one log-point increase in the number of new houses results in a ring rent index decrease of 4.38–6.66 index points, and (iii) a one percentage point increase of a citys vacancy rate leads to a ring rent index decrease of 5.04–11.16 index points. The negative estimated coefficient on the vacancy rate matches the standard economic reasoning that—holding demand fixed—higher supply of a good (more vacant apartments to rent out) should ceteris paribus result in lower prices of that good, i.e. rents. Also, according to the models in Table 4.4, building more detached and semi-detached houses (i.e having more owner-occupiers and fewer renters) results in a decrease of apartments rents, and this smoothly coincides with the mechanism of tenure choice and housing segment prices postulated in the introduction. Finally, the positive estimated coefficient on the number of new apartments may mirror the fact that newly built apartments are generally more expensive than older ones and developers are—taking the anecdotally present shortage of rental housing in many German cities as granted—able to capitalize that premium in rents.[28]

Besides that, attention should also be paid to the results in column (6) for cities where the "Mietpreisbremse" did not apply. No central rent premium (for the ten year period before BK) is found here, neither in expensive, nor in affordable cities (as $\widehat{\beta_1}$ and $\widehat{\beta_4}$ are not statistical significant from zero). This might at least be suggestive evidence that these cities also did not have the kind of "overheated" rental market with which politicians usually justify introducing rent controls.

Table 4.5 has the OLS results for equation Equation 4.3, again gradually introducing controls for building completions and the vacancy rate (col. (2) and (3)). Columns (4), (5) and (6) additionally include City $\times$ BK fixed effects using the whole sample of cities, only west German cities, and only cities without the "Mietpreisbremse", respectively. Again, equate the estimated coefficient of $\left(\overline{\text{PRICE}} - \text{PRICE}\right) \times \text{CENTER} \times \text{BK}$ with the subsidys effect on central rent premium, while the estimated coefficient of $\text{CENTER} \times \text{BK}$ represents the changes for the most expensive city (Mu-

[28] Of course, I am aware of the potential problems with including these control variables: On the one hand, omitting important variables from the regression leads to omitted variable bias. On the other hand, these control variables are likely to be endogenous and thus "bad controls" that should be (i) left out of the regression or (ii) instrumented. Note that an IV-approach is not feasible here, as I would need at least three time-varying instruments. I decided to describe these (probably) biased estimated coefficients anyway because my results regarding the causal effects of the subsidy introduction on rents do not depend on their inclusion in the regression (see col (1)).

Tab. 4.5: Rent Index and Landprice

	All Cities				Only West	No "Bremse"
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	146.92***	160.88***	172.75***	172.34***	178.26***	165.80***
	(0.56)	(3.71)	(3.97)	(3.94)	(3.85)	(3.81)
Center	21.69**	21.75**	22.31**	22.23**	21.84**	2.87
	(6.02)	(6.10)	(6.34)	(6.30)	(6.87)	(17.78)
BK	38.37***	36.89***	30.88***	26.33***	26.45***	28.09***
	(5.58)	(5.57)	(4.83)	(0.80)	(0.82)	(1.08)
Center × BK	20.55***	19.64***	15.33**	17.21**	17.78**	11.20
	(3.75)	(3.74)	(4.14)	(5.10)	(5.72)	(8.31)
($\overline{\text{Price}}$ - Price) × BK	−21.77**	−28.08***	−25.31***			
	(6.81)	(6.94)	(5.98)			
($\overline{\text{Price}}$ - Price) × Center	−22.15**	−22.28**	−23.20**	−23.05**	−23.20**	−2.86
	(7.43)	(7.54)	(7.85)	(7.80)	(8.63)	(20.55)
($\overline{\text{Price}}$ - Price) × Center × BK	−21.45***	−20.34***	−15.23**	−18.93**	−20.52**	−11.44
	(4.53)	(4.54)	(5.20)	(6.38)	(7.38)	(10.11)
Log(New Houses)		−6.60***	−5.55***	−5.58***	−5.10***	−4.38***
		(0.99)	(0.83)	(0.85)	(0.91)	(0.93)
Log(New Apartments)		9.16***	6.45***	6.54***	6.33***	5.18***
		(0.82)	(0.65)	(0.65)	(0.74)	(0.69)
Vacancy Rate			−721.28***	−737.17***	−1114.82***	−503.70***
			(147.86)	(142.86)	(168.20)	(85.89)
City FE	Yes	Yes	Yes	Yes	Yes	Yes
City FE × BK	No	No	No	Yes	Yes	Yes
Adj. R^2	0.46	0.51	0.49	0.50	0.53	0.41
Num. obs.	47030	45289	40915	40915	33786	21610
N Clusters	105	105	103	103	86	61

OLS regressions with quarterly ring rent index as the response variable. Clustered standard errors (at city level) in parentheses.
Col (5) excludes all cities on territory of former GDR, Col (6) excludes all cities that (at any point in time) introduced a rent control.
Data on building completions is from the federal and state statistical offices, data on the vacancy rate from empirica AG.
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

nich), where subsidy introduction has had least effect and may thus be equated with all other (unobservable) trends on that premium. $\widehat{\beta_6}$ carries the expected (negative) sign and is statistically highly significant, which means that the more affordable the city (the more sensitive to subsidy introduction), the weaker its central rent premium surge. To ease interpretation, consider now two cities with a difference in 2007 land prices of € 100 per square meter. The more affordable of the two cities, according to column (1), would find its central rent premium rise by 2.15 index points *less* post subsidy introduction.

These results also have economic significance. Consider two specific cities, Munich (the most expensive city in the sample) and Regensburg (falling € 675 short of Munich's price) and, to ease interpretation, results without the additional controls (i.e. col (1)). For Munich, $\left(\overline{\text{PRICE}} - \text{PRICE}\right)$ equals 0. Rent development of Munichs rings can thus be directly inferred from the estimated coefficients $\widehat{\beta_2}$ and $\widehat{\beta_3}$. The average rent of Munichs baseline ($P_1^0 = 100$) is € 1072 and its estimated city fixed effect $\widehat{\mu_M}$ equals −61.23. Predicted average rent of apartments in the second and last third of rings pre-subsidy introduction is thus € 919 ($\widehat{\alpha} + \widehat{\mu_M}$) while predicted central rents average € 1151 ($\widehat{\alpha} + \widehat{\mu_M} + \widehat{\beta_1}$). The difference between both rents equals the predicted central rent premium before BK, € 233. Post subsidy introduction, Munichs peripheral apartments experienced a rent increase of

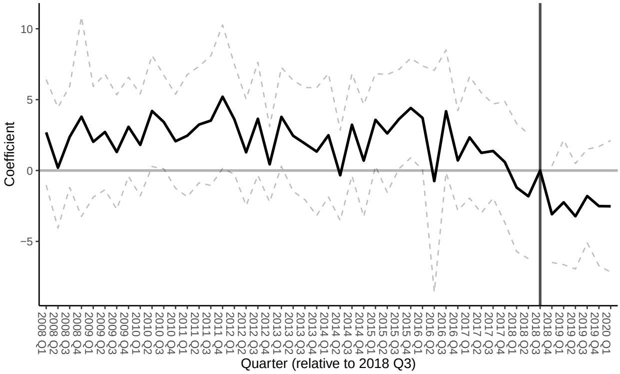
38.37 ($\widehat{\beta_2}$) index points while its central rings saw an even larger increase of 58.92 ($\widehat{\beta_2} + \widehat{\beta_3}$) index points. This translates into a predicted average peripheral rent post subsidy introduction of € 1330 while the average central apartment rents out for € 1783. Central rent premium has increased to € 453, which means, in absolute terms, it almost doubled in the wake of subsidy introduction.

Regensburgs average baseline rent equals € 684 and its corresponding regression city fixed effect $\widehat{\mu_R}$ equals -63.53. Predicted peripheral and central rent equals € 570 and € 616 respectively, so central rent premium is just 6.7 ($\widehat{\beta_1} + (0.675 \cdot \widehat{\beta_5})$) index points or € 46. Peripheral rents are predicted to rise with subsidy introduction by 23.67 index points ($\widehat{\beta_2} + (0.675 \cdot \widehat{\beta_4})$), so the average peripheral apartment rents out for € 732. Lastly, the predicted central rent after subsidy introduction equals € 820.[29] Predicted central rent premium after subsidy introduction hence equals 12.8 index points or € 87. In summary, both cities central rent premiums have risen with subsidy introduction, but Munichs did so by 20.6 index points while Regensburgs rose only by 6.1. The difference of 14.5 index points between the two can be attributed to the BK drawing residents out of Regensburgs rental apartments in the city center into owner-occupancy.

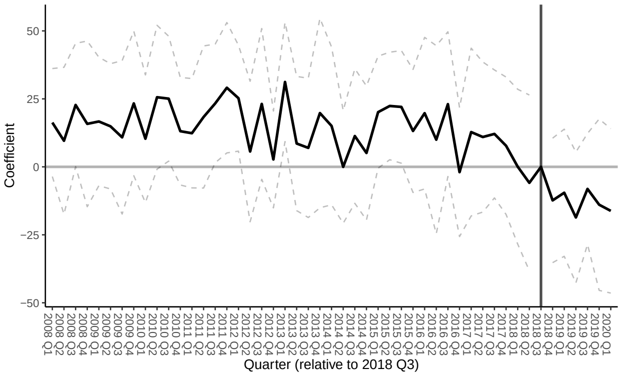
To show the dynamics of the dampening effect on (more) affordable cities' central rent premiums, I re-estimate Equation 4.2 and Equation 4.3 in an "event study-style" by replacing the treatment dummy BK with a set of dummies, one for each quarter. Figure 4.6 shows the estimated coefficient on the triple difference parameters $\text{AFF} \times \text{CENTER} \times \text{QUARTER}$ and $\left(\overline{\text{PRICE}} - \text{PRICE}\right) \times \text{CENTER} \times \text{QUARTER}$ as black solid lines along with 95% confidence intervals. Clearly, there is a longer-term effect of subsidy introduction, rather than just a short term effect of a few quarters, and it is only after subsidy introduction that there is a reversal in the coefficients' signs. Only after subsidy introduction, (more) affordable cities' central rent premium exhibited a permanent markdown. Unfortunately, I do not observe a longer post-treatment period in my data, but I suspect that the longer-term effect would be even more pronounced, i.e. negative, as more and more owner-occupiers that built new housing rather than moving into existing housing move out of their rental apartments.

Several robustness checks are reported in the appendix. My results are robust against (i) using another method of index construction (a "double im-

[29] Obtained by evaluating the full set of estimated coefficients: $\widehat{\alpha} + \widehat{\mu_R} + \widehat{\beta_1} + \widehat{\beta_2} + \widehat{\beta_3} + (0.675 \cdot \widehat{\beta_4}) + (0.675 \cdot \widehat{\beta_5}) + (0.675 \cdot \widehat{\beta_6}))$



(a) Estimated event-study style coefficients on $\mathrm{AFF} \times \mathrm{CENTER} \times \mathrm{QUARTER}$



(b) Estimated event-study style coefficients on $\left(\overline{\mathrm{PRICE}} - \mathrm{PRICE}\right) \times \mathrm{CENTER} \times \mathrm{QUARTER}$

Fig. 4.6: Dynamics of (more) affordable cities' central rent premium markdown

Note: This figure plots estimated coefficients along with 95 % confidence intervals of the estimated coefficients on the TD-parameters $\mathrm{AFF} \times \mathrm{CENTER} \times \mathrm{QUARTER}$ (Figure 4.6a) and $\left(\overline{\mathrm{PRICE}} - \mathrm{PRICE}\right) \times \mathrm{CENTER} \times \mathrm{QUARTER}$ (Figure 4.6b) from (adapted) Equation 4.2 and Equation 4.3. Standard errors are clustered at the city level.

putation" index), (ii) the grouping of cities as "affordable" (demonstrated by stable results in the continuous treatment) and (iii) other definitions of the the city center. These checks conclusively strengthen the validity of this papers identification strategy.

4.6 Conclusions

This paper empirically analyzed the effects of a new homeownership subsidy on rents within cities. Its main finding is that subsidy introduction induced falling central city rent premiums, likely caused by population sorting from rental into owner-occupied housing. With this analysis, I offer a complementary building block for analyzing housing subsidies' effects on housing prices. While other authors solely focus on subsidies for a certain tenure type on prices of the same tenure type, this paper complements how subsidies to *homeownership* influence *rents*.

Using triple difference frameworks, I suggest that subsidy introduction had a dampening effect on affordable cities' central rent premium growth. In a setup with a continuously varying treatment intensity by cities' land price, I show that this dampening effect is prevalent throughout the distribution of city affordability, measured by cities' prices of building land.

These results have important policy implications. Subsidies to homeownership are usually justified on the grounds of making the move to owner-occupancy more affordable. The literature, however, suggests that they often fail to achieve this goal. Subsidies get capitalized in house prices, benefiting the selling developer rather than the prospective owner-occupant. Interestingly and novel, this paper reveals that renters benefit most, as the homeownership subsidy's introduction dampens growth in central rents.[30]

Germany is in many regards rather unique in terms of its housing sector: It has a rather low homeownership rate and, in comparison to high ownership countries like the United States, an extensive social housing sector, high transfer taxes when buying real estate, and no tax deductions for mortgage interest payments for owner-occupiers (Kaas et al. 2021). And Germany also had a rather unique program setup for promoting homeownership: the subsidy was based on the number of children in the household and not, e.g.,

[30] I leave aside in this exaggeration many other aspects the subsidy might cause, e.g., a higher tax burden, a reduction in government spending elsewhere, or that in areas of inelastic supply, part of the subsidy will lead to higher prices of existing properties, which might very well increase the wealth of existing owners.

on the home loan amount, as it is the case with the mortgage interest deduction in the U.S. The MID in many cases does not lead the marginal resident to form homeownership. Rather, future homeowners use the savings from the tax breaks to increase the intensive margin, i.e. consume more living space (Glaeser and Shapiro 2003; Hanson 2012; Hilber and Turner 2014). This paper however is silent on the direct effects of lump-sum type homeownership subsidies on the extensive or intensive margins. It may very well be that different types of homeownership subsidies have different effects. But then also the indirect effects on the rental market carved out by this paper might be ambiguous in other settings with nominally varying subsidy rates, and one should exercise caution in generalizing the findings to other countries and subsidy programs.

This is a paper of eminent importance as, in the particular case of Germany, the subsidy was set to expire after a three years term and thus within current government's legislative period. This fall, after the elections, a new government must decide about its stance on housing policies. This contribution offers advice in the sense that it carved out the subsidy's dampening effect on central rent premium growth. It is likely that terminating the subsidy also means terminating this central city rent relief. Government must be clear that, when deciding to not reviving the program, it will likely hit both types of housing tenure: homeowners *and* renters.

Conclusion

5

This dissertation offers three novel contributions to the literature on homeownership subsidies. First, the repeal of homeownership subsidies stops urban suburbanization and leads to a "renaissance of downtown living." Second, from a regional perspective, repealing these subsidies puts an end to residents' migration to rural areas and facilitates living in cities. And third, homeownership subsidies and the move of new homeowners away from city centers lead to a decline in rental housing demand and thus to an affordability effect on the rental market.

But, of course, this literature could be enhanced even further: While I have examined the spatial distribution of population (chapters 2 and 3) and the spatial distribution of rents (chapter 4) separately, a worthwhile contribution could be to bring the two together. If both spatial population distribution and spatial rent distribution data were available for the same time period, one could, for example, attempt to explore causal relationships and interactions using simultaneous equation models (SEM).

Further, more accurate studies of the relationship between homeownership subsidies and suburbanization could be conducted if the exact geo-coordinates of subsidized owner-occupied housing were available. And lastly, another contribution to the literature could be to look at these German direct subsidies through the lens of the "classic" literature on policy evaluation: As is already well documented for preferential tax treatments of owner-occupied housing, do cash subsidies also tend to increase the intensive rather than the extensive margin of homeownership? And what are the effects on the spatial distribution of house prices? It seems that there are still many avenues to advance the understanding of the effects of homeownership subsidies.

But now what is to be said about homeownership in general and the related government subsidies in particular? Is there a “horrible housing blunder”? As economists, we can also approach an answer through an analysis of opportunity costs. How much taxpayers’ money could have been used elsewhere if there had been no homeowner subsidies?

Describing a few examples might help capture the sheer magnitude of governments’ subsidies to homeownership: With the possibility to claim mortgage interest as a tax deduction (MID), the United States of America offers some of the greatest tax incentives to become a homeowner. For 2017, the Joint Committee on Taxation (JCT) estimated lost tax revenue from the MID at $66.4 billion. Although changes in the tax code made the MID less attractive, the lost tax revenue burden for 2020 is still estimated at $30.2 billion while only about 14 million claimants are subsidized (JCT 2019; Keightley 2020).

The Economist magazine summed up all U.S. subsidies to homeowners and reported that the government forgoes a staggering amount of over $200 billion a year with policies such as deducting mortgage interest, excluding capital gains from the sale of a residence, or not taxing the income homeowners implicitly earn by avoiding rental payments (“End of an Era” 2020).

The design of the MID is certainly not identical to the German cases discussed in this dissertation: while in Germany, in fact, only relatively low-income households are subsidized, in the United States every household can deduct mortgage interest on loans up to $1 million. However, the real value of the deduction increases with income because, first, marginal tax rates rise with income under progressive taxation, and second, higher earners buy more expensive homes, which leads to higher mortgage interest payments and thus larger deductions. This fact is also reflected in the distribution of MID benefits by income class: Claimants with annual incomes higher than $100’000 accounted for nearly 80% of all claimants in 2018, and more than 90% of foregone taxes by the MID benefited them (JCT 2019). In that sense, claiming that the MID has almost exclusively helped the wealthy to build (larger) houses, as Hilber and Turner (2014) do, is not unfounded.

The German “Eigenheimzulage” (1996–2005), which was analyzed in chapter 2 and chapter 3, was granted in almost 4.5 million cases.[1] According to the Federal Ministry of Finance, the final subsidy payments were made in

[1] In just over 2 million cases, new construction was subsidized, which–according to the analysis in this dissertation–likely also subsidized suburban living.

2017. The total cost over the life of the program was more than €106 billion, of which the basic subsidy accounted for nearly two thirds and the additional child allowance for the rest. To put that sum in perspective: While in 2002 the federal and state governments paid a total of €4.5 billion in rental housing assistance (*Wohngeld*), subsidies to homeownership through the "Eigenheimzulage" totaled to more than double that amount (€9.2 billion).[2]

And although the 2018 reintroduction of subsidies to homeownership through the "Baukindergeld" restricted the group of recipients by imposing stricter selection criteria and, moreover, only subsidized owner-occupied housing aquired over the limited period of three years, a total of 350,000 owner-occupied homes will be subsidized with 7.3 billion euros over the next ten years.[3]

So even if the societal benefits from homeownership mentioned in the introduction truly exist, and thus might justify its subsidization, we still cannot know whether the benefits outweigh the disadvantages, such as the indirect subsidization of urban suburbanization. And additionally, subsidies to homeownership come with high opportunity costs, as the examples of the two German subsidy programs and the mortgage interest deductibility in the U.S. show.

Since the expiration of the "Baukindergeld", Germany currently has no subsidies to homeownership on the scale of the last programs. But by fall 2021, after the elections to the German Bundestag, a new federal government will have to decide whether or not to incentivize homeownership with taxpayer money. Especially in view of the negative environmental consequences of suburbanization, which likely will also be an indirect effect of any new subsidies, this decision should be weighed carefully.

[2] These figures are from the subsidy reports of the federal government (*16.–26. Subventionsbericht der Bundesregierung)*.

[3] These figures and also the regional breakdown of cases by county can be found in the KfW Bankengruppe 2017–2021 promotional reports (*Förderreport KfW Bankengruppe*).

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Appendix Chapter 2

A

A.1 Filtering and Subsidy Repeal

Utility is $\theta s_i + x_i$, where θ denotes taste for housing quality, s_i indexes housing segment i's quality and x_i is the numeraire. Taste θ is distributed according to cdf F, on support $[a, b]$ with $a < b$. Rent in segment i is $q_i - \sigma_i$, where σ_i is the subsidy that may apply to i. Hence utility becomes $\theta s_i + w - (q_i - \sigma_i)$ when residing in segment i. There are n city residents altogether (where now n is set to 1, for simplicity). Each household picks the quality that suits it best. We identify the threshold tastes $\underline{\theta}$ and $\overline{\theta}$ – owners of which are indifferent between segments 1 and 2, and between 2 and 3, respectively – as

$$\underline{\theta}(q_1, q_2 - \sigma_2) = (q_2 - \sigma_2 - q_1)/(s_2 - s_1) \quad \text{(A.1)}$$
$$\overline{\theta}(q_2 - \sigma_2, q_3 - \sigma_3) = (\overline{q}_3 - q_2 - (\sigma_3 - \sigma_2))/(s_3 - s_2). \quad \text{(A.2)}$$

We let $\underline{\theta}_1$ denote the derivative of $\underline{\theta}$ with respect to q_1, $\underline{\theta}_2$ the derivative of $\underline{\theta}$ with respect to $q_2 - \sigma_2$, and so on. We note that $\overline{\theta}_2 = -\overline{\theta}_3$.

In an interior equilibrium, households with tastes in $[a, \underline{\theta}]$ sort into rental housing, those (with tastes) in $(\underline{\theta}, \overline{\theta}]$ sort into existing homes, and those in $(\overline{\theta}, b]$ opt for a new home. Individual choices translate into aggregate housing demands, equal to $n_1 = F(\underline{\theta})$, $n_2 = (F(\overline{\theta}) - F(\underline{\theta}))$ and $n_3 =$

$(1 - F(\overline{\theta}))$. Let n_{ij} denote the derivative of aggregate housing demand for housing quality i with respect to price j. The following properties apply:

$$n_{11} = f(\underline{\theta})\,\underline{\theta}_1 < 0 \quad \text{and} \quad n_{12} = f(\underline{\theta})\,\underline{\theta}_2 > 0 \tag{A.3}$$

$$n_{21} = -f(\underline{\theta})\,\underline{\theta}_1 > 0 \quad \text{and} \quad n_{22} = \left(f(\overline{\theta})\overline{\theta}_2 - f(\underline{\theta})\,\underline{\theta}_2\right) < 0, \tag{A.4}$$

$$\text{and} \quad n_{23} = -f(\overline{\theta})\,\overline{\theta}_3 > 0, \tag{A.5}$$

to the extent that $n_{11} + n_{21} = 0 > n_{12} + n_{22}$.

New homes are supplied outside the city center, in the periphery, only.[1] Space constraints have much less of a role in the periphery, and so we will take the liberty to assume that new homes are supplied perfectly elastically at constant marginal cost $\overline{q}_3$. In this segment suppliers satisfy any demand at price $\overline{q}_3$. The cum-subsidy (i.e. consumer) price becomes $\overline{q}_3 - \sigma_3$. We set out the equilibrium conditions for the inter-connected segments of apartments and existing homes as follows.

$$\begin{aligned} n_1(q_1, q_2 - \sigma_2) &= s_1(q_1) \\ n_2(q_1, q_2 - \sigma_2, \overline{q}_3 - \sigma_3) &= s_2(q_2), \end{aligned} \tag{A.6}$$

where s_i is supply in segment i (never at risk of confusion with quality s_i as we suppress the quality index in what follows). For consistency, increases in s_2 (following increases in q_2) come about as existing vacant housing is supplied more; while increases in s_3 (following increases in q_3) we interpret as new construction. Let s_{ii} denote supply i's (strictly positive) derivative with respect to its own price below.

We translate Germany's full EZ-subsidy removal into policy changes $d\sigma_2 = -\sigma_2 < 0$ and $d\sigma_3 = -\sigma_3 < 0$, where $\sigma_2 < \sigma_3$.[2] We are interested in these policy changes' effects on qualities' prices and quantities, and on the distribution of city population across all three qualities. Removing the subsidy for new homes reduces equilibrium demand in that segment. But changes in the neighboring two segments are less obvious. To sort out the

[1] Glaeser (2011) suggests as much, emphasizing the coincidence of owner-occupied housing with peripheral location for the US. Ahlfeldt and Maennig (2015) observe strong positive correlation between a ring's share of owner-occupiers and its distance to the city center for Berlin.

[2] These changes are not "small", and so our emphasis below is on direction, and not so much size, of the endogenous changes implied.

filtering flows involved, totally differentiate the equilibrium, keep in mind $d\overline{q}_3 = 0$, and rearrange to give

$$\begin{pmatrix} n_{11} - s_{11} & n_{12} \\ n_{21} & n_{22} - s_{22} \end{pmatrix} \begin{pmatrix} dq_1 \\ dq_2 \end{pmatrix} = \begin{pmatrix} n_{12}\, d\sigma_2 \\ n_{22}\, d\sigma_2 + n_{23}\, d\sigma_3 \end{pmatrix} \tag{A.7}$$

or $A\, dq = db$ for short. Immediately we see that $|A| = (n_{11} - s_{11})(s_{22} - s_{22}) - n_{21}n_{12}$ is ambiguous in sign, and so with no further assumption nothing can be said about the sign of dq_1.

And then, the coefficient matrix A has three features we have not exploited yet. The first of these is its dominant diagonal, easily verified by summing all elements of a column and exploiting eq. A.3 or A.4. Already we conclude that A's inverse has negative entries only (Sweeney 1974). Two more of A's properties obtain once we rewrite matrix inverse A^{-1} as $G = (g_{ij})_{i,j=1,2}$. For G it must be true that $g_{11} < g_{12}$ as well as $g_{22} < g_{21}$. To these inequalities we refer to as "Sweeney's first and second property" below.[3]

Write the solution to the differentiated system of equilibrium equations as $dq = A^{-1} db$. The price change in segment 1, dq_1, can then be rewritten as

$$\begin{aligned} dq_1 &= g_{11}\, n_{12}\, d\sigma_2 + g_{12}\, n_{22}\, d\sigma_2 + g_{12}\, n_{23}\, d\sigma_3 \\ &= \underbrace{f(\underline{\theta})\, \underline{\theta}_2\, d\sigma_2}_{-} \underbrace{(g_{11} - g_{12})}_{-} + \underbrace{g_{12}\, f(\overline{\theta})\, \overline{\theta}_2}_{+} \underbrace{(d\sigma_2 - d\sigma_3)}_{+} > 0 \end{aligned} \tag{A.8}$$

where the first and last term on the first line of (A.8) are positive, while the second term on that line is negative. And yet we are able, after signing all individual terms on the second line of (A.8), to also sign dq_1 as positive nonetheless.

Replacing n_{12}, n_{22} and n_{23} on the first line of (A.8) by making use of (A.3) through (A.5), exploiting $\overline{\theta}_2 = -\overline{\theta}_3$, and also rearranging translates into the second line of (A.8). Given Sweeney's first property, i.e. $g_{11} < g_{12}$, the first term on the r.h.s. of the second line of (A.8) must be positive. Moreover, given the structure of subsidy phase-out, i.e. $d\sigma_3 < d\sigma_2$, the second term on the r.h.s. of (A.8) is positive also. Thus $0 < dq_1$.

[3] These inequalities are implied by Sweeney's (1974) general "commodity hierarchy"-type preferences (of which ours are a special case). They are easily shown when recalling that $A^{-1}A = I$ and exploiting the two component equations corresponding to the two zero entries of the identity matrix. For example, $g_{21}(n_{11} - s_{11}) + g_{22}n_{21} = 0$ and hence $g_{21}/g_{22} < 1$.

Lifting both of EZ's component subsidies does raise the price of rental housing. (Note how this result hinges on being able to sign $(d\sigma_2 - d\sigma_3)$.) Now, because $s_{11} > 0$, apartment supply must have risen, too, as must have equilibrium rental housing demand. Hence $\underline{\theta}$. Yet $d\underline{\theta} > 0$ in turn implies that $dq_1 < d(q_2 - \sigma_2)$. Recalling $-d\sigma_2 < -d\sigma_3$, we conclude that all three qualities' (consumer) prices have gone up, and that

$$0 < dq_1 < d(q_2 - \sigma_2) < d(\overline{q}_3 - \sigma_3). \quad \text{(A.9)}$$

A.2 Data Description

We use Regional Database Germany, provided by the Statistical Offices of the Federation and Lander, GENESIS-Database by the German Federal Statistical Office, and the INKAR database by the Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR) to obtain the following variables.

Price The variable PRICE is "price per square meter of building land". This is the average of the square meter prices of (undeveloped) building land sales in 1995 and 2000 in 1,000 Euros. The price per square meter of building land in city *i* and year *t* is calculated as the sum of all purchase prices in *i* at *t* divided by the aggregate land area sold in *i* at *t*. We use the average of the years 1995 and 2000 for reasons of data availability, and to mitigate the issue of outliers.

Tab. A.1: Descriptive Statistics

	Variable	N	Mean	SD	Min	Max
EXPENSIVE CITIES	Population	65	321,580	457,869	63,379	3,455,575
	Households with Children	65	40,724	55,042	6,163	351,564
	Price per sqm land in €, 2000)	65	219.98	157.44	68.63	939.47
	Share Female Labor Participation	65	0.464	0.046	0.367	0.570
	Share Employment in Hospitality Sector	65	0.254	0.037	0.150	0.331
	Share Employment in Financial Sector	65	0.193	0.047	0.099	0.350
	Share Employment in Public Sector	65	0.343	0.073	0.161	0.482
AFFORDABLE CITIES	Population	12	133,143	64,440	64,157	247,483
	Households with Children	12	18,719	2,498	15,664	21,251
	Price per sqm land in €, 2000	12	57.77	6.39	46.42	68.54
	Share Female Labor Participation	12	0.518	0.027	0.460	0.545
	Share Employment in Hospitality Sector	12	0.241	0.026	0.194	0.271
	Share Employment in Financial Sector	12	0.206	0.024	0.164	0.233
	Share Employment in Public Sector	12	0.386	0.037	0.325	0.424

Source: POPULATION: Authors' calculations using KOSTAT data. HOUSEHOLDS WITH CHILDREN: Authors' calculations using BBSR data. PRICE & SECTORAL SHARES: Regional Database Germany.

Female Labor The variable FEMALE LABOR is the share of female employees subject to compulsory social insurance in all women of working age. Employees subject to social insurances are manual and non-manual workers and persons in vocational training who are compulsorily insured under statutory pension, health and/or unemployment insurance schemes, i.e. excluding civil servants, self-employed persons, family workers, and marginally employed persons. The female employment rate in city i in year t is calculated as the number of female employees subject to compulsory social insurance at place of residence i at t divided by the number of female residents between ages 15 and 65 in i at t.

Share Hospitality Sector The variable SHARE HOSPITALITY SECTOR is the share of working population working in trade, transport, hospitality, and information & communication industries in city i in year t. This industry includes the following sections: "Sale, maintenance and repair of motor vehicles and motorcycles", "Transport and storage", "Hotels and restaurants", and "Information and Communication".

Share Financial Sector The variable SHARE FINANCIAL SECTOR is the share of working population working in financial, insurance and corporate service, and land and housing industries in city i in year t. This industry includes the following sections: "Financial and insurance activities", "Real estate activities', "Professional, scientific and technical activities", and "Other business activities".

Share Public Sector The variable SHARE PUBLIC SECTOR is the share of working population working in public and other service, education, and health industries in city i in year t. This industry includes the following sections: "Public administration, defense and compulsory social security", "Education', "Health and social work", "Arts, entertainment and recreation", "Other services not elsewhere classified", and "Households with domestic staff".

Tab. A.2: Sample of Cities

No.	City	Years	$\tilde{r}$	Affordable	Price	$\overline{\text{Age}}_{02}$
1	Aachen	2002–2017	15	0	146.6	–
2	Augsburg	2002–2017	13	0	207.4	42.0
3	Bergisch Gladbach	2002–2017 \{2015}	9	0	164.3	–
4	Berlin	2002–2017	26	0	387.5	41.0
5	Bielefeld	2002–2017	12	0	192.8	41.4
6	Bochum	2002–2017	10	0	227.8	42.6
7	Bonn	2002–2016	12	0	202.4	40.7
8	Brandenburg an der Havel	2003–2017 \{2010}	18	1	46.4	43.8
9	Braunschweig	2003–2016	12	0	102.0	42.5
10	Bremen	2003–2017	27	0	122.4	42.4
11	Chemnitz	2002–2017	14	1	54.4	45.0
12	Darmstadt	2004–2017	10	0	319.4	41.4
13	Dortmund	2002–2017	12	0	224.6	42.0
14	Dresden	2002–2017	16	0	81.7	42.5
15	Duisburg	2002–2017	15	0	192.1	42.0
16	Düsseldorf	2002–2017	15	0	311.4	42.4
17	Erfurt	2002–2017	13	1	62.5	41.9
18	Erlangen	2002–2017	9	0	312.3	40.8
19	Essen	2002–2017	14	0	214.1	43.2
20	Frankfurt am Main	2002–2017	15	0	624.8	41.5
21	Freiburg im Breisgau	2002–2016	14	0	262.0	39.5
22	Fürth	2011–2017	7	0	231.5	40.7
23	Gelsenkirchen	2002–2016	14	0	131.0	42.0
24	Gera	2002–2017	11	1	55.5	43.9
25	Göttingen	2002–2017	9	1	59.8	–
26	Hagen	2002–2014	11	0	124.0	42.2
27	Halle (Saale)	2002–2017	9	0	101.3	42.8
28	Hamburg	2002–2017	25	–	–	41.4
29	Hamm	2002–2017 \{2003}	12	0	89.2	40.3
30	Hannover	2002–2017	13	–	–	–
31	Heidelberg	2002–2017	10	0	574.9	40.0
32	Herne	2002–2017	7	0	101.7	42.3
33	Ingolstadt	2002–2017	13	0	249.6	40.4
34	Jena	2002–2017	8	0	80.8	40.9
35	Karlsruhe	2002–2017	12	0	355.4	41.8
36	Kassel	2010–2017	10	–	–	42.2
37	Kiel	2002–2017	12	0	146.9	41.0
38	Koblenz	2002–2017	9	0	119.6	42.7
39	Köln	2002–2017	19	0	319.0	41.0
40	Konstanz	2002–2017	12	0	127.7	–
41	Krefeld	2002–2017	10	0	169.0	41.9
42	Leipzig	2002–2017	14	0	135.4	43.1
43	Lübeck	2002–2017	19	1	65.4	42.8
44	Lüdenscheid	2002–2017 \{2006–2012}	6	–	–	–
45	Ludwigshafen am Rhein	2002–2017	11	0	187.5	41.4
46	Magdeburg	2002–2017	12	0	82.9	43.5
47	Mainz	2002–2017	12	0	320.7	40.6
48	Mannheim	2002–2017	11	0	424.6	41.5
49	Mönchengladbach	2002–2017	12	0	182.9	41.2
50	Mülheim an der Ruhr	2006–2017	9	0	202.2	43.9
51	München	2002–2017	16	0	939.5	41.5
52	Münster	2002–2017	14	0	183.0	39.9
53	Neubrandenburg	2012–2016	14	1	57.1	–
54	Neuss	2002–2016 \{2008}	9	0	148.6	–
55	Nürnberg	2002–2017	15	0	310.6	42.5
56	Oberhausen	2002–2017	12	0	120.8	41.9
57	Offenbach am Main	2002–2017	7	0	541.9	40.3
58	Oldenburg	2013–2017	8	0	77.6	–
59	Pforzheim	2002–2017	8	0	210.9	–
60	Potsdam	2002–2017	15	0	154.1	41.0
61	Recklinghausen	2002–2017 \{2003,2007}	7	0	135.5	–

Sample of Cities (continued)

No.	City	Years	$\bar{r}$	Affordable	Price	$\overline{\text{Age}}_{02}$
62	Regensburg	2002–2017	8	–	–	41.7
63	Remscheid	2002–2017	9	0	116.0	41.5
64	Reutlingen	2002–2017	10	0	184.2	–
65	Rheine	2002–2017 \{2005,2008}	12	1	59.0	–
66	Rostock	2002–2017	12	1	48.0	42.4
67	Saarbrücken	2002–2017	12	–	–	–
68	Salzgitter	2002–2016 \{2005–2009}	20	0	71.8	42.2
69	Schwerin	2002–2017 \{2003}	10	1	68.5	42.4
70	Siegen	2002–2016 \{2010}	10	1	59.9	–
71	Solingen	2002–2017	10	0	160.7	41.8
72	Stuttgart	2002–2017	11	0	545.6	41.4
73	Trier	2002–2017 \{2003,2004}	12	0	105.3	41.1
74	Tübingen	2002–2017	9	0	151.8	–
75	Ulm	2002–2017	12	0	219.8	40.4
76	Villingen-Schwenningen	2003–2017	13	0	71.3	–
77	Weimar	2002–2017	8	0	76.3	41.3
78	Wiesbaden	2002–2017	11	0	441.5	41.7
79	Witten	2002–2017	9	0	132.7	–
80	Wolfsburg	2002–2017	14	0	68.6	42.8
81	Wuppertal	2002–2017	13	0	136.1	42.1
82	Würzburg	2014–2017	10	0	240.9	41.3
83	Zwickau	2002–2016 \{2010}	11	1	56.6	–

Note: For some cities we are lacking information on prices of land (Hamburg, Hannover, Kassel, Lüdenscheid, Regensburg, and Saarbrücken). Source: Population data is from BBSR and KOSTAT, shapefiles to construct city profiles are either openly available online or directly requested from city administrations. Price of land and mean population age data is from Regional Database Germany (www.regionalstatistik.de) and INKAR database (www.inkar.de).

A.3 Robustness

Tab. A.3: Ring Households by Number of Children

	OLS			2SLS		
	1 child	2 children	3(+) children	1 child	2 children	3(+) children
Intercept	8.80***	5.61***	4.58***	6.34***	5.71***	4.42***
	(0.23)	(0.14)	(0.14)	(0.14)	(0.13)	(0.16)
Distance	0.22***	0.28***	0.29***	0.23***	0.28***	0.29***
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Peri × (Distance - $\tilde{r}/3$)	−0.63***	−0.68***	−0.69***	−0.64***	−0.69***	−0.70***
	(0.10)	(0.10)	(0.09)	(0.10)	(0.10)	(0.10)
Peri × Post	0.09	0.09	0.01	0.74	0.55	−0.42
	(0.18)	(0.18)	(0.19)	(1.36)	(1.38)	(1.55)
Peri × Post × Aff	−0.63*	−0.76**	−0.85*	−0.61*	−0.75**	−0.90*
	(0.24)	(0.22)	(0.30)	(0.23)	(0.23)	(0.33)
Peri × Post × Female Labor				−1.32	−0.90	0.93
				(2.91)	(2.95)	(3.32)
City FE	Yes	Yes	Yes	Yes	Yes	Yes
City FE × Post	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.79	0.78	0.77	0.79	0.78	0.77
Num. obs.	6960	6953	6910	6639	6632	6589
N Clusters	46	46	46	44	44	44

Note: OLS (col 1–3) and 2SLS (col 4–6) regressions with the log of ring households with a variable number of children as the response variable. Peri × Post × Female Labor instrumented by sectoral employment shares in the hospitality, financial and public service sector. Clustered standard errors (at city level) in parentheses. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Tab. A.4: Ring Population with continuous treatment

	OLS				2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	9.55***	9.50***	9.36***	9.32***	9.36***	9.32***
	(0.14)	(0.16)	(0.14)	(0.15)	(0.14)	(0.15)
Distance	0.20***	0.21***	0.21***	0.21***	0.21***	0.21***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Peri × (Distance - $\tilde{r}/3$)	−0.64***	−0.64***	−0.63***	−0.63***	−0.63***	−0.63***
	(0.07)	(0.07)	(0.08)	(0.07)	(0.08)	(0.07)
Peri × Post	0.71*	0.93	1.61***	2.00***	1.41*	1.92*
	(0.34)	(0.48)	(0.37)	(0.49)	(0.78)	(1.05)
($\overline{\text{Price}}$ - Price) × Peri × Post	−0.96*	−1.30*	−0.80*	−1.07*	−0.80*	−1.07*
	(0.43)	(0.59)	(0.35)	(0.49)	(0.36)	(0.50)
Peri × Post × Female Labor			−2.14***	−2.59***	−1.72	−2.42
			(0.65)	(0.78)	(1.68)	(2.27)
City FE	Yes	Yes	Yes	Yes	Yes	Yes
City FE × Post	No	Yes	No	Yes	No	Yes
Adj. R^2	0.78	0.78	0.78	0.78	0.78	0.78
Num. obs.	13933	13933	11705	11705	11705	11705
N Clusters	77	77	62	62	62	62

OLS regressions (col (1) - (4)) with the Log of Ring Population as the response variable. 2SLS-Regression (col (5) & (6)) with Peri × Post × Female Labor instrumented by sectoral employment shares in the hospitality, financial and public service sector. Clustered standard errors (at city level) in parentheses. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Tab. A.5: Ring Households with children and continuous treatment

	OLS				2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	6.96***	6.59***	6.79***	6.86***	6.79***	6.86***
	(0.15)	(0.13)	(0.16)	(0.15)	(0.16)	(0.15)
Distance	0.25***	0.24***	0.26***	0.25***	0.26***	0.25***
	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
Peri × (Distance - $\tilde{r}/3$)	−0.66***	−0.66***	−0.67***	−0.67***	−0.67***	−0.67***
	(0.09)	(0.09)	(0.10)	(0.10)	(0.10)	(0.10)
Peri × Post	0.77	1.00	1.96***	2.31***	2.04*	2.42**
	(0.46)	(0.57)	(0.47)	(0.55)	(1.05)	(1.14)
($\overline{\text{Price}}$ - Price) × Peri × Post	−1.09	−1.37	−1.11*	−1.37*	−1.11*	−1.37*
	(0.57)	(0.68)	(0.46)	(0.57)	(0.45)	(0.55)
Peri × Post × Female Labor			−2.43**	−2.67***	−2.57	−2.91
			(0.89)	(0.94)	(2.12)	(2.34)
City FE	Yes	Yes	Yes	Yes	Yes	Yes
City FE × Post	No	Yes	No	Yes	No	Yes
Adj. R^2	0.79	0.79	0.79	0.79	0.79	0.79
Num. obs.	7125	7125	6804	6804	6804	6804
N Clusters	46	46	44	44	44	44

OLS regressions (col (1) - (4)) with the Log of Ring Households with Children as the response variable. 2SLS-Regression (col (5) & (6)) with Peri × Post × Female Labor instrumented by sectoral employment shares in the hospitality, financial and public service sector. Clustered standard errors (at city level) in parentheses. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Tab. A.6: Ring Households with varying number of children and continuous treatment

	OLS			2SLS		
	1 child	2 children	3(+) children	1 child	2 children	3(+) children
Intercept	6.02***	5.60***	6.63***	6.35***	5.73***	4.45***
	(0.14)	(0.14)	(0.28)	(0.15)	(0.15)	(0.18)
Distance	0.21***	0.27***	0.28***	0.22***	0.28***	0.29***
	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)
Peri × (Distance - $\tilde{r}/3$)	−0.63***	−0.67***	−0.69***	−0.64***	−0.68***	−0.70***
	(0.09)	(0.09)	(0.09)	(0.10)	(0.10)	(0.09)
Peri × Post	0.87	1.02	1.17	2.23*	2.39*	2.00
	(0.55)	(0.56)	(0.59)	(1.16)	(1.16)	(1.25)
($\overline{\text{Price}}$ - Price) × Peri × Post	−1.20	−1.41	−1.77*	−1.20*	−1.41*	−1.77**
	(0.64)	(0.67)	(0.69)	(0.52)	(0.54)	(0.62)
Peri × Post × Female Labor				−2.78	−2.79	−1.69
				(2.44)	(2.39)	(2.60)
City FE	Yes	Yes	Yes	Yes	Yes	Yes
City FE × Post	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R^2	0.79	0.78	0.78	0.79	0.78	0.78
Num. obs.	6960	6953	6910	6639	6632	6589
N Clusters	46	46	46	44	44	44

OLS (col 1–3) and 2SLS (col 4–6) regressions with the log of ring households with varying number of children in custody as the response variable. Peri × Post × Female Labor instrumented by sectoral employment shares in the hospitality, financial and public service sector. Clustered standard errors (at city level) in parentheses. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Tab. A.7: Ring Households with Children with spline knot at $\widetilde{r}/4$

	OLS				2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	6.32***	6.37***	6.22***	6.43***	6.20***	6.43***
	(0.19)	(0.16)	(0.21)	(0.15)	(0.21)	(0.15)
Distance	0.47***	0.46***	0.48***	0.47***	0.48***	0.47***
	(0.07)	(0.06)	(0.08)	(0.07)	(0.08)	(0.07)
Peri × (Distance - $\tilde{r}/3$)	−0.84***	−0.84***	−0.86***	−0.85***	−0.86***	−0.85***
	(0.11)	(0.10)	(0.11)	(0.10)	(0.11)	(0.10)
Peri × Post	0.14	0.21	0.58	0.77	−0.12	0.01
	(0.10)	(0.16)	(0.43)	(0.49)	(1.16)	(1.41)
Aff × Peri × Post	−0.58**	−0.83**	−0.57**	−0.82**	−0.62**	−0.88**
	(0.15)	(0.23)	(0.16)	(0.24)	(0.18)	(0.26)
Peri × Post × Female Labor			−0.89	−1.10	0.56	0.48
			(0.89)	(0.99)	(2.43)	(2.93)
City FE	Yes	Yes	Yes	Yes	Yes	Yes
City FE × Post	No	Yes	No	Yes	No	Yes
Adj. R^2	0.78	0.78	0.78	0.78	0.78	0.78
Num. obs.	7125	7125	6804	6804	6804	6804
N Clusters	46	46	44	44	44	44

OLS regressions (col (1) - (4)) with the Log of Ring Households with Children as the response variable. 2SLS-Regression (col (5) & (6)) with Peri × Post × Female Labor instrumented by sectoral employment shares in the hospitality, financial and public service sector. Clustered standard errors (at city level) in parentheses. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Tab. A.8: Ring Population with spline knot at $\widetilde{r}/4$

	OLS				2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	9.09***	9.17***	8.93***	8.99***	8.91***	8.98***
	(0.16)	(0.17)	(0.18)	(0.17)	(0.19)	(0.17)
Distance	0.39***	0.38***	0.40***	0.39***	0.40***	0.39***
	(0.06)	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)
Peri × (Distance - $\tilde{r}/3$)	−0.78***	−0.78***	−0.78***	−0.78***	−0.78***	−0.78***
	(0.08)	(0.08)	(0.09)	(0.09)	(0.09)	(0.08)
Peri × Post	0.13**	0.19*	0.54*	0.74**	−0.14	−0.08
	(0.06)	(0.11)	(0.27)	(0.35)	(0.79)	(1.16)
Aff × Peri × Post	−0.55***	−0.85***	−0.48**	−0.74**	−0.56**	−0.85**
	(0.14)	(0.22)	(0.18)	(0.29)	(0.21)	(0.33)
Peri × Post × Female Labor			−0.88	−1.16	0.56	0.56
			(0.56)	(0.71)	(1.68)	(2.48)
City FE	Yes	Yes	Yes	Yes	Yes	Yes
City FE × Post	No	Yes	No	Yes	No	Yes
Adj. R^2	0.77	0.77	0.77	0.77	0.76	0.76
Num. obs.	13933	13933	11705	11705	11705	11705
N Clusters	77	77	62	62	62	62

OLS regressions (col (1) - (4)) with the Log of Ring Population as the response variable. 2SLS-Regression (col (5) & (6)) with Peri × Post × Female Labor instrumented by sectoral employment shares in the hospitality, financial and public service sector. Clustered standard errors (at city level) in parentheses. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Appendix Chapter 3

B

B.1 Robustness

B.1.1 Other categorizations of regions

In addition to my analysis with labor market regions, I also conducted the analysis with so-called spatial planning regions and self-constructed "synthetic regions". Spatial planning regions are formed by the Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR) using German NUTS-3 municipalities (counties, independent cities as well as Hamburg and Berlin). They also describe cities (the economic center) and its surrounding hinterlands as in the case of labor market regions, but on much coarser scale. Results do not change qualitatively when using spatial planning regions (n = 55).

As a second robustness check, I constructed "synthetic regions" using NUTS-3 municipalities. Guided by the Standard Urban Model and using NUTS-3 level shapefiles, I first selected all German independent cities (107). Second, I identified all counties sharing an administrative boundary with an independent city using geographical information system (GIS) methods. Defining all of a independent citys neighboring counties as its urban hinterland, I created a total of 80 regions consisting of a core (an independent city) and a periphery (one or multiple neighboring counties). I exclude from the analysis all independent cities whose synthetic urban hinterland includes another independent city, such that each region has one and only one city and at least one neighboring county. In this approach, I create 60 synthetic regions that share common borders but for which I cannot ensure that they are indeed connected through commuter flows. Also with this approach, results are similar to the ones obtained by using labor market regions.

B.2 Data description

Female Employment Rate The variable FEMALE EMPLOYMENT RATE is the share of female employees subject to compulsory social insurance in all employees. Employees subject to social insurances are manual and non-manual workers and persons in vocational training who are compulsorily insured under statutory pension, health and/or unemployment insurance schemes, i.e. excluding civil servants, self-employed persons, family workers, and marginally employed persons. The female employment rate in city *i* in year *t* is calculated as the number of female employees subject to compulsory social insurance at place of work *i* at *t* divided by the number of employees in *i* at *t*. Source: INKAR database (`http://www.inkar.de`)

Household Income The variable HOUSEHOLD INCOME is the average disposable household income. Households' disposable income is calculated by adding to primary income the social benefits and other current transfers in kind that households mainly receive from the general government and deducting taxes on income and wealth, social contributions and other current transfers payable by households. The disposable income of private households thus corresponds to the income that ultimately accrues to private households and that they can use for consumption and saving purposes. Source: Federal and state statistical offices (`http://www.statistikportal.de/`)

Share Hospitality Sector The variable SHARE HOSPITALITY SECTOR is the share of working population working in trade, transport, hospitality, and information & communication industries in city *i* in year *t*. This industry includes the following sections: "Sale, maintenance and repair of motor vehicles and motorcycles", "Transport and storage", "Hotels and restaurants", and "Information and Communication". Source: Federal and state statistical offices (`http://www.regionalstatistik.de`)

Share Financial Sector The variable SHARE FINANCIAL SECTOR is the share of working population working in financial, insurance and corporate service, and land and housing industries in city *i* in year *t*. This industry includes the following sections: “Financial and insurance activities”, “Real estate activities’, “Professional, scientific and technical activities”, and “Other business activities”. Source: Federal and state statistical offices (http://www.regionalstatistik.de)

Share Public Sector The variable SHARE PUBLIC SECTOR is the share of working population working in public and other service, education, and health industries in city *i* in year *t*. This industry includes the following sections: “Public administration, defense and compulsory social security”, “Education’, “Health and social work”, “Arts, entertainment and recreation”, “Other services not elsewhere classified”, and “Households with domestic staff”. Source: Federal and state statistical offices (http://www.regionalstatistik.de)

Unemployment Rate The variable UNEMPLOYMENT RATE is the share of unemployed as a percentage of the civilian labor force. These represent the labor supply (employed + unemployed). They are estimated on the basis of the census and the microcensus. The unemployment rate in municipality *i* at time *t* is calculated as the numer of unemployed in *i* at *t* divided by all civilian labor force in *i* at *t*. Source: INKAR database (http://www.inkar.de)

Business Tax Revenues The variable BUSINESS TAX REVENUES is the business tax revenue in € per inhabitant. In addition to income tax revenues, business tax revenues are particularly important for municipal task planning. Business tax revenues depend primarily on the degree of industrialization and the production structure, but also on the development of the tertiary sector. Municipalities can influence trade tax revenue by setting assessment rates. The variable business tax revenues in municipality *i* at time *t* is calculated as the sum of business tax revenues in *i* at *t* divided by all residents in *i* at *t*. Source: INKAR database (http://www.inkar.de)

Tab. B.1: Descriptives on municipalities (mean over time)

	N	Mean	SD	Min	Max
Population	196	203,690	200,413	34,918	1,747,648
Building-Age (30-50y)	196	60,981	63,275	9,587	555,872
Children (0-16y)	196	19,031	18,477	2,614	156,955
Average Household Income	196	18,600	2,479	14,207	31,054
Business Tax Revenue p.c.	196	422	270	133	1,917
Unemployment rate	196	0.078	0.033	0.027	0.184
Female Employment Rate	196	0.456	0.045	0.286	0.561
Share Employment in the Hospitality Sector	196	0.263	0.041	0.152	0.456
Share Employment in the Financial Sector	196	0.148	0.046	0.068	0.340
Share Employment in the Public Sector	196	0.311	0.069	0.157	0.511

Source: POPULATION, BUILDING-AGE, CHILDREN, HOUSEHOLD INCOME & SECTORAL SHARES: Federal and state statistical offices. BUSINESS TAX REVENUE, UNEMPLOYMENT RATE & FEMALE EMPLOYMENT RATE: INKAR database.

Tab. B.2: Sample of Labor Market Regions

No.	Region ID	Munic. ID	Munic. name	Munic. type
1	4	1001	Flensburg, kreisfreie Stadt	City
2	4	1059	Schleswig-Flensburg, Landkreis	County
3	5	1003	Lübeck, kreisfreie Stadt	City
4	5	1055	Ostholstein, Landkreis	County
5	6	1002	Kiel, kreisfreie Stadt	City
6	6	1004	Neumünster, kreisfreie Stadt	City
7	6	1057	Plön, Landkreis	County
8	6	1058	Rendsburg-Eckernförde, Landkreis	County
9	8	2000	Hamburg, kreisfreie Stadt	City
10	8	1056	Pinneberg, Landkreis	County
11	8	1060	Segeberg, Landkreis	County
12	8	1062	Stormarn, Landkreis	County
13	8	3353	Harburg, Landkreis	County
14	9	3101	Braunschweig, kreisfreie Stadt	City
15	9	3157	Peine, Landkreis	County
16	9	3158	Wolfenbüttel, Landkreis	County
17	11	3103	Wolfsburg, kreisfreie Stadt	City
18	11	3151	Gifhorn, Landkreis	County
19	31	3402	Emden, kreisfreie Stadt	City
20	31	3452	Aurich, Landkreis	County
21	33	3403	Oldenburg (Oldenburg), kreisfreie Stadt	City
22	33	3458	Oldenburg, Landkreis	County
23	34	3404	Osnabrück, kreisfreie Stadt	City
24	34	3459	Osnabrück, Landkreis	County
25	35	3405	Wilhelmshaven, kreisfreie Stadt	City
26	35	3455	Friesland, Landkreis	County
27	35	3462	Wittmund, Landkreis	County
28	42	3401	Delmenhorst, kreisfreie Stadt	City
29	42	4011	Bremen, kreisfreie Stadt	City
30	42	3356	Osterholz, Landkreis	County
31	43	4012	Bremerhaven, kreisfreie Stadt	City
32	43	3352	Cuxhaven, Landkreis	County
33	45	5111	Düsseldorf, kreisfreie Stadt	City
34	45	5158	Mettmann, Landkreis	County
35	45	5162	Rhein-Kreis Neuss	County
36	46	5112	Duisburg, kreisfreie Stadt	City
37	46	5119	Oberhausen, kreisfreie Stadt	City
38	46	5170	Wesel, Landkreis	County
39	57	5315	Köln, kreisfreie Stadt	City
40	57	5362	Rhein-Erft-Kreis	County
41	57	5378	Rheinisch-Bergischer Kreis	County
42	59	5314	Bonn, kreisfreie Stadt	City
43	59	5382	Rhein-Sieg-Kreis	County
44	63	5512	Bottrop, kreisfreie Stadt	City

Sample of Labor Market Regions (continued)

No.	Region ID	Munic. ID	Munic. name	Munic. type
45	63	5513	Gelsenkirchen, kreisfreie Stadt	City
46	63	5916	Herne, kreisfreie Stadt	City
47	63	5562	Recklinghausen, Landkreis	County
48	64	5515	Münster, kreisfreie Stadt	City
49	64	5558	Coesfeld, Landkreis	County
50	64	5570	Warendorf, Landkreis	County
51	67	5711	Bielefeld, kreisfreie Stadt	City
52	67	5758	Herford, Landkreis	County
53	73	5913	Dortmund, kreisfreie Stadt	City
54	73	5915	Hamm, kreisfreie Stadt	City
55	73	5978	Unna, Landkreis	County
56	81	6611	Kassel, kreisfreie Stadt	City
57	81	6633	Kassel, Landkreis	County
58	91	6414	Wiesbaden, kreisfreie Stadt	City
59	91	6439	Rheingau-Taunus-Kreis	County
60	92	6412	Frankfurt am Main, kreisfreie Stadt	City
61	92	6413	Offenbach am Main, kreisfreie Stadt	City
62	92	6433	Gross-Gerau, Landkreis	County
63	92	6434	Hochtaunuskreis	County
64	92	6436	Main-Taunus-Kreis	County
65	92	6438	Offenbach, Landkreis	County
66	92	6440	Wetteraukreis	County
67	94	6411	Darmstadt, kreisfreie Stadt	City
68	94	6432	Darmstadt-Dieburg, Landkreis	County
69	100	7111	Koblenz, kreisfreie Stadt	City
70	100	7137	Mayen-Koblenz, Landkreis	County
71	100	7141	Rhein-Lahn-Kreis	County
72	105	7211	Trier, kreisfreie Stadt	City
73	105	7235	Trier-Saarburg, Landkreis	County
74	109	7312	Kaiserslautern, kreisfreie Stadt	City
75	109	7333	Donnersbergkreis	County
76	109	7335	Kaiserslautern, Landkreis	County
77	109	7336	Kusel, Landkreis	County
78	110	7313	Landau in der Pfalz, kreisfreie Stadt	City
79	110	7337	Südliche Weinstrasse, Landkreis	County
80	111	7315	Mainz, kreisfreie Stadt	City
81	111	7339	Mainz-Bingen, Landkreis	County
82	112	7319	Worms, kreisfreie Stadt	City
83	112	7331	Alzey-Worms, Landkreis	County
84	113	7317	Pirmasens, kreisfreie Stadt	City
85	113	7320	Zweibrücken, kreisfreie Stadt	City
86	113	7340	Südwestpfalz, Landkreis	County
87	114	7311	Frankenthal (Pfalz), kreisfreie Stadt	City
88	114	7314	Ludwigshafen am Rhein, kreisfreie Stadt	City

Sample of Labor Market Regions (continued)

No.	Region ID	Munic. ID	Munic. name	Munic. type
89	114	7316	Neustadt an der Weinstrasse, kreisfreie Stadt	City
90	114	7318	Speyer, kreisfreie Stadt	City
91	114	7332	Bad Dürkheim, Landkreis	County
92	114	7338	Rhein-Pfalz-Kreis	County
93	120	8111	Stuttgart, kreisfreie Stadt	City
94	120	8115	Böblingen, Landkreis	County
95	120	8116	Esslingen, Landkreis	County
96	120	8118	Ludwigsburg, Landkreis	County
97	120	8119	Rems-Murr-Kreis	County
98	122	8121	Heilbronn, kreisfreie Stadt	City
99	122	8125	Heilbronn, Landkreis	County
100	127	8211	Baden-Baden, kreisfreie Stadt	City
101	127	8216	Rastatt, Landkreis	County
102	128	8212	Karlsruhe, kreisfreie Stadt	City
103	128	8215	Karlsruhe, Landkreis	County
104	129	8221	Heidelberg, kreisfreie Stadt	City
105	129	8226	Rhein-Neckar-Kreis	County
106	130	8222	Mannheim, kreisfreie Stadt	City
107	130	6431	Bergstrasse, Landkreis	County
108	135	8311	Freiburg im Breisgau, kreisfreie Stadt	City
109	135	8315	Breisgau-Hochschwarzwald, Landkreis	County
110	135	8316	Emmendingen, Landkreis	County
111	145	8421	Ulm, kreisfreie Stadt	City
112	145	8425	Alb-Donau-Kreis	County
113	145	9775	Neu-Ulm, Landkreis	County
114	154	9163	Rosenheim, kreisfreie Stadt	City
115	154	9187	Rosenheim, Landkreis	County
116	159	9162	München, kreisfreie Stadt	City
117	159	9174	Dachau, Landkreis	County
118	159	9175	Ebersberg, Landkreis	County
119	159	9177	Erding, Landkreis	County
120	159	9178	Freising, Landkreis	County
121	159	9179	Fürstenfeldbruck, Landkreis	County
122	159	9184	München, Landkreis	County
123	159	9188	Starnberg, Landkreis	County
124	160	9161	Ingolstadt, kreisfreie Stadt	City
125	160	9176	Eichstätt, Landkreis	County
126	160	9185	Neuburg-Schrobenhausen, Landkreis	County
127	160	9186	Pfaffenhofen an der Ilm, Landkreis	County
128	162	9261	Landshut, kreisfreie Stadt	City
129	162	9274	Landshut, Landkreis	County
130	165	9262	Passau, kreisfreie Stadt	City
131	165	9275	Passau, Landkreis	County
132	169	9263	Straubing, kreisfreie Stadt	City
133	169	9278	Straubing-Bogen, Landkreis	County

Sample of Labor Market Regions (continued)

No.	Region ID	Munic. ID	Munic. name	Munic. type
134	171	9362	Regensburg, kreisfreie Stadt	City
135	171	9375	Regensburg, Landkreis	County
136	173	9361	Amberg, kreisfreie Stadt	City
137	173	9371	Amberg-Sulzbach, Landkreis	County
138	175	9363	Weiden in der Oberpfalz, kreisfreie Stadt	City
139	175	9374	Neustadt an der Waldnaab, Landkreis	County
140	177	9464	Hof, kreisfreie Stadt	City
141	177	9475	Hof, Landkreis	County
142	178	9462	Bayreuth, kreisfreie Stadt	City
143	178	9472	Bayreuth, Landkreis	County
144	179	9461	Bamberg, kreisfreie Stadt	City
145	179	9471	Bamberg, Landkreis	County
146	182	9463	Coburg, kreisfreie Stadt	City
147	182	9473	Coburg, Landkreis	County
148	184	9562	Erlangen, kreisfreie Stadt	City
149	184	9474	Forchheim, Landkreis	County
150	184	9572	Erlangen-Höchstadt, Landkreis	County
151	185	9563	Fürth, kreisfreie Stadt	City
152	185	9564	Nürnberg, kreisfreie Stadt	City
153	185	9565	Schwabach, kreisfreie Stadt	City
154	185	9573	Fürth, Landkreis	County
155	185	9574	Nürnberger Land, Landkreis	County
156	185	9576	Roth, Landkreis	County
157	187	9561	Ansbach, kreisfreie Stadt	City
158	187	9571	Ansbach, Landkreis	County
159	190	9663	Würzburg, kreisfreie Stadt	City
160	190	9679	Würzburg, Landkreis	County
161	191	9662	Schweinfurt, kreisfreie Stadt	City
162	191	9678	Schweinfurt, Landkreis	County
163	196	9661	Aschaffenburg, kreisfreie Stadt	City
164	196	9671	Aschaffenburg, Landkreis	County
165	196	9676	Miltenberg, Landkreis	County
166	200	9761	Augsburg, kreisfreie Stadt	City
167	200	9771	Aichach-Friedberg, Landkreis	County
168	200	9772	Augsburg, Landkreis	County
169	201	9764	Memmingen, kreisfreie Stadt	City
170	201	9778	Unterallgäu, Landkreis	County
171	202	9762	Kaufbeuren, kreisfreie Stadt	City
172	202	9777	Ostallgäu, Landkreis	County
173	203	9763	Kempten (Allgäu), kreisfreie Stadt	City
174	203	9780	Oberallgäu, Landkreis	County
175	206	12051	Brandenburg an der Havel, kreisfreie Stadt	City
176	206	12054	Potsdam, kreisfreie Stadt	City

Sample of Labor Market Regions (continued)

No.	Region ID	Munic. ID	Munic. name	Munic. type
177	206	12063	Havelland, Landkreis	County
178	206	12069	Potsdam-Mittelmark, Landkreis	County
179	207	12052	Cottbus, kreisfreie Stadt	City
180	207	12066	Oberspreewald-Lausitz, Landkreis	County
181	207	12071	Spree-Neisse, Landkreis	County
182	208	12053	Frankfurt (Oder), kreisfreie Stadt	City
183	208	12064	Märkisch-Oderland, Landkreis	County
184	208	12067	Oder-Spree, Landkreis	County
185	242	16051	Erfurt, kreisfreie Stadt	City
186	242	16068	Sömmerda, Landkreis	County
187	243	16052	Gera, kreisfreie Stadt	City
188	243	16076	Greiz, Landkreis	County
189	244	16053	Jena, kreisfreie Stadt	City
190	244	16074	Saale-Holzland-Kreis	County
191	245	16054	Suhl, kreisfreie Stadt	City
192	245	16069	Hildburghausen, Landkreis	County
193	246	16055	Weimar, kreisfreie Stadt	City
194	246	16071	Weimarer Land, Landkreis	County
195	247	16056	Eisenach, kreisfreie Stadt	City
196	247	16063	Wartburgkreis	County

Note: This table has all labor market regions consisting of at least one city (the core) and at least one county (the hinterlands), as used in the analysis in the body of the paper.

Appendix Chapter 4

C

C.1 Index Construction (Hedonic Dummy Method)

For the construction of the rental index, I rely on the following hedonic regression, which is estimated for each city in the sample (n=109) separately. As the index' goal is to represent the main section of the rental market, I clean the sample from observations likely to be luxury apartments, following Klick and Schaffner (2019). I exclude observations with the following: a net rent above €5,000, a living space larger 400 square meters or more than seven rooms.

The conditional expectation of the log net rent (i.e. rent excluding utilities) $\mathrm{NET\,RENT}$ of apartment advertisement h in city ring j at time t is a function all the covariates on the right-hand side, for simplicity denoted by x_{ij}^{t}:

$$
\begin{aligned}
\mathrm{E}(\ln(\text{NET RENT}^t_{hj})|x^t_{hj}) = \alpha &+ \beta_1\,\text{LIVING SPACE}^t_{hj} + \beta_2\,\text{ROOMS}^t_{hj} \\
&+ \beta_3\,\text{QUALITY}^t_{hj} + \beta_4\,\text{BUILT}^t_{hj} \\
&+ \beta_5\,\text{FIRST OCCUPANCY}^t_{hj} + \beta_6\,\text{GARDEN}^t_{hj} \\
&+ \beta_7\,\text{GUEST BATH}^t_{hj} + \beta_8\,\text{CELLAR}^t_{hj} \\
&+ \beta_9\,\text{FITTED KITCHEN}^t_{hj} + \beta_{10}\,\text{BALCONY}^t_{hj} \\
&+ \sum_{j=2}^{J} \gamma_j\,\text{RING}_{hj} + \sum_{t=1}^{T} \theta^t\,\text{TIME}^t_h \\
&+ \sum_{j=2,t=1}^{J,T} \lambda^t_j\,\text{RING}_{hj} \times \text{TIME}^t_h \qquad \text{(C.1)}
\end{aligned}
$$

Variables LIVING SPACE and ROOMS are metric variables, indicating the amount of living space and the number of rooms in the apartment, respectively. QUALITY is a categorical variable measuring the quality of materials and equipment used in the apartment.[1] BUILT is a dummy variable specifying the year of construction,[2] FIRST OCCUPANCY turning 1 if the perspective move-in is the first occupancy of a newly built or substantially renovated apartment, GARDEN indicating if the apartment has access to a private garden, GUEST BATH if object includes a guest toilet, CELLAR if a cellar room is available, FITTED KITCHEN if the object comes with a fitted kitchen and BALCONY if the property has a balcony.

RING is a spatial dummy variable showing in which city ring the property is located. TIME constitutes a quarter-year dummy for the quarter and year the advertisement on the web page ended, indicating a successful transaction/lease. Lastly, RING $\times$ TIME is an interaction term between RING and TIME.

Methodology-wise, consider for now the case of a simple Dummy Time Hedonic (DTH) index to construct a city-wide rent index (i.e. skipping rings as the subject of indexes for now). Hill (2011, 13 ff.) describes the semi-log formulation as

[1] Conditions are: 0 = information missing, 1 = simple, 2 = normal, 3 = sophisticated, 4 = exclusive
[2] 1 = missing, 2 = before 1900, 3 = 1900-1945, 4 = 1946-1959, 5 = 1960-1969, 6 = 1970-1979, 7 = 1980-1989, 8 = 1990-1999, 9 = 2000-2009, 10 = after 2009

$$y = Z\beta + D\delta + \epsilon, \tag{C.2}$$

where y is an $H \times 1$ vector with elements $y_h = \ln p_h$, Z is an $H \times K$ matrix of property characteristics, β is a $K \times 1$ vector of characteristic shadow prices, D is an $H \times T - 1$ matrix of period dummy variables, δ is a $T - 1 \times 1$ vector of period prices and ϵ is an $H \times 1$ vector of random errors. H, K and T denote, respectively, the number of observations, characteristics and time periods, and p_h denotes the rent of property h. The base period rent index is normalized to 100.

To construct a quality-adjusted rent index, the primary interest lies in the δ parameters, which measure the period specific fixed effects on the logarithm of the price level after controlling for the effects of the differences in the attributes of the dwellings. To obtain rent index P^t for period t, one simply exponentiates the estimated $\widehat{\delta}^t$ obtained from the hedonic model and multiples that by 100, such that

$$\widehat{P}^t = \exp\left(\widehat{\delta}^t\right) \cdot 100. \tag{C.3}$$

I will briefly note for completeness Hill (2011) and Silver (2016) pointing out that, in theory, this rent index P^t is a biased estimate since it entails taking a nonlinear transformation of a random variable, i.e. $E[\exp(\hat{\delta})] \neq \exp(\delta)$. One should correct for that by adding half of the variance of the estimated coefficient when calculating the rent index, such that the corrected rent index for period t is

$$\overset{*}{\widehat{P}^t} = \exp\left[\widehat{\delta}^t + \frac{1}{2}\left(\widehat{\sigma}^t\right)^2\right] \cdot 100 \tag{C.4}$$

while $\left(\widehat{\sigma}^t\right)^2$ is an estimate of the variance of $\hat{\delta}_t$. Importantly note that both authors point out that the difference between $\widehat{P}^t$ and $\overset{*}{\widehat{P}^t}$ is in practice typically negligible as $\widehat{\sigma}^t$ is usually very small. I carry out this index correction but also find that it leads to changes only in the fourth decimal of $\widehat{P}_j^t$ and hence is of no great importance in this application.

For this papers purposes, I generalize this Dummy Time Hedonic (DTH) method to a Hedonic Dummy (HD) method as I have not only period dummy

Coefficient	Estimate	Standard Error	t statistic
γ_2	-0.1508	0.01679	-8.98
γ_3	-0.1767	0.01662	-10.63
$\theta^{08\text{-}2}$	0.0063	0.01597	0.40
$\theta^{08\text{-}3}$	-0.0504	0.01550	-3.25
$\theta^{08\text{-}4}$	-0.0415	0.01744	-2.38
$\lambda_2^{08\text{-}2}$	-0.0313	0.02463	-1.27
$\lambda_2^{08\text{-}3}$	0.0488	0.02268	2.15
$\lambda_2^{08\text{-}4}$	0.0499	0.02567	1.95
$\lambda_3^{08\text{-}2}$	-0.0234	0.02464	-0.95
$\lambda_3^{08\text{-}3}$	0.0362	0.02293	1.58
$\lambda_3^{08\text{-}4}$	0.0198	0.02632	0.75

Tab. C.1: Estimated coefficients for Regensburg

variables (TIME) but also ring dummy variables (RING) and their interactions (RING$\times$TIME). For illustration purposes, I estimate Equation C.1 for the city of Regensburg but report only the resulting estimated coefficients $\widehat{\gamma}_j$, $\widehat{\theta}^t$ and $\widehat{\lambda}_j^t$ for time periods t = 2008 Q2, ..., 2008 Q4 and rings j = 2,3. Quarter t^0 = 2008 Q1 and ring j = 1 are the base period and base ring, respectively.

Table C.1 has the regression results for this set of estimated coefficients; Table C.2 uses them to illustrate the construction of ring indexes $\widehat{P}_j^t$. Importantly note that, although all coefficients are reported in the tables, all those that were estimated with a p-value > 0.1 are not used for index construction or are set to 0.

Tab. C.2: Calculation of $\widehat{P}_j^t$ for Regensburg

Ring	Time	Calculation	$\widehat{P}_j^t$
1	08-Q1	$\exp(0)$	100.0
1	08-Q2	$\exp\left(\theta^{08\text{-}2}\right)$	100.0
1	08-Q3	$\exp\left(\theta^{08\text{-}3}\right)$	95.1
1	08-Q4	$\exp\left(\theta^{08\text{-}4}\right)$	95.9
2	08-Q1	$\exp(\gamma_2)$	86.0
2	08-Q2	$\exp\left(\gamma_2+\theta^{08\text{-}2}+\lambda_2^{08\text{-}2}\right)$	86.0
2	08-Q3	$\exp\left(\gamma_2+\theta^{08\text{-}3}+\lambda_2^{08\text{-}3}\right)$	85.9
2	08-Q4	$\exp\left(\gamma_2+\theta^{08\text{-}4}+\lambda_2^{08\text{-}4}\right)$	86.7
3	08-Q1	$\exp(\gamma_3)$	83.8
3	08-Q2	$\exp\left(\gamma_3+\theta^{08\text{-}2}+\lambda_3^{08\text{-}2}\right)$	83.8
3	08-Q3	$\exp\left(\gamma_3+\theta^{08\text{-}3}+\lambda_3^{08\text{-}3}\right)$	79.7
3	08-Q4	$\exp\left(\gamma_3+\theta^{08\text{-}4}+\lambda_3^{08\text{-}4}\right)$	80.4

Note: Bias correction (eq (8)) skipped for illustration purposes.

C.2 Robustness

C.2.1 Alternative Rent Index (Hedonic Double Imputation Index)

The results of subsidy introduction effects could depend on the way cities ring rent indexes are constructed. I therefore check whether the results obtained by using the Hedonic Dummy Method for index construction change if the index is instead constructed by the Hedonic Double Imputation Method (HDI). HDI relaxes the assumption that housing characteristics' valuations are time-invariant. In particular, it combines features of an index number (e.g. Laspeyres, Paasche or Dutot) and uses hedonic methods to estimate predicted values for missing prices. First, I estimate separate hedonic models of the form

$$y_h^t = Z_h^t \beta + \epsilon_h^t, \tag{C.5}$$

for each city i in each period t. The dependent variable y is, an $H \times 1$ vector with elements $y_h = \ln p_h$, Z is an $H \times K$ matrix of property characteristics,

β is a $K \times 1$ vector of characteristic shadow prices and ϵ is an $H \times 1$ vector of random errors. Again, H, K and T denote, respectively, the number of observations, characteristics and time periods, and p_h denotes the rent of property h.

The estimated coefficients from the hedonic models are then used to impute net rents for individual apartments. Following Hill and Scholz (2018) and Silver (2016) let $\ln\left(\widehat{p^t_{h|z^0_h}}\right)$ be the imputed log-rent (the predicted value) in period t of an apartment rented out in period 0. This log-rent is imputed by substituting the characteristics of apartment h rented out in period 0, z^0_h, into the estimated hedonic model of period t as

$$\ln\left(\widehat{p^t_{h|z^0_h}}\right) = \sum_{k=1}^{K} \widehat{\beta}^t_k \, z^0_{k,h}. \tag{C.6}$$

These imputed prices can be inserted into standard price formulas. I group observations ring j and time t and use a Dutot/Laspeyres-type imputation index of the form

$$P^t_j = \frac{\exp\left(1/H^0_j \sum_{h \in H^0_j} \ln \widehat{p^t_{h|z^0_h}}\right)}{\exp\left(1/H^0_j \sum_{h \in H^0_j} \ln \widehat{p^0_{h|z^0_h}}\right)} \times 100 \tag{C.7}$$

Hence, the price index of ring j at time t (in city i) is the mean of the predicted prices of apartments offered in ring j in t^0 valued at prices at t divided by the mean of these same apartments predicted prices at t^0. Practically, I take all rental observations in ring j and time t^0 and, predicting prices from quarterly hedonic regression results, look at how much these same rental observations would have cost in other periods. Also note that I use a double imputation method, which means that rents both in t and in base period t^0 are imputed/predicted from the quarterly regressions. This index baseline is different from the dummy hedonic method, as here every ring j is treated separately and hence each ring js index value at t^0 serves as baseline = 100 (and not, as in the Hedonic Dummy Method, j_1 at t^0).[3]

Let me again show this index technique using one of my samples cities, Regensburg, as an example. Table C.3 shows estimated coefficients for a rent estimation like Equation C.5, where property characteristics vector z only consists of variables listed in the table (and hence shortening the original rent regression for ease of presentation). Further, assume that both of these rings in t^0 only consist of three rental observations each. Table C.4 illustrates the imputation of the rent index for two rings of Regensburg, j = 1,5. For each observation, I calculate the predicted log-rent in t using its housing characteristics (in t=0) from Table C.4 and valuations of property characteristics in t from Table C.3.

For example, the calculation of the log of predicted rent of the first apartment (first row of Table Table C.4) in t^0 is

$$
\begin{aligned}
\ln \widehat{p^{08\text{-}1}}(z_{h=1}^{08\text{-}1}) &= 5.5421 + (0.0113 \times 77.5) + (0.0268 \times 3) + (-0.0698 \times 1) \\
&= 6.42866
\end{aligned}
$$

Calculation for the numbers of the 2015 Q1 predicted column proceed the same way, except they use the estimated coefficients from the hedonic regression for 2015 Q1. Next, I take the average of the predicted log-rents for each ring j and proceed to calculate index P_j^t. In my example, the HDI-Index value for the first ring at 2015 Q1 is

$$
P_1^{15\text{-}1} = [\exp(6.50390)/\exp(6.43655)] \times 100 = 106.97.
$$

Table C.5 has the regression results for the re-estimation of Equation 4.2 and Equation 4.3, but this time with the HDI-Index as response variable. The estimated DDD-coefficient does change in amplitude but - more importantly - retains its sign and statistical significance. Consequently, the papers main result, lower central rent premiums post subsidy introduction in affordable cities, is not driven by how apartment rent and rent growth is measured.

[3] For “hands-on” explanations of a variety of index constructions, see Aizcorbe (2014).

Tab. C.3: Hedonic regression results for Regensburg (shortend set of variables)

	Quarter-by-Quarter-Regressions		
	2008 Q1	2015 Q1	2020 Q1
Intercept	5.5421	5.4125	5.7629
living space	0.0113	0.0091	0.0113
rooms	0.0268	0.1427	0.0511
garden	0.0175	0.0571	0.0431
ring	-0.0698	-0.0216	-0.0435
adj rsq	0.748	0.764	0.817

Note: All reported estimated coefficients have p-values < 0.1.

Tab. C.4: Calculation of imputed price indexes for Regensburg

Illustration of predicted rents					(log) Actual and predicted rents			P_j^t
Raw data for apartment rents in 2008 Q1					Actual	Predicted		
net rent	living space	rooms	garden	ring	2008 Q1	2008 Q1	2015 Q1	
700	**77.5**	**3**	**0**	**1**	**6.55108**	**6.42866**	**6.52250**	
570	76	2	0	1	6.34564	6.38488	6.36620	
720	80	3	1	1	6.57925	6.49610	6.60230	
		Averages j = 1			6.49199	6.43655	6.50390	**106.97**
390	56	2	1	5	5.96615	5.91874	6.15534	
402	74	3	0	5	5.99645	6.11015	6.40459	
630	90	3	0	5	6.44572	6.29064	6.54957	
		Averages j = 5			6.13611	6.10651	6.36983	**104.12**

C.2.2 Different Definitions of the City Center

Results of central rings rent developments of course, may also heavily depend on which rings one defines as “central”. The papers results are obtained with ring j being central, i.e. dummy CENTER turning 1, if j lies within the first third of a cities set of rings or $j \leq J_i/3$.

Table C.6 serves as a robustness check and presents results from a re-estimation of Equation 4.2 and Equation 4.3 but with different definitions of CENTER. Col (1) and (4) use an *absolute* definition of the city center, as in every city—irrespective of its size—Dummy CENTER is 1 only for the first two rings from the CBD, i.e. $j = 1{,}2$. Col (2) and (5) use again a *relative* measure, as ring j is considered central when it lies within the first quarter of a citys set of rings or $j \leq J_i/4$. Finally, Col (3) and (6) show results when Dummy CENTER turns 1, if j lies within the first half of a citys set of rings.

Tab. C.5: Estimations with the Hedonic Double Imputation Index

	All Cities								Only West		No Rent Control	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Intercept	112.81***	128.07***	142.19***	142.93***	113.33***	128.05***	142.70***	143.36***	149.52***	143.36***	133.59***	133.98***
	(0.57)	(5.40)	(5.50)	(5.42)	(0.53)	(4.97)	(5.38)	(5.35)	(5.22)	(5.35)	(4.66)	(4.59)
Center	2.24*	2.23*	2.22*	2.21*	0.98	0.95	1.25	1.28	1.11	1.28	1.19	−12.78
	(1.18)	(1.18)	(1.17)	(1.17)	(3.32)	(3.30)	(3.22)	(3.20)	(0.72)	(3.20)	(0.79)	(7.77)
BK	24.99***	19.30***	14.79***	11.87***	52.28***	49.95***	40.71***	11.86***	11.85***	11.86***	14.51***	14.03***
	(2.08)	(2.00)	(1.84)	(1.19)	(9.96)	(9.74)	(8.92)	(1.16)	(1.27)	(1.16)	(1.26)	(1.23)
Center × BK	7.37***	7.22***	6.09***	6.24***	25.49***	25.19***	22.26***	21.53***	5.18***	21.53***	5.00***	15.65**
	(1.36)	(1.39)	(1.24)	(1.30)	(3.36)	(3.35)	(3.64)	(3.00)	(1.07)	(3.00)	(0.97)	(7.37)
Aff × BK	−5.94**	−7.77***	−6.95***									
	(2.40)	(2.49)	(2.54)									
Aff × Center	1.72	1.81	1.62	1.63					2.05*		2.75**	
	(1.43)	(1.42)	(1.42)	(1.42)					(1.12)		(1.25)	
($\overline{\text{Price}}$ - Price) × Center					2.64	2.71	2.24	2.21		2.21		18.57*
					(3.93)	(3.90)	(3.81)	(3.79)		(3.79)		(9.45)
Log(New Houses)		−6.83***	−5.47***	−5.65***		−6.75***	−5.49***	−5.65***	−4.94***	−5.65***	−4.15***	−4.15***
		(1.44)	(1.13)	(1.14)		(1.35)	(1.11)	(1.14)	(1.15)	(1.14)	(1.09)	(1.09)
Log(New Apartments)		9.32***	6.41***	6.48***		9.52***	6.41***	6.48***	6.30***	6.48***	4.60***	4.60***
		(1.10)	(0.83)	(0.81)		(1.09)	(0.82)	(0.81)	(0.81)	(0.81)	(0.79)	(0.79)
Vacancy Rate			−818.00***	−835.16***			−818.55***	−835.16***	−1286.42***	−835.16***	−536.99***	−536.99***
			(205.37)	(198.80)			(203.22)	(198.80)	(185.39)	(198.80)	(127.79)	(127.79)
DDD-estimates												
Aff × Center × BK	−4.47***	−4.52***	−3.76**	−4.01**					−3.38**		−2.97**	
	(1.53)	(1.55)	(1.60)	(1.60)					(1.57)		(1.31)	
($\overline{\text{Price}}$ - Price) × Center × BK					−25.61***	−25.50***	−22.76***	−22.19***		−22.19***		−14.83
					(3.94)	(3.90)	(4.50)	(3.71)		(3.71)		(8.97)
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City FE × BK	No	No	No	Yes	Yes	No	No	No	Yes	Yes	Yes	Yes
Adj. R^2	0.42	0.48	0.47	0.49	0.44	0.50	0.47	0.49	0.51	0.49	0.39	0.39
Num. obs.	46550	44724	40348	40348	46550	44724	40348	40348	33276	40348	21472	21472
N Clusters	105	105	103	103	105	105	103	103	86	103	61	61

OLS regressions with ring rent index as the response variable. Clustered standard errors (at city level) in parentheses.
Cols (9) & (10) exclude all cities on territory of former GDR, Col (11) & (12) exclude all cities that (at any point in time) introduced a rent control.
Data on building completions is from the federal and state statistical offices, data on the vacancy rate from empirica AG.
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Tab. C.6: Different Definitions of Central Rings

	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	171.70^{***}	172.34^{***}	176.34^{***}	171.54^{***}	172.24^{***}	174.85^{***}
	(4.02)	(4.03)	(4.74)	(3.94)	(3.97)	(4.45)
Center	7.82^{***}	7.45^{***}	4.32^{***}	20.84^{***}	23.31^{***}	17.75
	(1.32)	(1.41)	(1.55)	(4.57)	(5.84)	(8.44)
BK	14.42^{***}	13.90^{***}	14.30^{***}	32.92^{***}	31.32^{***}	37.72^{***}
	(1.33)	(1.34)	(2.03)	(5.24)	(5.07)	(5.41)
Center × BK	6.18^{***}	5.26^{***}	1.61	24.42^{***}	18.75^{***}	−1.00
	(0.92)	(0.89)	(1.39)	(5.45)	(4.18)	(6.70)
Aff × BK	-5.23^{***}	-4.68^{**}	−3.57			
	(1.87)	(1.85)	(2.84)			
Aff × Center	-3.86^{**}	-4.63^{**}	-6.83^{**}			
	(1.68)	(1.84)	(2.94)			
Aff × Center × BK	-4.28^{***}	-4.06^{***}	−2.82			
	(1.30)	(1.26)	(2.56)			
($\overline{\text{Price}}$ - Price) × Center				-19.05^{**}	-23.31^{**}	−21.53
				(5.60)	(7.18)	(10.99)
($\overline{\text{Price}}$ - Price) × Center × BK				-25.48^{***}	-19.42^{***}	2.54
				(6.60)	(5.17)	(9.00)
Log(New Houses)	-5.52^{***}	-5.53^{***}	-5.54^{***}	-5.53^{***}	-5.54^{***}	-5.55^{***}
	(0.85)	(0.85)	(0.85)	(0.83)	(0.83)	(0.83)
Log(New Apartments)	6.45^{***}	6.44^{***}	6.44^{***}	6.45^{***}	6.45^{***}	6.46^{***}
	(0.66)	(0.66)	(0.66)	(0.65)	(0.65)	(0.65)
Vacancy Rate	-721.36^{***}	-720.93^{***}	-721.62^{***}	-721.51^{***}	-721.24^{***}	-721.53^{***}
	(149.27)	(149.26)	(149.38)	(147.86)	(147.86)	(147.92)
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Center	$j < 2$	$j \leq J_i/4$	$j \leq J_i/2$	$j < 2$	$j \leq J_i/4$	$j \leq J_i/2$
Adj. R^2	0.48	0.49	0.47	0.49	0.49	0.47
Num. obs.	40915	40915	40915	40915	40915	40915
N Clusters	103	103	103	103	103	103

OLS regressions with ring rent index as the response variable. Clustered standard errors (at city level) in parentheses.
Data on building completions is from the federal and state statistical offices, data on the vacancy rate from empirica AG.
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Results of central rent premium development do not depend on the exact definition of central rings. Significant different central rent premium development by city affordability can be seen up to "mid-town", i.e. $j \leq J_i/2$ and this may closely coincide with the spatial cut-off of the predominance of apartment development in most cities.

C.3 Further Descriptives

Tab. C.7: Sample of Cities

No.	City	J	State	Former GDR	Affordable	$\overline{\text{Price}}$ - Price
1	Amberg	6	BY	0	1	0.869
2	Ansbach	8	BY	0	1	0.859
3	Aschaffenburg	7	BY	0	0	0.674
4	Augsburg	13	BY	0	0	0.696
5	Baden-Baden	9	BW	0	0	0.748
6	Bamberg	7	BY	0	0	0.768
7	Bayreuth	6	BY	0	1	0.854
8	Berlin	26	B	1	0	0.665
9	Bielefeld	12	NW	0	0	0.799

Sample of Cities (continued)

No.	City	J	State	Former GDR	Affordable	$\overline{\text{Price}}$ - Price
10	Bochum	10	NW	0	0	0.800
11	Bonn	13	NW	0	0	0.718
12	Bottrop	13	NW	0	0	0.758
13	Brandenburg an der Havel	18	BB	0	1	0.947
14	Braunschweig	11	NI	0	1	0.846
15	Bremen	27	HB	0	1	0.833
16	Bremerhaven	9	HB	0	1	0.932
17	Chemnitz	12	SN	1	1	0.959
18	Coburg	6	BY	0	1	0.901
19	Cottbus	11	BB	1	1	0.946
20	Darmstadt	9	HE	0	0	0.533
21	Delmenhorst	7	NI	0	1	0.915
22	Dessau-Rosslau	14	ST	0	1	0.958
23	Dortmund	13	NW	0	0	0.799
24	Dresden	16	SN	1	0	0.794
25	Duisburg	13	NW	0	0	0.797
26	Dusseldorf	15	NW	0	0	0.676
27	Eisenach	10	TH	1	1	0.938
28	Emden	11	NI	0	1	0.922
29	Erfurt	12	TH	1	1	0.924
30	Erlangen	8	BY	0	0	0.720
31	Essen	14	NW	0	0	0.671
32	Flensburg	5	SH	0	1	0.926
33	Frankenthal (Pfalz)	7	RP	0	1	0.820
34	Frankfurt (Oder)	12	BB	1	1	0.957
35	Frankfurt am Main	15	HE	0	0	0.527
36	Freiburg im Breisgau	14	BW	0	0	0.656
37	Fuerth	7	BY	0	0	0.753
38	Gelsenkirchen	11	NW	0	1	0.844
39	Gera	11	TH	1	1	0.945
40	Goettingen	8	NI	0	1	0.910
41	Hagen	11	NW	0	1	0.820
42	Halle (Saale)	9	ST	1	1	0.916
43	Hamburg	25	HH	0	0	0.494
44	Hamm	12	NW	0	1	0.885
45	Hannover	11	NI	0	1	0.850
46	Heidelberg	9	BW	0	0	0.462
47	Heilbronn	10	BW	0	0	0.749
48	Herne	6	NW	0	1	0.832
49	Ingolstadt	12	BY	0	0	0.706
50	Jena	8	TH	1	1	0.895
51	Kaiserslautern	9	RP	0	1	0.820
52	Karlsruhe	11	BW	0	0	0.671
53	Kassel	10	HE	0	1	0.880
54	Kaufbeuren	5	BY	0	1	0.851
55	Kempten (Allgäu)	8	BY	0	1	0.818
56	Kiel	13	SH	0	1	0.855

Sample of Cities (continued)

No.	City	J	State	Former GDR	Affordable	$\overline{\text{Price}}$ - Price
57	Köln (Cologne)	18	NW	0	0	0.729
58	Krefeld	10	NW	0	0	0.736
59	Landau in der Pfalz	10	RP	0	0	0.801
60	Landshut	11	BY	0	0	0.743
61	Leipzig	13	SN	1	1	0.921
62	Leverkusen	10	NW	0	1	0.848
63	Luebeck	18	SH	0	1	0.885
64	Ludwigshafen am Rhein	10	RP	0	0	0.772
65	Magdeburg	11	ST	1	1	0.936
66	Mainz	11	RP	0	0	0.662
67	Memmingen	8	BY	0	1	0.861
68	Moenchengladbach	13	NW	0	0	0.793
69	Muelheim an der Ruhr	8	NW	0	1	0.842
70	Munich	16	BY	0	0	0.000
71	Munster	12	NW	0	0	0.731
72	Neumunster	9	SH	0	1	0.910
73	Neustadt an der Weinstrasse	12	RP	0	0	0.758
74	Nuremberg	12	BY	0	0	0.669
75	Oberhausen	12	NW	0	0	0.846
76	Offenbach am Main	6	HE	0	0	0.669
77	Osnabrueck	9	NI	0	1	0.820
78	Passau	11	BY	0	1	0.873
79	Pforzheim	9	BW	0	0	0.788
80	Pirmasens	6	RP	0	1	0.922
81	Potsdam	14	BB	1	1	0.854
82	Regensburg	8	BY	0	0	0.676
83	Remscheid	9	NW	0	1	0.818
84	Rosenheim	6	BY	0	0	0.652
85	Rostock	18	MV	1	1	0.937
86	Salzgitter	19	NI	0	1	0.919
87	Schwabach	6	BY	0	0	0.801
88	Schweinfurt	4	BY	0	1	0.810
89	Schwerin	9	MV	1	1	0.917
90	Solingen	9	NW	0	0	0.761
91	Speyer	6	RP	0	0	0.736
92	Straubing	7	BY	0	1	0.839
93	Stuttgart	11	BW	0	0	0.266
94	Suhl	10	TH	1	1	0.960
95	Trier	10	RP	0	0	0.804
96	Ulm	10	BW	0	0	0.795
97	Weiden i.d. OPf.	7	BY	0	1	0.862
98	Weimar	8	TH	1	1	0.944
99	Wiesbaden	11	HE	0	0	0.578
100	Wilhelmshaven	12	NI	0	1	0.926
101	Wolfsburg	13	NI	0	1	0.901
102	Worms	13	RP	0	1	0.810
103	Wuppertal	13	NW	0	0	0.792

Sample of Cities (continued)

No.	City	J	State	Former GDR	Affordable	$\overline{\text{Price}}$ - Price
104	Wuerzburg	9	BY	0	0	0.720
105	Zweibruecken	8	RP	0	1	0.926

Note: B = Berlin, BB = Brandenburg, BW = Baden-Wuerttemberg, BY = Bavaria, HB = Bremen, HE = Hesse, MV = Mecklenburg-Western Pomerania, NI = Lower Saxony, NW = North Rhine-Westphalia, RP = Rhineland-Palatinate, SH = Schleswig-Holstein, SN = Saxony, ST = Saxony-Anhalt, TH = Thuringia.

		Expensive (N=26479)		Affordable (N=27475)			
		Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
Ring Rent Index		104.7	20.4	109.5	19.1	4.8	2.1
J		13.2	5.0	11.8	4.8	−1.3	1.3
($\overline{\text{Price}}$ - Price)		675.5	159.6	887.2	46.4	211.7	27.4
New Houses		296.0	392.0	126.3	161.1	−169.7	83.0
New Apartments		88.5	137.4	15.2	19.3	−73.3	23.4
		N	%	N	%		
Former GDR	No	24421	92.2	19737	71.8		
	Yes	2058	7.8	7738	28.2		
“Mietpreisbremse”	No	9361	35.4	19929	72.5		
	Yes	17118	64.6	7546	27.5		

Tab. C.8: Balance table of of the data

Colophon

This thesis was typeset with $\LaTeX\,2_\varepsilon$. It uses the *Clean Thesis* style developed by Ricardo Langner.