Homeowner Subsidy Repeal and Housing Recentralization

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Abstract: Subsidizing homeownership decentralizes cities, so Muth (1967) suggested over half a century ago. This paper's interest is in the related question of whether repealing a homeownership subsidy recentralizes cities. This question is relevant today, given homeownership subsidies' ubiquity. We provide a first quasi-experimental test of a subsidy repeal's spatial effects by turning to Germany's 2005 homeownership subsidy reform. We find that repealing the subsidy contributed to recentralizing Germany's cities. Inasmuch as recentralization helps abate carbon dioxide emissions, repealing a homeownership subsidy also helps mitigate global warming.

Appendix materials can be accessed online at:
https://uwpress.wisc.edu/journals/pdfs/LE-99-2-Daminger-appA.pdf
https://uwpress.wisc.edu/journals/pdfs/LE-99-2-Daminger-appB.pdf
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1. Introduction

Subsidizing homeownership decentralizes cities, so Muth (1967) suggested over half a century ago. More recently, Voith (1999) and Glaeser (2011) have renewed this proposition. This paper's interest is in the related question of whether repealing a home- ownership subsidy recentralizes cities. This question is relevant today. Many countries around the world are known to pay a subsidy towards homeownership. We provide a first quasi-experimental test of a repeal's spatial effects by turning to Germany's 2005 homeownership subsidy reform. Because housing in Germany's city centers is predominantly rental, subsidizing homeownership coaxed owner-occupiers to move out. Repealing the subsidy ended that allure.

Germany's cities have recentralized conspicuously ever since. Controlling for distance from the city center and for city fixed effects, we find that the population in every central ring (i.e., a ring among the third of rings closest to the center) grew by over 6% between 2005 and 2017; while the population in every peripheral ring (i.e., a ring among the two thirds of rings closer to the urban fringe) contracted by 0.3%. We label this asymmetric adjustment *recentralization*, even as we understand that cities are open and that adjustments in rings are more than mere rearrangements of the existing city population. It is tempting to attribute recentralization to subsidy repeal.

However, recentralization may also be driven by other forces. To address these potential confounders, we repeatedly make use of the stylized fact that subsidy repeal tended to affect only a subset of households yet left alone all others–even if the distinction between those who were affected and those who were not is "fuzzy" rather than clear-cut (de Chaisemartin and d'Haultfœuille (2018)). Specifically, we exploit the differences in treatment implied by the subsidy repeal's timing and by the original subsidy's design. In terms of timing, repealing the subsidy tended to hurt those too young to have applied prior to repeal. And in terms of design, repealing the subsidy tended to hurt those living where real estate was not expensive to begin with (i.e., built on land costing no more than 70 Euro per sqm). In a nutshell, subsidy repeal "treated" the young and those in affordable places; it "never treated" the older (who had long bought their home or had decided against it) or those in expensive places (who would never have bought a home in the first place).

This dichotomy suggests the following differences in how different strata of the population should respond to subsidy repeal. 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 approximately 200,000 homes extra in city peripheries had the subsidy not been repealed. From the perspective of design, our estimates imply that affordable-city households would have added approximately 130,000 homes to city peripheries had the subsidy not been repealed. These figures help us assess subsidy repeal's recentralizing impact. We conclude that, when- ever buying a home in the city center is more difficult than acquiring one in the city periphery, unsubsidizing homeownership discourages further suburbanization. We also tentatively suggest that understanding subsidy repeal may help us assess the effect of the original subsidy itself whenever implementing the subsidy can be argued to be the "reverse" of repealing it.

So, ultimately, we intend to contribute to the literature on understanding a home-ownership subsidy's impact on the spatial distribution of housing. While there is an extensive literature on the homeownership subsidy, much of it 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 (Hilber and Turner (2014) and Sommer and Sullivan (2018), or Kaas et al. (2021)), rather than on urban form. There is also a vast literature on program evaluation. Yet except for 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 a homeownership subsidy's effect on urban form.

Our paper is, however, related to Gruber et al. (2021). As indicated, those 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 suggest that repeal had no effect on Danish cities' form. This finding 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 strictly less for most of its duration). That subsidy repeal mattered little to affluent individuals' tenure decisions appears perfectly consistent with a strong role of subsidy repeal for the tenure decisions 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 numerous forms. Remarkably, we 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, by estimating a 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 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. 2022), surely pulling at least some 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 childcare facilities at their centers, enabling parents to re-enter the labor market earlier yet also drawing them closer to those 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 away 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 the subsidy repeal's impact.

Figure 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 as dots to the left of both diagrams. Initial gradients are equal to, or at least close to, zero. *Changes of gradient* ("DD") can be read off the dotted and solid graphs' slopes next. We note that gradient estimates for the treated–i.e., the young and households in affordable cities–go down (solid graphs). Recalling the gradient as a peripheral ring's extra in population (vis-à-vis a central ring), we see that centers become more popular with the young and in more affordable cities.

None of this, however, need be a convincing indication of the subsidy repeal's effects. Possibly, recentralization is similar, or even stronger, for the untreated? To address this concern, we turn from gradient changes to the *differences in changes of gradients* ("DDD"). In Figure 1, these differences can easily be gauged from the differences in the solid and dotted graphs' respective ascents. Where estimated gradients for the treated went down (as just explained, see the solid graphs), the estimated gradients for the untreated went *up* (and certainly not down, dotted graphs), and this is true irrespective of type of treatment. Put differently, far from also getting stronger, city centers become weaker both with the old and in expensive places. *A fortiori*, this divergence implies that the gradient change for the treated (solid graphs) is less than that for the untreated (dotted graphs). Peripheries' population extras suffer 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*.

[Figure 1 about here.]

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) and 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. Depending on the demographic we focus on, available data cover either the full sample of 83 of the largest German cities or a subset thereof, for all years from 2002 to 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 analyzing a homeownership subsidy. First, the repeal was for a federal, not for a local, subsidy. All cities saw their subsidy expire simultaneously, and so we do not need to concern ourselves with the methodological difficulties known to afflict difference-in-differences estimation when the treatment is staggered across units (Goodman-Bacon 2021). From any individual city's perspective, moreover, repeal was exogenous. Repeal 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 may, at least in part, offset the benefits from subsidizing homeownership. 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 to 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 greater 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 surface, 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).

The paper has six sections. Section 2 lays out the subsidy's design. Section 3 details the assembly of our geospatial city-ring-year panel and presents some preliminary and coarse observations on urban structure. Section 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 4.1) and housing affordability (subsection 4.2), to identify the subsidy repeal's impact on urban form. Section 5 provides a discussion of our results and pursues the various counterfactuals made possible by them. These may also provide insight into the strength of the homeownership subsidy itself. Section 6 concludes.

2. Subsidy Timing and Design

Germany's homeownership subsidies start with the housing shortage following WW II. One can distinguish roughly five 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, 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. During the fourth phase (2018 to 2020), federal government temporarily restored the homeownership subsidy, by introducing a variant of EZ for another three years.[5] Since 2021, homeownership is no longer subsidized. This paper exploits the transition from phase 2 to phase 3.

Appendix Table A1 provides an overview of essential features of the homeownership subsidy as they applied in phase 2. The subsidy was, 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 below). Then, for every year over a period of eight years altogether, subsidy payments amounted to $\min\{0.05 \cdot q_3, 2556\}$ Euros per newly built home, as opposed to only $\min\{0.025 \cdot q_2, 1\,278\}$ Euros for an existing home.[6] Common to all specifications for phase 2, households with children were always entitled to another €767 per child and year.[7] The more children the household had, the greater was the subsidy it was entitled to. Unlike the baseline subsidy, the child bonus was not capped with respect to the home value.

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, nominal subsidy payments were highly similar across cities. This nominal similarity was particularly true if there were children. Take, as one not overly contrived example, a married couple with two children and with a combined 2-year taxable income of no more than €163,614 buying a new home in 2003 at €120,000 (i.e., in an "expensive" city). This family would have received €2,556 + 2 · €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 costed 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 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 filtering 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 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 B 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]

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 population inhabiting the 1-km-wide ring starting at r km away from the CBD. Let r denote the maximum distance from the CBD to the city's administrative boundary, i.e., "city size". Then, as r ranges from 0 to 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 areal weighting via standard geospatial techniques. Highly detailed subdivision data are provided by KOSTAT[13] and BBSR[14] for the largest German cities,[15] and (in most cities) for all years 2002 through 2017. We often (i.e., whenever possible) choose city hall as the city's CBD.[16] We partition the city into 1 km wide concentric rings around the CBD, and then intersect this partition with the city shapefile polygons.[17] Appendix Figure A1 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. From 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 areal weighting for every city in the sample yields the full set of population profiles, $\{g_i\}$. Appendix Figure A1 highlights the procedure for Berlin's first two rings. For example, 92% of the centermost subdivision's population are assigned to the first ring, while 8% are assigned to the second ring. Appendix Figure A2's two diagrams show the profiles g_i (normalized by city population) we obtain for Berlin and the substantially smaller, more affordable Halle. Central rings have gained weight in either city. This gain is the recentralization of population apparent from the raw data, and it represents a common trend present in almost all cities in the sample.

[Figure 2 about here.]

Whenever possible, we make use of the full sample of 83 cities. Data are not always available for the full sixteen years 2002–2017, and thus our (unbalanced) panel comes to somewhat less than the full number of observations. At best (i.e., for the analysis in subsection 4.2), our sample cities account for slightly over 22 million individuals (in 2002), representing nearly one fourth of the country's population.

To provide some preliminary insight into the sample's 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 Figure 2 shows the change in the sample average of ring thirds' population over time. On average, the 1st third of rings (filled dots) grows by over 20,000 residents between 2002 and 2017. Residents in the 2nd third of rings (unfilled dots) on average also become more numerous, if only later and less so. Average population in the last third of rings (squares) 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 (Figure 2's second panel). Here we see that the 1st third's share on average grew by almost 1.5 percentage points, 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 the subsidy repeal's causal impact, we now turn to our full panel of finer profiles g.

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

$$g'(r) = 2\pi D(r) + 2\pi r D'(r). \qquad [1]$$

The first term on the right-hand side of Equation 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 Appendix Figure A2. Setting the r.h.s. of Equation 1 equal to zero and rearranging the resulting equation 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 "linearizes" $g(r)$ in piece-wise fashion, by explaining the logarithm of the population (or some stratum thereof further down) inhabiting city i, ring j and period t, y_{ijt}, with a simple spline. We set the spline's knot r_0 such that one third of rings are closer to, while two thirds of rings are further away from, the CBD. Next, $PERI_{ij}$ is a city periphery dummy equal to 1 if ring j belongs to the last two thirds of city i's rings (and zero else). Further, $POST_t$ is the treatment period dummy and equal to 1 if year t dates to after 2005, the year of subsidy repeal (and zero else). So, our point of departure is the following diff-in-diff specification:

$$y_{ijt} = \alpha_0 + \alpha_1 \, \mathrm{DIST}_j + \alpha_2 \left(\mathrm{DIST}_j - \tilde{r}_i/3\right) \times \mathrm{PERI}_{ij}$$

$$+ \beta_1 \, \mathrm{PERI}_{ij} + \beta_2 \, \mathrm{POST}_t + \beta_3 \, \mathrm{PERI}_{ij} \times \mathrm{POST}_t + \varepsilon_{ijt}. \qquad [2]$$

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, 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, β_3, captures the extent to which the population gradient has adjusted from before to after reform. Ignoring the confounders that we discuss shortly, we expect $\beta_3 < 0$ (joint with $\alpha_2 < 0 < \alpha_1$). With subsidy repeal, the population gradient should *fall*.

[Table 1 about here.]

Table 1 shows the OLS coefficient estimates we obtain after augmenting baseline Equation 2 by various types of fixed effects–as indicated by the corresponding checkmarks in the bottom half of the table. Column 1 includes ("one-way") city fixed effects first. This column's coefficient estimate for PERI × POST shows that the population gradient actually *did decrease* from before to after subsidy repeal, by substantial and significant 0.069. Where, before subsidy repeal, each peripheral ring had an extra 2.2% of population over and above what accounting for the spline would have us expect for the typical central ring, after reform it had 4.6% *less*. In Columns 2 through 4, we address potential endogeneity from failing to include further relevant (un-)observables. We generalize Equation 2 by also including year fixed effects (giving rise to "two-way" fixed effects (Baltagi 2021)), ring fixed effects, and the interaction between city and year effects.[21] Year fixed effects capture year-on-year shocks impacting the entire city system (e.g., the international financial crisis). Ring fixed effects generalize the dependence of population on distance to a potentially non-linear relationship. City and year fixed effects' interactions capture year-on-year shifts specific to each city (e.g., adjustments in the real estate transfer tax, international immigrants settling in cities closer to the country's borders or settling more in cities with existing migrant communities). None of these extensions overturn our conclusions from Column 1. The coefficient estimate on PERI × POST essentially remains the same throughout Columns 1–4, and highly significant. Only when city and ring fixed effects' interaction is also included in Column 5 does the coefficient estimate of interest drop noticeably, to −0.026. However, even then the estimate remains significant, at 10%. These preliminary estimates give a flavor of the strength of the recentralization underway. Moreover, they also are consistent with what we expect of subsidy repeal. Nonetheless, we want to check for the existence of a pre-trend. Recentralization may have started *prior*, and hence unrelated, to subsidy repeal. To rule out a pre-trend, we re-estimate Equation 2 by replacing POST with a full set of year fixed effects D_t, and by replacing PERI × POST with the full set of interactions between PERI and those year fixed effects. This is

$$\begin{aligned} y_{ijt} &= \alpha_0 + \alpha_1 \,\mathrm{DIST}_j + \alpha_2 \left(\mathrm{DIST}_j - \tilde{r}_i/3\right) \times PERI_{ij} \\ &+ \beta_1 \, PERI_{ij} + \sum_{\substack{t=2002 \\ t\neq 2005}}^{2017} \beta_t \,\mathrm{D}_t + \sum_{\substack{t=2002 \\ t\neq 2005}}^{2017} \gamma_t \, PERI_{ij} \times \mathrm{D}_t + \varepsilon_{ijt}. \quad [3] \end{aligned}$$

Figure 3 plots the estimated yearly shifts in the gradient relative to the 2005 gradient, γ_t, over time, 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, coefficient estimates did not just drop; they continued dropping for the full decade following repeal. Intuitively, this ongoing drop 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 arising in *unison with* repeal. The 0.069 points decrease of the population gradient shown in Table 1 might also partly 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. 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 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 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, will not (subsection 4.2).

[Figure 3 about here.]

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 to have applied for the subsidy, by then, though.[22] We define as "young" in any given year those who are between 15 and 29 years, as "old" all middle-aged individuals in the age brackets 30–44, and as "very old" those who are 45 through 59. Over the course of the 15 years following the year 2002, the young turned old as the old 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 $\mathrm{YOUNG_g}$ equal 0 (one) if the ring stratum g is from 30 to below 45 (15–29) in 2002 and from 45 to below 60 (30–44) in 2017. Our baseline equation is the following diff-in-diff-in-diff specification (DDD):

$$
\begin{aligned}
y_{ijtg} \quad = \quad & \alpha_0 + \alpha_1 \mathrm{DIST}_j + \alpha_2\left(\mathrm{DIST}_j - \widetilde{r}_i/3\right) \times \mathrm{PERI}_{ij} \\
+ \quad & \beta_1\, POST_t + \beta_2\, YOUNG_g + \beta_3\, PERI_{ij} \\
+ \quad & \gamma_1\, POST_t \times YOUNG_g + \gamma_2\, POST_t \times PERI_{ij} + \beta_3\, YOUNG_g \times PERI_{ij} \\
+ \quad & \delta\, POST_t \times PERI_{ij} \times YOUNG_g + \varepsilon_{ijtg}. \qquad [4]
\end{aligned}
$$

[Table 2 about here.]

In Equation 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 have undergone to be smaller than the change in gradient undergone by the old. Now, from the first column of Table 2, our DDD-estimate is -0.238. This estimate is highly significant.

For convenience and readability, in what follows we round off estimates to the first two digits after the decimal point, e.g., as in $-0.238 \approx -0.24$. It is instructive now to decompose the DDD-estimate. Let us recall how, in the introduction, in Figure 1's panel on the left, we represented gradient changes by the two graphs' slopes. There, for the old, we can immediately read a gradient change of 0.10 off the slope of the dotted graph and representing the coefficient estimate for $\mathrm{POST} \times \mathrm{PERI}$. Likewise, for the young, we read a gradient change of -0.14 off the slope of the solid graph, reflecting the difference between coefficient estimates for $\mathrm{POST} \times \mathrm{PERI}$ and $\mathrm{POST} \times \mathrm{PERI} \times \mathrm{YOUNG}$. Simply comparing these two slopes revealed the difference in gradient changes across cohorts, i.e., the DDD-estimate.

Appendix Figure A3 offers an alternative illustration, now also providing information on (log) population (rather than just on differences in it). Appendix Figure A3's two panels graph (the log of) ring population. These graphs' slopes are no longer gradient changes; they are the gradients themselves. That is, Appendix Figure A3's four slopes are the gradients pre- and post-repeal for either old (left-hand side) or young (right-hand side). Where the panel on the left-hand side indicates an upward shift in the slope for the old, the panel on the right-hand side indicates a downward shift of the slope for the young. In addition to these familiar effects, now we also see that the reported changes are driven by an underlying immigration by the young into both central and peripheral rings. No similar pattern is apparent for the old.

Subsequent variations of the young-old baseline specification in Equation 4 again also allow for year fixed effects (Column 2), ring fixed effects (Column 3), two-way interactions between city and year fixed effects as well as city and ring fixed effects (Column 4), and two types of three-way interactions (Column 5). Remarkably, our DDD-estimate δ remains negative and highly significant throughout. The estimate is robust to all these extensions, and this makes it a reasonable basis for experimenting with subsidy's repeal. Let us assume that, in the absence of subsidy repeal, the gradient change for the young would have mimicked that of the old.[23] Under this assumption, the dashed graphs in Figure 1 and Appendix Figure A3 show the counterfactual change in (the log of) the young. This is the change that would have been observed in each peripheral ring if the subsidy had not been repealed. We conclude that cities would have gone on to decentralize in the absence of subsidy repeal.

4.2 Treatment by Affordability

We next address treatment by housing affordability. We let dummy $\mathrm{AFF_i}$ equal 1 if city i's land price in year 2000 is €70 per square meter or less, and 0 else. This amounts to partitioning our sample into the 15% most "affordable" cities on the one hand and the more "expensive" remainder on the other. Our particular choice of €70 as the cut-off is not essential here, i.e., we have also allowed for a 2000 land price cut-off of €85 (corresponding to the 25th-percentile), or of even €150 (median) and neither adjustment substantially changes our most robust estimate of the (DDD) coefficient of interest below. Letting ourselves be guided by our discussion of the subsidy's design (Section 2 and Appendix Table A1), we now settle on 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 $\mathrm{PERI} \times \mathrm{POST}$ with AFF. We expect the change in the "population gradient" in less-affordable cities, as the coefficient of $\mathrm{PERI} \times \mathrm{POST}$, to exceed that in affordable ones. That is, we expect the estimated coefficient of $\mathrm{AFF} \times \mathrm{PERI} \times \mathrm{POST}$ to be negative.[24]

[Table 3 about here.]

As laid out in Section 2, the homeowner subsidy was identical across cities. This was particularly true for the substantial bonus per child of nearly €800 prior to 2004, and of exactly €800 in 2004 and 2005 (Appendix Table A1). Arguably, in affordable cities subsidy repeal treated (i.e., hurt) those households strongest who would have been eligible for receiving the most. Therefore, we first test our DDD-design on the narrow stratum of families with dependent children, rather than on population totals. It is families with dependent children that should have responded strongest. Table 3 has the corresponding coefficient estimates.

Column 1 shows that the coefficient estimate of $\mathrm{AFF} \times \mathrm{PERI} \times \mathrm{POST}$ *is* significantly negative, and large in absolute value. Affordable cities see their population gradient drop by 0.50, while expensive cities witness an increase in their gradient, of 0.07. So here our DDD-estimate is -0.57. As before, we assume that in the absence of repeal the treated would have mimicked the untreated. Then in affordable cities, peripheral housing stocks would have been substantially larger had the subsidy not been repealed.

Again, we may illustrate our results by plotting gradient changes. Figure 1's right-hand panel showed gradient changes as the graphs' respective slopes. Affordable cities' gradient falls, while expensive cities' gradient rises. Alternatively, Appendix Figure A4 plots (log) population. That figure's four slopes are the gradients pre- and post-repeal for those in expensive (left-hand side) vs. those in affordable cities (right-hand side). Going beyond Figure 1, now we also see the underlying trends in (log) population. Affordable cities see families emigrating out of, while expensive cities witness families immigrating into, their respective peripheral rings.

[Table 4 about here.]

As discussed, the affordable cities' gradient experiences a reduction of -0.57 relative to the change in population gradient in unaffordable cities. This reduction is our alternative DDD-estimate of the subsidy repeal's impact. For robustness, Table 3's subsequent columns again allow for adding various fixed effects. Including fixed effects with respect to cities, years, and rings has no effect on the three-way interaction, nor has including city-specific time trends. Column 5 shows that the coefficient estimate on $\text{AFF} \times \text{PERI} \times \text{POST}$ is not robust relative to allowing for the interaction between city and ring fixed effects, however. Table 4 alternatively reports our results for estimating the affordability "premium" on all households in the sample, rather than just households with children, and accounting for similar fixed effects.

Corresponding estimates of the coefficient of AFF $\times$ PERI $\times$ POST parallel those from Table 3.

Appendix A offers various robustness checks. First, Appendix Table A2 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 per child-year, see Table 1), we expect subsidy repeal’s impact on affordable cities to become stronger as the number of children increases. This expectation is not fully borne out in the data: Families with 3 or more children recentralize more than families with 1 child; however, families with 2 children do not recentralize less than those with 3 children or any more than those with one child only.

Second, we also replace dummy AFF with a continuous, if rough, indicator of affordability, $\overline{\mathrm{PRICE}} - \mathrm{PRICE}$, where PRICE is the highest average real estate price among all cities in the sample (i.e., in Munich) for the year 2000 and thus predating our analysis. Appendix Tables A3 to A5 in Appendix A show that the coefficient on the three-way interaction $(\overline{\mathrm{PRICE}}\text{-}\mathrm{PRICE}) \times$ PERI $\times$ POST retains its negative sign throughout. Third, we also vary the position of the spline’s knot. Appendix Tables A6 and A7, also in Appendix A, 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 have re-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 above and hence are suppressed.

5. Discussion

Stable Unit Treatment Value (SUTVA). We have argued for subsidy repeal treating the young but not the old. Yet the repeal of a generous subsidy should also have us expect concomitant changes in prices. These changes, in turn, likely affect the old, too. For example, should a city's young recentralize post-repeal, suburbs become cheaper. The city center's old could embrace the suburb rather than stay put, thereby changing the control group's spatial allocation. In the presence of such general equilibrium price adjustments, our DDD-estimates are less likely to be able to capture the subsidy repeal's causal effect (Angrist, Imbens, and Rubin (1996)). A similar, if slightly less compelling, general equilibrium objection may apply to comparing households in affordable cities with those in less-affordable cities.

Some support for maintaining SUTVA, however, comes from our spatial context. Each of the sample's cities is surrounded by its own densely populated hinterland, full of young ready to immigrate into the city at the slightest hint of falling rents or prices. These cities fit the notion of an open-city equilibrium (e.g., Brueckner 1987). Suppose the hinterland young are the quickest to fill any housing vacated by the urban young (themselves recentralizing towards the city center, or now no longer moving out into the urban periphery). Then it is the hinterland young, rather than the city old, who adjust to changes in rent and price. This notion of greater mobility among the young is certainly consistent with the large migration flows for the young and the small flows for the old apparent in Appendix Figure A3. We may think of the old as being not treated, not even by way of endogenous price effects.[25] At the same time, to the extent that the hinterland young are less than perfectly mobile, the urban old may find it easier to adjust their location, too. Then our assigning the old as the comparison group becomes less tenable.

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).[26]

Cohort-Specific Shifts. One may wonder if our strong result in subsection 4.1 could also be due to unobservable differences in cohort-specific trends, e.g., millennials' preferential shifts, with a small but growing literature asserting gentrification, and even a degree of city center renaissance, for certain population strata in US metro areas' urban cores (Baum-Snow and Hartley 2020; Couture and Handbury 2020; Owens III et al. 2020). Such trend differences, however, are unlikely to be 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. Suppose the young want to recentralize *more* than the old.

But then these same cohort-specific shifts must also have more affordable cities, with their older populations, recentralize *less*, instead of more, than expensive cities. This contradicts what we just learned in subsection 4.2 on more affordable cities recentralizing *more* than expensive cities. Subsidy repeal, in contrast, is well able to explain stronger recentralization both of the young and in more affordable places. Of course, this is a stylized reply only. We cannot rule out affordable cities attracting the young *more* than expensive cities do, by offering cheaper accommodation and amenities. At the same time, we note that our explanation of recentralization is based on observable changes in individuals' constraints (i.e., based on subsidy repeal), rather than based on assumed unobservable changes in preferences (i.e., ad-hoc changes in cohort-specific preferences), and thus keeps with economics tradition (Silberberg and Suen 2000).

Counterfactual Analysis. Consider our "accessibility" estimates from subsection 4.1. There, we assumed that the gradient for the young would have moved in tandem with that for the old had the subsidy not been repealed. No 0.238 would have been shaved off the (log) number of young in each ring. As discussed, the dashed line in the right-hand panel of Figure 1 indicates this counterfactual change in slope, while that of Appendix Figure A3 shows the corresponding counterfactual change in the (log) population of young.

Let $\hat{y}_{iy\bar{t}}$ 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 Equation 5).[27] Then

$$e^{\hat{y}_{iy\bar{t}}} - e^{\left(\hat{y}_{iy\bar{t}}-0.238\right)} \qquad [5]$$

is the number of young individuals who, post-reform, never bought the home in city i and peripheral ring j they otherwise would have bought. Summing over all cities' peripheral rings gives a total of 397,607 young individuals who never turned to homeownership. On assuming that it is always two young individuals who buy a house jointly, the number of home purchases "averted" by the subsidy repeal is 198,804. These approximately 200,000 purchases would have translated into the additional construction of 200,000 actual homes in city peripheries had no homes been vacant there.[28]

Alternatively, consider our "affordability" estimates from subsection 4.2. Had the subsidy not been repealed, now no 0.58 points would have been taken off the (log) number of residents in affordable cities' rings (Table 4). Using these latter estimates, and proceeding along the analogue of Equation 5, an additional 256,092 first-time buyers would now have owner-occupied their home extra. Repealing the subsidy prevented these purchases from happening. Again, on assuming a household size of 2 (and on presuming vacant housing largely irrelevant), an extra 128,046 homes would have been built in city peripheries had the subsidy not been scrapped.

Rents. Building on a filtering logic (e.g., as the one laid out in Appendix B), 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 (no longer moving) into the rental housing left behind (not left behind).[29] From this perspective, repealing the homeowner subsidy can also help contribute to explaining the more recent surge in Germany's rents. Daminger (2021b) is able to document hedonic rent for city rings during the transition from phase 3 (of no homeownership subsidy) to phase 4 (when home- ownership was subsidized via BK (see Section 2) and finds that BK indeed alleviated pressure on rents in cities that were affordable (but not in those that were expensive) to begin with.

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 more for the young than for the old. We conclude that it was not just that 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).

Subsidy Repeal vs. Subsidy Introduction. Our focus has been on the subsidy repeal's effects. This focus reflects the quasi-experiment at hand. But this focus also reflects the policy relevance of the fact that many countries pay the subsidy today. A country with an existing subsidy can only consider repealing, not introducing, it. Nonetheless, it is of interest to inquire into the extent to which the homeownership subsidy's effects on implementation can be gauged from our analysis of subsidy repeal. That is, can we assume that the recentralizing effect of revoking the subsidy equals (in absolute value) the decentralizing effect of introducing it? Surely there are several reasons why this assumption may fail, and why we cannot infer the original decentralizing effect of the subsidy–not least because roughly a decade separates the subsidy's introduction from its repeal. It is very unlikely that all relevant circumstances will have remained the same.

Certainly at least one endogenous variable change suggests that the subsidy's decentralizing effect may actually exceed, rather than fall short of, the subsidy repeal's recentralizing effect. Introducing the subsidy initially meets with few suburban amenities and little commuting infrastructure. But the decade of subsequent decentralization contributes to building up suburban amenities and commuter infrastructure that do not disappear simply because the subsidy does. "Path-dependence", "hysteresis" or even "lock-in" may induce households to remain in the city periphery, or even keep coming. Then the subsidy repeal's recentralizing effects–such as those identified in our analysis–actually understate the decentralizing effects of introducing the subsidy. From this perspective, at least, the subsidy repeal's recentralizing impact puts a lower, rather than upper, bound on the original subsidy's decentralizing impact.

6. Conclusions

On a large sample of city rings, this paper shows how Germany's repealing a lump-sum subsidy for 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 already) 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 untreated. It is this latter empirical observation that the economics of subsidy repeal has us expect. Our estimates are for diff-in-diff and triple-diff specifications, each augmented by various combinations of fixed effects, and interactions between them. These specifications appear suited to removing the bias in coefficient estimates that many potential confounders would otherwise introduce.

Homeownership subsidies are near-to-ubiquitous. We expect repealing a homeownership subsidy to drive recentralization in many countries–even if we must be careful to observe the institutional context, too. What may be true for repealing a lump-sum subsidy with tight caps on income and housing value may look quite different for repealing a mortgage-interest deductible by everyone and on every home. We add that subsidy repeal may be yet another, and novel, policy option for reducing greenhouse gas emissions whenever decentralized cities imply longer commutes, larger cars, and bigger housing.

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Tables

Table 1: Diff-in-Diff on Population

	(1)	(2)	(3)	(4)	(5)
Distance	0.230***	0.230***			
	(0.036)	(0.036)			
Peri × (Distance − $\tilde{\mathbf{r}}$/3)	−0.658***	−0.658***	−0.749***	−0.749***	
	(0.059)	(0.059)	(0.075)	(0.075)	
Peri	0.022	0.022	0.809***	0.810***	
	(0.139)	(0.139)	(0.095)	(0.095)	
Post	0.066***				
	(0.015)				
Peri × Post	−0.069***	−0.069***	−0.068***	−0.067***	−0.026***
	(0.023)	(0.023)	(0.020)	(0.021)	(0.014)
City FE	✓	✓	✓	✓	✓
Year FE		✓	✓	✓	✓
Ring FE			✓	✓	✓
City × Year FE				✓	✓
City × Ring FE					✓
Adj. R^2	0.786	0.786	0.823	0.808	0.997
Num. obs.	14939	14939	14939	149339	14939
Num. clusters (city)	83	83	83	83	83

Note: OLS regressions with the logarithm of ring population as the response variable. Clustered standard errors (at city level) in parentheses. Data: full sample of BBSR and KOSTAT cities (see Appendix Table C2). *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table 2: Old vs. Young Individuals

	(1)	(2)	(3)	(4)	(5)
Post	−0.072**				
	(0.030)				
Young	−0.854***	−0.854***	−0.854***	−0.854***	
	(0.018)	(0.018)	(0.018)	(0.018)	
Peri	0.058	0.058	0.677***		
	(0.144)	(0.144)	(0.086)		
Post × Young	0.962***	0.962***	0.962***	0.962***	
	(0.026)	(0.026)	(0.026)	(0.026)	
Post × Peri	0.102**	0.102**	0.099**	0.192***	0.190***
	(0.047)	(0.047)	(0.046)	(0.024)	(0.024)
Young × Peri	−0.139***	−0.139***	−0.140***	−0.147***	
	(0.016)	(0.016)	(0.015)	(0.014)	
Post × Peri × Young	−0.238***	−0.238***	−0.237***	−0.230***	−0.226***
	(0.018)	(0.018)	(0.018)	(0.019)	(0.020)
City FE	✓	✓	✓	✓	✓
Year FE		✓	✓	✓	✓
Ring FE			✓	✓	✓
City × Year FE				✓	✓
City × Ring FE				✓	✓
City × Year × Cohort FE					✓
City × Ring × Cohort FE					✓
Adj. R^2	0.820	0.820	0.852	0.993	0.993
Num. obs.	4658	4658	4658	4658	4658
Num. clusters (city)	50	50	50	50	50

Note: OLS regressions with the logarithm of the population count (in age strata) as the response variable. We match up age cohorts of years 2002/203 (before subsidy repeal) and years 2016/2017 (post subsidy repeal). For years 2002/2003, dummy Young equals 1 (0) for residents aged 15–29 (30–44). For years 2016/2017, dummy Young equals 1 (0) for residents aged 30–44 (45–59). To improve table clarity, estimated coefficients on α1 and α2 and are not reported. Clustered standard errors (at city level) in parentheses. Data: cities in BBSR sample (see Appendix Table C2). *** p < 0.01; ** p < 0.05; * p < 0.1.

Table 3: Ring Households with Children

	(1)	(2)	(3)	(4)	(5)
Distance	0.250***	0.249***			
	(0.049)	(0.044)			
Peri × (Distance − $\tilde{\mathbf{r}}$/3)	−0.661***	−0.660***	−0.581***	−0.587***	
	(0.090)	(0.087)	(0.094)	(0.094)	
Peri × Post	0.073	0.085	0.347***	0.367***	−0.041**
	(0.116)	(0.162)	(0.090)	(0.097)	(0.015)
Aff × Peri × Post	−0.574***	−0.572***	−0.483***	−0.642***	−0.077
	(0.153)	(0.155)	(0.154)	(0.205)	(0.050)
City FE	✓	✓	✓	✓	✓
Year FE		✓	✓	✓	✓
Ring FE			✓	✓	✓
City × Year FE				✓	✓
City × Ring FE					✓
Adj. R^2	0.786	0.785	0.835	0.824	0.999
Num. obs.	7125	7125	7125	7125	7125
Num. clusters (city)	46	46	46	46	46

Note: OLS regressions with the logarithm of ring households with children as the response variable. Clustered standard errors (at city level) in parentheses. Data: cities in BBSR sample (see Appendix Table C2). *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table 4: All Residents

	(1)	(2)	(3)	(4)	(5)
Distance	0.208***	0.211***			
	(0.039)	(0.037)			
Peri × (Distance − $\tilde{\mathbf{r}}$/3)	−0.639***	−0.641***	−0.693***	−0.696***	
	(0.068)	(0.066)	(0.079)	(0.079)	
Peri × Post	0.086	0.055	0.389***	0.425***	−0.015
	(0.062)	(0.104)	(0.065)	(0.075)	(0.014)
Aff × Peri × Post	−0.581***	−0.581***	−0.563***	−0.781***	−0.073
	(0.140)	(0.139)	(0.132)	(0.183)	(0.051)
City FE	✓	✓	✓	✓	✓
Year FE		✓	✓	✓	✓
Ring FE			✓	✓	✓
City × Year FE				✓	✓
City × Ring FE					✓
Adj. R^2	0.784	0.784	0.816	0.801	0.997
Num. obs.	13933	13933	13933	13933	13933
Num. clusters (city)	77	77	77	77	77

Note: OLS regressions with the logarithm of ring population as the response variable. Clustered standard errors (at city level) in parentheses. Data: full sample of BBSR and KOSTAT cities with available land price information (see Appendix Table C2). *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Figure Captions

Figure 1: Population Gradient Before and After Repeal, by Type of Treatment

Figure 2: 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 cities' population living in the centermost third of rings rises while the 2nd and 3rd thirds' shares both shrink. Data: Authors' calculations using KOSTAT and BBSR data.

Figure 3: Additions to Population Gradient between 2002 and 2017

Note: This figure shows the estimated coefficients of γt from Equation 3. For this regression, we restrict our sample to the 57 cities for which we continuously have yearly data from 2002–2017 (see Appendix Table C2). Data: Authors' calculations using BBSR and KOSTAT data.

Footnotes

[1] For Denmark's three income brackets, repeal "raised the 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 add that the number of individuals with higher incomes is smaller than the number of those with incomes below the eligibility threshold, and so behavioral changes induced in lower income brackets must matter more.
[3] That is, in subsections 4.1 and 4.2. Rather than show a "difference-in-differences-in-differences" of *population*, Figure 1 shows the equivalent "difference-in-differences" of the *population gradient*.
[4] Federal government's aggregate yearly expenditures of homeownership subsidies had attained a staggering €11 billion 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.
[5] This variant is the so-called *Baukindergeld*, or BK below. The state of Bavaria topped up BK by an extra €300.
[6] Our term „home" here applies to condos, apartments, detached or semi-detached housing alike, if 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 $\min\{0.01 \cdot p, 1{,}250\}$ Euros, $p \in \{q_2, q_3\}$ for both types of property.
[7] Subsidies applied to first homes, but couples were eligible for second homes, too.
[8] In fact, the subsidy pay out period could be pushed back even further if, for example, applications for subsidy and building permission had been filed 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.
[10] Such a tenure-quality-hierarchy can 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 empirical analysis of the changes in rents implied by BK during the phase 4 set our above.
[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, data were not consolidated anywhere. This lack of centralized information may also help explain the dearth of studies on EZ.
[13] KOSTAT: KOSIS-Gemeinschaft Kommunalstatistik. This dataset provides information on the total resident population in city subdivisions.
[14] BBSR: Bundesinstitut für Bau-, Stadt und Raumforschung. This dataset provides information on (i) the total resident population and (ii) resident population in various age strata in city subdivisions.
[15] We had to omit 21 among the 100 largest cities from this list because for those cities, shapefiles (see below) and/or data on population were missing. These cities are Osnabruck (48th in a list ordered by city size), Leverkusen (49th), Paderborn (56th), Heilbronn (62nd), Bottrop (66th), Bremerhaven (70th), Hildesheim (79th), Cottbus (80th), Kaiserslautern (81st), Gutersloh (82nd), Hanau (84th), Ludwigsburg (87th), Esslingen am Neckar (88th), Iserlohn (89th), Duren (90th), Flensburg (93rd), Giessen (94th), Ratingen (95th), Lunen (96th), Marl (99th), and Worms (100th) – see Appendix Table B2 for a full list of remaining cities in our sample.
[16] When a historic 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. See Holian (2019) for an overview of this procedure and related approaches.
[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.
[19] This also is why we add city and ring fixed effects later.
[20] This assumption holds if the density profile D(r) is not too convex in r.
[21] Due to collinearity, including these effects drives successively more variables out of the r.h.s. of Equation 2, ultimately leaving us only with the interaction term of interest, PERI × POST.

[22] As indicated in the introduction, this distinction is not perfect. Not all the old are never treated and not all the young are treated. Older households may have delayed buying their home to the extent of being "surprised" by the subsidy repeal. Younger households may have prioritized buying their home, buying early in their twenties. While correlated with household age, individual preferences and household wealth have roles of their own in the tenure decision. Notwithstanding this "fuzziness", post-repeal we expect the rate of the treated among the young to exceed that among the old.

[23] This is the common-trends-assumption adapted to our DDD context. It replaces the assumption of common trends in levels (as suited to a DD application) by an assumption of common trends in gradients.

[24] Again, our distinction between the treated and the untreated is "fuzzy". Households in expensive cities may call off buying a house due to subsidy repeal, while households in affordable cities might buy a house, nonetheless. Notwithstanding, we expect the rate of the treated in affordable cities to exceed that in non-affordable cities.

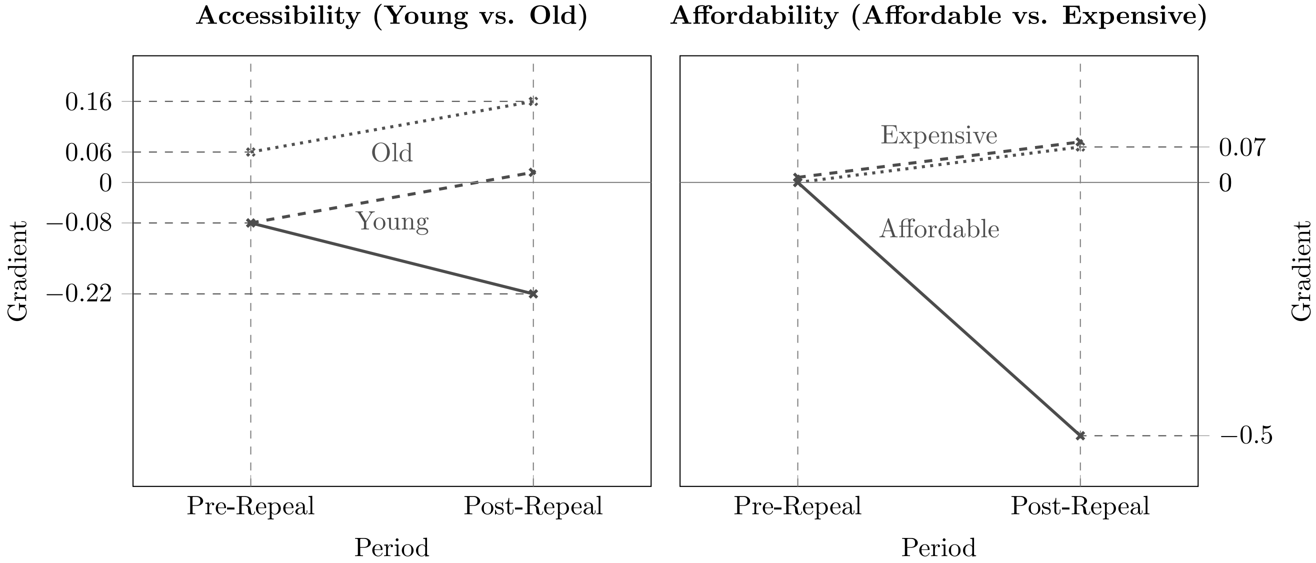
[25] This discussion appears to be at odds with the filtering model in Appendix B, which assumes a fixed city population. However, the filtering model's focus was on showing how a repeal of two related subsidies can be cast in terms of repealing a single subsidy. A full-fledged analysis can address both within-city-filtering and inter-city-migration (e.g., Dascher (2014)).

[26] 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.

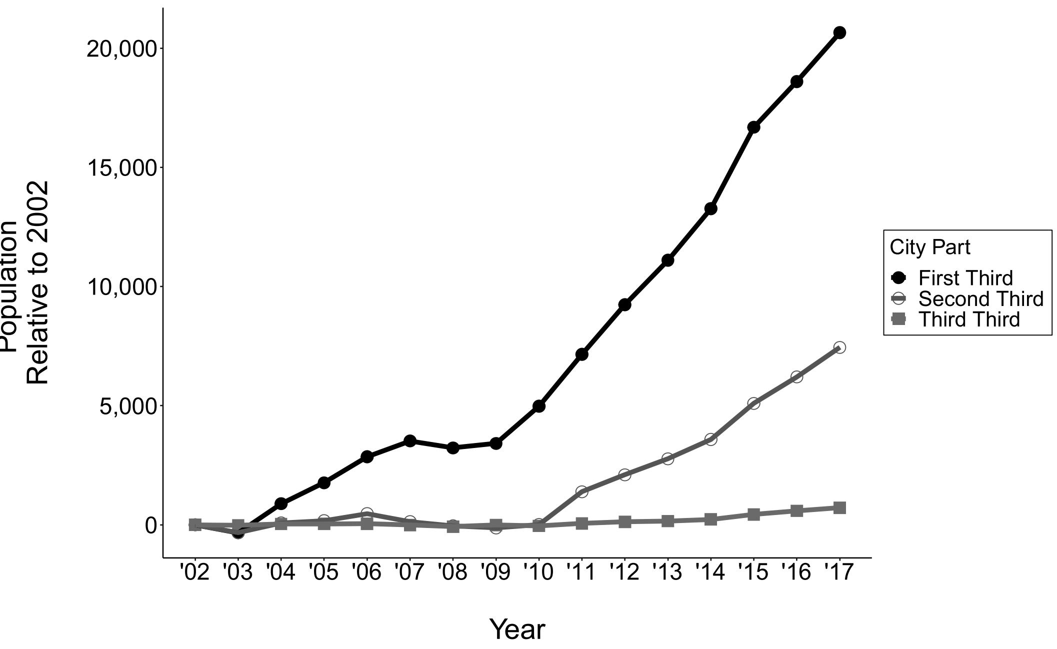
[27] I.e., $\hat{y}_{iy\bar{t}} = \hat{\alpha}_0 + \hat{\mu}_i + \widehat{\alpha}_1 DIST_j + \widehat{\alpha}_2 \left(DIST_j - \widetilde{r}_i/3\right) + \hat{\beta}_1 + \hat{\beta}_2 + \hat{\beta}_3$.

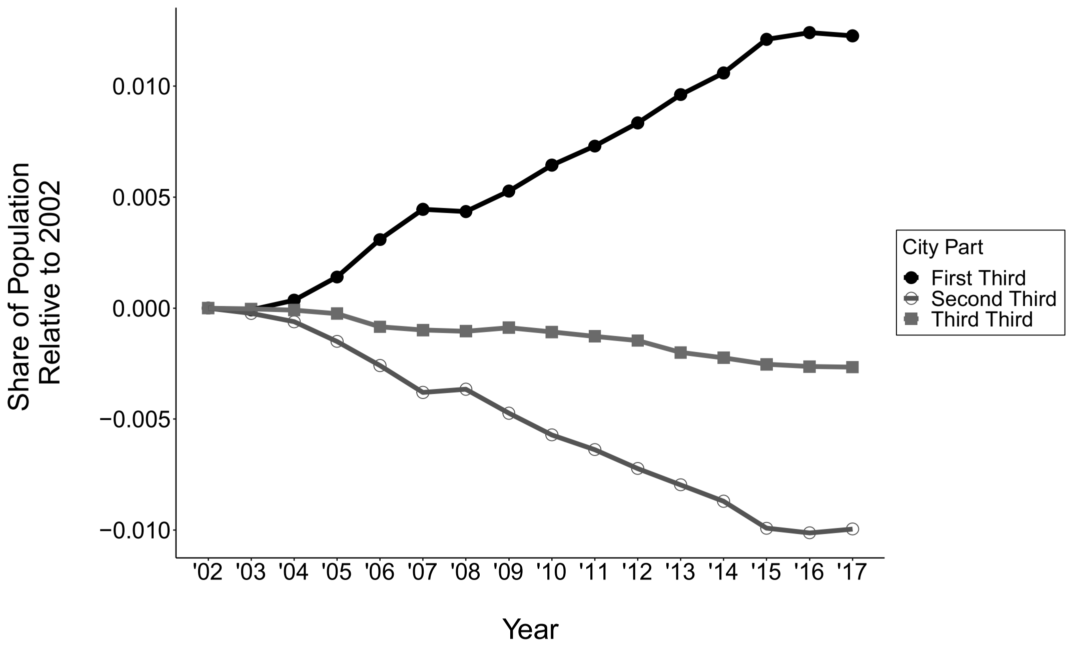
[28] That the vacancy rate in peripheries was zero is certainly not true. (These vacant (older) homes were of lower quality than the homes first-time buyers were observed to buy.) In any case, we do not, unfortunately, have ring-specific data on vacant housing.

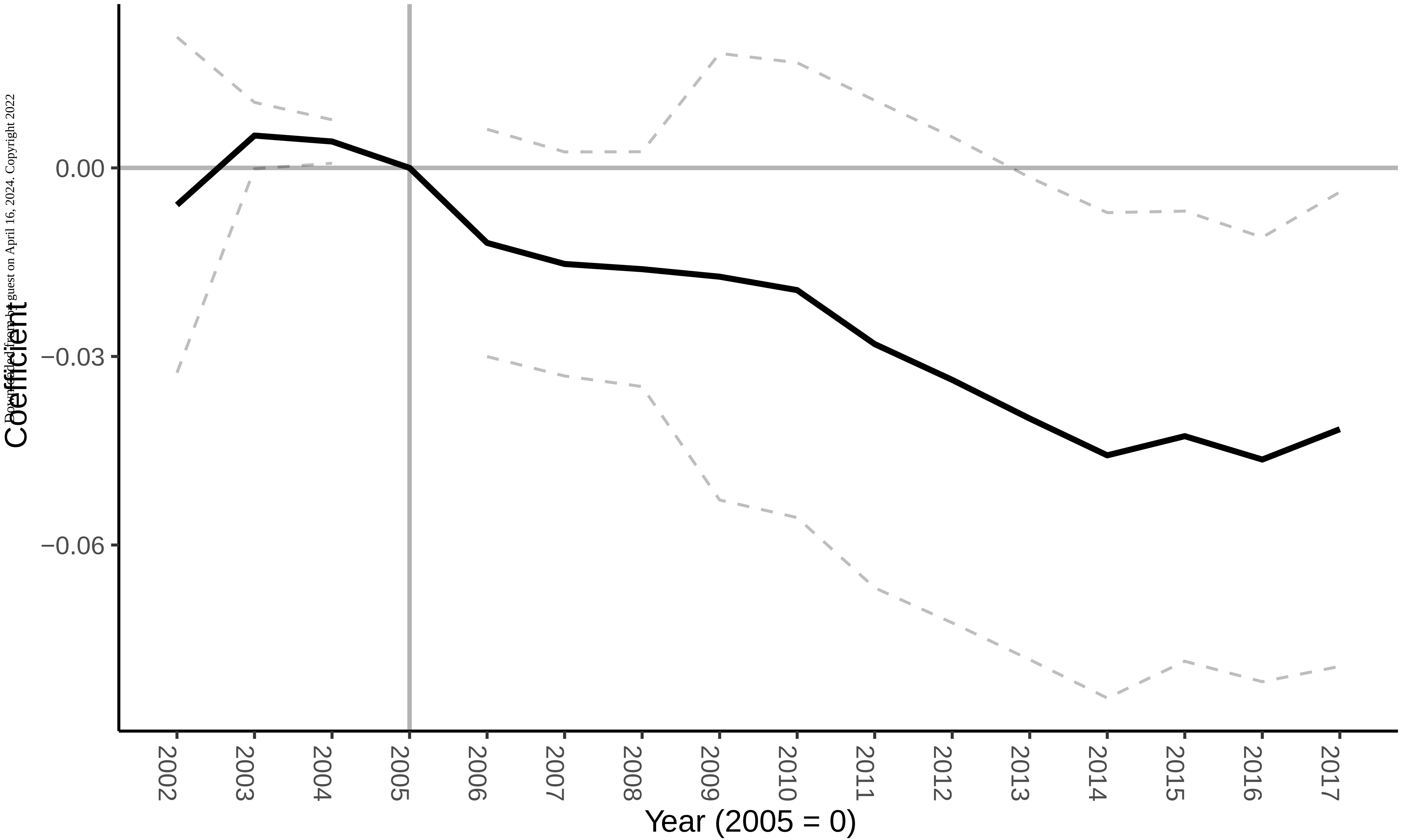
[29] Recall that, in Appendix B's model, the change in the equilibrium rental price is shown to be strictly positive, $dq_1 > 0$. Conversely, introducing the subsidy is easily shown to drive rents down, $dq_1 < 0$.



Accessibility (Young vs. Old)
Affordability (Affordable vs. Expensive)
0.16
0.06
0
−0.08
−0.22
Gradient
Old
Young
Pre-Repeal
Post-Repeal
Period
Expensive
Affordable
0.07
0
−0.5
Gradient
Pre-Repeal
Post-Repeal
Period


Population
Relative to 2002
20,000
15,000
10,000
5,000
0
'02 '03 '04 '05 '06 '07 '08 '09 '10 '11 '12 '13 '14 '15 '16 '17
Year
City Part
First Third
Second Third
Third Third


Share of Population
Relative to 2002
0.010
0.005
0.000
−0.005
−0.010
'02 '03 '04 '05 '06 '07 '08 '09 '10 '11 '12 '13 '14 '15 '16 '17
Year
City Part
First Third
Second Third
Third Third



Coefficient
0.00
−0.03
−0.06
2002
2003
2004
2005
2006
2007
2008
2009
2010
2011
2012
2013
2014
2015
2016
2017
Year (2005 = 0)