Homeowner Subsidy Repeal and Housing Recentralization

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Abstract: Subsidizing homeownership makes cities decentralize, so Muth (1967) suggested over half a century ago, and so Voith (1999) and Glaeser (2011) 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. 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.

Keywords: Homeownership Subsidy, Subsidy Repeal, Housing Recentralization, Global Warming, Suburban Land Use
JEL-Classifications: R12, D72, R52

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1 Introduction

Subsidizing homeownership makes cities decentralize, so Muth (1967) suggested over half a century ago, and so Voith (1999) and Glaeser (2011) 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Germany’s cities have recentralized conspicuously ever since the subsidy was repealed.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), or 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[2] (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.[3]

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 residents closer to the city center.

[2]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).

[3]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.

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. 1 showcases our estimates of all three differences, as obtained further below in the paper's empirical section.[4] 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? To address this concern, we turn from gradient changes to the *differences in changes of gradient* ("DDD"). In Fig. 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.

[4] That is, in subsections 4.1 and 4.2. Rather than show a "difference-in-differences-in-differences" of *population*, Fig. 1 shows the equivalent "difference-in-differences" of the *population gradient*.

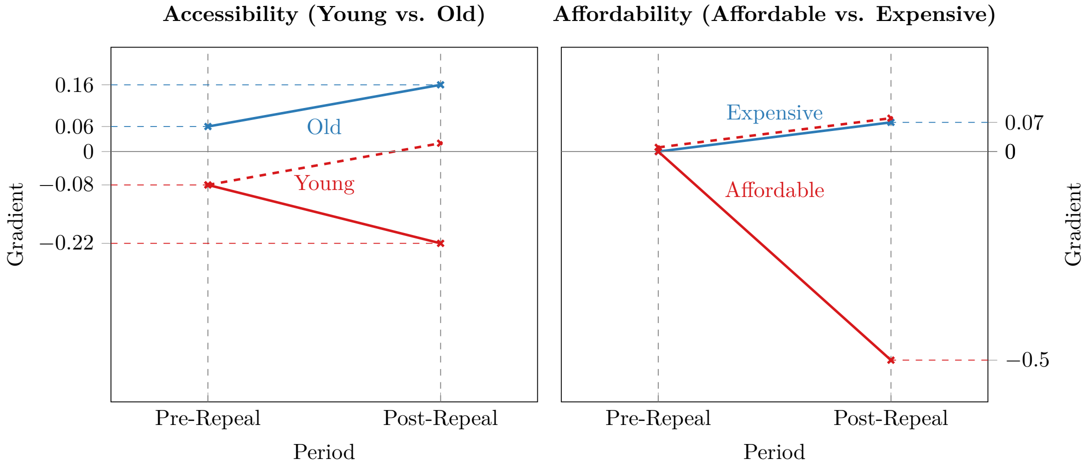


Figure 1: Population Gradient Before and After Repeal, By Treatment

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[5], 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

[5]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.

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 (Henninghausen 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 subsidy repeal's impact on urban form. Section 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 6 concludes.

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).[6] This paper exploits the transition from phase 2 to phase 3.

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

Table 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)	max {5.0% of q_3, €2,556}	max {5.0% of q_3, €2,556}	max {1.0% of q_3, €1,250}
Existing Property (q_2)	max {2.5% of q_2, €1,278}	max {2.5% of q_2, €1,278}	max {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, 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.

Table 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.[7] Common to all specifications for phase 2, households with children were always entitled to another €767 per child and year.[8]

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.[9] 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 $2,556 + 2 \cdot 767$ 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

[7] 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.

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

[9] 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.

only half as much.[10]

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, Sweeny(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.[11] 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 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.[12]

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.[13] 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 $\tilde{r}$ denote the maximum distance from the CBD to the city's administrative boundary, i.e. "city size". Then as r ranges from

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

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

[12] 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.

[13] 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.

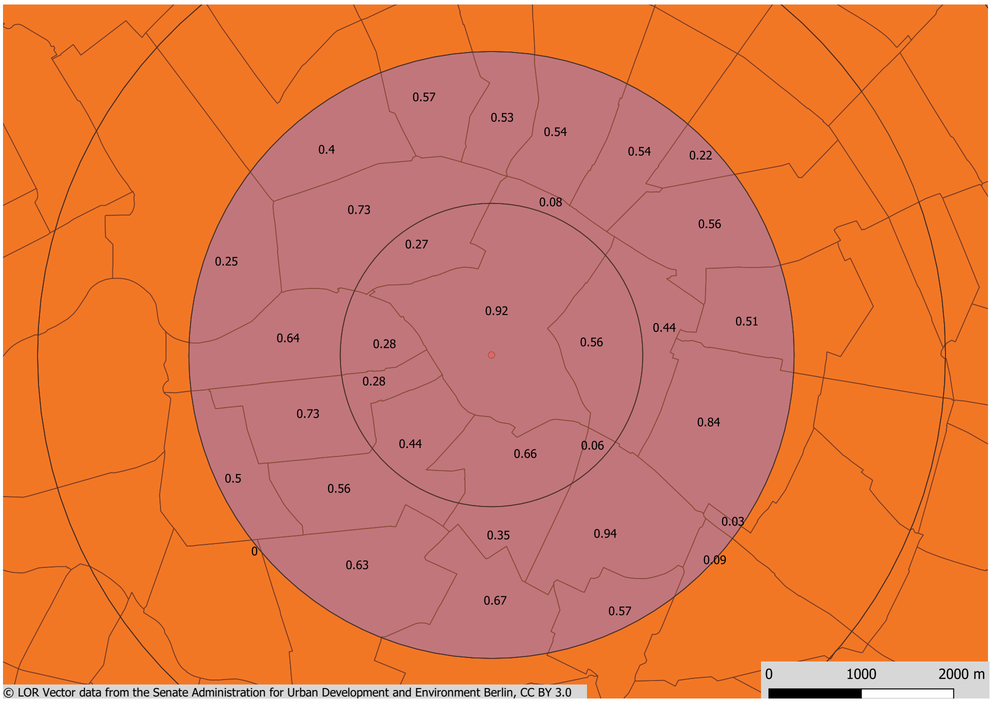


Figure 2: Shares α_{11} and α_{12} for Berlin's first two rings

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.

0 to $\tilde{r}$, $g(r)$ 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[14] and KOSTAT[15] for the largest German cities[16], and (in most cities) for all years 2002 through 2017. As the city's CBD we often (i.e. whenever possible) choose city hall.[17] Given the CBD's geo-coordinates, we next equate $\tilde{r}$ 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 then intersect this partition with the city shapefile polygons.[18] Fig. 2 gives one example of the

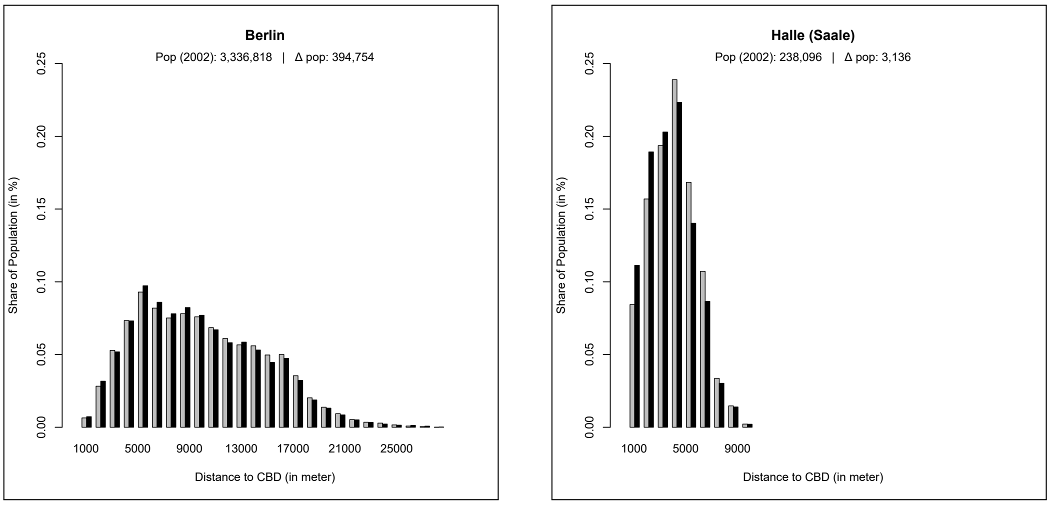
[14] BBSR: Bundesinstitut für Bau-, Stadt- und Raumforschung.

[15] KOSTAT: KOSIS-Gemeinschaft Kommunalstatistik.

[16] 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).

[17] 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.

[18] City shapefiles indicate subdivisions' polygonal boundaries. Where shapefiles are not publicly avail-



Berlin, 2002-2017 Halle, 2002-2017

Figure 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.

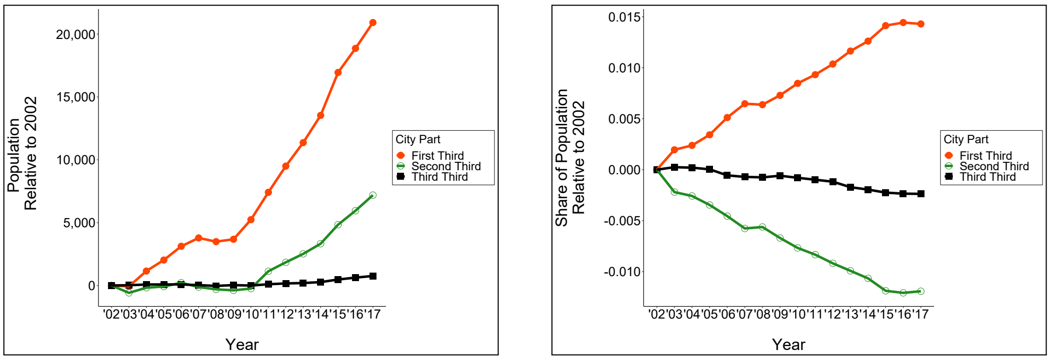
procedure, for Berlin's first two concentric rings around the historic city hall (itself shown as a small circle at the center of the map).

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.[19] 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 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. 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 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 4.2) our sample cities account for slightly over 22 million individuals (in 2002), and represent nearly one fourth of the country's population.

able we contacted municipal cadastral offices.

[19]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

Figure 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 and BBSR data.

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. 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.[20] So we alternatively cast our diagrams in terms of ring thirds' shares in city population (Fig. 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.

[20]This also is why we consistently add city fixed effects later.

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

$$g'(r) \;=\; 2\pi D(r) \;+ 2\pi r D'(r). \tag{1}$$

The first term on the r.h.s. of eq. 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.[21] Concavity captures the hump-shape we observed earlier, in Fig. 3. Setting eq. 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 "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 subsidy repeal (and 0 else). So our point of departure is the following diff-in-diff specification:

$$\begin{aligned} E(y_{ijt}|x_{ijt}) \;&=\; \alpha_0 + \mu_i + \alpha_1 \,\mathrm{DIST}_j \;+\; \alpha_2\,(\mathrm{DIST}_j - \widetilde{r}_i/3) \times \mathrm{PERI} \\ &+\; \beta_1\,\mathrm{PERI} \;+\; \beta_2\,\mathrm{POST} \;+\; \beta_3\,\mathrm{PERI} \times \mathrm{POST}, \end{aligned} \tag{2}$$

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$\times$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 shows our OLS coefficient estimates for eq. 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

[21] This assumption holds as long as the density profile $D(r)$ is not too convex in r.

Table 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. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

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. 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 on replacing POST with a full set of yearly dummies D_t, then estimating

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

Fig. 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

Figure 5: Additions to Population Gradient between 2002 and 2017

Note: This figure shows the estimated coefficients of γ_t from Equation 3. For this regression we restrict our sample to the 57 cities for which we have gap-free data from 2002–2017 (see Table 7 in the Appendix). Data: Authors' calculations using BBSR and KOSTAT data.

joint with repeal. For example, the 7%-decrease of the population gradient according to Table 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 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, must not (subsection 4.2).

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

Table 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. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

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 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):

$$\begin{aligned} E(y_{ijt}|x_{ijt}) &= \alpha_0 + \mu_i + \alpha_1 \,\mathrm{DIST}_j + \alpha_2 \,(\mathrm{DIST}_j - \widetilde{r}_i/3) \times \mathrm{PERI} \\ &+ \beta_1 \,\mathrm{POST} + \beta_2 \,\mathrm{YOUNG} + \beta_3 \,\mathrm{PERI} \\ &+ \gamma_1 \,\mathrm{POST} \times \mathrm{YOUNG} + \gamma_2 \,\mathrm{POST} \times \mathrm{PERI} + \gamma_3 \,\mathrm{YOUNG} \times \mathrm{PERI} \\ &+ \delta \;\mathrm{POST} \times \mathrm{PERI} \times \mathrm{YOUNG}. \end{aligned} \tag{4}$$

In eq. 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

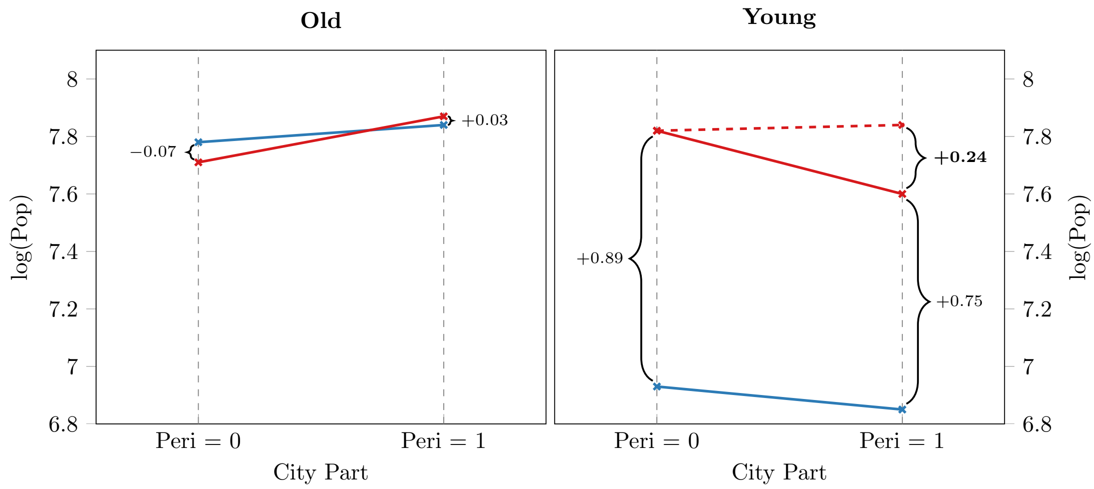


Figure 6: Population Gradients for the Old and Young

Note: This figure illustrates the estimates from Table 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.

scrutiny. We expect $\delta < 0$, i.e. that whatever change in gradient the young undergo to fall short of (be smaller than) the change in gradient undergone by the old. Now, from the first column of Table 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. 3).[22] 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.

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 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

[22] Fig. 1's panel on the left, in the introduction, provides an alternative, yet equivalent, picture.

Table 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. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

design (section 2 and Table 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 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). 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. 7 illustrates these estimates.[23] 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 percentage 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

[23] Again, Fig. 1's panel on the right, in the introduction, provides an alternative, yet equivalent, picture.

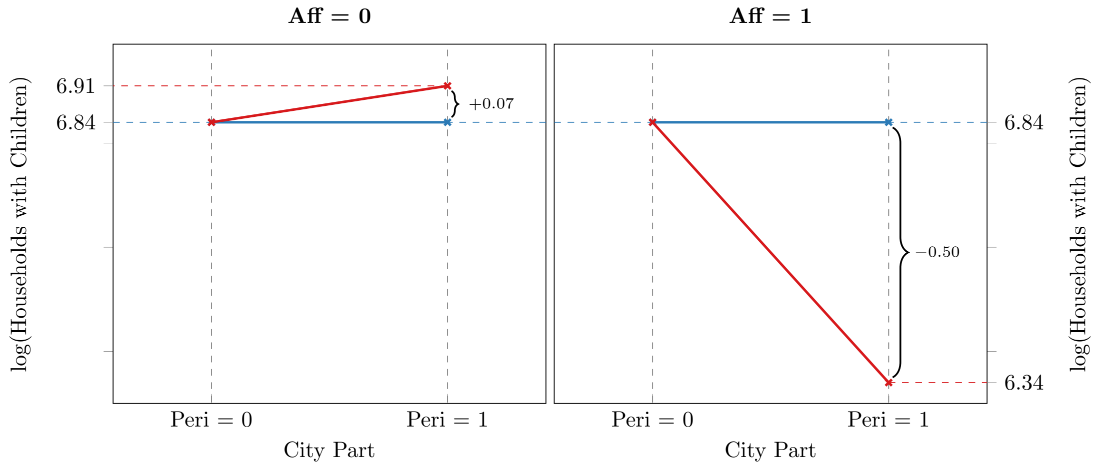


Figure 7: Population Gradients for Affordable and Expensive Cities

Note: This figure shows the results from Table 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.

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.[24] The coefficient estimate on PERI×POST×AFF remains negative, significant, and substantial, throughout.

Table 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 × POST × AFF parallel those from Table 4.

Appendix C offers various robustness checks. First, Table 8 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 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

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

Table 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. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

in the sample and in the year predating our analysis (i.e. in Munich)). Tables (9) to (11) 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 12 and 13 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.

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).[25]

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 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 2020; Couture and Handburg 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 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 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. 1 indicates this counterfactual change in slope; while that of Fig. 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).[26] Then

$$e^{\widehat{y}_{ij\bar{t}}} \; - \; e^{(\widehat{y}_{ij\bar{t}} - 0.24)} \tag{5}$$

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

[25] Ahlfeldt and Maenning (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.

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

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 been vacant in city peripheries.[27]

Alternatively, consider our "affordability" estimates from subsection 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 5). Again, dashed red lines in Fig. 1 and 3 indicate counterfactuals. In Fig. 1's panel on the right, the dashed line shows the counterfactual change in the gradient for households with children; while in Fig. 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 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 paper's Appendix), 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).[28] 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,[29] 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 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).

[27] 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.

[28] 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, $dq_1 < 0$.

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

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Appendices

A 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

$$\begin{aligned} \underline{\theta}(q_1, q_2 - \sigma_2) &= (q_2 - \sigma_2 - q_1)/(s_2 - s_1) &\quad (6) \\ \overline{\theta}(q_2 - \sigma_2, q_3 - \sigma_3) &= (\overline{q}_3 - q_2 - (\sigma_3 - \sigma_2))/(s_3 - s_2). &\quad (7) \end{aligned}$$

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:

$$\begin{aligned} n_{11} &= f(\underline{\theta})\,\underline{\theta}_1 < 0 \quad \text{and} \quad n_{12} = f(\underline{\theta})\,\underline{\theta}_2 > 0 &\quad (8) \\ n_{21} &= -f(\underline{\theta})\,\underline{\theta}_1 > 0 \quad \text{and} \quad n_{22} = (f(\overline{\theta})\overline{\theta}_2 - f(\underline{\theta})\,\underline{\theta}_2) < 0, &\quad (9) \\ &\qquad\qquad\qquad\quad\;\; \text{and} \quad n_{23} = -f(\overline{\theta})\,\overline{\theta}_3 > 0, &\quad (10) \end{aligned}$$

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.[30] 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 interconnected segments of apartments and existing homes as follows.

$$\begin{aligned} n_1(q_1, q_2 - \sigma_2) &= s_1(q_1) \\ n_2(q_1, q_2 - \sigma_2, \overline{q}_3 - \sigma_3) &= s_2(q_2), &\quad (11) \end{aligned}$$

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 $\sigma_2 < \sigma_3$.[31] We are interested in these policy changes' effects

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

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

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

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

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. 8 or 9. 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.[32]

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

$$\begin{aligned} dq_1 &= g_{11}\, n_{12}\, d\sigma_2 + g_{12}\, n_{22}\, d\sigma_2 + g_{12}\, n_{23}\, d\sigma_3 \\ &= \underbrace{f(\underline{\theta})\, \underline{\theta}_2\, d\sigma_2}_{-} \underbrace{(g_{11} - g_{12})}_{-} + \underbrace{g_{12}\, f(\bar{\theta})\, \bar{\theta}_2}_{+} \underbrace{(d\sigma_2 - d\sigma_3)}_{+} > 0, \end{aligned} \tag{13}$$

where the first and last term on the first line of (13) 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 (13), to also sign dq_1 as positive nonetheless.

Replacing n_{12}, n_{22} and n_{23} on the first line of (13) by making use of (8) through (10), exploiting $\bar{\theta}_2 = -\bar{\theta}_3$, and also rearranging translates into the second line of (13). Given Sweeney's first property, i.e. $g_{11} < g_{12}$, the first term on the r.h.s. of the second line of (13) 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 (13) is positive also. Thus $0 < dq_1$.

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

$$0 < dq_1 < d(q_2 - \sigma_2) < d(\bar{q}_3 - \sigma_3). \tag{14}$$

B 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.

[32]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 $g_{21}/g_{22} < 1$.

Table 6: 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.

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.

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".

Table 7: 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

Sample of Cities (continued)

No.	City	Years	$\tilde{r}$	Affordable	Price	$\overline{\text{Age}}_{02}$
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	–
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).

C Robustness

Table 8: 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. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table 9: 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. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table 10: 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. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table 11: 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. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table 12: Ring Households with Children with spline knot at $\tilde{r}/4$

	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. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table 13: Ring Population with spline knot at $\tilde{r}/4$

	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. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.