

# The Impact of Upzoning on Housing Construction in Auckland\*

Ryan Greenaway-McGrevy<sup>†</sup>      Peter C. B. Phillips<sup>‡</sup>

March 2023

## Abstract

There is a growing debate about whether upzoning is an effective policy response to housing shortages and unaffordable housing. This paper provides empirical evidence to further inform debate by examining the various impacts of recently implemented zoning reforms on housing construction in Auckland, the largest metropolitan area in New Zealand. In 2016, the city upzoned approximately three quarters of its inner suburban land to facilitate construction of more intensive housing. We use a quasi-experimental approach to analyze the short-run impacts of the reform on construction, allowing for potential shifts in construction from non-upzoned to upzoned areas (displacement effects) that would, if unaccounted for, lead to an overestimation of treatment effects. We find strong evidence that upzoning stimulated construction. Treatment effects remain statistically significant even under implausibly large displacement effects that would necessitate more than a seven-fold increase in the trend rate of construction in control areas under the counterfactual of no-upzoning. Our findings support the argument that upzoning can stimulate housing supply and suggest that further work to identify factors that mediate the efficacy of upzoning in achieving wider objectives of the policy would assist policymakers in the design of zoning reforms in the future.

*Keywords:* Upzoning, Land Use Regulations, Redevelopment, Housing Construction, Housing Affordability, Spillovers.

*JEL Classification Codes:* R14, R31, R52

---

\*We thank Auckland Council for providing the building permit and urban extent datasets, and Land Information New Zealand for providing the land parcel dataset. This research was funded by the Royal Society of New Zealand under Marsden Fund Grant UOA2013. Phillips also acknowledges research support from the NSF under Grant No. SES 18-50860 and a Kelly Fellowship at the University of Auckland. We thank two anonymous referees and the Editor for their many helpful suggestions.

<sup>†</sup>University of Auckland. Corresponding author. Postal address: The University of Auckland, Private Bag 92019 Auckland 1142, New Zealand. Email: r.mcgregvy@auckland.ac.nz.

<sup>‡</sup>Yale University, University of Auckland, Singapore Management University, and University of Southampton. Email: peter.phillips@yale.edu.

# 1 Introduction

Housing has become prohibitively expensive in many of the world’s major cities, precipitating serious and widespread housing affordability crises (Wetzstein, 2017). A growing coalition of researchers argue that part of the solution is to ‘upzone’ cities by relaxing land use regulations (LURs) to allow construction of more intensive housing, such as townhouses, terraced housing and apartment buildings (Glaeser and Gyourko, 2003; Freeman and Schuetz, 2017; Manville et al., 2019). Policy-makers have begun to listen to these supply-side solutions and, in response, several metropolitan and gubernatorial authorities have pursued zoning reform in recent years (National Public Radio, 2019).

These policy reforms are underpinned by the argument that LURs increase house prices by restricting housing supply (Glaeser et al., 2005; Quigley and Raphael, 2005; Quigley and Rosenthal, 2005; Ihlanfeldt, 2007; Zabel and Dalton, 2011; Gyourko and Molloy, 2015). Relaxing those regulations through upzoning, it is argued, enables new more intensive development, thereby increasing housing supply and putting downward pressure on prices. However, these arguments are not universally accepted and many commentators remain skeptical of the capacity for these market-led policies to deliver affordable and inclusive housing (Rodríguez-Pose and Storper, 2020; Wetzstein, 2021). Instead it is suggested that government intervention is needed to tackle the problem through means such as state-led construction (Wetzstein, 2021; Favilukis et al., 2023), the repurposing of public space (Wetzstein, 2021; Freemark, 2021), and policies that limit demand (Wetzstein, 2021).

Unfortunately our understanding of the manifold impact of upzoning is presently limited by an acute lack of empirical research on the subject (Schill, 2005; Freeman and Schuetz, 2017; Freemark, 2019). Only a handful of studies have offered empirical evidence of the effects of a relaxation of LURs (Atkinson-Palombo, 2010; Freemark, 2019; Gray and Millsap, 2020; Limb and Murray, 2022; Peng, 2023), and these have tended to concentrate on small-scale policy changes, often involving transit-oriented rezoning, not the large-scale policy reforms currently being implemented in many US cities.<sup>1</sup> The limited empirical work that is available has findings that often contravene the outcomes anticipated by proponents of supply-side regulatory reforms. For example, Freemark (2019) found that transit-oriented upzoning in Chicago failed to encourage construction, calling into question the fundamental premise of the supply-side argument (Rodríguez-Pose and Storper, 2020). Meanwhile, Limb and Murray (2022) argue that transit-oriented upzoning in Brisbane, Australia, generated no significant increase in housing construction.

The lack of empirical evidence on the effects of large-scale upzoning is largely due to the fact that, until very recently, no city has systematically upzoned large shares of land as a mechanism to promote affordability (Freeman and Schuetz, 2017, p. 229). In 2016, however, the city of Auckland, New Zealand, implemented large-scale zoning reforms under the Auckland Unitary Plan (AUP). Motivated by housing affordability concerns (Auckland Unitary Plan Independent Hearings Panel, 2016), the plan removed single dwelling restrictions on three-quarters of residential land and trebled

---

<sup>1</sup>Gray and Millsap (2020) is an exception, showing that the 1998 reduction in minimum lot sizes in Houston preceded an increased concentration of development activity in middle-income, less dense, under-built neighborhoods. The net effect of the reforms on the aggregate housing stock or building permits is unaddressed.

the number of dwellings that could be built within the residential areas (Greenaway-McGrevy et al., 2021), providing researchers with a unique opportunity to study large-scale upzoning reform of the kind that was hitherto lacking.

The present paper examines impact of upzoning on housing construction in Auckland to provide further evidence on whether zoning reforms can fulfill the fundamental premise of the supply-side policy response, namely, that upzoning increases housing supply. A quasi-experimental framework is adopted that exploits geographic variation in the incidence of upzoning to estimate causal effects through the comparison of outcomes in upzoned areas with outcomes in non-upzoned areas. Our dataset consists of geocoded building permits that are matched to planning maps that detail the geographic incidence and intensity of upzoning.<sup>2</sup> Because the empirical design exploits temporal changes in zoning rules via a policy intervention, it has the capacity to mitigate many of the concerns stemming from the endogeneity of regulations that afflict studies which rely only on spatial variation in LURs (Gyourko and Molloy, 2015). By exploiting time series and spatial variation in LURs, we are able to allow for a wider set of time-invariant confounders since their impact on outcomes is differenced out in the DID procedure. The approach is concordant with other work where changes in the geographic variation in zoning has been used in quasi-experimental designs to examine casual impacts (Thorson, 1997; Cunningham, 2006; Zhou et al., 2008; Kahn et al., 2010; Freemark, 2019).

Our empirical strategy pays particular attention to the possibility that housing construction in upzoned areas displaced housing that would have otherwise been constructed in non-upzoned areas.<sup>3</sup> These displacement effects would manifest as negative spillovers from treatment areas to control areas, violating the stable unit treatment value assumption (SUTVA) in the Neyman-Rubin causal framework, and generating an overstatement of conventional treatment effects that are based on simple comparisons of outcomes in treatment and control groups. Present techniques for addressing spatial spillover effects require the spillovers to be localized, in the sense that the magnitude of the spillover between geographically distant areas is assumed to be negligible (Clarke, 2017; Butts, 2021; Huber and Steinmayr, 2021). As we demonstrate below, the evidence suggests that upzoning in Auckland reallocated permits over large distances (from distant areas with more vacant land to near areas with less vacant land), meaning that methods that model spillovers under the assumption that they are highly localized and dissipate with distance are untenable in our setting. We therefore develop an approach to accommodating spillover effects that does not rely on spillovers dissipating with distance.

We adapt the set identification approach suggested by Rambachan and Roth (2023) (hereafter ‘RR’) for remediating violations of the standard parallel trends (or ‘pre-trends’) assumption that is required under the difference-in-differences (DID) framework. RR extrapolate pre-treatment trends to generate a set of counterfactual outcomes in the treatment group. In the present paper we repurpose this strategy by using pre-treatment trends in the *control* group to extrapolate a set of

---

<sup>2</sup>Throughout the paper, “permits” refers to permitted housing units or, in the New Zealand parlance, “consented dwellings”.

<sup>3</sup>Spatial displacement effects go by varying terms in the urban literature. Neumark and Simpson (2015) characterise displacement effects from place-based policies as negative spillovers, while Redding and Turner (2015) refer to “reorganisation” effects in response to transport policies.

counterfactual outcomes that are used to bound the magnitude of the spillover effect. The intuition underpinning both the RR strategy and our strategy is the idea that observed trends immediately prior to the policy intervention are informative of the counterfactual scenario. Adapting the RR method to our application yields a confidence set of treatment effects that is robust to spillover effects and amenable to inference.

The empirical findings using this methodology reveal strong statistical evidence that upzoning increased housing construction. Our preferred model specification shows a statistically significant increase in permits even under counterfactual sets that span approximately seven times the extrapolated linear trend in the control group. For example, a linear trend fitted to pre-treatment observations in the control group implies that 1,118 additional dwellings would have been built in non-upzoned areas in 2021 under the counterfactual of no upzoning. We find that the estimated treatment effect for 2021 remains statistically significant even when we allow an additional 8,768 dwellings in the counterfactual set of outcomes. Put differently, counterfactual scenarios that imply more than a seven-fold increase in permits over the pre-treatment trend (since  $7.84 = 8,768/1,118$ ) would be needed in order for the estimated treatment effects to become statistically insignificant. There is no policy change concurrent to the upzoning policy that could plausibly generate such a substantive increase in construction.

We also use the extrapolated counterfactual trend in control areas to generate a spillover-robust estimate of the number of additional dwellings enabled through upzoning. To do so, we restrict the counterfactual set to the extrapolated linear trend, so that the set collapses to a point. This approach implies that 27,183 additional dwellings were permitted over the five years following upzoning, corresponding to approximately 5.13% of the dwelling stock of the Auckland region.<sup>4</sup>

Our results have implications for ongoing debates about the efficacy of upzoning. In particular, the findings support the view that large-scale upzoning can encourage construction. This is particularly important in the light of recent work by [Freemark \(2019\)](#) and [Limb and Murray \(2022\)](#), who find that zoning changes had minimal impact on housing construction in Chicago and Brisbane, respectively. Further work examining potential mediating factors that enabled increased construction will hopefully help explain why the policy was more effective in Auckland, and assist policymakers in tailoring rezoning and housing policies to facilitate construction elsewhere.

The remainder of the paper is organized as follows. Section two provides the background institutional context and timeline of the key events in the city of Auckland and section three describes the dataset used in our empirical work. Section four presents the empirical DID model. Section five describes and applies our methodology for dealing with potential spillover effects, and presents spillover-robust estimates of treatment effects. Section six concludes.

---

<sup>4</sup>Unfortunately we do not have precise measures of dwellings demolished when properties are redeveloped. This is because demolition permits are only required for buildings that are less than three storeys.

## 2 Institutional Background

This section provides some background demographic and administrative features of Auckland city with information concerning relevant policies and processes preceding the relaxation of land use regulations under the Auckland Unitary Plan. It also shows how the policy informs our empirical design.

Auckland is the largest city in New Zealand with a population of approximately 1.5 million within the greater metropolitan region (as of 2017). Prior to 2010, the metropolitan region comprised seven different District Councils. Since 2010, the entire metropolitan area, as well as several towns, populated islands, and a large amount of the rural land beyond the fringes of its outermost suburbs, has been under the jurisdiction of a single local government, the Auckland Council. Centered on a long isthmus of land between two harbors, this jurisdiction extends over 4,894 km<sup>2</sup> of land area. Figure 1 illustrates the Auckland region, decomposed into Statistical Area (SA) units, which, as discussed below, are used as the geographic unit of analysis. The shaded areas are the Auckland Council region. The lighter shaded area in the center is the urban core.<sup>5</sup>

[Insert Figure 1 here]

In March 2013, the Auckland Council announced the ‘draft’ version of the Auckland Unitary Plan (AUP). The draft version of the plan went through several rounds of consultations, reviews and revisions before the final version became operational on 15 November 2016. Each version of the AUP contained new Land Use Regulations (LURs) that would potentially change restrictions on the extent of site development. In most areas these LURs were relaxed in order to enable residential intensification and greater population density, including multi-family housing such as terraced housing and apartments. These proposed changes could be viewed online, so that any interested member of the public could observe the specific LURs proposed for a given parcel of land. This meant that it was relatively simple for developers to observe the new land use regulations and to commence planning prior to the policy becoming operational.

The amount of development allowed on a given site is restricted by the LURs of its assigned planning zone. In this study we focus on four residential zones introduced under the AUP, listed in declining levels of permissible site development: Terrace Housing and Apartments (THA); Mixed Housing Urban (MHU); Mixed Housing Suburban (MHS); and Single House (SH). Thus THA permits the most site development, and SH permits the least. Table 1 summarizes the various LURs for each of the four residential zones considered. These regulations include site coverage ratios, height restrictions, setbacks and building envelopes, among others. For example, five storeys and a maximum site coverage of 50% is allowed in THA, whereas only two storeys and 35% site coverage is allowed in SH.<sup>6</sup>

---

<sup>5</sup>For the urban core, we use Statistical Areas inside or overlapping the ‘Major Urban Area’ of the greater Metropolitan region, as defined by Statistics New Zealand.

<sup>6</sup>There are two additional zones in the AUP that are classified as “Residential”: “Large Lot” and “Rural and

[Insert Table 1 here]

Figure 2 depicts the geographic distribution of the four zones. For clarity we zoom in on the central urban area of Auckland. Evidently MHS covers the most area, closely followed by MHU. SH is predominantly located either very close to the Auckland city central business district (CBD) or at the outskirts of the city. THA covers the least amount of area. Parcels zoned as THA, MHU or MHS collectively comprise 75.1% of residential land, while SH accounts for the remaining 24.9%. Within what we refer to as the urban core of Auckland (the yellow area in Figure 1), THA, MHU and MHS account for 84.5% of residential land.<sup>7</sup>

[Insert Figure 2 here]

Our empirical design uses the introduction of the AUP as a quasi-natural experiment in which residential areas that were upzoned to either MHS, MHU or THA are designated as treatment areas, while residential areas that were not upzoned are control areas. In order to identify upzoned areas, we compare the LURs that applied before and after the AUP. Prior to the AUP, each of the seven city and district councils set their own zoning regulations, which remained in effect until the AUP became operational in 2016. Upzoned areas are identified by comparing the maximum floor area ratios (FARs) under the previous zone to those that applied under the zone introduced under AUP. Areas where the FAR increased are classified as upzoned. Although neither the AUP nor the seven city and district plans placed direct restrictions on FARs, all sets of plans imposed site coverage ratios and height limits that cap the maximum allowable FAR on a parcel. Under a plausible mapping of building height to storeys, the FAR is straightforwardly the product of the storey limit and the site coverage ratio. Further details on the classification method are given in the Appendix.<sup>8</sup>

Using this approach, approximately 98.3% of the land zoned as MHS, MHU or THA under the AUP is classified as upzoned, and 96.2% of residential land had a FAR no greater than that of SH under the AUP. Thus, while the previous set of regulations did allow for housing intensity that was roughly equivalent to MHS, MHU or THA, these zones were restricted to very small, targeted

---

Coastal Settlement". We exclude these areas from our analysis as they are an intermediate, semi-rural zone between outright rural and urban housing areas. We also omit residential land on the islands in the Hauraki Gulf, which have their own unique zoning under the AUP.

<sup>7</sup>Areas calculated using geocoded dataset of cadastral land parcels, defined as at November 2016.

<sup>8</sup>FARs are often used as a measure of LUR stringency; see Brueckner et al. (2017), Brueckner and Singh (2020) and Tan et al. (2020). By using a FAR, our upzoning identification algorithm is based on increases in maximum floorspace density, rather than restrictions on dwelling density, such as minimum lot sizes (MLS). While the majority of the zones prior to the AUP also had MLS in addition to height and site coverage restrictions, MLS do not apply to extant residential parcels under the AUP. MLS on extant parcels were therefore an additional restriction that only applied prior to the AUP, which means that our upzoning classification method may understate the amount of upzoned land.

locations. The vast majority – over 98% – of the land designated as either THA, MHU or MHS under the AUP was upzoned.<sup>9</sup>

Our sample period spans several demand-side policies intended to curb housing demand to promote affordability, including severe restrictions on foreign ownership, disincentives to investor speculation, and macroprudential banking restrictions<sup>10</sup>. Although these policies frequently exempted new builds, there is little reason to think that these policies would differentially impact upzoned areas. Moreover, as we illustrate below, the parallel trends assumption holds remarkably well in our sample, affirming that these policies did not have a differential impact. On the supply side, prior to the AUP, ‘Special Housing Areas’ (SpHA) incentivized developers to provide some housing units at below-market prices in exchange for accelerated processing of building permits. To guard against this policy having an impact on our findings, we exclude permits issued within SpHAs prior to 2017, as the AUP superseded SpHAs once it became operational in 2016. A robustness check reported in the appendix demonstrates that our findings are largely unaffected when these permits are included in the analysis.

### 3 Dataset

Our dataset is based on annual building permits for new dwelling units issued by the Auckland Council from 2010 to 2021.<sup>11</sup> The permits include the number of dwellings. Each observation includes the longitude and latitude of the building site, which have been used to map each permit to its corresponding zone under the Unitary Plan.

Figure 3 exhibits the aggregate permits in non-upzoned areas compared to upzoned areas over the 2010 to 2021 period. We also decompose permits into attached and detached dwellings. There is a clear increase in the number of permits in upzoned areas after the policy is implemented (from 2017 onward). The number of attached dwelling permits per year in upzoned areas increases from under 1,000 in 2016 to near 10,000 by 2021 – more than a tenfold increase. Over the same period, detached housing increases from just over 2,000 permits per year to approximately 4,500. By 2019, there were more attached dwelling permits than detached, consistent with the upzoning goal of incentivising more capital intensive structures. In addition, there is an interesting fall in detached dwelling permits between 2019 and 2021 in upzoned areas.

Prior to treatment, permits in subsequently upzoned areas consistently exceeded permits in non-upzoned areas by between 1400 to 1900 permits annually. This difference remains remarkably consistent even as permits in non-upzoned areas more than doubled between 2011 and 2015. These patterns are consistent with modeling permits in levels in the DID framework, since it implies an

---

<sup>9</sup>We also consider empirical designs in which THA, MHU and MHS are simply classified as the treatment group and SH areas are the control group, and where downzoned areas are excluded from the analysis. Our results and findings do not substantively change.

<sup>10</sup>Loan to value ratios on new residential mortgages were introduced in 2013; a limited capital gains tax was introduced in 2015; and legislation preventing foreign ownership (excepting Australia and Singapore) in 2018. See [Greenaway-McGrevy and Phillips \(2021\)](#) for additional details.

<sup>11</sup>Permits for extensions to existing dwellings are not included in our analysis.



absolute difference in the level of the two series under the counterfactual (Kahn-Lang and Lang, 2020).

[Insert Figure 3 here]

There is a notable decrease in the number of permits in the non-upzoned areas after 2015. Permits follow a steady upwards trend until 2015. Thereafter there is a discrete shift as permits trend downwards. This break in trend is statistically significant (refer to the Appendix for details). This outcome is consistent with a negative spillover effect, as construction that would otherwise have occurred in non-upzoned areas may have been relocated to upzoned areas as a result of the treatment. The outcome is mainly driven by a decline in detached dwelling permits, which lends further support to the negative spillover interpretation of the switch in trend from 2015.

Economic theories of urban development suggest that upzoning will encourage construction in desirable locations where zoning regulation was previously binding, such as areas close to job locations. The canonical Alonso-Muth-Mills (AMM) spatial equilibrium model of the monocentric city predicts that a relaxation of Land Use Regulations (LURs) will result in more housing close to the city to the center (Bertaud and Brueckner, 2005). To explore whether this is the case in Auckland, Figure 4 divides the sample into urban core and non-core areas of Auckland (see Figure 1 above for the geographic delineation of the urban core and non-core areas). Consistent with this prediction, most of the increase in permits in upzoned areas is occurring in the urban core.

[Insert Figure 4 here]

Figure 5 breaks down the upzoned areas into constituent Terraced Housing and Apartments (THA), Mixed Housing Urban, (MHU) and Mixed Housing Suburban (MHS) zones. Despite having more restrictive constraints than MHU and THA, it is unsurprising that MHS accounts for most of the increase in permits because it covers the largest geographic area.

[Insert Figure 5 here]

## 4 Empirical Model and Results

Let  $y_{i,j,t}$  denote the (log) number of permits in zone  $j$  in area  $i = 1, \dots, n$  in period  $t = -\underline{T}, \dots, 0, \dots, \bar{T}$ , where  $\underline{T}$  denotes the number of time series observations prior to the treatment, and  $\bar{T}$  denotes the number of time series observations post-treatment. Treatment occurs in period  $t = 0$ . We use  $j = 0$  to indicate the control group (i.e, permits in non-upzoned areas) and  $j = 1$  to signify the



treatment group (permits in upzoned areas). The causal impact of upzoning is then estimated using a multi-period difference-in-differences (DID) specification of the form

$$y_{i,j,t} = \alpha_{i,j} + \sum_{s=-\underline{T}, s \neq 0}^{\bar{T}} \phi_s \mathbf{1}_{s=t} + \sum_{s=-\underline{T}, s \neq 0}^{\bar{T}} \beta_s \mathbf{1}_{s=t, j=1} + \varepsilon_{i,j,t} \quad (1)$$

where  $\alpha_{i,j}$  are suburb-zone fixed effects,  $\phi_s$  are time  $s$  specific fixed effects, and  $\mathbf{1}_{s=t}$  are indicators for each time period except the period of implementation, which occurs at  $t = 0$ .  $\mathbf{1}_{s=t, j=1}$  are indicators for each time period (except the period of policy implementation) interacted with the treatment effect indicator. Thus  $\{\beta_s\}_{s=1}^{\bar{T}}$  represent the treatment effects over time of upzoning. The empirical estimates of these parameters capture the increase in permits in treatment areas relative to the control areas in each period after upzoning is implemented. Following convention, estimates of  $\{\beta_s\}_{s=-\underline{T}}^{-1}$  will be used to assess whether any potential confounding pre-treatment trends are evident. The time specific fixed effects  $\phi_s$  measure outcomes in the control group relative to the treatment period  $t = 0$ . They also capture common variation in permits across different zones and suburbs that is due to macroeconomic or city-wide shocks and policy changes.

We use “Statistical Areas” (SAs), which are similar to census tracts in the US, as the geographic unit of analysis indexed by  $i$ . SAs have a target population of between 2000-4000 people in cities (such as Auckland) and were drawn to reflect communities that interact socially and economically. There are 510 SAs in Auckland. <sup>12</sup>

One of the strengths of DID is that spatial and time series variation in Land Use Regulations (LURs) can be used to control for a wide set of time invariant confounding factors. Although studies frequently employ spatial discontinuity designs to control for these confounders by assuming they are equally salient on either side of the boundary (Turner et al., 2017; Anagol et al., 2021), these approaches necessarily constrain the geographic area of analysis to areas spanning zone boundaries, making the empirical design ill-suited to aggregating treatment effects without imposing strong assumptions on the saliency of the policy intervention in areas distant to the boundary.

Dwelling permits are modeled in levels. As discussed above, levels seem more appropriate given the observed pre-treatment trends in upzoned and non-upzoned areas, which differ over time by a near constant amount until the Auckland Unitary Plan (AUP) is implemented. An additional benefit of modeling outcomes in levels is that it allows us to define counterfactual sets in terms of model parameters. By definition, spillovers are measured in levels. For example, consider a spillover that generates  $\epsilon \in \mathbb{R}^+$  fewer permits in non-upzoned areas – and  $\epsilon$  more permits in upzoned areas – in the first treatment period. The corrected treatment effect would be  $\hat{\beta}_1 - \epsilon$  and the corrected period effect would be  $\hat{\phi}_1 + \epsilon$ . This direct mapping is lost or transformed if permits are instead modeled in logs or another non-linear transformation.

<sup>12</sup>SAs were introduced in 2018, as the previous classification system had not been revised since 1992. The previous statistical geographies no longer reflect current land use and population patterns. The revision was also implemented in order to align the geographic unit standards with international best practice. Population data from the previous census (conducted in 2013) and associated projections were used in the design of the 2018 boundaries. For additional details, see <https://www.stats.govt.nz/assets/Uploads/Retirement-of-archive-website-project-files/Methods/Statistical-standard-for-geographic-areas-2018/statistical-standard-for-geographic-areas-2018.pdf>

## 4.1 Selection of the Treatment Date

Throughout this study 2015 is used as the final year prior to treatment (i.e. the treatment date).<sup>13</sup> There are three main reasons for this selection.

First, as discussed in Section 2, the AUP became operational in November 2016, meaning that the final month and a half of 2016 fall under the new regulations. Using 2015 as the treatment date ensures that any differential increase in permits between upzoned and non-upzoned areas over this final month and a half of 2016 are correctly identified as a treatment effect.

Second, this date yields a more conservative estimate of the treatment effect if (a) negative spillovers from non-upzoned to upzoned areas are present, and (b) developers begin to respond to new regulations prior to those regulations coming into effect. Policy interventions can begin to manifest prior to the policy change if agents are notified of the policy change in advance. This is likely in the case of the Unitary Plan because the first version of the plan, with clear notification of the intent for dwelling intensification, was released in 2013, more than three years before the final version became operational (see Section 2 above). Negative spillovers can manifest as a decrease in dwelling permits in non-upzoned areas prior to 2016 if developers delay and shift planned construction to upzoned areas. The observed trend in permits in non-upzoned areas depicted in Figure 3 is consistent with a negative spillover effect beginning prior to the policy change. In fact, permits trend upwards until reaching a peak in 2015, a year before the policy change in late 2016, before trending downwards to a nadir in 2019.

Third, the most appropriate alternative treatment date, 2016 (the year the policy is enacted), results in larger estimated treatment effects because permits in non-upzoned areas are lower in 2016 than 2015. This is also the case for the set identified treatment effects, since the extrapolated linear trend that provides the basis for the counterfactual sets is flatter when the treatment date does not coincide with the peak in the control group. The result is a smaller spillover effect in the counterfactual set, and thus a larger treatment effect in the identified set, compared to that obtained with 2015 as the treatment date.

## 4.2 Results

Figure 6 shows the estimates of the coefficients alongside 95% confidence intervals (standard errors are clustered by suburb). Recall that we set the treatment to occur in 2016 since only the final month and a half of 2016 falls under the AUP. The top panel of the Figure displays results for all dwellings, while the middle and bottom panels display results for detached and attached dwellings, respectively.

Evidently there is no apparent trend in the estimated coefficients prior to treatment in any of the three samples, so the parallel trends assumption appears to hold. Further buttressing the DID framework, we find no evidence of anticipation in land prices or sales volumes prior to policy announcement, nor selection into treatment based on specifications that admit neighbourhood-specific trends (see the Robustness Checks in the Appendix). We proceed under the assumption

---

<sup>13</sup>We use other dates in our set of robustness checks.

that there is no confounding variable generating a difference in permits between treatment and control areas prior to policy implementation.

The response to the zoning change is immediate: The estimated treatment effects increase every year after implementation in 2016. (Recall that we set 2015 as the treatment date.) By 2021, six years after the policy was introduced, some 21.52 additional permits are issued, on average, in upzoned areas compared to non-upzoned areas in each of the 510 SAs. This would correspond to an additional 10,975 permits across the city. Cumulating the corresponding figures for 2016 through 2021 yields 32,869 additional permits in upzoned areas compared to non-upzoned areas. These are mostly attached dwellings. The estimated treatment effect for 2021 is 16.83 for attached dwellings and 4.69 for detached dwellings.

[Insert Figure 6 here]

### 4.3 Spillover Effects

It is possible that the implementation of upzoning reallocated construction to upzoned areas that would have otherwise occurred in non-upzoned areas. This negative spillover effect would lead to an overstatement of the treatment effect, since some of the construction in upzoned areas would have occurred in non-upzoned areas under the counterfactual scenario.

The time series plots of dwelling permits in Figure 3 appear to be consistent with a negative spillover effect. There is a mild upward trend in permits built in non-upzoned areas until 2015, one year prior to policy implementation. Thereafter, there is a mild decrease until 2019. Meanwhile, permits in upzoned areas trend upwards from 2016 onwards. This pattern is consistent with a negative spillover effect that shifted construction from non-upzoned to upzoned areas as a result of the policy. The initial upward trend in the control group, and subsequent downturn from 2015 onwards, is most evident in the detached dwellings sample, which is also consistent with a negative spillover.

These patterns are also evident in the estimated period effects (i.e.,  $\{\hat{\phi}_s\}$ ), which are depicted in Figure 7. The coefficients measure outcomes (average number of dwelling permits) in the control group relative to the treatment period (2015). In the all dwellings sample, the coefficients trend upwards until 2015; thereafter they trend down until 2019.

[Insert Figure 7 here]

To examine the potential mechanisms generating spillover effects, we specify a regression function that explains variation in the period fixed effects (PFEs) depicted in Figure 7. To do so, we

use a set of location-specific explanatory variables contained in the vector  $X_i$ , and fit the following regression to the data

$$y_{i,j,t} = \alpha_{i,j} + \sum_{s=-\underline{T}, s \neq 0}^{\bar{T}} \phi_s \mathbf{1}_{s=t} + \sum_{s=-\underline{T}, s \neq 0}^{\bar{T}} \mathbf{1}_{s=t} \gamma'_s X_i + \sum_{s=-\underline{T}, s \neq 0}^{\bar{T}} \beta_s \mathbf{1}_{s=t, j=1} + \sum_{s=-\underline{T}, s \neq 0}^{\bar{T}} \mathbf{1}_{s=t, j=1} \xi'_s X_i + \varepsilon_{i,j,t},$$

where  $X_i$  is a vector of variables specific to Statistical Area  $i$ , comprising: (i) Manhattan distance to the CBD from the SA's centroid; (ii) Manhattan distance to the nearest highway onramp from the centroid; (iii) undeveloped residential land area; and (iv) zoning concentration. Variables are standardized (but not demeaned) to ensure that the magnitude of fitted coefficients map to the explanatory power of the variable.

The sequence of parameter vectors  $\{\gamma_s\}_{s=-\underline{T}}^{s=\bar{T}}$  captures time series variation in control group permits that can be attributed to cross sectional variation in  $X_i$ . If a given variable contained in  $X_i$  accounts for the spillover effect, we anticipate the associated sequence of coefficients to mimic the pattern depicted in Figure 7 – particularly the switch from an upward trend to a downward trend in 2015, which, as discussed above, is consistent with a negative spillover effect. (i) is included to examine whether this pattern is more prevalent in more distant areas, which would imply that spillovers manifest as a re-allocation of permits from distant exurbs to inner suburbs. (ii) is a conceptually similar variable, but uses highway network access points that facilitate access to all other regions of the city, not just the CBD, as the relevant measure of distance. (iii) is the amount of residential land in the SA that falls outside of the “urban extent” of Auckland (as defined at the time of the zoning reform), which is used as an approximation of developed land.<sup>14</sup> It is included to examine whether the observed patterns in control group permits are more prevalent in locations with more undeveloped land, which would imply that spillovers manifest as a shift from greenfield housing development to redevelopment and infill housing. (iv) is the average minimum distance between upzoned and non-upzoned parcels within the SA,<sup>15</sup> and it is included to examine whether there are local spillovers within the same local area. The larger the measure, the greater the distance, on average, between upzoned and non-upzoned parcels, and thus the greater the concentration of zoning within the statistical area. If there are negative local spillover effects, whereby development is reallocated from non-upzoned to upzoned areas within statistical areas, areas with more concentrated zoning are more likely to experience a smaller (in magnitude) post-reform decrease in permits in control (non-upzoned) areas. If there are positive

<sup>14</sup> “Urban extent” is a geographical measure of Auckland’s developed urban area that excludes rural, peri-urban (i.e., semi-rural) and open space areas. It is based on cadastral land parcels. See [Fredrickson \(2014\)](#) for further description of the concept and classification methodology. Auckland Council has used development outside the urban extent to approximate the concept of greenfield development; see <https://www.aucklandcouncil.govt.nz/about-auckland-council/business-in-auckland/docsoccasionalpapers/the-brownfield-bounce-march-2018.pdf>

<sup>15</sup> For each upzoned residential parcel in the geographic unit, we calculate the Haversine distance to the nearest non-upzoned parcel; and for each non-upzoned parcel, we calculate the Haversine distance to the nearest upzoned parcel. Nearest parcels are not restricted to within the SA. We then average this minimum distance across all parcels within the SA. The measure builds on the empirical strategy used by [Turner et al. \(2017\)](#), who use distance to zoning boundaries to measure the external effects of zoning regulation.

local spillover effects, statistical areas with more concentrated zoning are likely to experience a larger (in magnitude) post-reform decrease in permits in control areas.<sup>16</sup>

Figure 14 presents the point estimates of the period fixed effects  $\phi_s$  and the elements of  $\gamma_s$  for each of the explanatory variables listed above. Each variable in  $X_i$  is standardized (but not demeaned), so the magnitude of each variable’s coefficient reflects the variable’s explanatory power. The  $y$  axis span is uniform across all variables to facilitate comparability.

The upward and downward trend in the period fixed effects that is evident in Figure 7 is gone, and the estimated coefficients are very close to zero, suggesting that our set of variables  $X_i$  explains time series variation in permits in non-upzoned areas prior to 2015. Looking to the plots for the explanatory variables, the amount of vacant land area has the most explanatory power for the observed patterns in permits in non-upzoned areas over the time period. The pattern in the coefficients mimic those observed in the period fixed effects exhibited in Figure 7, steadily increasing through to 2015, before falling to a nadir in 2019. The coefficients for the remaining variables are smaller in magnitude and statistically indistinguishable from zero in all periods. The evidence is therefore consistent with the spillovers reallocating construction across different suburbs – specifically from relatively undeveloped suburbs to more developed suburbs – rather than within suburbs.

[Insert Figure 14 here]

## 5 Set Identification of Treatment Effects under Spillovers

To account for spillover effects we adopt a confidence set identification approach by repurposing methods recently proposed in [Rambachan and Roth \(2023\)](#). First, we specify a set of plausible counterfactual scenarios based on observed pre-treatment trends in the control group. The period fixed effects depicted in Figure 7 form the basis for specifying this set of counterfactual scenarios. We then test whether the estimated treatment effects are significantly different from the set of counterfactuals.

Pre-treatment trends are frequently used to infer information about the counterfactual in DID frameworks. For example, RR propose extrapolating pre-treatment trends in estimated treatment effects to make inferences about counterfactual outcomes. We propose extrapolating trends in outcomes in the control group (i.e., the estimated period fixed effects) to learn about the counterfactual.

Figure 8 superimposes a linear trend on the outcomes in the control group. The linear trend is only fitted to the pre-treatment sample (2010–2015), and is fitted to pass through the coefficients in the first period (2010) and the treatment date (2015). Extrapolating a linear trend into the

<sup>16</sup>[Turner et al. \(2017\)](#) present evidence consistent with positive spillover effects. They find that external effects of land use regulation on land values are negative but statistically insignificant in the US. A negative external effect indicates that residents prefer locations that allow higher density.

treatment period yields 2.19 additional permits in the control group in the final period of observation (2021) relative to the treatment date (2015). However, we actually observe 0.44 fewer permits (since  $\hat{\phi}_{\bar{T}} = -0.44$ ) in the control group. Using the extrapolated trend as the counterfactual implies that the treatment effect in the final period is overstated by 2.63 ( $= 2.19 + 0.44$ ) permits, or approximately 1,341 permits across  $n = 510$  statistical areas.

[Insert Figure 8 here]

It is desirable to permit some margin for error when using pre-treatment trends to extrapolate a counterfactual scenario. This permits local deviations to potential nonlinearities in the trends. The approach suggested by [Rambachan and Roth \(2023\)](#) is to adopt a set of counterfactuals around the extrapolated trend. For example, in Figure 8, the space in between the dot-dashed pink lines permits a set of counterfactual scenarios in the control group, such that there is a margin of error of 5 permits in the final period of the analysis (2021). This means that the control group counterfactual for 2021 lies anywhere between 7.63 ( $= 2.63 + 5$ ) and -2.37 ( $= 2.63 - 5$ ) permits built relative to the treatment period (2015). Note that for earlier periods, we specify the sets to grow linearly in  $t \geq 1$ .

We provide some further examples to instruct the reader.

Let  $\beta = (\beta'_{pre}, \beta'_{post})'$ , where  $\beta_{pre} = (\beta_{-T}, \dots, \beta_{-1})'$  and  $\beta_{post} = (\beta_1, \dots, \beta_{\bar{T}})'$ . Following RR, we partition  $\beta_{post} = \delta_{post} + \tau_{post}$ , where  $\tau_{post}$  is the true treatment effect, and  $\delta_{post}$  is the difference between the treatment and control groups under the counterfactual scenario. The conventional assumptions required for DID analysis ensure that  $\delta_{post} = 0$ . In the absence of spillover effects,  $\delta_{post} = 0$ . The quantities  $\delta_{post}$  and  $\tau_{post}$  are unobserved and unidentified.

**Example 1.** Suppose that  $\bar{T} = 1$  (one post-treatment period) and that upzoning only re-allocated construction from control to treatment areas, so that no additional construction was generated. Then  $\beta_1 = \delta_1$  and  $\tau_1 = 0$ .

RR use  $\beta_{pre}$  to generate a set of possible counterfactual outcomes when the parallel trends assumption ( $\beta_{pre} = 0$ ) does not hold. For example, in the three period DID case, they discuss setting  $\delta_1 = -\beta_{-1} \pm M$  for some  $M \in \mathbb{R}^+$ . The intuition is that observed pre-treatment trends in the treatment group relative to the control group are informative of post-treatment trends under the counterfactual. The case where  $M = 0$  imposes a linear extrapolation, which is highly restrictive. Permitting general  $M \in \mathbb{R}^+$  allows for nonlinear patterns within a set of counterfactual scenarios.

In our application we want to account for negative spillovers that cause the estimated treatment effects to overstate the true treatment effects. To do so, we need to place bounds on the size of the spillover effect. In this regard, pre-trends in the treatment effect  $\beta_{pre}$  are uninformative.<sup>17</sup> Instead, we propose using pre-treatment trends in control group outcomes to bound the counterfactual

---

<sup>17</sup>However, our approach to modeling counterfactual scenarios based on pre-treatment trends in the control group could easily be extended to incorporate trends in *both* the treatment and the control group. Under such a scenario,  $\beta_{pre}$  is informative and would be used to bound  $\delta_{post}$ .

outcomes in the treatment group relative to the control group. We therefore similarly define and partition  $\phi = (\phi'_{pre}, \phi'_{post})'$ , where  $\phi_{pre} = (\phi_{-T}, \dots, \phi_{-1})'$  and  $\phi_{post} = (\phi_1, \dots, \phi_T)'$ .

**Example 2.** Suppose that  $\bar{T} = T = 1$  (so that we have three periods) and that  $\phi_{-1} < 0$  (outcomes in the control group trend upwards prior to the treatment) and  $\phi_1 < 0$  (outcomes in the control group trend downwards after the treatment). If control group outcomes remained on trend under the counterfactual, negative spillovers account for the observed downward deviation from trend after the policy is implemented. Then  $\delta_1 = -\phi_{-1} - \phi_1$  and  $\tau_1 = \beta_1 + \phi_{-1} + \phi_1$ . In practice, we permit deviations from the linear trend assumption, such that  $\delta_1 = -\phi_{-1} - \phi_1 \pm M$  for some  $M \in \mathbb{R}^+$ .

Once the set of counterfactuals is articulated, we can rely on the inferential architecture supplied by [Rambachan and Roth \(2023\)](#). In our application we select counterfactual sets that are convex and centrosymmetric, which ensures that fixed length confidence intervals (FLCI) are consistent.

Specifically, for each  $t = 1, \dots, \bar{T}$  we adopt sets given by

$$\Delta_t = \left\{ \delta_t : \delta_t \in \left( -\hat{\phi}_{-T} \cdot \frac{t}{\bar{T}} - \hat{\phi}_T - M \frac{t}{\bar{T}}, -\hat{\phi}_{-T} \cdot \frac{t}{\bar{T}} - \hat{\phi}_T + M \frac{t}{\bar{T}} \right) \right\}, \quad (2)$$

where  $M$  denotes the half-length of the counterfactual set centered on  $\hat{\phi}_T$ .

Identification of the treatment effect is based on the difference between observed outcomes in treated areas and the set of counterfactuals defined by  $\Delta_t$ , which naturally gives rise to set (rather than point) identification of treatment effects. For a given significance level  $\alpha \in (0, 0.5]$ , the coverage of FLCIs converge to  $1 - \alpha$ . The remaining technicalities of the set identification approach are formulated in the Appendix.

Returning to our empirical application, Figure 9 superimposes the confidence regions for  $\alpha = 0.05$  and  $M = 5$  on the conventional treatment effects depicted in Figure 6. Notably, the confidence intervals sit above zero for 2018, 2019, 2020 and 2021, meaning that we can reject the null of no treatment effect at the 95% level when allowing for a counterfactual set defined under 2 with  $M = 5$ . They are also centered at a point below  $\hat{\beta}_{\bar{T}}$ , which is consistent with a negative spillover effect.

[Insert Figure 9 here]

## 5.1 Set-Identified Treatments Effects

We now consider set identification under various counterfactual set lengths. Figure 10 exhibits the set-identified treatment effects for  $M = 5, 10$  and  $15$ . In all cases, the identified set lies above zero for 2021. The largest counterfactual set,  $M = 15$ , spans  $8,768 (= 510 \times (1.827 \times \frac{6}{5} + 15))$  to  $-6,532 (= 510 \times (1.827 \times \frac{6}{5} - 15))$  permits. The upper bound of this set is approximately eight times the extrapolated linear trend (since  $(15 + 1.827 \times \frac{6}{5}) / (1.827 \times \frac{6}{5}) = 7.84 \simeq 8$ ). This serves to pin down the magnitude of the increase in permits that would be necessary under a counterfactual scenario to render the treatment effects statistically insignificant. Specifically, we must allow for



counterfactual scenarios that imply at least a seven-fold increase in permits over the pre-treatment trend in order for the estimated treatment effects to become statistically insignificant. Such a scenario appears highly improbable. There is no concurrent policy change in the narrative record that could plausibly generate such a substantive increase in construction.

To help illustrate the substantive growth in permits that would be required under the counterfactual to render all of the treatment effects insignificant, Figure 15 in the Appendix presents the counterfactual set when  $M = 15$ . The counterfactual set can even accommodate limited forms of exponential growth in permits over the six year post-treatment period, including a year-on-year growth rate of 27.4%.<sup>18</sup>

[Insert Figure 10 here]

## 5.2 Set-Identified Treatments Effects for Attached and Detached Housing

This section applies the set identification approach to the subsamples of attached and detached housing.

### 5.2.1 Attached Housing

The empirical model is first fitted to dwelling permits for attached housing. Figure 15 in the Appendix exhibits the estimated period effects and extrapolated linear trend between the first period and the treatment period. Evidently, there is only a slight upward trend in attached dwelling permits prior to treatment, with the extrapolated trend yielding only an additional 0.374 permits in 2021 compared to 2015. This implies an additional 191 ( $= 0.374 \times 510$ ) permits in total across the 510 statistical areas under the extrapolated counterfactual scenario.

Figure 11 exhibits the estimated treatment effects and set-identified confidence intervals under various counterfactual sets ( $M = 4, 9, 14$ ). The 95% confidence interval for the identified set for the  $M = 14$  case sits above zero for 2021. Thus, treatment effects are statistically significant at the 95% level even when the counterfactual set spans an additional 7,331 ( $= (14 + 0.364) \times 510$ ) permits under the counterfactual scenario. This is about thirty-eight times the 191 additional permits in the control area implied by the extrapolated linear trend. The conclusion seems unequivocal that upzoning has had a substantive impact on the construction of attached dwellings.

[Insert Figure 11 here]

---

<sup>18</sup>In 2015 there were 2,050 permits in non-upzoned areas. The upper bound allows up to 8,768 permits in 2021, corresponding to an annual growth rate of 27.4%.

### 5.2.2 Detached Housing

Next we fit the model to the subsample of detached dwelling permits. Figure 15 in the Appendix exhibits the estimated period effects and extrapolated linear trend between the first period and the treatment period. Evidently, there is a marked increase in detached dwelling permits prior to treatment. The extrapolated trend then yields 1.831 additional permits in non-upzoned areas in 2021 compared to 2015. This would entail an additional 934 ( $= 1.831 \times 510$ ) permits in total under the extrapolated counterfactual scenario.

Figure 12 exhibits the estimated treatment effects and set-identified confidence intervals under various counterfactual sets ( $M = 1, 2, 3$ ). For the largest  $M = 3$  set, the confidence intervals sits above zero for only 2018 and 2019. The counterfactual extrapolated trend is 0.915 in 2018, while the counterfactual set spans  $(-0.585, 2.415)$ . Thus, the treatment effect is statistically significant at the 95% level even when the counterfactual set spans approximately two-and-a half times the additional permits in the control area implied by the extrapolated linear trend (since  $2.63 \simeq 2.415/0.915$ ). We conclude that upzoning had a substantive impact on the construction of detached dwellings, albeit a smaller impact than that realized for attached dwellings.

[Insert Figure 12 here]

### 5.3 Set-Identified Treatment Effects by Zone

We now analyze the impact of upzoning in each of the three treatment zones separately. Figure 16 in the Appendix exhibits the treatment effects for areas upzoned to Terraced Housing and Apartments (THA), Mixed Housing Urban, (MHU) and Mixed Housing Suburban (MHS). Treatment effects in all three areas are generally positive and statistically significant in the post-treatment period. There is some evidence of a downward pre-treatment trend for THA. The treatment effects in this zone may therefore be under-estimated.

We also consider set identification of confidence intervals. Under the set identification approach there is no obvious method to allocate spillovers from the control group to the treatment groups. For example, all of the spillover could be allocated to a single area, such as upzoned to MHS. This would be consistent with *all* of the construction in the control zone spilling over into areas upzoned to MHS, and none into the MHU and THA areas.

In the absence of an obvious alternative, we elect to allocate the spillover to each of the three zones according to baseline levels of construction in each treatment area prior to upzoning. Over 2010-2014, 16.0% of permits were in areas upzoned to THA, 31.0% were in upzoned to MHU, and 53.1% were in upzoned to MHS. Let  $w_j$  denote the weights in zone  $j$ . For each  $t = 1, \dots, \bar{T}$  the set of counterfactuals is given by

$$\Delta_t = \left\{ \delta_t : \delta_t \in w_j \left( -\hat{\phi}_{-T} \cdot \frac{t}{\bar{T}} - \hat{\phi}_T - M \frac{t}{\bar{T}}, -2\hat{\phi}_{-T} \cdot \frac{t}{\bar{T}} - \hat{\phi}_T + M \frac{t}{\bar{T}} \right) \right\}.$$

Figure 16 exhibits the results with  $M = 15$ , which is the largest set considered earlier. For upzoned to THA and MHU, the treatment effects are statistically significant for 2019, 2020 and 2021. Meanwhile, the estimated effects for upzoned to MHS are statistically insignificant at the 5% level (two-sided). This is due to the fact that the treatment effects are small relative to the proportion of permits issued in this zone prior to the policy. For example, the treatment effect for upzoned to MHU was 8.0, which is indistinguishable from the 8.0 treatment effect in MHS (rounded to 1 d.p.). However, upzoned to MHS is allocated 53% of the spillover, whereas MHU receives only 31%.

#### 5.4 How Many Additional Permits Did Upzoning Enable?

To estimate the impact of upzoning on the total housing stock, confidence sets were constructed with the counterfactual set restricted to the counterfactual trend, i.e.  $M = 0$ . This implies that the treatment effect is estimated by subtracting  $-\hat{\phi}_{-T} \cdot \frac{t}{T} - \hat{\phi}_t$  from  $\hat{\beta}_t$  for each  $t = 1, \dots, \bar{T}$ . In other words, the difference between the extrapolated linear trend and observed permits in the control group is subtracted from the estimated treatment effect. This difference represents the additional permits that would have occurred in the control group had the policy not been implemented. Figure 13 depicts the spillover-robust treatment effects.

The spillover-robust treatment effects for each year between 2016 and 2021 are  $-1.58, 3.08, 8.21, 10.84, 13.87$  and  $18.89$ , respectively, yielding a cumulative total of  $27,183$  ( $= 53.30 \times 510$ ) additional permits as a result of the upzoning policy. This figure implies that permits issued per year have approximately doubled as a result of the policy, which averaged 4,148 permits per year between 2010 and 2015.<sup>19</sup> The additional 27,183 permits corresponds to 5.13% in the city's extant housing stock. Statistics New Zealand estimates that there were 530,300 dwellings in Auckland by the end of 2016.<sup>20</sup> Because completions of permits range between 95% and 99% (outside of recessionary periods), the cumulative completed construction enabled by the policy is likely to be between 4.87% ( $= 5.13\% \times 0.95$ ) and 5.08% ( $= 5.13\% \times 0.99$ ) of the dwelling stock of Auckland. It is important to note, however, that permits in upzoned areas are still trending upwards as of 2021, so the full impact of the policy will likely not be known for several more years.

[Insert Figure 13 here]

**Residential Zones.** The analysis was repeated for estimated treatment effects by different residential zone. We used the same weights as used in Section 5.4 above to allocate the spillovers in each of the three zones.

Figure 17 in the Appendix shows the results. For THA, the spillover-robust treatment effects for each year between 2016 and 2021 are 0.1, 0.2, 2.8, 3.8, 3.9 and 5.4, respectively, yielding a

<sup>19</sup>This number excludes houses built in the special housing areas. As stated above, these are excluded from the analysis.

<sup>20</sup>Source: <https://www.stats.govt.nz/experimental/experimental-dwelling-estimates>

cumulative total of 16.3 permits per Statistical Area, or 8,290 additional permits in total. For MHU, the corresponding estimates are 0.3, 1.4, 3.1, 5.5, 5.8 and 7.0, yielding a cumulative total of 23.1 permits per Statistical Area, or 11,769 across all statistical areas. Finally, for MHS the corresponding estimates are 0.0, 1.6, 4.1, 4.5, 5.5 and 6.2, yielding a cumulative total of 22.0 permits per Statistical Area, or 11,215 in total. These figures imply that 26.5% of the overall increase in permits occurred in areas upzoned to THA, 35.8% in areas upzoned to MHU, and 37.6% in areas upzoned to MHS if we allocate the counterfactual change in permits equally to each of the three treatment areas.<sup>21</sup>

## 5.5 Robustness Checks

This section provides a brief description of results under various robustness checks.<sup>22</sup> Results and additional details are provided in the appendix.

### 5.5.1 Selection into Treatment and Policy Anticipation

Although the parallel trend assumption appears to hold in our sample, we perform three robustness checks to examine whether upzoning was anticipated or whether there was selection into treatment.

The first two robustness checks are based on a geocoded dataset of residential housing sales. Because the policy was first announced in March 2013, we use 2012 as our treatment date, as the housing market could respond to the policy prior to it being implemented. First, we fit the multi-period DID model to a spatial panel of (log) sales volumes. Interestingly, treatment effects become positive and statistically significant in 2017, suggesting sales activity increased in upzoned areas only after the policy was operationalized. Prior to 2017, coefficients are statistically indistinguishable from zero, indicating that there were no differential trends in sales volumes between upzoned or non-upzoned areas prior to policy implementation. Second, we fit the multi-period DID model to sales prices, wherein the site intensity ratio of the property is interacted with the treatment indicators in order to obtain treatment effects on land values, rather than the overall value of the property (Greenaway-McGrevy et al., 2021).<sup>23</sup> Abstracting from external effects of increased density or new construction, upzoned land should be priced higher than non-upzoned land due to its greater potential floorspace capacity (Turner et al., 2017; Greenaway-McGrevy et al., 2021), all else equal. Relative to non-upzoned areas, land prices in upzoned areas increase from 2014 through

<sup>21</sup>The sum of the treatment effects across the three treatment areas is not equal to the treatment effect when the treatment effect is obtained when the three areas are aggregated. Let  $\beta_s^k$ ,  $k \in \{\text{ALL}, \text{THA}, \text{MHU}, \text{MHS}\}$  denote the parameter when  $k$  is the treatment group ('ALL' denotes the case where all three zones are in the treatment group). Then  $\sum_{k \in \{\text{THA}, \text{MHU}, \text{MHS}\}} (\hat{\beta}_s^k + \hat{\phi}_s) = \hat{\beta}_s^{\text{ALL}} + \hat{\phi}_s$  for each year  $s$ , where  $\hat{\phi}_s$  is the same as when (1) is run using each  $k$  as the treatment group. To allocate  $\hat{\beta}_s^{\text{ALL}}$  to each of the three areas, we use  $\hat{\beta}_s^k + \frac{2}{3}\hat{\phi}_s$  since  $\hat{\beta}_s^{\text{ALL}} = \sum_{k \in \{\text{THA}, \text{MHU}, \text{MHS}\}} (\hat{\beta}_s^k + \frac{2}{3}\hat{\phi}_s)$ . This is equivalent to assuming that the counterfactual change in permits in each area is equivalent to one-third of the counterfactual implied by  $\hat{\phi}_s$ .

<sup>22</sup>We thank two anonymous referees for suggesting many of these robustness checks.

<sup>23</sup>The site intensity ratio is the value of improvements divided by the total value of the property, so that vacant land has an intensity ratio of zero. Valuations are local government estimates used for the purpose of levying property taxes. See Greenaway-McGrevy et al. (2021) for additional details.

to 2016, indicating that upzoning was capitalized into land prices soon after the policy was first announced in March 2013. Prior to 2013, coefficients are statistically indistinguishable from zero, indicating that there were no differential trends in land prices between upzoned or non-upzoned areas prior to policy announcement.

Another method to account for selection into treatment is to adopt an empirical model with period fixed effects that vary by geographic unit (indexed by  $i$ ), which control for neighborhood-level demand trends. These trends may be a concern if policymakers direct development-intensive zones to neighborhoods that are experiencing upward or downward demand trends. The estimated treatment effects are robust to heterogeneous period fixed effects.

### 5.5.2 Alternative Treatment Dates and Samples

We consider alternate specifications in which 2013, 2014 and 2016 are used as the date of treatment. Point estimates of treatment effects and spillover robust treatment effects are reported in the appendix. Estimates of the cumulative increase in the number of permits are slightly larger when alternative treatment dates are used, which is unsurprising given that permits in non-upzoned areas peaked in 2015.

We also consider empirical designs in which treatment and control areas are defined differently. In the first, THA, MHU and MHS zones are the treatment group and SH areas are the control group. This approach ignores whether the floor area ratio (FAR) has changed. In the second, downzoned areas, where the FAR decreased, are excluded from the analysis. Our results and findings from these designs do not change substantively.

Finally, we also consider a larger sample that includes permits issued in Special Housing Areas (see Section 2 above). Again, our findings do not change, although the estimated net increase in permits decreases slightly to 24,767.

## 6 Discussion

The empirical findings show strong evidence to support the conclusion that upzoning raised dwelling construction in the city of Auckland. Our fitted model shows that 26,903 additional dwellings have been permitted as a result of the policy over the first five years subsequent to the policy. This figure is equivalent to 5.07% of the existing housing stock prior to policy implementation. The data reveal that much of this increase is in the form of the more capital intensive, attached (or multifamily) structures in the suburban core of the city.

These findings are a positive sign for proponents of upzoning as a solution to unresponsive housing supply, particularly when compared to recent studies that show that zoning reforms have not had a substantial impact on housing construction ([Freemark, 2019](#); [Limb and Murray, 2022](#)). Although the present paper does not explicitly identify factors that mediate the efficacy of zoning reforms in different contexts, comparisons to other reforms may shed light on the underlying mechanisms and thereby assist in directing future research. For example, the large-scale upzon-

ing in Auckland may have fostered greater competition in land supply to developers, resulting in lower upzoned land prices than would have occurred if the policy had instead been restricted to specific neighborhoods or transit corridors. Alternatively, demand for housing in Auckland has significantly outstripped supply over the past few decades, resulting in housing that is amongst the most expensive in the world when measured against local incomes: Auckland may have significantly more latent demand for housing than other cities. We anticipate future research will address these questions to further guide policymakers in the design and implementation of zoning reforms.

We conclude by noting that the impact of upzoning on housing construction and housing markets will continue to be felt over coming years. Permits for attached dwellings are still trending upwards and permits for detached dwellings remain significantly above their pre-upzoning average. In future work, and as new data become available, the impact of the policy on the housing stock will be updated and new research will seek to determine the particular characteristics of parcels that predict the uptake of redevelopment. Such findings should be useful in assisting the design and refinement of future upzoning policies.

## **7 Figures and Tables to appear in the Main Text**

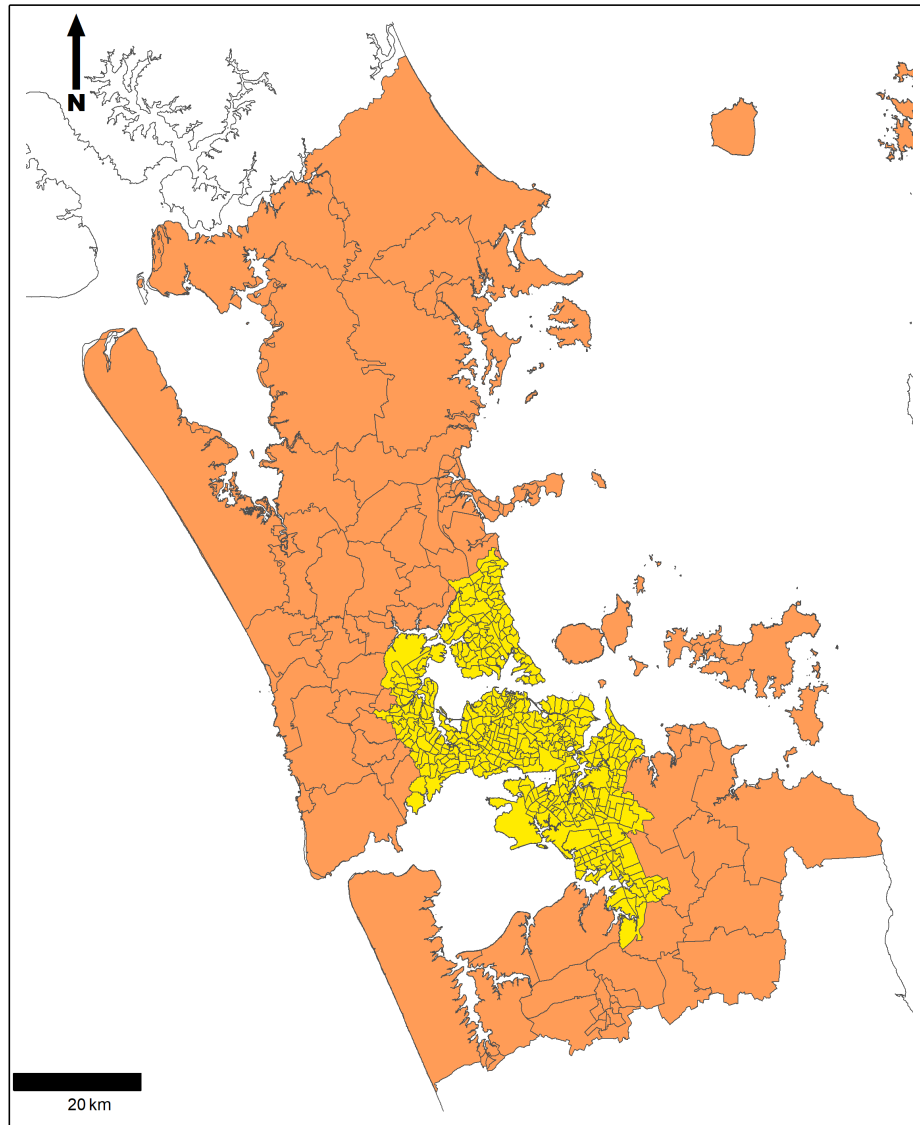


Table 1: Summary of Land Use Regulation by Residential Zone under the Unitary Plan

Regulation	Terraced Housing Apartments Zone	Mixed Housing Urban Zone	Mixed Housing Suburban Zone	Single House Zone
Max. height	16m (five storeys)	11 to 12m (three storeys)	8 to 9m (two storeys)	8 to 9m (two storeys)
Height in relation to boundary	3m vertical + 45° recession plane	3m vertical + 45° recession plane	2.5m vertical + 45° recession plane	2.5m vertical + 45° recession plane
Setback	0m	1m	1m	1m
Site Coverage	50%	45%	40%	35%
Impervious Area	70%	60%	60%	60%
Min. dwelling size (1 bedroom)	45m <sup>2</sup>	45m <sup>2</sup>	45m <sup>2</sup>	n/a
Max. dwellings (on existing parcels)	does not apply	3	3	1
Min. Lot Size (subdivision)	1200m <sup>2</sup>	300m <sup>2</sup>	400m <sup>2</sup>	600m <sup>2</sup>

Notes: Tabulated restrictions are ‘as of right’ and can be exceeded through resource consent notification. Height in relation to boundary and Setbacks apply to side and rear boundaries. Less restrictive height in relation to boundary rules than those tabulated apply to side and rear boundaries within 20m of site frontage. Maximum dwellings per site are the number permitted as of right in the Unitary Plan. Minimum lot sizes do not apply to extant residential sites. Site coverage is the area under the dwelling structure. Impervious area is the area under the dwelling and structures such as concrete driveways that prevent rainwater absorption into the soil.

Figure 1: Auckland Region



Notes: Auckland region (shaded) decomposed into Statistical Area units. Major urban area in yellow.

Figure 2: Residential Zones in inner Auckland introduced under the Auckland Unitary Plan

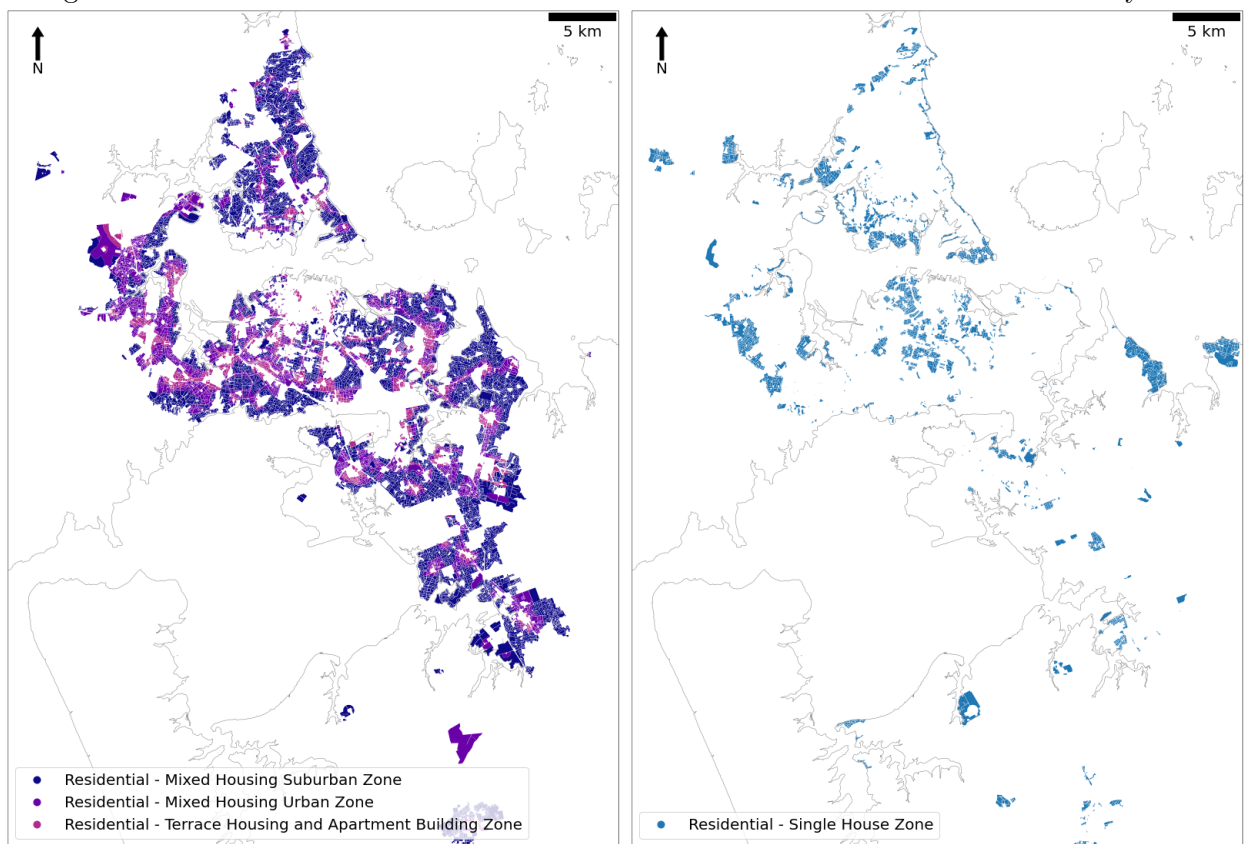
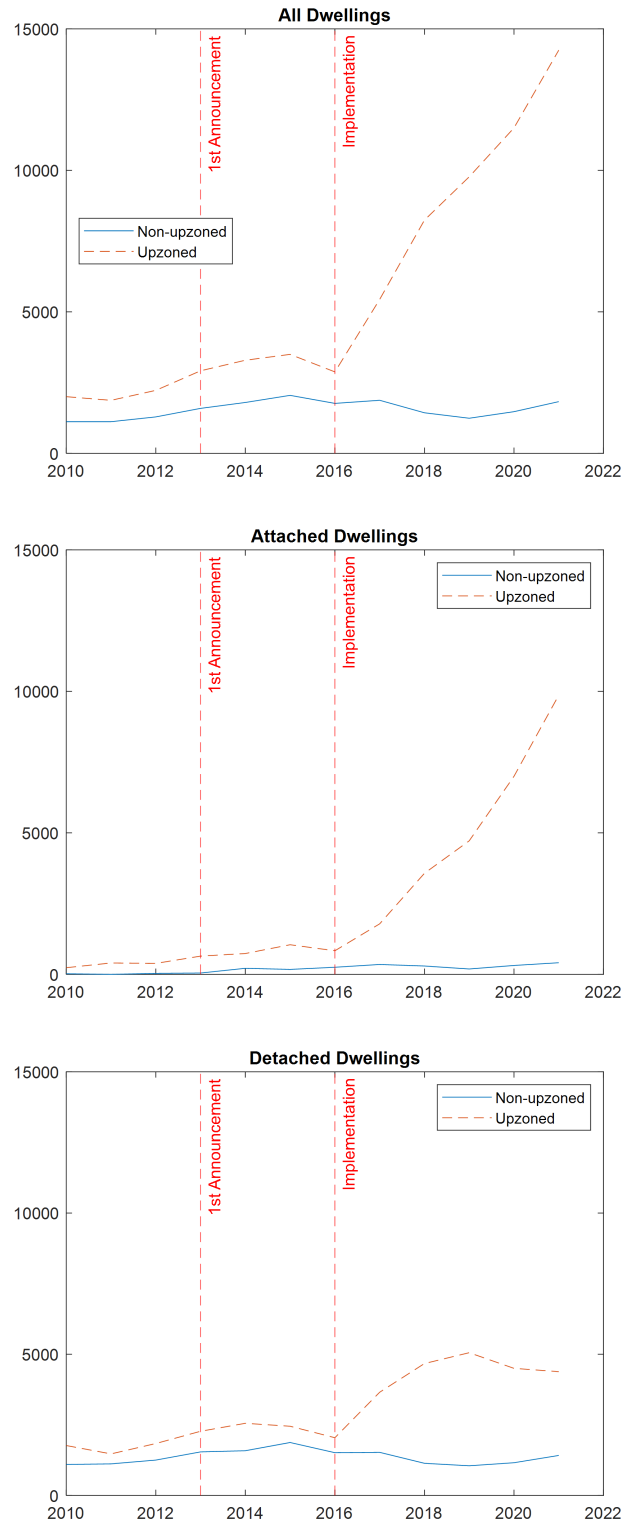
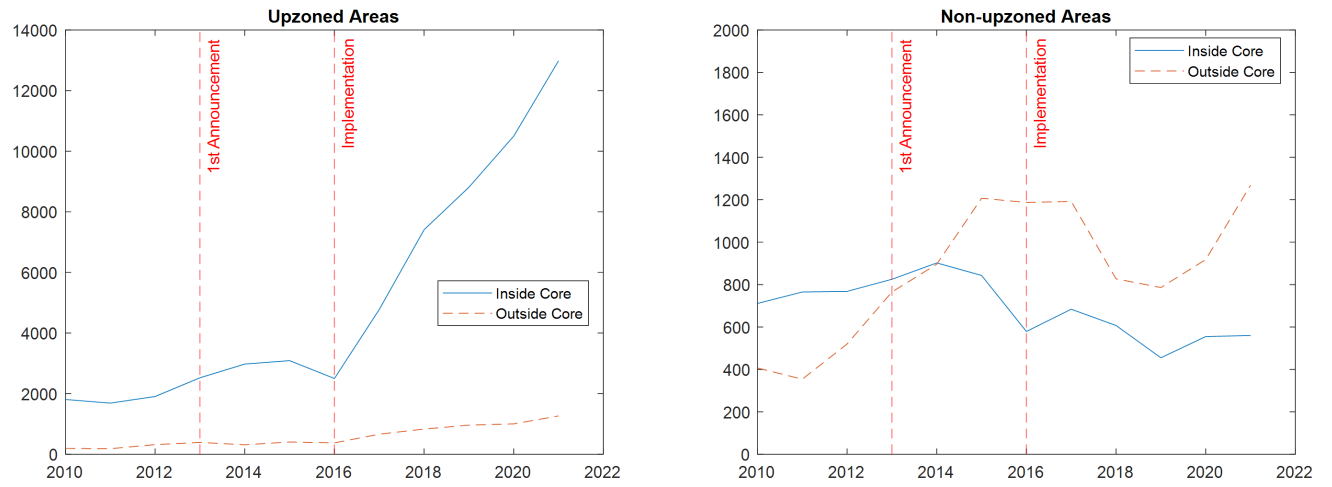


Figure 3: Dwelling permits, 2010-2021



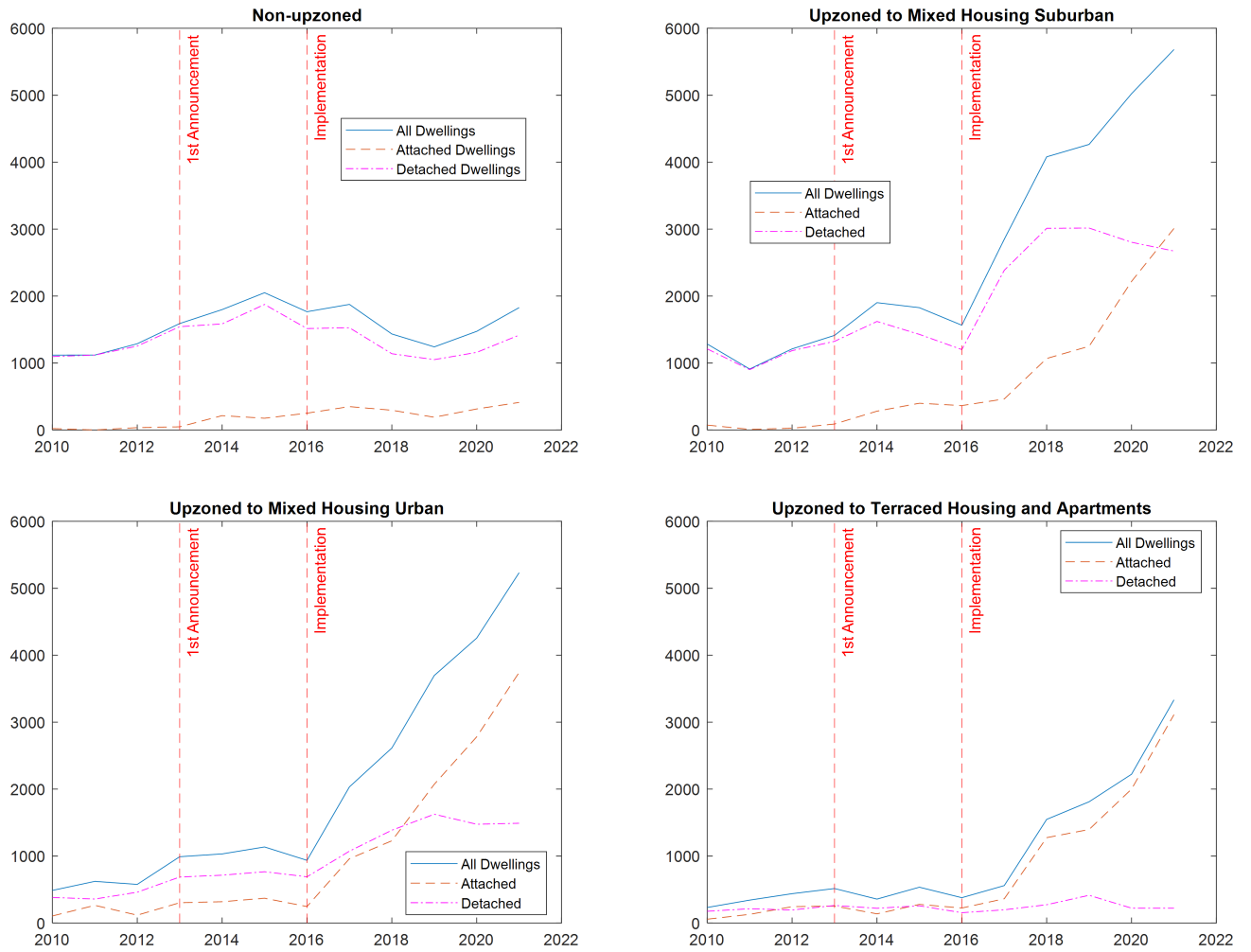
Note: Dwelling permits issued in upzoned and non-upzoned areas.

Figure 4: Dwelling permits inside and outside urban core, 2010-2021



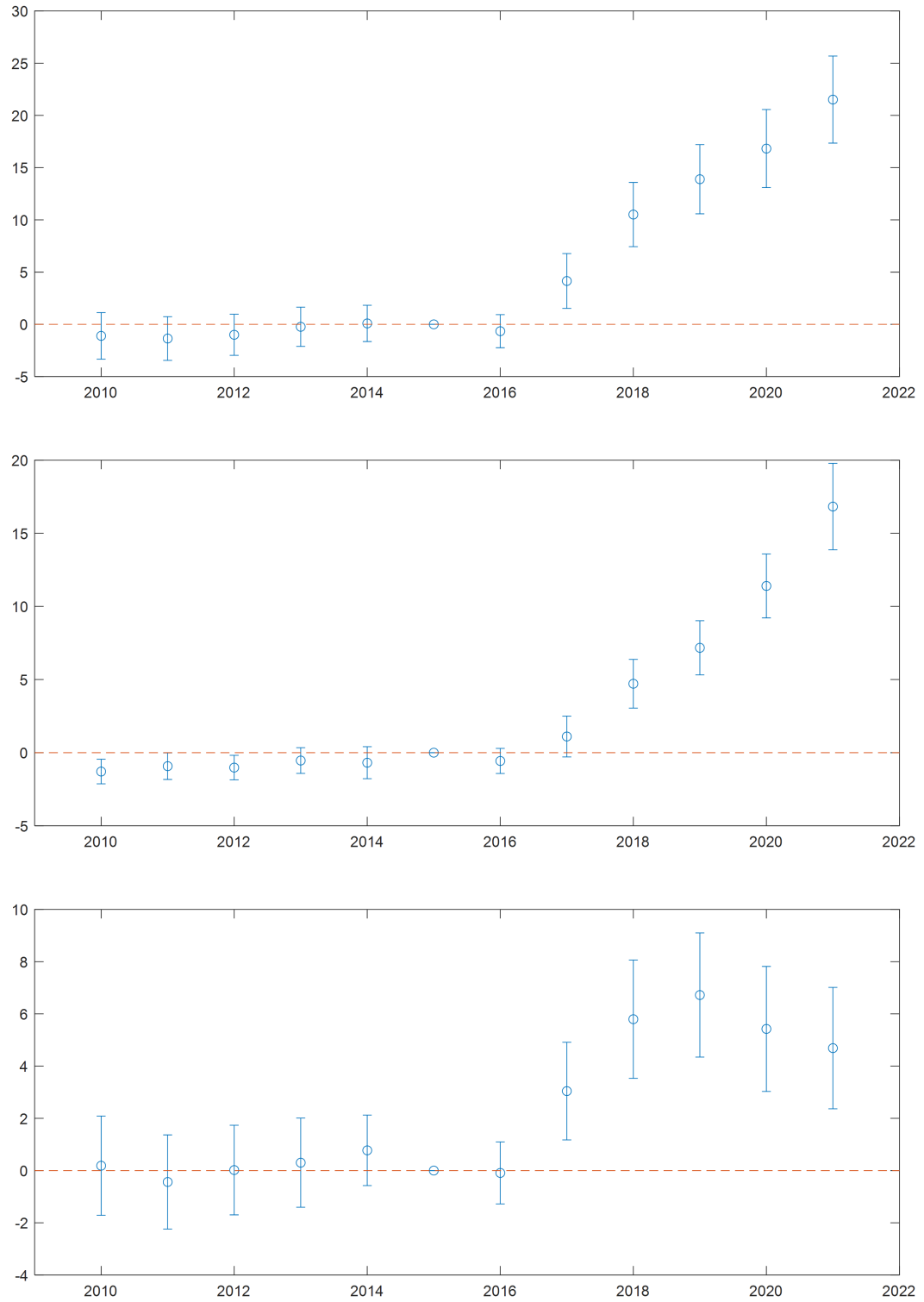
Note: Dwelling permits issued inside and outside urban core by upzoned (left) and non-upzoned areas (right).

Figure 5: Dwelling permits by residential zone, 2010-2021



Note: Dwelling permits issued in the four Auckland Unitary Plan residential zones considered.

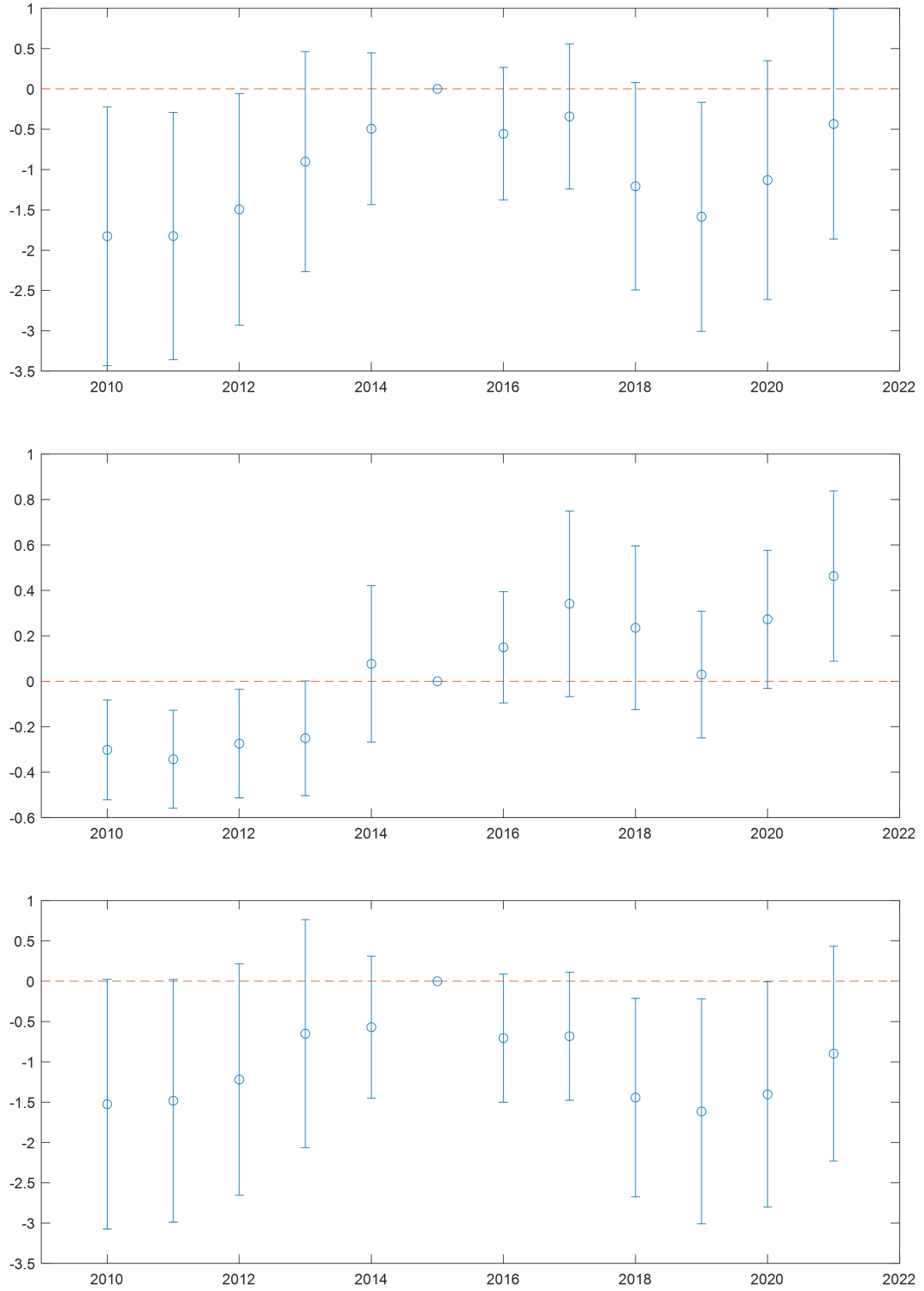
Figure 6: Estimated Treatment Effects, 2010-2021



Notes: Estimated multi-period treatment effects for all dwellings (top), attached dwellings (middle) and detached dwellings (bottom). OLS estimates of  $\{\beta_s\}$  (circles) and 95% confidence intervals (lines).

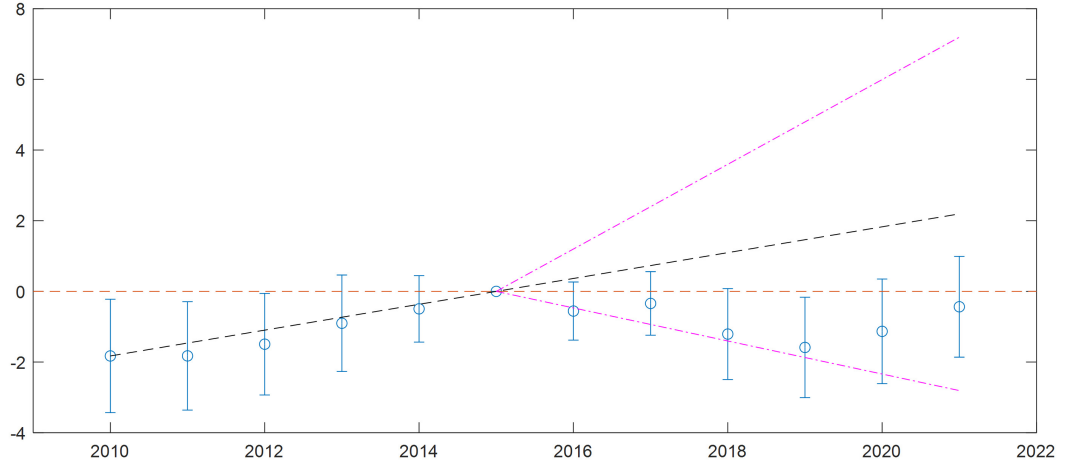


Figure 7: Estimated Period Effects, 2010-2021



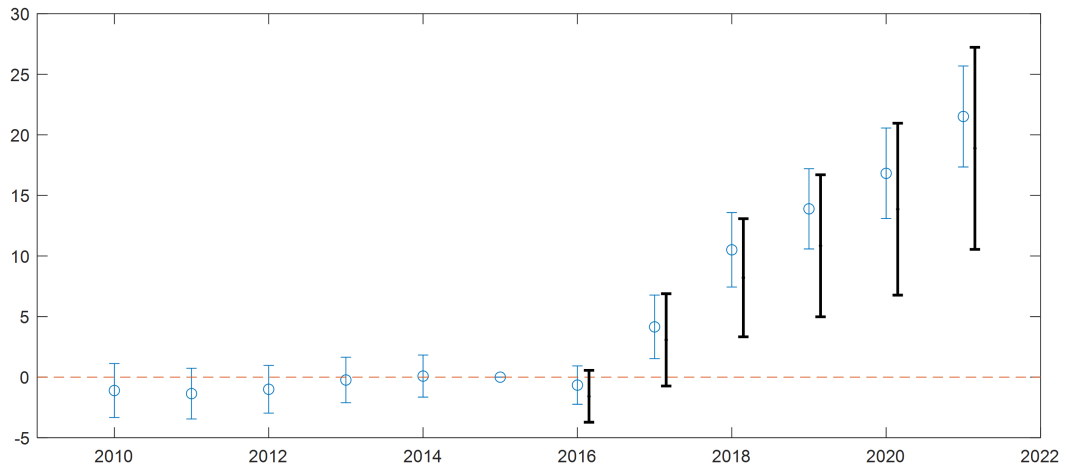
Notes: Estimated period effects for all dwellings (top), attached (middle) and detached dwellings (bottom). The estimated period effects are the average dwelling permits (across statistical areas) in non-upzoned areas relative to the treatment year 2016. OLS estimates of  $\{\phi_s\}$  (circles) and 95% confidence intervals (lines).

Figure 8: Estimated Period Effects and Counterfactual Sets, 2010-2021



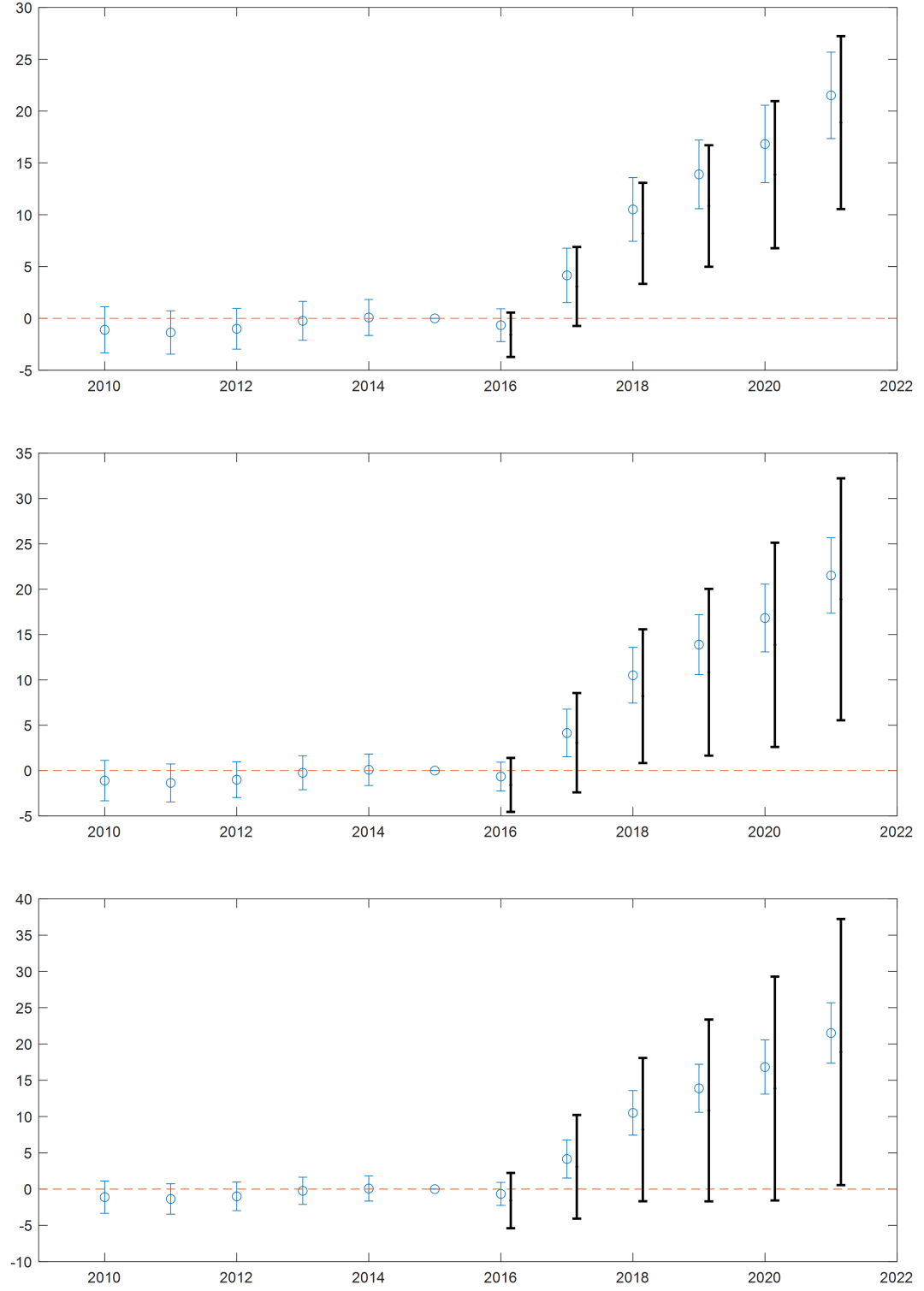
Notes: Outcomes in control group. OLS estimates of  $\{\phi_s\}$  (blue circles) alongside 95% confidence intervals (blue lines). Extrapolated pre-treatment trend given by the (black) dashed line. Set of counterfactual outcomes for 2016 to 2021 given by the space between the (pink) dot-dash lines. The counterfactual set is specified as  $\pm 5$  dwellings in the final period (equivalent to  $\pm 2550$  dwellings over the 510 statistical areas).

Figure 9: Estimated Treatment Effects and Confidence Sets, 2010-2021



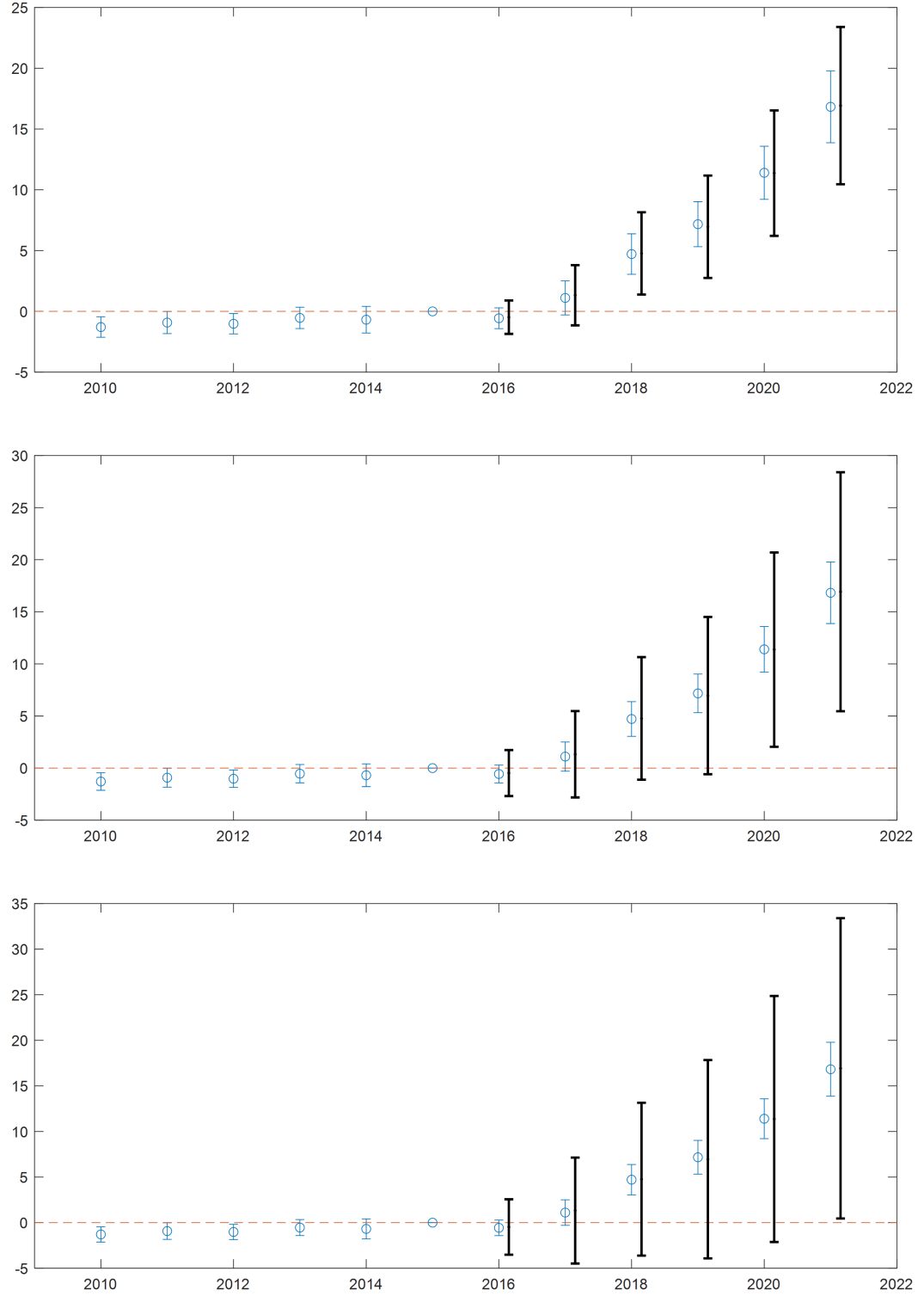
Notes: Multi-period treatment effects and 95% confidence intervals (blue circles and thin lines), and confidence sets for post-treatment interventions, 2016 to 2021 (thick black lines). The counterfactual set is specified as  $M = 5$  dwellings in the final period.

Figure 10: Estimated Treatment Effects and Confidence Sets, 2010-2021



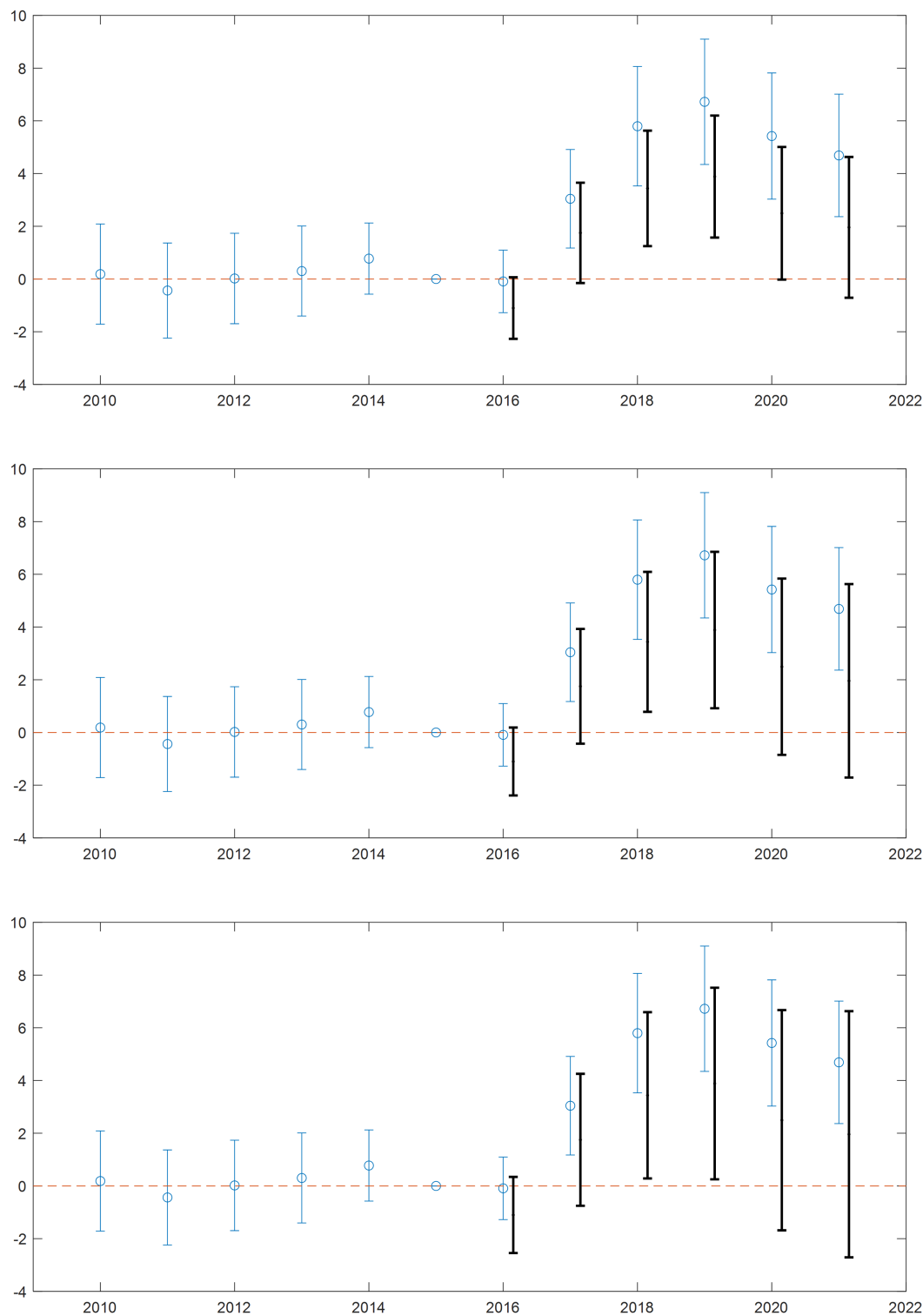
Notes: Multi-period treatment effects and 95% confidence intervals (blue circles and thin lines), and confidence sets for post-treatment interventions, 2016 to 2021 (thick black lines). Treatment occurs in 2015. Top panel  $M = 5$ ; middle panel  $M = 10$ ; bottom panel  $M = 15$ .

Figure 11: Estimated Treatment Effects and Confidence Sets, Attached Housing, 2010-2021



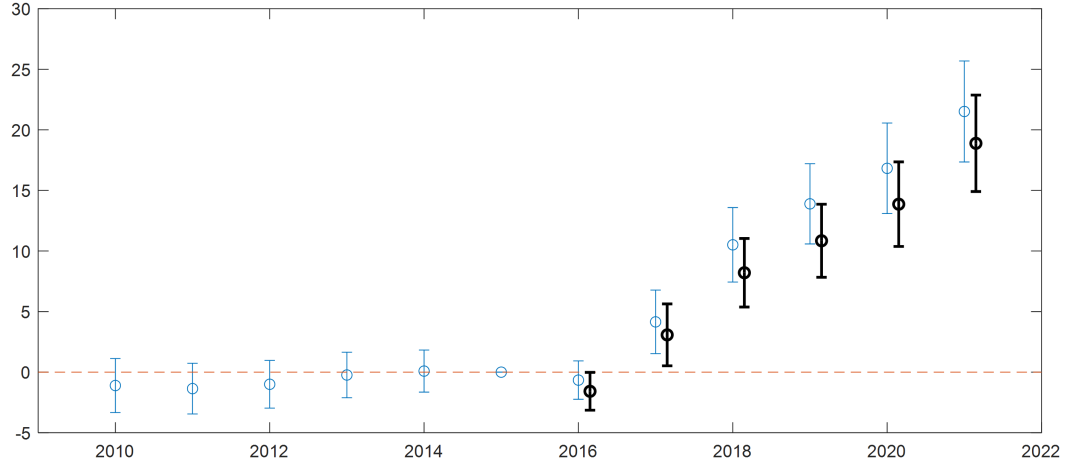
Notes: Multi-period treatment effects and 95% confidence intervals (blue circles and thin lines), and confidence sets for post-treatment interventions, 2016 to 2021 (thick black lines). Treatment occurs in 2015. Top panel  $M = 4$ ; middle panel  $M = 9$ ; bottom panel  $M = 14$ .

Figure 12: Estimated Treatment Effects and Confidence Sets, Detached Housing, 2010-2021



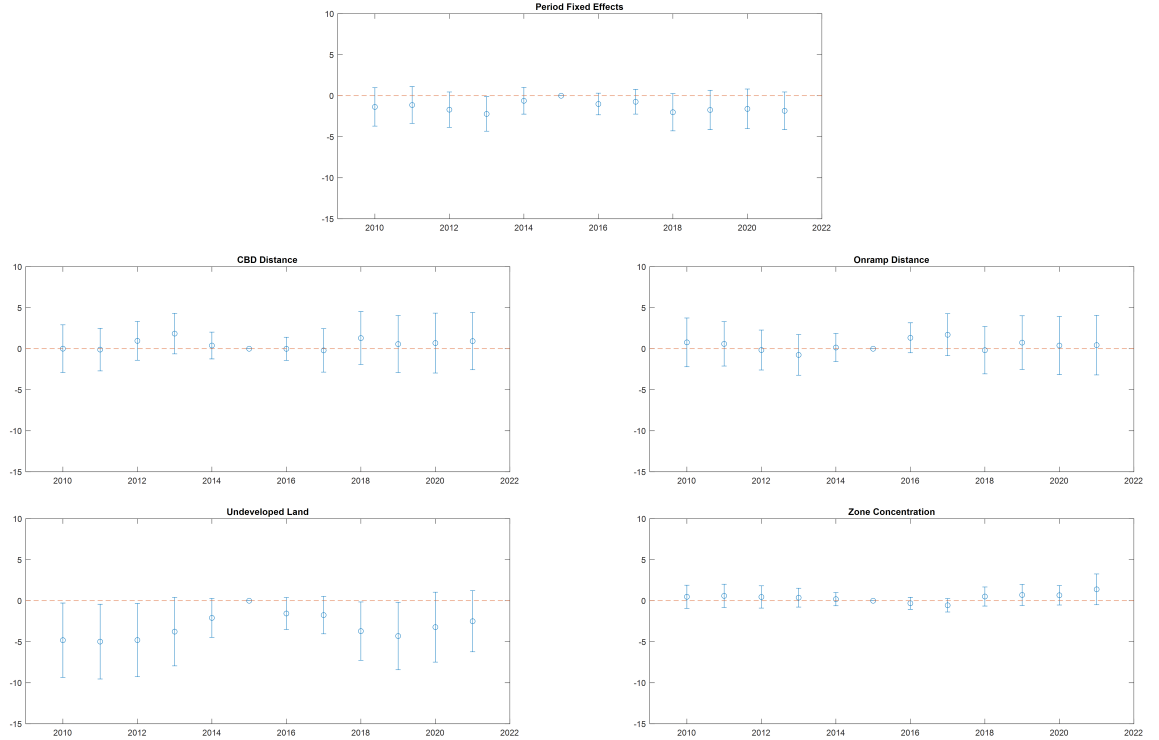
Notes: Multi-period treatment effects and 95% confidence intervals (blue circles and thin lines), and confidence sets for post-treatment interventions, 2016 to 2021 (thick black lines). Treatment occurs in 2015. Top panel  $M = 1$ ; middle panel  $M = 2$ ; bottom panel  $M = 3$ .

Figure 13: Estimated Treatment Effects under Counterfactual Trend for All Dwellings, 2010-2021



Notes: Multi-period treatment effects and 95% confidence intervals (blue circles and thin lines) and treatment effects under counterfactual trend in the control group, 2016–2021 (thick black circles and lines). All dwellings. Estimates under the counterfactual trend are given by restricting the span of the counterfactual set to zero, i.e.  $M = 0$ . Thick black circles denote spillover-robust point estimates of treatment effects.

Figure 14: Explaining Variation in Period Fixed Effects, 2010-2021



## 8 Appendix

### 8.1 Background and Detailed Timeline of the Auckland Unitary Plan

Prior to 2010, the greater Auckland metropolitan region comprised one regional and seven city and district councils: Auckland City Council, North Shore City Council, Waitākere City Council, Manukau City Council, Rodney District Council, Papakura District Council, and Franklin District Council. On 1 November 2010, Auckland Council (AC) was formed when the eight previous governing bodies in the region were amalgamated. Special legislation was also passed by the central government requiring AC to develop a consistent set of planning rules for the whole region under the Local Government Act 2010. This set of planning rules is embodied in the Auckland Unitary Plan (AUP).

Key dates in the development and implementation of the AUP are as follows:

- 15 March 2013: AC releases the draft AUP. The next 11 weeks comprised a period of public consultation, during which AC held 249 public meetings and received 21,000 items of written feedback.
- 30 September 2013: AC released the Proposed AUP (PAUP) and notified the public that the PAUP was open for submissions. More than 13,000 submissions (from the public, government, and community groups) were made, with over 1.4 million separate points of submission.
- April 2014 to May 2016: an Independent Hearings Panel (IHP) was appointed by the central government, which subsequently held 249 days of hearings across 60 topics and received more than 10,000 items of evidence.
- 22 July 2016: the IHP set out recommended changes to the PAUP. One of the primary recommendations was the abolition of minimum lot sizes for existing parcels. The AC considered and voted on the IHP recommendations over the next 20 working days. On 27 July the public could access and view the IHP's recommendations.
- 19 August 2016: AC released the 'decisions version' of the AUP, including the new zoning maps. Several of the IHP's recommendations were voted down, including a IHP recommendation to abolish minimum floor sizes on apartments. However, the abolition of minimum lot sizes for existing parcels was maintained. This was followed by a 20-day period for the public to lodge appeals on the 'decisions version' in the Environment Court. Appeals to the High Court were only permitted if based on points of law.
- 8 November 2016: A public notice was placed in the media notifying that the AUP would become operational on 15 November 2016.
- 15 November 2016: AUP becomes operational. There were two elements of the AUP that were not fully operational at this time: (i) any parts that remain subject to the Environment Court and High Court under the Local Government Act 2010; and (ii) the regional coastal plan of the PAUP that required Minister of Conservation approval.

All versions of the AUP ('draft', 'proposed', 'decisions' and 'final') could be viewed online.



## 8.2 Upzoning Classification

For each geocoded permit in our sample, we identify the zone that previously applied to the permit's location using GIS databases for each of the seven plans. Then, for each of the approximately 115 residential zones (across the seven Councils) that previously existed, we calculate the floor area ratio (FAR) based on the height limits and site coverage ratios in the District and City Plans. Height limits are translated into storey limits based on building on with minimum 0.6 meters ground clearance, 2.68 meters per storey, and 1 meter for a roof. The maximum FAR is then calculated as the product of the effective storey limit and the site coverage ratio. Permits that are located in areas that were previously not zoned as residential (business, rural, or open space) are classified as upzoned since they have been converted to enable residential housing.

We can apply the same algorithm to identify the zones of individual land parcels in order to quantify the amount of upzoned land. We obtain GIS information on land parcels for November 2016, when the AUP was operationalized. We then assign each parcel to a pre- and post-AUP zone using the polygon's centroid, and apply the same algorithm described above to classify upzoned parcels. This process allows us to quantify the amount of upzoned land, as discussed in the section two.

The parcel dataset is also used to calculate the amount of undeveloped residential land and the zoning concentration measure for each SA. These measures are used in the spillover analysis in section 4.3.

## 8.3 Set Identification

This section is based on section 3.1 of RR.

Let  $\theta = l'\tau_{post}$  be a linear combination of the treatment parameters of interest, where  $l \in \mathbb{R}^{\bar{T}}$ . For example, if we are interested in the treatment effect in the final period,  $l = (0, \dots, 0, 1)'$ . Next, let  $\hat{\lambda}_n$  be a relevant  $m$ -subvector of the estimate  $\hat{\Lambda}_n = (\hat{\phi}'_n, \hat{\beta}'_n)'$ , where  $\hat{\lambda}_n \sim \mathcal{N}(\lambda, \Sigma_n)$ . That is, there exists a full column rank  $(\underline{T} + \bar{T}) \times m$  selection matrix  $\mathbf{J}$  such that  $\hat{\lambda}_n = \mathbf{J}'\hat{\Lambda}_n$ . The choice of  $\hat{\lambda}_n$  depends on both the parameter of interest  $\theta$  and the counterfactual set – a specific example is given below. We similarly define  $\lambda = \mathbf{J}'\Lambda$ , where  $\Lambda = (\phi', \beta')'$  can be decomposed as follows

$$\Lambda = \begin{bmatrix} \phi_{pre} \\ \phi_{post} \\ \beta_{pre} \\ \beta_{post} \end{bmatrix} = \begin{bmatrix} \phi_{pre} \\ \phi_{post} \\ \beta_{pre} \\ \delta_{post} \end{bmatrix} + \begin{bmatrix} 0 \\ 0 \\ 0 \\ \tau_{post} \end{bmatrix} =: \delta + \tau.$$

The decomposition above accords with that given for  $\beta$  in (2) of RR. We simply extend their framework to include  $\phi$  in the parameter space of interest.

We consider FLCIs based on affine estimators of  $\theta$  of the general form

$$\mathcal{C}_{\alpha,n}(a, v, \chi) = (a + v'\hat{\lambda}_n) \pm \chi,$$

where  $\alpha$  and  $\chi$  are scalars,  $v \in \mathbb{R}^m$ , and  $\alpha \in (0, 0.5]$  denotes a significance level. We choose  $a$  and  $v$  to minimize

$$\chi_n(a, v; \alpha) = \sigma_{v,n} \cdot cv_\alpha(\bar{b}(a, v) / \sigma_{v,n}),$$

where  $\sigma_{v,n} = \sqrt{v' \Sigma_n v}$ , and  $cv_\alpha(\cdot)$  denotes the  $1 - \alpha$  quantile of the folded normal distribution with unit variance,  $|\mathcal{N}(\cdot, 1)|$ . The quantity  $\bar{b}(a, v)$  denotes the worst-case bias of the affine estimator for a given  $a$  and  $v$ , namely

$$\bar{b}(a, v) := \sup_{\delta \in \Delta, \tau_{post} \in \mathbb{R}^{\bar{T}}} |a + v'(\mathbf{J}'\delta + \mathbf{J}'\tau) - l'\tau_{post}|, \quad (3)$$

where  $\Delta$  denotes the set of permissible values of  $\delta$  articulated under the counterfactual.

To showcase the method, we take  $\theta = \tau_{\bar{T}}$  (so that interest is focused on the final treatment effect) and define the set of counterfactuals based on the observed trend in the treatment group between  $t = \underline{T}$  and  $t = 0$  as

$$\Delta_{\bar{T}} = \left\{ \delta_{\bar{T}} : \delta_{\bar{T}} \in \left( -\hat{\phi}_{-\underline{T}} \cdot (\bar{T}/\underline{T}) - \hat{\phi}_{-\underline{T}} - M, -\hat{\phi}_{-\underline{T}} \cdot (\bar{T}/\underline{T}) - \hat{\phi}_{-\underline{T}} + M \right) \right\}, \quad (4)$$

where  $-\hat{\phi}_{-\underline{T}} \cdot (\bar{T}/\underline{T})$  is the counterfactual trend in the final period  $\bar{T}$ . Thus  $\lambda = (\phi_{-\underline{T}}, \phi_{\underline{T}}, \beta_{\bar{T}})$  is the subvector of parameters of interest. Using the estimates of  $\hat{\phi}_{-\underline{T}} = -1.827$  and  $\hat{\phi}_{\underline{T}} = -0.435$  from our empirical application means  $\Delta_{\bar{T}}$ , as defined in (4), allows  $\delta_{\bar{T}} \in (1.827 \times \frac{6}{5} + 0.435 - 5, 1.827 \times \frac{6}{5} + 0.435 + 5) = (-2.37, 7.63)$ . Figure 8 depicts the corresponding set of counterfactual values of  $\phi_{\bar{T}}$  when  $M = 5$ , leading to the interval  $(1.827 \times \frac{6}{5} - 5, 1.827 \times \frac{6}{5} + 5) = (-2.81, 7.19)$ . The affine estimator is then defined on  $\hat{\lambda}_n = (\hat{\phi}_{-\underline{T}}, \hat{\phi}_{\underline{T}}, \hat{\beta}_{\bar{T}})$ , and (3) can be more succinctly expressed as

$$\bar{b}(a, v) := \sup_{\delta_{\bar{T}} \in \Delta_{\bar{T}}, \tau_{\bar{T}} \in \mathbb{R}} \left| a + v' \left( \begin{bmatrix} \hat{\phi}_{-\underline{T}} \\ \hat{\phi}_{\underline{T}} \\ \delta_{\bar{T}} \end{bmatrix} + \begin{bmatrix} 0 \\ \phi_{\underline{T}} \\ \tau_{\bar{T}} \end{bmatrix} \right) - \tau_{\bar{T}} \right|.$$

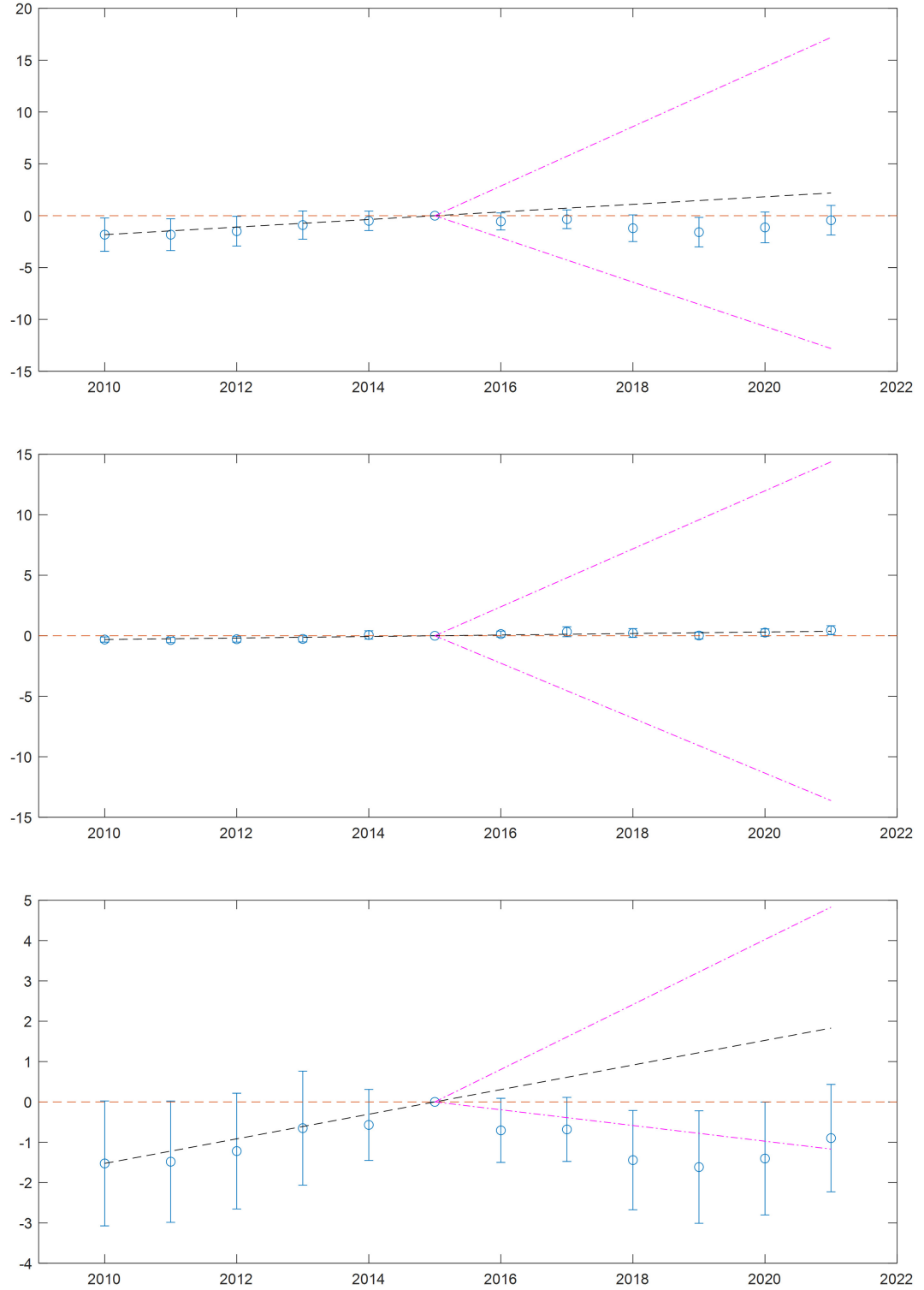
## 8.4 Structural Break in Trend in Permits

For  $j = 0$  (i.e., control group), we estimate

$$c_{i,j,t} = \alpha_{i,j} + \beta_j 1_{t \geq 1} + \delta_j t + \gamma_j 1_{t \geq 1} t + \varepsilon_{i,j,t}$$

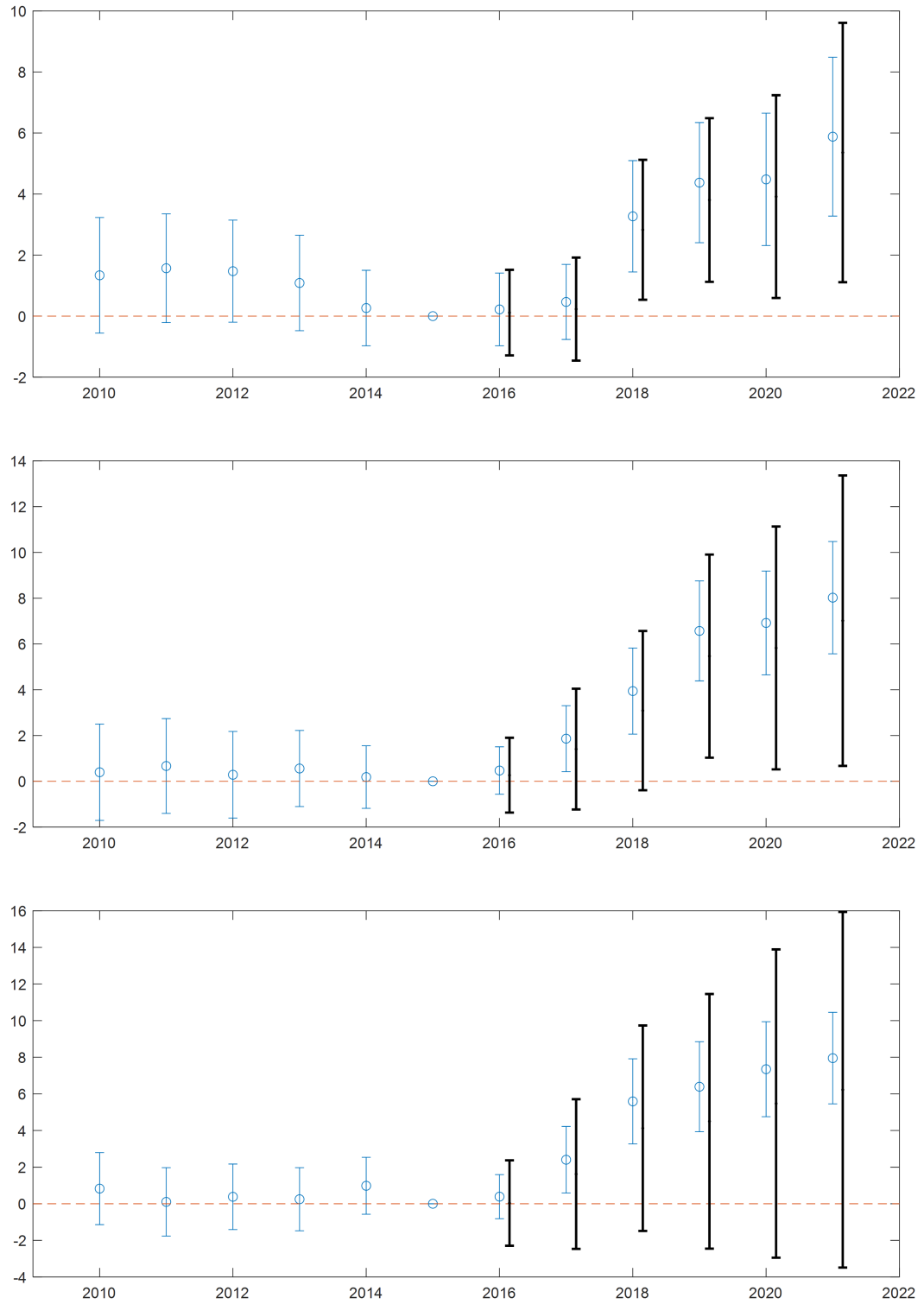
where recall that  $c_{i,j,t}$  is the number of permits in zone  $j$  in area  $i$  in year  $t$ ,  $1_{t \geq 1}$  denotes an indicator set to one for time periods after the treatment date (2015). OLS estimates are  $\hat{\beta}_0 = 2.168$  (t-statistic = 1.643),  $\hat{\delta}_0 = 0.392$  (t-stat = 3.176) and  $\hat{\gamma}_0 = -0.453$  (t-stat = -2.419), indicating a statistically significant reduction in the upwards linear trend after 2015. t-statistics are based on panel data Newey-West standard errors with the temporal bandwidth set to two periods.

Figure 15: Estimated Periods Effects and Post-Treatment Counterfactual Sets, 2010-2021.



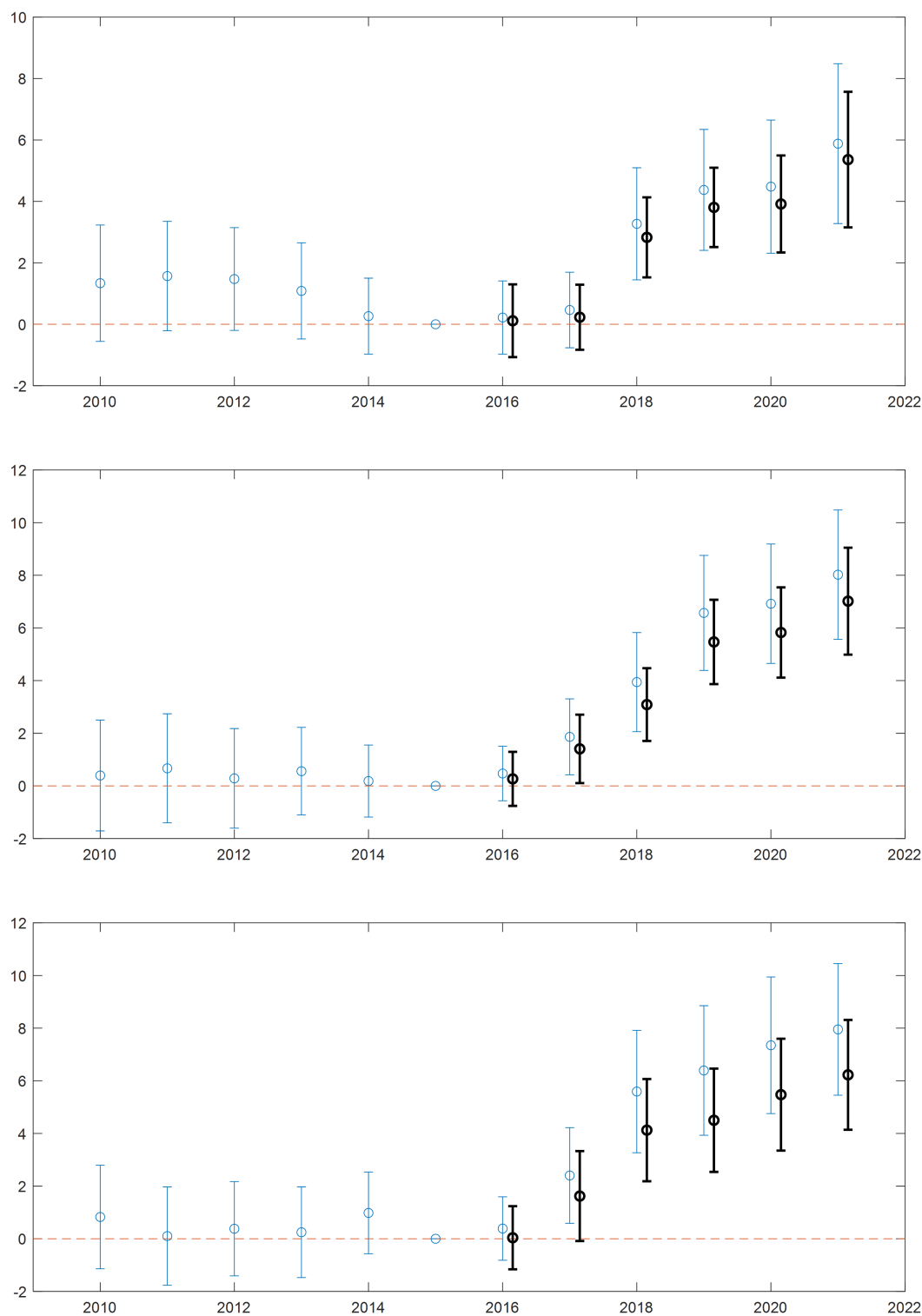
Notes: Outcomes in control group. OLS estimates of  $\{\phi_s\}$  (blue circles) alongside 95% confidence intervals (blue lines). Extrapolated pre-treatment trend over 2016 to 2021 given by the (black) dashed line. Set of counterfactual outcomes for 2016 to 2021 given by the space between the (pink) dot-dash lines. Top panel is all dwellings with counterfactual set specified as  $\pm 200$  dwellings in the final period; middle panel is attached dwellings with counterfactual set specified as  $\pm 175$  dwellings in the final period; bottom panel is detached dwellings with counterfactual set specified as  $\pm 50$  dwellings in the final period

Figure 16: Estimated Treatment Effects by Residential Zone, 2010-2021



Notes: Multi-period treatment effects (blue) and set-identified confidence intervals for post-treatment interventions (black). THA (top), MHU (middle) and MHS (bottom).  $M = 15$ .

Figure 17: Estimated Treatment Effects by Residential Zone under Counterfactual Trend, 2010-2021



Notes: Multi-period treatment effects and 95% confidence intervals (blue circles and thin lines) and treatment effects under counterfactual trend in the control group, 2016–2021 (thick black circles and lines). THA (top), MHU (middle) and MHS (bottom). Estimates under the counterfactual trend are given by restricting the span of the counterfactual set to zero, i.e.  $M = 0$ . Thick black circles denote spillover-robust point estimates of treatment effects.

## 8.5 Additional Figures

## 8.6 Robustness Checks

### 8.6.1 Alternative Treatment Dates

Figure 18 exhibits estimated treatment effects when 2013, 2014 or 2016 is used as the treatment date instead of 2015. Constraining the counterfactual to the linear trend results in 29,866, 27,786 and 33,191 additional permits when 2013, 2014 and 2016 are used as the treatment date, respectively. The corresponding upzoned differentials for these three treatment dates are 34,784, 33,464 and 35,789 permits, respectively.

### 8.6.2 Alternative Treatment and Control Areas

Figure 19 exhibits estimated treatment effects under different delineations of treatment and control areas. In the top panel, THA, MHU and MHS zones comprise the treatment areas, and SH is the control area. Constraining the counterfactual to the linear trend results in 27,073 additional permits. The upzoned differential is 34,569 permits. In the middle panel, downzoned areas are removed from the baseline sample, as defined in the main text. Constraining the counterfactual to the linear trend results in 26,418 additional permits. The upzoned differential is 34,124 permits. In the bottom panel, permits issued in Special Housing Areas are included in the sample used in the main text. Constraining the counterfactual to the linear trend results in 27,073 additional permits. The upzoned differential is 32,539 permits.

### 8.6.3 Local Period Fixed Effects

We estimate

$$c_{i,j,t} = \alpha_{i,j} + \sum_{s=-\underline{T}, s \neq 0}^{\bar{T}} \phi_{i,s} \mathbf{1}_{s=t} + \sum_{s=-\underline{T}, s \neq 0}^{\bar{T}} \beta_s \mathbf{1}_{s=t, j=1} + \varepsilon_{i,j,t} \quad (5)$$

where the period fixed effects  $\phi_{i,s}$  are now indexed by the statistical area  $i$ . Figure 20 exhibits estimated treatment effects.

### 8.6.4 Sales

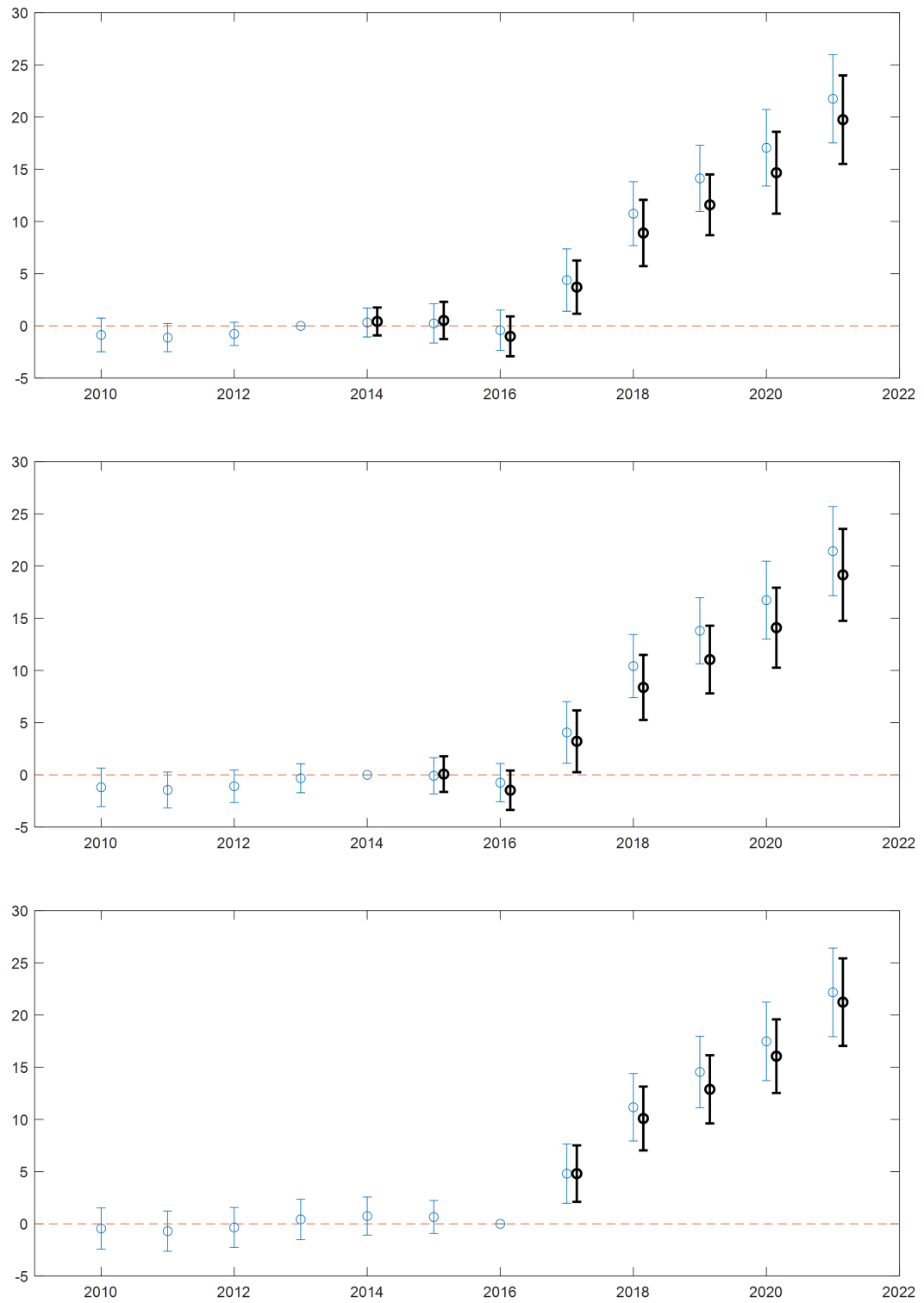
We fit the multi-period DID model to log of residential housing sales, showing that there is no significant increase in sales prior to implementation in 2016. We set 2013 as the treatment date, since the policy is first announced in March 2013.

### 8.6.5 Land Prices

Let  $p_{i,t}$  denote the log sales price of house  $i$  in year  $t$ . Our empirical model is

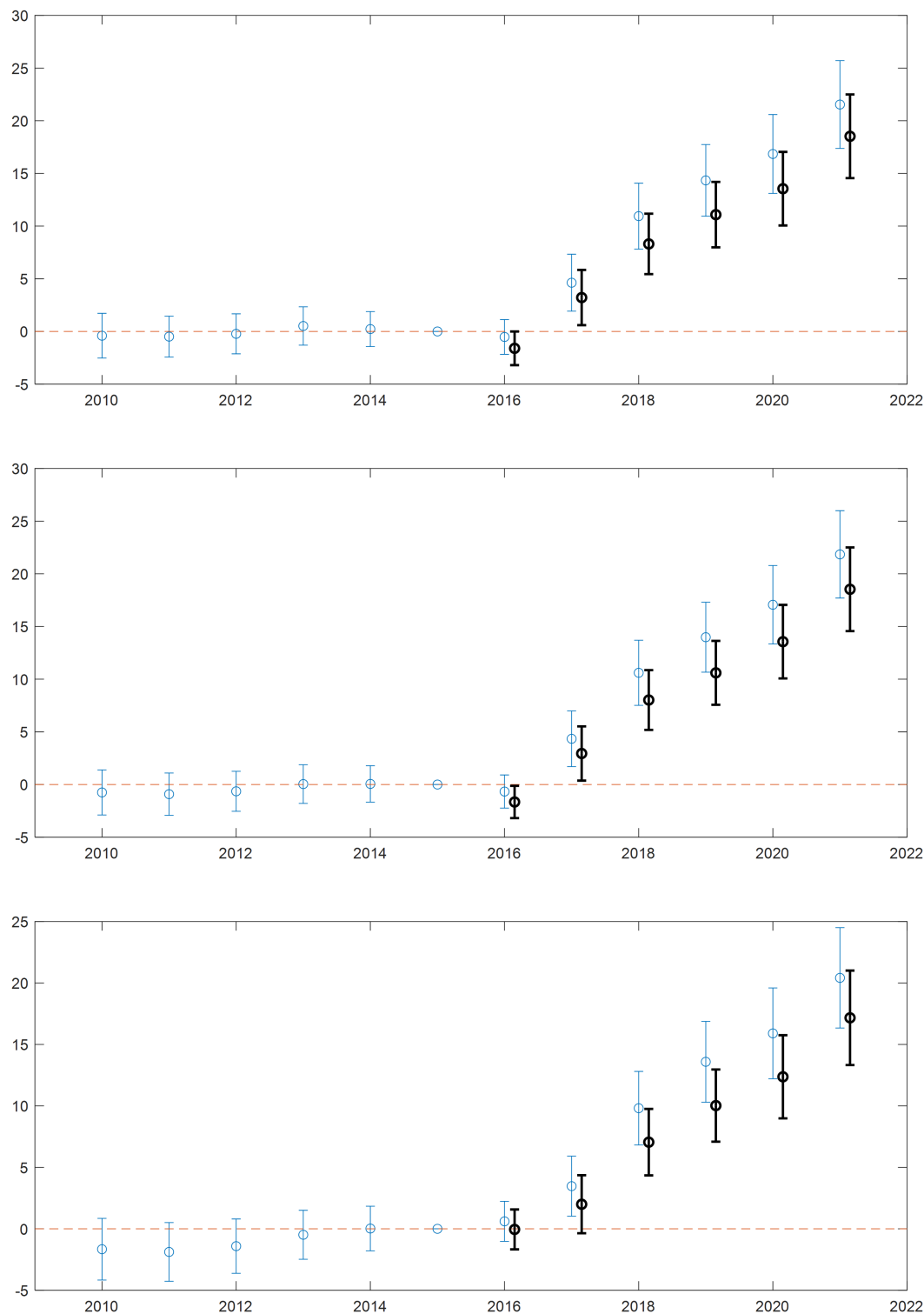
$$p_{i,t} = \delta'_0 Z_i + \sum_{s=-\underline{T}, s \neq 0}^{\bar{T}} \mathbf{1}_{s=t} \delta'_s Z_i + \mathbf{1}_{i \in u} \beta'_{0,j} Z_i + \sum_{s=-\underline{T}, s \neq 0}^{\bar{T}} \mathbf{1}_{s=t} \mathbf{1}_{i \in u} \beta'_{s,j} Z_i + \varepsilon_{i,t}, \quad (6)$$

Figure 18: Estimated Treatment Effects under Alternative Treatment Dates



Notes: Estimated multi-period treatment effects for log land prices. OLS estimates (circles) and 95% confidence intervals (lines). Treatment date is 2013 (top), 2014 (middle) and 2016 (bottom).

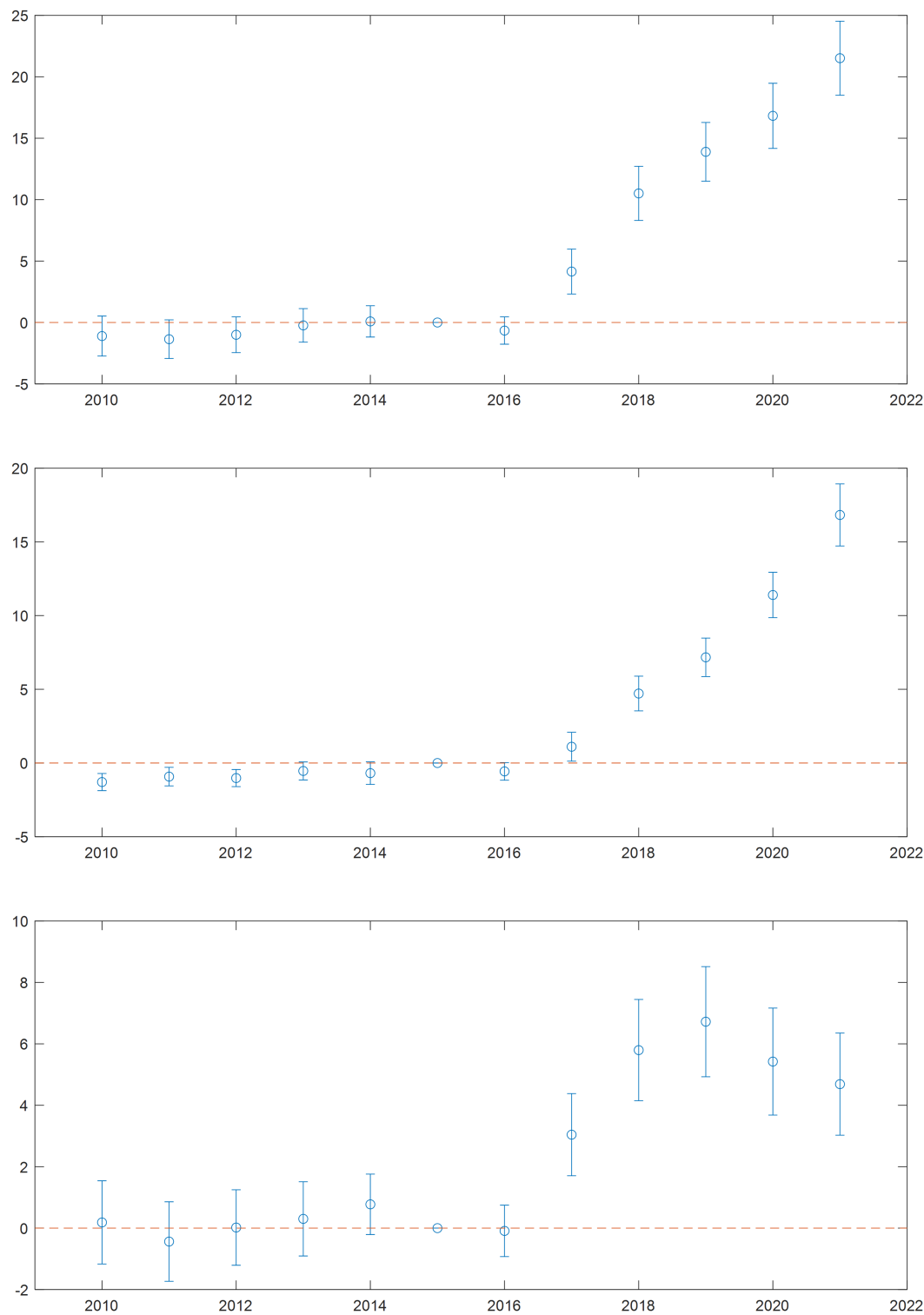
Figure 19: Estimated Treatment Effects under Alternative Treatment and Control Areas



Notes: Estimated multi-period treatment effects for log land prices. OLS estimates (circles) and 95% confidence intervals (lines). Top panel: THA, MHU and MHS zones comprise the treatment group and SH zones comprise the control group. Middle: Downzoned areas removed from the sample. Bottom: Permits issued under Special Housing Areas authority are included in the sample.

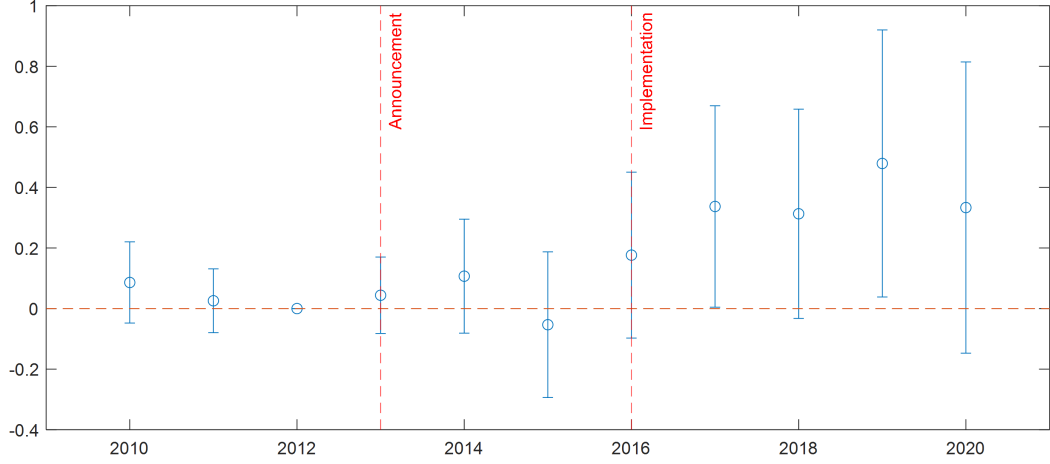


Figure 20: Estimated Treatment Effects under Alternative Treatment and Control Areas



Notes: Estimated multi-period treatment effects for log land prices. OLS estimates (circles) and 95% confidence intervals (lines). Top panel: THA, MHU and MHS zones comprise the treatment group and SH zones comprise the control group. Middle: Downzoned areas removed from the sample. Bottom: Permits issued under Special Housing Areas authority are included in the sample.

Figure 21: Estimated Treatment Effects for Sales, 2010-2021

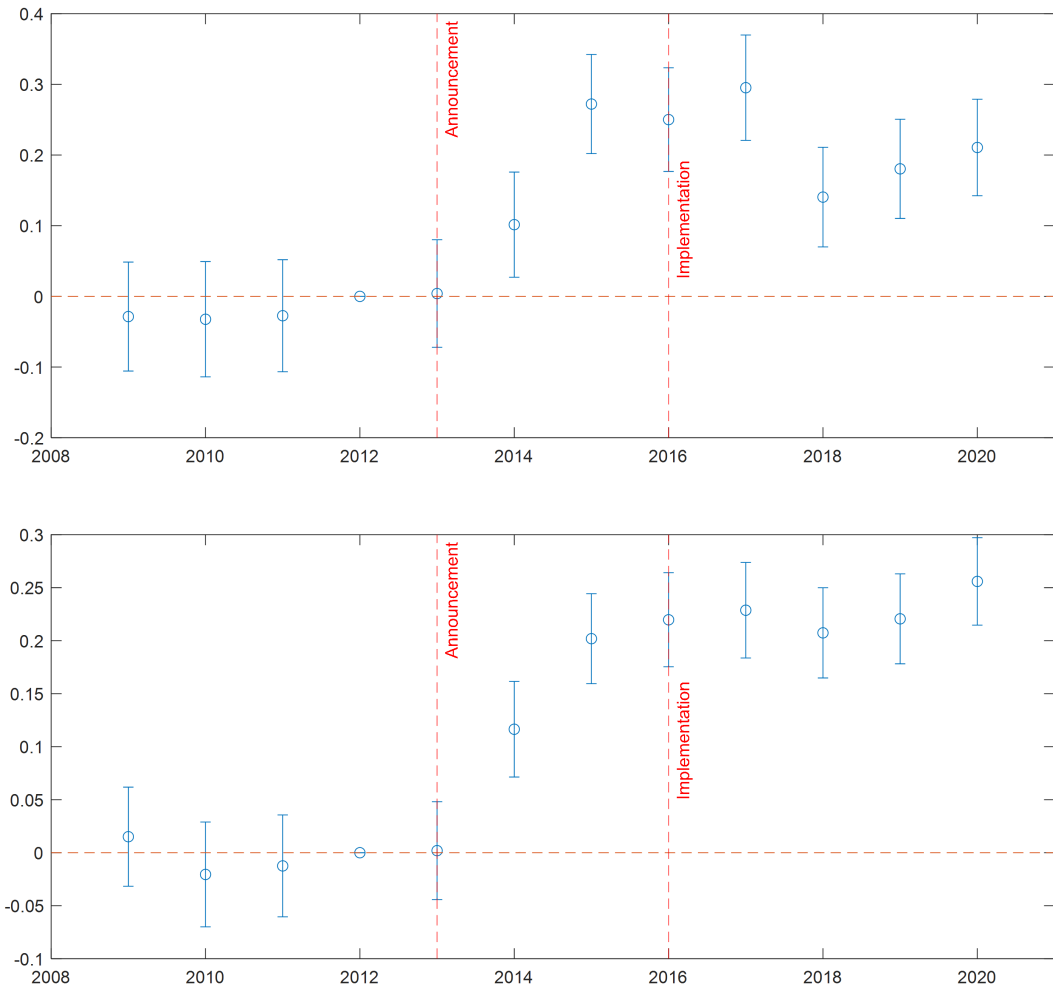


Notes: Estimated multi-period treatment effects for log sales. OLS estimates (circles) and 95% confidence intervals (lines).

where  $Z_i = (1, z_i, X_i')$ , and where  $z_i$  is site intensity, and  $X_i$  is a vector of standardized control variables, including floorspace, building age, number of garages, Manhattan distance to CBD, distance to nearest highway onramp, and distance to the nearest rapid transit station. The sample is a repeated sample of cross sections, since properties are not sold in every year of the sample period.  $\mathbf{1}_{i \in u}$  is an indicator equal to one if house  $i$  is located in an upzoned area.

Site intensity is the ratio of improvements to total value of the property. Valuations are constructed by Auckland Council for levying property taxes. Vacant land, and properties with improvements valued at zero, have a site intensity of zero. Thus, the average change in the land price differential between upzoned and non-upzoned properties in period  $s$  is given by the first element of  $\beta_{s,j}$ , holding all else equal. We plot point estimates of these coefficients in Figure 22, both with and without control variables in the model specification. Land prices in upzoned areas increase relative to non-upzoned areas soon after announcement, but not before, indicating that the policy was not anticipated by the market. The model with controls indicates that land prices in upzoned areas increased by between 20 and 25% relative to non-upzoned areas, holding all else constant.

Figure 22: Estimated Treatment Effects for Land Prices, 2010-2021



Notes: Estimated multi-period treatment effects for log land prices. OLS estimates (circles) and 95% confidence intervals (lines). Models with (bottom) and without (top) control variables.

## References

- ANAGOL, S., F. V. FERREIRA, AND J. M. REXER (2021): “Estimating the Economic Value of Zoning Reform,” *NBER Working Paper Series*, 29440. [9](#)
- ATKINSON-PALOMBO, C. (2010): “Comparing the capitalisation benefits of light-rail transit and overlay zoning for single-family houses and condos by neighbourhood type in metropolitan phoenix, Arizona,” *Urban Studies*, 47, 2409–2426. [2](#)
- AUCKLAND UNITARY PLAN INDEPENDENT HEARINGS PANEL (2016): “Report to Auckland Council Overview of recommendations on the proposed Auckland Unitary Plan,” . [2](#)
- BERTAUD, A. AND J. K. BRUECKNER (2005): “Analyzing building-height restrictions: Predicted impacts and welfare costs,” *Regional Science and Urban Economics*, 35, 109–125. [8](#)
- BRUECKNER, J. K., S. FU, Y. GU, AND J. ZHANG (2017): “Measuring the stringency of land use regulation: The case of China’s building height limits,” *Review of Economics and Statistics*, 99, 663–677. [6](#)
- BRUECKNER, J. K. AND R. SINGH (2020): “Stringency of land-use regulation: Building heights in US cities,” *Journal of Urban Economics*, 116, 103239. [6](#)
- BUTTS, K. (2021): “Difference-in-Differences Estimation with Spatial Spillovers,” *arXiv:2105.03737*. [3](#)
- CLARKE, D. (2017): “Estimating Difference-in-Differences in the Presence of Spillovers,” *Munich Personal RePEc Archive*, 52. [3](#)
- CUNNINGHAM, C. R. (2006): “House price uncertainty, timing of development, and vacant land prices: Evidence for real options in Seattle,” *Journal of Urban Economics*, 59, 1–31. [3](#)
- FAVILUKIS, J., P. M. STIJN, V. NIEUWERBURGH, P. MABILLE, AND S. V. NIEUWERBURGH (2023): “Affordable Housing and City Welfare,” *The Review of Economic Studies*, 90, 293–330. [2](#)
- FREDRICKSON, C. (2014): “Measuring Auckland’s Urban Extents: Background and Methodology,” <https://knowledgeauckland.org.nz/media/1922/measur>. [12](#)
- FREEMAN, L. AND J. SCHUETZ (2017): “Producing Affordable Housing in Rising Markets: What Works?” *Cityscape: A Journal of Policy Development and Research* 19. [2](#)
- FREEMARK, Y. (2019): “Upzoning Chicago: Impacts of a Zoning Reform on Property Values and Housing Construction,” *Urban Affairs Review*, 56, 758–789. [2](#), [3](#), [4](#), [20](#)
- (2021): “Doubling housing production in the Paris region: a multi-policy, multi-jurisdictional response,” *International Journal of Housing Policy*, 21, 291–305. [2](#)

- GLAESER, E. AND J. GYOURKO (2003): “The Impact of Building Restrictions on Housing Affordability,” *Economic Policy Review*, 9, 21–39. [2](#)
- GLAESER, E. L., J. GYOURKO, AND R. SAKS (2005): “Why is Manhattan so expensive? Regulation and the rise in housing prices,” *Journal of Law and Economics*, 48, 331–369. [2](#)
- GRAY, M. N. AND A. A. MILLSAP (2020): “Subdividing the Unzoned City: An Analysis of the Causes and Effects of Houston’s 1998 Subdivision Reform,” *Journal of Planning Education and Research*. [2](#)
- GREENAWAY-MCGREY, R., G. PACHECO, AND K. SORENSEN (2021): “The effect of upzoning on house prices and redevelopment premiums in Auckland, New Zealand,” *Urban Studies*, 58, 959–976. [3](#), [19](#)
- GREENAWAY-MCGREY, R. AND P. C. B. PHILLIPS (2021): “House prices and affordability,” *New Zealand Economic Papers*, 55, 1–6. [7](#)
- GYOURKO, J. AND R. MOLLOY (2015): “Regulation and Housing Supply,” *Handbook of Regional and Urban Economics*, 5, 1289–1337. [2](#), [3](#)
- HUBER, M. AND A. STEINMAYR (2021): “A Framework for Separating Individual-Level Treatment Effects From Spillover Effects,” *Journal of Business and Economic Statistics*, 39, 422–436. [3](#)
- IHLANFELDT, K. R. (2007): “The effect of land use regulation on housing and land prices,” *Journal of Urban Economics*, 61, 420–435. [2](#)
- KAHN, M. E., R. VAUGHN, AND J. ZASLOFF (2010): “The housing market effects of discrete land use regulations: Evidence from the California coastal boundary zone,” *Journal of Housing Economics*, 19, 269–279. [3](#)
- KAHN-LANG, A. AND K. LANG (2020): “The Promise and Pitfalls of Differences-in-Differences: Reflections on 16 and Pregnant and Other Applications,” *Journal of Business and Economic Statistics*, 38, 613–620. [8](#)
- LIMB, M. AND C. K. MURRAY (2022): “We zoned for density and got higher house prices: Supply and price effects of upzoning over 20 years,” *Urban Policy and Research*, DOI: 10.1080/08111146.2022.2124966. [2](#), [4](#), [20](#)
- MANVILLE, M., P. MONKKONEN, AND M. LENS (2019): “It’s Time to End Single-Family Zoning,” *Journal of the American Planning Association*, 86, 106–112. [2](#)
- NATIONAL PUBLIC RADIO (2019): “Across America: How U.S. Cities Are Tackling The Affordable Housing Crisis,” . [2](#)
- NEUMARK, D. AND H. SIMPSON (2015): *Place-Based Policies*, vol. 5, Elsevier B.V., 1 ed. [3](#)

- PENG, X. E. (2023): “The Dynamics of Urban Development : Evidence From Zoning Reform in New York,” *Working Paper*, [https://www.dropbox.com/s/3ojjh5vs8q9vchg/Peng\\_JMP](https://www.dropbox.com/s/3ojjh5vs8q9vchg/Peng_JMP). [2](#)
- QUIGLEY, J. M. AND S. RAPHAEL (2005): “Regulation and the High Cost of Housing in California,” *American Economic Review*, 95, 323–328. [2](#)
- QUIGLEY, J. M. AND L. A. ROSENTHAL (2005): “The Effects of Land Use Regulation on the Price of Housing: What Do We Know? What Can We Learn? The Effects of Land Use Regulation on the Price of Housing: What Do We Know? What Can We Learn? Cityscape 69,” Tech. Rep. 1. [2](#)
- RAMBACHAN, A. AND J. ROTH (2023): “A More Credible Approach to Parallel Trends,” *The Review of Economic Studies*, forthcomin. [3](#), [13](#), [14](#), [15](#)
- REDDING, S. J. AND M. A. TURNER (2015): *Transportation Costs and the Spatial Organization of Economic Activity*, vol. 5, Elsevier B.V., 1 ed. [3](#)
- RODRÍGUEZ-POSE, A. AND M. STORPER (2020): “Housing, urban growth and inequalities: The limits to deregulation and upzoning in reducing economic and spatial inequality,” *Urban Studies*, 57, 223–248. [2](#)
- SCHILL, M. H. (2005): “Regulations and Housing Development: What We Know,” *Cityscape: A Journal of Policy Development and Research*, 8, 5–19. [2](#)
- TAN, Y., Z. WANG, AND Q. ZHANG (2020): “Land-use regulation and the intensive margin of housing supply,” *Journal of Urban Economics*, 115, 103199. [6](#)
- THORSON, J. A. (1997): “The Effect of Zoning on Housing Construction,” *Journal of Housing Economics*, 6, 81–91. [3](#)
- TURNER, M., A. HAUGHWOUT, AND W. VAN DER KLAUW (2017): “Land Use Regulation and Welfare,” *Econometrica*, 82, 1341–1403. [9](#), [12](#), [13](#), [19](#)
- WETZSTEIN, S. (2017): “The global urban housing affordability crisis,” *Urban Studies*, 54, 3159–3177. [2](#)
- (2021): “Assessing post-GFC housing affordability interventions: a qualitative exploration across five international cities,” *International Journal of Housing Policy*, 21, 70–102. [2](#)
- ZABEL, J. AND M. DALTON (2011): “The impact of minimum lot size regulations on house prices in Eastern Massachusetts,” *Regional Science and Urban Economics*, 41, 571–583. [2](#)
- ZHOU, J., D. P. McMILLEN, AND J. F. McDONALD (2008): “Land Values and the 1957 Comprehensive Amendment to the Chicago Zoning Ordinance,” *Urban Studies*, 45, 1647–1661. [3](#)