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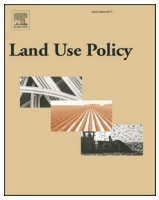

Dispelling myths: Reviewing the evidence on zoning reforms in Auckland☆

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ABSTRACT

In 2016, the city of Auckland adopted zoning reforms that enabled more housing on approximately three-quarters of its urban land. Three subsequent studies have found that these reforms increased housing supply and reduced rents. Two economists have, however, criticised these studies on blogs and social media, describing their findings as a "myth". Despite their informal nature, these critiques have been cited in formal planning and policy processes. Here, we review these critiques and find them to have little to no merit. Specifically, the critiques misunderstand the papers' methods and rely on inappropriate analyses. In our view, there is remarkably robust evidence that zoning reforms increased housing supply and reduced rents in Auckland.

> Myths which are believed in tend to become true.
> *Accredited to George Orwell*

1. Introduction

Economists have long been interested in the effects of planning policies on housing outcomes (for reviews, see Gyourko and Molloy, 2015; Molloy, 2020). Where planning policies act to constrain the development of housing, then conventional economic models predict they will lead to reduced housing supply (a "quantity" effect) and higher housing prices (a "price" effect), and vice versa where planning policies enable housing. The theoretical economic framework that produces these predictions is, however, hotly contested, especially by so-called "supply sceptics" who argue either that planning reforms do not increase housing supply or that increased housing supply does not lead to lower housing prices (for rebuttals of these arguments, see Manville et al. 2022 and Been et al. 2024). In this context, there is heightened need for empirical research to confirm whether the effects predicted by conventional economic models do indeed occur in practice and are large enough to make a meaningful contribution to housing affordability.

Previous efforts to undertake empirical research into the effects of planning policies on housing outcomes have, however, run into a simple problem: Large changes in planning policies are quite rare. For this reason, most empirical research has had to analyse the effects of small and/or gradual changes in planning policies on housing outcomes between locations and over time (see, e.g. Wallace, 1988; Mayer and Somerville, 2000; Glaeser and Gyourko, 2018). In these studies, the gradual and/or small size of the changes in planning policies vis-à-vis other factors make it more difficult to draw causal inferences about the effects of reforms on quantities and prices. Additionally, the empirical effects of small and/or gradual changes may be substantively different from major reforms. By significantly expanding the supply of developable parcels, for example, major reforms could increase competition between landowners and have larger effects than small (or, "spot") upzonings. Until recently, empirical analyses of housing outcomes before and after the adoption of *major* planning reforms have been a notable gap in the literature.

Nonetheless, growing concerns with housing affordability have

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prompted some jurisdictions to progress policy reforms to enable more housing, which is often described as "upzoning". The case for upzoning is often premised on the predictions of conventional economic models as noted above, where planning policies are seen as constraints on development that reduce housing supply and increase housing prices.[1] Perhaps the most notable example of upzoning comes from the city of Auckland, New Zealand, where the amalgamation of seven councils necessitated the development of a new set of planning rules known as the Auckland Unitary Plan, or "AUP" (for a background to the AUP, see Greenaway-McGrevy and Jones, 2023). By upzoning approximately three-quarters of Auckland's urban land, the AUP presented researchers with a rare opportunity to study the empirical effects of major zoning reforms on housing outcomes. As a result, Auckland is now home to the most well-studied case study of major zoning reforms globally. The findings from three quasi-experimental studies that seek to quantify the empirical effects of the AUP are central to this paper and are worth introducing briefly from the outset.[2]

First, Greenaway-McGrevy and Phillips (2023) analyse the impact of the AUP on building consents ("permits") for dwellings by comparing upzoned and non-upzoned residential areas within Auckland from 2010–2021. The authors estimate that the AUP led to an additional 21,808 consents after five years, which is around 4 % of Auckland's housing stock. Second, Greenaway-McGrevy (2023a) also analyses the impact of the AUP on consents but uses a different method that compares outcomes in Auckland to similar cities in New Zealand that did not upzone. This study finds even larger effects: The AUP led to an additional 43,500 consents within six years, or approximately 9 % of the housing stock. Third, Greenaway-McGrevy and So (2024) analyse the effects of the AUP on rents. Compared to similar cities in New Zealand that did not upzone, the authors find rents for comparable properties in Auckland six years after the AUP are 28 % lower than they would have been otherwise. Taken together, these studies provide evidence that, before the AUP, planning policies in Auckland were acting to constrain housing supply and increase housing prices — exactly as predicted by conventional economic models.[3]

At a time of growing concern with housing affordability, the three quasi-experimental studies of upzoning in Auckland have been received with interest by researchers, policy makers, and elected representatives alike — not just in New Zealand but also globally.[4]

Nevertheless, two economists — namely, Cameron Murray and Tim Helm (hereafter, "Murray and Helm") — have strongly criticised these three studies of the effects of upzoning in Auckland and somewhat controversially concluded that their findings are a "myth". Murray and Helm argue that *none* of the aforementioned evidence is credible. In contrast, our assessment finds that Murray and Helm's critiques have little to no merit: There is strong evidence that Auckland's upzoning has had large effects on housing outcomes. Indeed, even a cursory look at the data reveals that housing supply in Auckland has grown rapidly. As

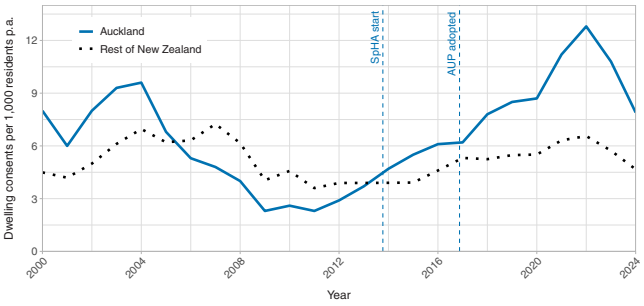

Fig. 1. Dwelling consents per 1000 residents in Auckland and rest of New Zealand 2000-2024. *Notes:* The "Rest of New Zealand" includes all other parts of New Zealand but exclude the Canterbury and Wellington regions, which were affected by an earthquake and zoning reforms in this period, respectively. The vertical line labelled "SpHA start" denotes when upzoning under the AUP was selectively applied to some areas of Auckland. The dwelling consent data for recent years is provisional and subject to revisions.

shown in Figure 1, dwelling consents in Auckland surged after the AUP to levels that were one-third higher than their previous peak, at the same time as consents in other parts of New Zealand remained fairly stable.

This discussion hints at a key question that is central to this paper: What housing outcomes would have been observed in Auckland in the *absence* of the AUP? Answers to this hypothetical question define the "counterfactuals" to which we can compare actual outcomes, such as consents and rents, in the wake of the AUP. In the three quasi-experimental studies discussed above, the counterfactuals are defined by non-upzoned areas of Auckland (cf. Greenaway-McGrevy and Phillips 2023) or non-upzoned cities in New Zealand (cf. Greenaway-McGrevy 2023a; Greenaway-McGrevy and So 2024).[5] More formally, these three quasi-experimental studies seek to infer the causal impacts of the AUP by comparing outcomes for locations that are subject to upzoning (the "treated" group) to outcomes for locations that are not (the "control" group). The underlying assumption is — in the absence of the AUP — outcomes for locations in the control group would be identical to those in the treated group. In turn, this implies the studies must carefully select the locations in the control group that define the counterfactual.

While there is room for debate on the most appropriate approach to defining the counterfactual for housing outcomes in Auckland, we find *all* reasonable methods imply the AUP had economically and statistically significant effects. The consistency of this finding suggests the impacts of upzoning are relatively robust to methodological choices. Although there is value in critiques of economic papers, including but not limited to these three studies from Auckland, we suggest Murray and Helm's arguments do not help to inform the debate. In Section 2, we provide a brief background to Murray and Helm's critiques and elaborate on our motivations for writing this paper.

2. Background

The first quasi-experimental study of the AUP was Greenaway-McGrevy and Phillips (2023), which was published online in the Journal of Urban Economics on 31 May 2023.[6] Five days later on 4 June 2023, Murray and Helm published a blog post titled "The Auckland Myth: There is no evidence that upzoning increased housing construction" (Murray and Helm, 2023a). As Greenaway-McGrevy and Phillips

[1] For an example of these arguments, see NZ Productivity Commission (2024), cf. p. 9).

[2] Two of these three studies are currently working papers. A fourth (published) quasi-experimental study analyses the impacts of the AUP on redevelopment premiums (Greenaway-McGrevy et al., 2021).

[3] The recent paper by Greenaway-McGrevy (2025) finds similar effects using a structural economic model that analyses the impact of upzoning in Auckland on residential floorspace and house prices.

[4] These three papers have, for example, been cited by the Australian Productivity Commission (2022); the NSW Productivity Commission (2023); the Grattan Institute (Coates and Moloney, 2023); the New South Wales Premier (NSW Parliament, 2024) and Housing Minister (ABC, 2024); the Chief Economist for Auckland Council (Jones et al., 2024); the Centre for Independent Studies (Tulip, 2024); Australian Treasury (2024); and the current Australian Government Housing Minister (O'Neil, 2024). In the US, Auckland's zoning reforms are discussed in, for example, West and Garlick (2023) and Politano (2024).

[5] Greenaway-McGrevy (2023a) also presents the results of a sensitivity test where outcomes in Auckland are compared to capital cities and states in Australia. The results of this test imply even larger effects.

[6] Versions of the working paper have been available to the public since 2021.

(2023) was at the time the only published quasi-experimental study of upzoning, Murray and Helm (2023a) claimed they had rebutted the evidence for upzoning.[7] Before publishing their blog post, Murray and Helm emailed some of their concerns to the lead author of Greenaway-McGrevy and Phillips (2023), who subsequently sought to address them in an extension paper, hereafter "Extension Paper" (Greenaway-McGrevy, 2023b). As the Extension Paper was also published on 31 May 2023 — *before* the publication of Murray and Helm (2023a) — we are unsure why the blog post did not address the Extension Paper in detail given that, as we shall argue below, it thoroughly addresses many of their concerns.

Soon after Murray and Helm's first blog post, one of the authors of this paper published a blog post titled "A response to Murray and Helm on Auckland's upzoning" (Maltman, 2023). This post considered Murray and Helm's critiques, noted the existence of the Extension Paper, and concluded Greenaway-McGrevy and Phillips (2023)'s methods and results appeared to be robust. On 27 August 2023, Murray and Helm released a second blog post that claimed to tackle Maltman's response, but instead mostly repackaged their initial concerns (Murray and Helm, 2023b).[8] In the wake of their two blog posts, Murray and Helm have posted on social media that the impacts of upzoning in Auckland are a "myth" (see, e.g., Helm, 2024a). Although these critiques initially did not engage with the two more recent papers noted above — that is, Greenaway-McGrevy (2023a) and Greenaway-McGrevy and So (2024) — this changed on 22 August 2024, when Helm claimed the results of these papers were also "utterly implausible" (Helm, 2024a).

Given that Murray and Helm have published their comments via informal channels *and* subsequent sections of this paper find them to have little to no merit, readers may wonder why these critiques would warrant our attention. We have three main reasons for wanting to formally document and assess Murray and Helm's critiques in this paper.

First, Murray and Helm have cited their blog posts in their submissions to formal policy and planning processes. Helm (2024b), for example, cited Murray and Helm (2023a) in evidence submitted to a planning process in Wellington, New Zealand. This evidence appears to have swayed Commissioners in this process, who determined that planning policies did not play a "dominant role in housing affordability" in Wellington (cf. p. 45 Independent Hearings Panel, 2024). Similarly, a parliamentary inquiry in Australia concluded that the evidence on Auckland's upzoning was contested, citing evidence submitted by Murray (see NSW Parliament, 2024a, paras 3.47–3.50). Given their apparent influence on policy and planning processes, we consider there is a public interest in formally documenting and assessing Murray and Helm's critiques in this paper.

Second, many of Murray and Helm's critiques diverge from the wider economic evidence. Not only is there robust quasi-experimental evidence that upzoning increased housing supply and reduced rents in Auckland, but this evidence dovetails with a large number of other economic studies that also find planning policies can affect both the supply and price of housing.[9] The combined weight of this evidence, moreover, appears to have persuaded a majority of economists. In a survey of notable economists conducted by the Economic Society of Australia, 65 % of respondents believed 'easing planning restrictions' is one of the top 3 measures that governments can take to improve housing affordability (Martin, 2023). Similarly, a survey undertaken by the New Zealand Association of Economists found around 95 % of respondents believed that land use restrictions reduced housing supply and affordability (Wesselbaum, 2023). In this context, we see value in contrasting Murray and Helm's critiques with the wider economic evidence.[10]

Third, this episode raises questions about how planning processes engage with economic evidence. In our view, the adverse influence of Murray and Helm's critiques provides a timely reminder of the value of more formal literature, such as working papers and peer-reviewed articles, compared to informal channels, such as blog posts and online comments. While formal literature is not immune to mistakes and misrepresentations, such problems are more likely to be identified and addressed — whether by the original researchers, peer reviewers, journal editors, or subsequent researchers. Interestingly, the Commissioners in Wellington admitted under questioning their decisions were informed more by Helm's *oral testimony* than his *written evidence* (cf. 48 mins, Wellington City Council, 2024). If the Commissioners had instead put more weight on Helm's written evidence, then they might have noticed that it relied heavily on a blog post that he had co-authored, rather than more formal sources. By documenting some of the most egregious errors that affect Murray and Helm's informal critiques, we hope to stimulate debate on how planning and policy processes can best engage with economic evidence.

The adverse influence of Murray and Helm's critiques on policy, their divergence from mainstream economic evidence and opinion, and the implications of this episode for planning and policy processes have motivated us to write this paper. In doing so, we hope to support more informed conversations, guide further research, and contribute to the adoption of evidence-based policies. We structure the following sections of this paper as follows: Section 3 considers Murray and Helm's critiques of Greenaway-McGrevy and Phillips (2023); Section 4 considers the reasonableness of various possible counterfactuals; Section 5 considers corroborating evidence; and Section 6 concludes.

3. Critiques of Greenaway-McGrevy and Phillips (2023)

In this section, we consider Murray and Helm's critiques of Greenaway-McGrevy and Phillips (2023). We decompose these critiques into three sub-sections: First, the selection of the treated and control groups; second, the distinction between consents and completions; and third, the econometric methods that underpin the analysis.

[7] As we shall discuss below, this claim contradicts a considerable body of evidence from other jurisdictions and contexts that also finds planning policies, like zoning, can constrain housing supply.

[8] Whereas the first blog post in Murray and Helm (2023a) mostly focused on the merits of the methods used in Greenaway-McGrevy and Phillips (2023), the second blog post in Murray and Helm (2023b) implied that others had ulterior motives, for example observing "… the story that upzoning produced a huge building boom is becoming an urban myth. Cherry-picking figures, uncritically citing a paper with known methodological issues, and writing fairy tales about a small and plucky city far away is well and good when pushing a policy agenda …But if that's your game with Auckland and upzoning, please be honest enough to admit you're playing politics, not doing economic science". The rhetoric was taken even further in a recent comment on social media, where Helm stated: "Do the people pushing it believe a data fudge or two is okay in service of a good cause? Because I don't. We need honesty. Good housing policy needs smart people to stop pretending to be stupid. Suspending your critical faculties because you like the policy story is not okay … right now, the public is being deceived. Presumably, no-one is orchestrating a conspiracy to enrich landowners at the expense of taxpayers, by misleading the public to ram through unpopular changes, but if they were, they couldn't do a better job" (Helm, 2024a).

[9] See, e.g. Hilber and Vermeulen (2016); Jackson (2016); Eriksen and Orlando (2022); Molloy et al. (2022); Ahlfeldt et al. (2023); Asquith et al. (2023); Maltman and Greenaway-McGrevy (2025); Greenaway-McGrevy (2025). For evidence of the microeconomic channels, or behavioural mechanisms, through which new housing can support housing affordability more widely see Mast (2023); Bratu et al. (2023). The latter use detailed data to track individual households over time and find that new housing creates vacancies that extend into the wider area via a series of household moves ("moving chains"), quickly alleviating housing pressures in middle- and low-income suburbs.

[10] Appendix A examines several additional related critiques advanced by Murray and Helm, such as landbanking, which are, in our view, unsupported by the economic evidence.

3.1. Selection of the treated and control groups

This aspect of Murray and Helm's critique focuses on the sample used in Greenaway-McGrevy and Phillips (2023), which excludes data for some areas of Auckland that — if they are included — appears to reduce the impacts of upzoning. Murray and Helm include this data in the chart in the left panel of Figure 2, which they argue is a more reasonable representation of trends in dwelling consents in upzoned and non-upzoned areas of Auckland than the chart from Greenaway-McGrevy and Phillips (2023) that is shown in the right panel. Murray and Helm's critique implies the increase in dwelling consents in upzoned areas is a continuation of existing trends before the AUP came into effect (the "pre-treatment period"), rather than an effect of the AUP in 2016.[11]

This critique is flawed for three reasons. First, Murray and Helm misunderstand how researchers select the treated and control groups in quasi-experimental studies. Second, Murray and Helm do not inform their readers that Greenaway-McGrevy and Phillips (2023) transparently disclose why they choose to remove these data and, most importantly, demonstrate that their results are robust to the inclusion of this data. Third, Murray and Helm mistakenly include all these data in the treated group when many of these consents were, in fact, in areas that were not upzoned. When correctly assigned, trends between upzoned and non-upzoned regions in the pre-treatment period appear comparable.

3.1.1. Designing a 'quasi'-experiment

Murray and Helm's critique misunderstands how quasi-experimental research works in practice. Unlike randomised control trials ("RCTs"), quasi-experimental research must assess the effects of interventions in the absence of randomization.[12] This requires researchers to construct a "quasi" experiment by using statistical methods to approximate randomization or by selecting a control group that closely matches the treated group in all respects except for exposure to the intervention. In quasi-experimental designs, researchers thus carefully select the data that goes into the control and treated groups to avoid bias; simply including all the available data is rarely appropriate.

In this spirit, Greenaway-McGrevy and Phillips (2023) deliberately select non-upzoned areas that they expect will provide an appropriate counterfactual to upzoned areas in Auckland. Accordingly, the authors intentionally contrast urban residential areas that were impacted by the AUP in 2016 with similar but unaffected urban residential areas. This approach *strengthens* the validity and reliability of the results by ensuring a meaningful "like-for-like" comparison. There is, however, one downside of allowing researchers to curate their sample to improve comparability: It could increase the risk that data is selectively used to support specific findings.[13] To mitigate this risk, quasi-experimental studies should ideally a) disclose any excluded data along with the rationale for exclusion and b) investigate the robustness of their results to the excluded data. As the following sub-section shows, Greenaway-McGrevy and Phillips (2023) does both.

3.1.2. Disclosure and robustness

As well as misunderstanding quasi-experimental methods, Murray and Helm's critique suffers from a second flaw: Greenaway-McGrevy and Phillips (2023) disclose their reasons for excluding some data and show their results are robust to their inclusion.

Specifically, to support the "like-for-like" comparisons that underpin the use of quasi-experimental methods, Greenaway-McGrevy and Phillips (2023)'s sample deliberately and transparently excludes two types of data. First, they limit the sample only to 'residential' areas, omitting rural and business areas. Although the latter sometimes allow residential development, the nature of housing tends to be quite different. Business areas, for example, may permit housing over commercial or retail space, but this is unlikely to be a suitable counterfactual for areas where single-detached dwellings are upzoned to medium-density housing. Second, Greenaway-McGrevy and Phillips (2023) also exclude data associated with Special Housing Areas, or "SpHA", which ran from September 2013 until the adoption of the AUP in December 2016, noting the following reasons (p. 5):

> "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. Developers could also use more relaxed planning rules from a preliminary version of the plan (the "Proposed AUP", notified in September 2013).
> SpHA were disestablished once the AUP became operational. We exclude permits issued in SpHA prior to 2017 as a disproportionate share of SpHA permits are in locations that were later upzoned. A robustness check reported in the Appendix demonstrates that our findings are not substantively affected when these permits are included in the analysis."

Here, Greenaway-McGrevy and Phillips (2023) observe that they chose to remove data in SpHA *because developments in these areas could make use of AUP rules in advance*. Given Greenaway-McGrevy and Phillips (2023)'s focus on the 2016 date at which the AUP applied to all of Auckland, it would not be appropriate to include SpHA in either the control group (that is, non-upzoned areas in Auckland) or in the treated group.

In our view, removing data for rural/business areas and SpHAs is both standard practice and ex ante reasonable. We expect most researchers who are familiar with quasi-experimental studies would be more concerned if these data were included.

Notwithstanding these transparent disclosures, Greenaway-McGrevy and Phillips (2023) and the Extension Paper also document the results of sensitivity tests that show *even if* these data are included, the AUP still has a substantial (in fact, larger) impact on dwelling consents. The Appendix to Greenaway-McGrevy and Phillips (2023), for example, presents a sensitivity test where SpHAs are included in the control group with the original 2016 timing. Although this test is conservative, it nonetheless still finds the AUP had a large and statistically significant impact on consents. The Extension Paper presents another sensitivity test that is, in our view, more appropriate: All data — including SpHAs, rural, and business areas — are included in the sample with an additional treatment date set to 2013 (Greenaway-McGrevy, 2023b). In this test, the structural break in 2016 disappears, but a new break emerges in 2013 when the SpHAs began to take effect. The Extension Paper describes the results of this sensitivity test as follows (p. 14):

> "Total permits no longer exhibit a substantial break in trend in 2016, when the AUP became operational. However, the decomposition into upzoned and remaining areas illustrates that much of this is due to permits in upzoned areas growing at a faster rate between 2013 and 2016. Thus, much of the increase in the interim period between 2013 and 2016 is occurring in areas targeted for upzoning under the AUP."

Crucially, the Extension Paper finds that including all the data serves

[11] Different trends in the 'treated' and 'control' groups also violates a key assumption of the difference-in-difference ("DiD") methods that are used in Greenaway-McGrevy and Phillips (2023).

[12] In RCTs, researchers can randomly assign subjects into treated and control (or, "placebo") groups. Comparing outcomes between the treated and control groups is then sufficient to identify the causal effect of the treatment. RCTs are standard practice in medical trials, although much less common in economics.

[13] Recently, Helm suggested the sample choices made by the authors of Greenaway-McGrevy and Phillips (2023) were *intended* to exaggerate the impacts of the AUP, arguing they "… omitted inconvenient data, creating a heavily biased sample with a structural break that did not exist in reality" (Helm, 2024a).

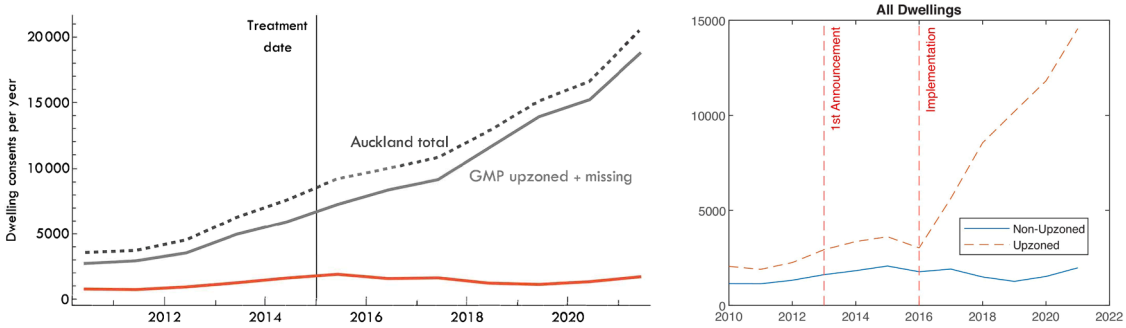

Fig. 2. Comparing trends in dwelling consents from Murray and Helm (2023a) to Greenaway-McGrevy and Phillips (cf Figure 3, (2023)).

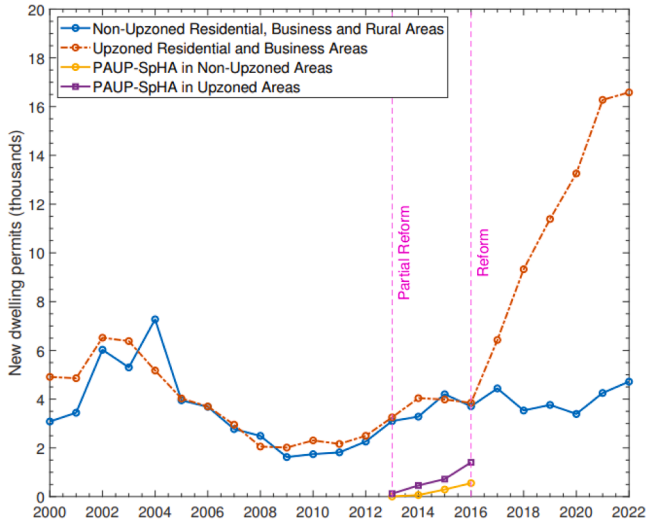

Fig. 3. Trends in total dwelling consents in Auckland 2000–2022 (Source: Greenaway-McGrevy and Phillips (cf Figure 3, 2023)).

to increase the estimated impacts of the AUP, because the latter now affects a larger area for a longer period (p. 19): "The incorporation of the SpHA generally lends support to the evidence that upzoning increased dwelling construction permits in Auckland. Set-identified treatment effects remain statistically significant under larger counterfactual sets, and point estimates of the increase in permits under linear trend counterfactuals are greater."

Together, these sensitivity tests show Greenaway-McGrevy and Phillips (2023)'s results are robust to the exclusion of these data. Indeed, including these data leads to larger impacts from upzoning. As Murray and Helm's blog posts and social media comments have not explicitly acknowledged nor engaged with the results of these sensitivity tests, we conclude that this aspect of their critiques has little to no merit.

3.1.3. *Clarifying the treatment*

The third flaw in Murray and Helm's critique is that it muddies the treatment of upzoning under the AUP in two crucial ways. First, Murray and Helm incorrectly assign data to the treated group and, second, they inaccurately represent the timing of the treatment.

As demonstrated in Figure 2, Murray and Helm lump all the missing data (that is, all business, rural, and SpHAs zones in Auckland) into the 'treated' or upzoned group. While *some* of these areas were affected by the AUP (notably SpHAs, as well as some rural/business areas being converted to residential or allowing for greater development), many were not. Placing all these missing data into the 'treated' or upzoned group is not accurate, and gives the false impression that a) non-upzoned areas were not a good counterfactual for upzoned areas, and b) that there was already strong growth in dwelling consents in upzoned areas prior to the adoption of the AUP.

In contrast, when undertaking the sensitivity test that was described in Section 3.1.2, the Extension Paper assigns these data to the correct group and with the correct timing (Greenaway-McGrevy, 2023b). Figure 3 illustrates the trends that result when the data is correctly assigned as upzoned under the AUP (or, "treated" in 2016), not upzoned under the AUP (or, "control"), and SpHAs ("treated" but with the timing of treatment occurring in 2013). We can see from Figure 3 that both the treated and control groups have comparable outcomes during the pre-treatment period. Additionally, we see growth in consents in SpHAs exceeded other areas in Auckland from 2013 to 2016, as upzoning began to impact dwelling consents in these areas. And, finally, outcomes in the treated group diverge rapidly following the adoption of the AUP in 2016.

The trends in Figure 3 differ from those in the left panel of Figure 2 because the latter incorrectly assigns data between the treated and control groups and inaccurately represents the timing of the treatment. Our assessment thus finds the data used in Greenaway-McGrevy and Phillips (2023) is ex ante reasonable and makes sense in practice. Moreover, the latter's results are robust to the choice of data; including all data tends to increase — rather than decrease — the estimated effect of upzoning in Auckland. For these reasons, we find this aspect of Murray and Helm's critiques has little to no merit.

3.2. *Consents are not completions, but both have hit record levels*

Another of Murray and Helm's methodological critiques considers the distinction between consents and completions. Specifically, this critique argues that because Greenaway-McGrevy and Phillips (2023) and Greenaway-McGrevy (2023a) use dwelling consents (comparable to a "permit" in other jurisdictions) rather than dwelling completions, the impacts on housing supply are over-estimated. Murray and Helm (2023a) argues:

> "A final note of caution concerns the interpretation of dwelling consents as extra dwellings. Historically, about 90 % of consents become completed dwellings after two years … Recently, however, net additional dwellings, as measured by the change in the number of residential electricity connections, have not grown as fast as completions would suggest. Net additional dwellings two years after approvals fell from 77 % prior to 2018, to 69% since 2020. This implies that more existing homes are being demolished for each new home."

There are two problems with this critique. First, although there are valid questions about what proportion of consents will result in completions and by when, Murray and Helm (2023a) ignore existing

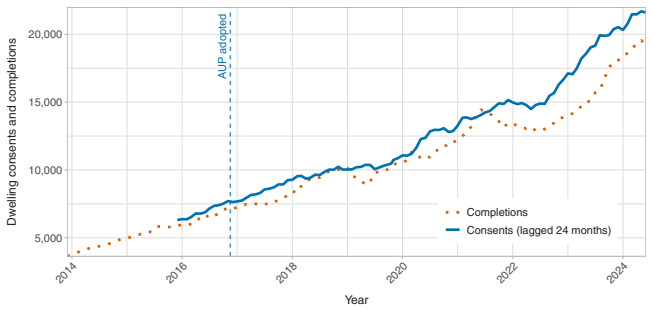

Fig. 4. Dwelling consents (lagged 24 months) and completions in Auckland 2014–2024. *Notes*: Dwelling consents are lagged 24 months from reported values. The consent data for recent years is provisional and can be subject to revisions.

evidence on these questions and misrepresent both the magnitude and timing of the gap that has emerged between consents and completions in Auckland.[14] Intuitively, consents for dwellings can take months if not years to be acted on, especially for larger and more complex developments, like major subdivisions and apartment buildings, with the duration of the lag fluctuating in response to prevailing macroeconomic conditions. In Figure 4, we plot annual rolling totals of both dwelling consents and completions but lag the former by 24 months. This reveals that completions have indeed closely tracked consents. Although the gap between completions and consents widens circa 2021–22, this timing appears unrelated to the AUP and more likely due to other economic factors, such as the COVID pandemic, higher costs for building materials, and/or higher interest rates. Most importantly, and despite the recent slowdown in the growth of consents in Auckland, completions remain at record levels.

Second, Murray and Helm (2023a) argue that new residential electricity connections imply net additions to the dwelling stock have fallen, possibly due to demolitions. Other researchers have, however, investigated this question and found it has no empirical support. Jones et al. (2024), for example, use Auckland Council valuation data to estimate changes to the dwelling stock over time and note (p. 13):

> "Between August 2018 and August 2023, the dwelling stock estimate increased by 61,209 units. This compares to 72,377 dwellings consented between September 2016 and August 2021. This implies that one dwelling was demolished for every nine constructed, on average, assuming a 95 % completion rate on consented dwellings. Estimated teardown ratios are higher if a lower completion rate is assumed."

At face value, this estimate implies that approximately 84 % of dwelling consents flow through into net additions to the dwelling stock, far higher than Murray and Helm's estimate of 69 %. A higher rate of net dwelling additions in the wake of the AUP is also supported by data from the 2023 Census: In the period from 2018 to 2023, Auckland added 64,836 dwellings, which would represent almost 90 % of consents. In our view, data from both valuation records and the Census are likely to provide a more reliable measure of net additions to Auckland's dwelling stock than electricity connections.

In short, data shows Auckland's dwelling stock has grown strongly in the wake of the AUP, with completions continuing to hit record levels. Thus, we find no evidence to support Murray and Helm's suggestion that falling completion rates or higher demolition rates serve to undermine the findings of Greenaway-McGrevey and Phillips (2023).

[14] The supplementary material of Greenaway-McGrevey and Jones (2023), for example, analyses data on housing completions in Auckland assuming a 24-month lag between consents and completions, like we do below. Although Greenaway-McGrevey and Jones (2023) acknowledges this lag is imprecise, we consider this analysis to be more reliable than Murray and Helm's calculations.

3.3. *Econometric methods: a tale of many-not-one counterfactuals, linear models, and inappropriate transformations*

We now consider a third aspect of Murray and Helm's critiques: That the econometric methods used by Greenaway-McGrevey and Phillips (2023) understate growth in consents in the pre-treatment period, which causes them to understate consents in the counterfactual and, in turn, overstate the impacts of the AUP. Specifically, Murray and Helm argue the counterfactual in Greenaway-McGrevey and Phillips (2023) assumes "(a) linear growth, and (b) identical trends in upzoned and non-upzoned areas prior to the AUP", which — in their view — introduces "… significant biases." We find this critique has no merit for two reasons. First, it misunderstands the methods adopted by Greenaway-McGrevey and Phillips (2023), which treats linear trends as a probabilistic outcome that, in turn, defines the bounds of a *set* of counterfactuals, rather than one counterfactual. Second, we argue the linear trend used in Greenaway-McGrevey and Phillips (2023) is ex ante appropriate, whereas Murray and Helm rely on inappropriate transformations of the data.

3.3.1. *Many-not-one counterfactuals*

The first problem with Murray and Helm's critique is it seems to misunderstand how linear trends are used in Greenaway-McGrevey and Phillips (2023). Helm, for instance has stated that 'the authors assumed that without upzoning, growth would have continued in a straight line' (Helm, 2024a). Although this is a common theme in Murray and Helm's critiques, it mistakenly implies that a) Greenaway-McGrevey and Phillips (2023) estimate only *one* counterfactual, and b) this counterfactual implies perpetual, linear growth.

Instead, most of Greenaway-McGrevey and Phillips (2023) focuses on the development and application of novel econometric methods that allow them to quantify uncertainty in the linear pre-treatment trend, which is then used to generate not just one but an *entire set of counterfactuals* (cf. Figure 9, Greenaway-McGrevey and Phillips, 2023). Naturally, this set encompasses many pre-treatment trends that imply much higher levels of consents in the post-treatment period as well as non-linear and non-parametric counterfactuals. Provided the "true" counterfactual for what would have happened in Auckland in the absence of the AUP exists somewhere within the bounds of this set, then we can be confident that upzoning had a statistically significant positive effect on consents.

This aspect of Greenaway-McGrevey and Phillips (2023)'s methodology is crucial to the robustness of their results and, as far as we can tell, it has never been acknowledged by Murray and Helm. It is crucial because it means Greenaway-McGrevey and Phillips (2023) allows for a wide range of outcomes, many of which differ markedly from the average linear pre-treatment trend. In this context, using a linear model to generate individual counterfactuals is less relevant than the range of possible outcomes that fall within the bounds of the resulting set. Greenaway-McGrevey and Phillips (2023)'s probabilistic treatment of pre-treatment trends is not a mere technical detail: Indeed, it is one of the paper's main econometric contributions and helps to greatly reduce the sensitivity of its results to the assumed functional form for the model of pre-treatment trends.

Greenaway-McGrevey and Phillips (2023)'s probabilistic approach to modelling pre-treatment trends also directly undermines Murray and Helm's critique. Greenaway-McGrevey and Phillips (2023) explains, for example, that the counterfactual set can even encompass a variety of *non-linear trends*, observing "… the counterfactual set can even accommodate limited forms of exponential growth … including a year-on-year growth rate of 13.68 %" (cf. p. 15, Greenaway-McGrevey and Phillips, 2023). Notably, this growth rate is higher than the rate of pre-treatment growth (cf. Section 3.3.2 for more details).

Murray and Helm's critique, therefore, appears to be largely limited to one sub-section of Greenaway-McGrevey and Phillips (2023), specifically Section 5.4 'How Many Additional Permits Did Upzoning Enable?' In this sub-section, Greenaway-McGrevey and Phillips (2023) uses the

midpoint of the counterfactual set — that is, the average rate of pre-treatment growth — to estimate the number of new consents that followed from upzoning. This is the only part of Greenaway-McGrevy and Phillips (2023) that presents a single, linear counterfactual for consents in Auckland in the absence of upzoning.

Presenting a critique of this one sub-section — without engaging with the broader econometric methods of the paper — is, in our view, inappropriate for two reasons. First, Murray and Helm claim to rebut Greenaway-McGrevy and Phillips (2023), rather than just one sub-section of Greenaway-McGrevy and Phillips (2023).[15] Second, Greenaway-McGrevy and Phillips (2023) notes the results in this sub-section should taken with caution, as they do not capture underlying uncertainty.[16] Methodologically, this distinction is similar to reporting a point estimate compared with a confidence interval.

Notwithstanding these points, the following sections note that Greenaway-McGrevy and Phillips (2023)'s use of linear trends is reasonable (cf. Section 3.3.2) and the counterfactual that results is plausible (cf. Section 4).

3.3.2. *Linear models and inappropriate transformations*

Many economic models assume there exists a linear relationship between the dependent (Y) and independent variables (X), which simply implies that each unit change in X has a constant effect on Y. Linear models are common for three main reasons: First, they are simple to estimate; second, they are easy to interpret; and third, their behaviour is predictable. In economic contexts where the effect of a variable on an outcome is theoretically or statistically unclear, linear models often provide a useful starting point.

Murray and Helm, however, dispute the use of linear trends to generate the set of counterfactuals in Greenaway-McGrevy and Phillips (2023). In their first blog post, for example, Murray and Helm write, "… there is no reason the counterfactual trend should be linear. Not many economic trends are. Fitting a curve to the pre-treatment trend fits that data better …" (Murray and Helm, 2023a).[17] On the surface, Murray and Helm's critique is not entirely without merit, as linear trends may not be appropriate in many situations. A case could also be made that economists tend to rely too heavily on linearity and the field would benefit from greater use of non-linear methods.

However, even if a non-linear trend "fits" the data better in the pre-treatment period, as Murray and Helm claim, it does not follow — either statistically or economically — that it is suited to generating counterfactuals in the post-treatment period. Non-linear methods also come with risks, notably overfitting. The latter arises when a model specification has superior *internal validity* (i.e. ability to predict data in the pre-treatment period) but inferior *external validity* (i.e. ability to predict data in the post-treatment period). A myopic focus on internal validity, as Murray and Helm seem to espouse, risks producing over-fitted models that perform better at predicting *observed data* but worse at predicting *new data*. As quasi-experimental studies like Greenaway-McGrevy and Phillips (2023) are interested in counterfactuals in the post-treatment period, external validity is crucial. This is why many econometric analyses, including but not limited to Greenaway-McGrevy and Phillips (2023), use linear models unless there is evidence to support the use of non-linear models. We return to the question of over-fitting in Section 4.

The benefits of linearity can be contrasted with a recurring problem in Murray and Helm's critiques: The use of inappropriate transformations of the data, such as growth rates and indices. In their second blog post, for example, Murray and Helm (2023b) write: "Greenaway-McGrevy and Phillips (2023) effectively assume that, without the AUP upzoning, growth in consents would suddenly have slowed down. Over the five years prior to the AUP, annual growth in consents in Greenaway-McGrevy and Phillips (2023)'s sample averaged 12.1 %. But Greenaway-McGrevy and Phillips (2023)'s counterfactual for the six years following involves an average annual growth of just 5.7 %". These sentiments are then repeated on social media, where Helm comments, "In the real world, property moves in cycles. In the paper, it does not. The authors assumed that without upzoning, growth would have continued in a straight line, inexplicably halving from 12 % per annum to 6 %" (Helm, 2024a).[18] The essence of Murray and Helm's critique is the linear models used in Greenaway-McGrevy and Phillips (2023) are unreasonable because they imply the percentage growth in consents in Auckland would fall over time.

To see why this argument is statistically absurd, consider a simple linear model: $Y = X + 1$, where Y measures consents and X measures time. For each one period increase in X, Y also increases by one unit. In turn, this model implies the *percentage growth rate* of Y will fall with time, X. For example, an increase from $X = 1$ to $X = 2$ causes Y to increase from 2 to 3, or 50 %, whereas an increase from $X = 2$ to $X = 3$ causes Y to increase from 3 to 4, or only 33 %. Put simply, the outcome in a linear model grows faster in percentage terms when the explanatory variables, for example time, are starting from low levels.

Economic factors are also at play. Starting from low levels of economic activity usually means there is spare capacity available. When considering housing in Auckland, Greenaway-McGrevy and Phillips (2023)'s pre-treatment period begins in 2010, *just* as Auckland emerges from the Global Financial Crisis. Indeed, 2009 was the worst year on record for consents in Auckland. Improving macroeconomic conditions after a recessionary period should, prima facie, give rise to fast growth. And, importantly for the counterfactuals in Greenaway-McGrevy and Phillips (2023), we would expect growth to slow once activity approached the long-run average and spare capacity was absorbed. In this context, the lower growth rate in the post-treatment period that is implied by the counterfactuals in Greenaway-McGrevy and Phillips (2023) is entirely plausible. This is not complicated: Economic variables often grow faster when they are starting from low levels. Most tellingly, growth in consents also abruptly slowed in other New Zealand cities in the years after the AUP, which is a key point that we return to in Section 4.[19]

[15] This is relevant for Murray and Helm's main argument that "there is no evidence that upzoning increased supply in Auckland." Consider a hypothetical situation where Murray and Helm definitively proved that the midpoint counterfactual in Greenaway-McGrevy and Phillips (2023) was unreasonable compared to one that allowed for a higher rate of supply. Nevertheless, provided the latter counterfactual still existed within the bounds of the set of counterfactuals estimated by Greenaway-McGrevy and Phillips (2023), the AUP would still be found to have a positive and statistically significant causal effect on consents.

[16] The paper notes: "A point of caution should be made in interpreting these findings. Mounting any counterfactual such as an extrapolated linear trend or any set of fixed points inevitably introduces potential misspecification due to the absence of an observable counterfactual scenario and the ambiguities in model selection. In this work a particular method for specifying a counterfactual has been used and point estimates will consequently be sensitive to changes in that specification. Importantly, set-identification mitigates such specification problems by constructing a set that covers a wide-range of possible unobservable counterfactuals" (Greenaway-McGrevy and Phillips, 2023).

[17] Cryptically, Murray and Helm (2023a) argue, "Extrapolating growth this far forward is unrealistic. And this is part of our point: whether using a linear or non-linear trend, extrapolating a short and highly-cyclical series a long way into the future is an inherently unreliable way of defining a counterfactual." We make two comments. First, we are unsure of how one can define a counterfactual without using either linear or non-linear trends. Second, Murray and Helm subsequently define a non-linear, non-cyclical counterfactual.

[18] We are especially confused by the latter comment because we would have expected that the main implication of a "property cycle" is that growth rates change over time, rather than remaining constant.

[19] In the three years before and after 2016, for example, consents in Hamilton City grew by 21 % and 6 %, respectively, whereas in Tauranga City consents grew by 23 % and then *fell* by 3 %.

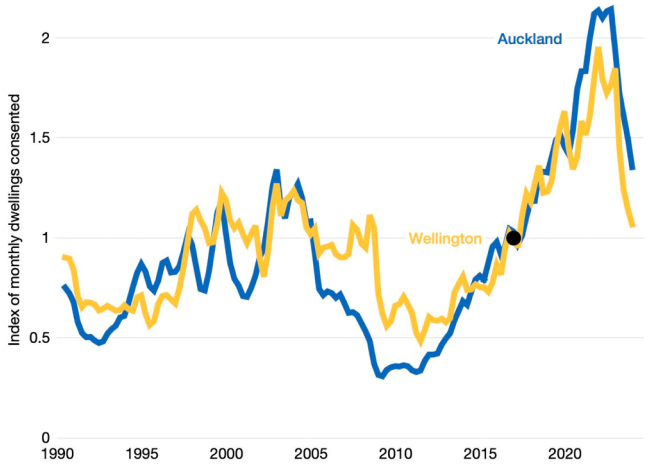

Fig. 5. Dwelling consents in Auckland and Wellington regions 1990–2024 indexed to 2016 levels (Source: Murray 2024).

To support their erroneous arguments about growth rates, Murray and Helm often adopt inappropriate and misleading transformations, such as indexation. Murray has, for example, regularly published charts such as Figure 5, which purport to show that consents in the Wellington region have tracked those in upzoned Auckland (Murray, 2024). This chart suffers from three problems. First, it uses an *index*. Although both regions approximately doubled consents after 2016, consents in Auckland increased from approximately 6 to 12 per 1000 residents whereas those in Wellington increased from less than 4–7 per 1000 residents. Indexation compresses the variance in the data, giving the impression that Auckland and Wellington experienced similar outcomes, when they did not. Second, the graph indexes consents *to 2016*. Per Section 3.1, Greenaway-McGrevy and Phillips (2023) find evidence that SpHA were already having positive impacts on consents in Auckland from 2013. That is, consents are indexed to a point in time where Auckland is already being affected by upzoning. Third, the graph compares Auckland to the Wellington *region*, which comprises several councils. One of these councils — namely Lower Hutt — also upzoned from circa 2017 onwards, which quasi-experimental research finds had a significant positive impact on consents (Maltman and Greenaway-McGrevy, 2025). The Wellington data shown in Figure 5 is thus also affected by upzoning.

In contrast, Figure 6 on the following page compares dwelling consents per 1000 residents in Auckland to those in the Wellington region as well as the (non-upzoned) rest of New Zealand. Notwithstanding that the Wellington data includes the effects of upzoning in Lower Hutt, a different picture emerges from Figure 6 vis-à-vis Figure 5.

In short, Murray and Helm's critiques of the linear trends that are used in Greenaway-McGrevy and Phillips (2023) suffer from basic errors and are highly misleading. For these reasons, we consider this aspect of Murray and Helm's critique to have no merit.

4. Reasonableness of the counterfactuals

We now consider a unifying theme in Murray and Helm's critiques that is alluded to above: A scepticism of the counterfactuals presented in Greenaway-McGrevy and Phillips (2023). Murray and Helm (2023b), for example, argue:

> "Was the counterfactual Greenaway-McGrevy and Phillips (2023) used to estimate growth in consents due to upzoning realistic? Here's a test for you. It's the end of 2015 … After a marathon debate, the proposed Auckland Unitary Plan (AUP) is rejected … Zoning rules stay as they are. If you lived in 2015 in this alternative no-AUP world, which path would you bet on in the image below for dwelling consents?"

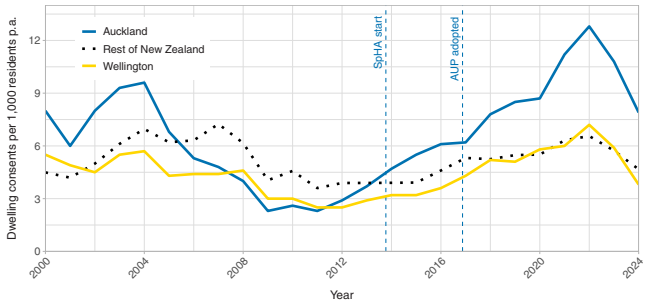

Fig. 6. Dwelling consents per 1000 residents in Auckland, Wellington, and rest of New Zealand 2000–2024. *Notes*: The "Rest of New Zealand" includes all other parts of New Zealand with the exception of Canterbury and Wellington regions, which were affected by earthquakes and upzonings in this period. The vertical line labelled "SpHA start" denotes when upzoning under the AUP was selectively applied to some areas of Auckland. The dwelling consent data for recent years is provisional and subject to revisions.

Murray and Helm (2023b) present the chart illustrated in Figure 7 and then ask:

> "Would you have picked D? We wouldn't have either. But D is the counterfactual used by Greenaway-McGrevy and Phillips (2023) to conclude that anything above this is the effect of the AUP on new dwelling consents …"

Murray and Helm's comments are mistaken for two reasons: First, even if D was the counterfactual in Greenaway-McGrevy and Phillips (2023), the available evidence indicates this is ex ante reasonable. Second, D is *not* the counterfactual used in Greenaway-McGrevy and Phillips (2023). We expand on both of these two points below.

4.1. Counterfactual D is ex ante reasonable

To proceed, assume we want to predict consents per 1000 residents in Auckland from 2016–24 in the hypothetical situation where the AUP was not implemented. Consider three simple methods for generating such a prediction. As a first pass, we might assume consents in the future continue at their historical mean.[20] The second method is almost as simple but subtly different: We could calculate average consents in other regions of New Zealand that did not upzone (per Figure 1). Third, we could calculate average consents in the Northland, Waikato, Bay of Plenty, Wellington, and Otago regions, which are either geographically close to Auckland and/or home to larger urban centres.

In Figure 8, the grey shaded area shows the range in consents per 1000 residents that we might expect for Auckland in the period from 2016–2024 based on these three methods. The solid dark and light blue lines, in contrast, illustrate observed consents in Auckland for the periods 2000–2015 and 2016–2024, respectively. Similarly, Murray and Helm (2023b)'s interpretation of the counterfactuals in Greenaway-McGrevy and Phillips (2023) and Greenaway-McGrevy (2023a) are denoted by the higher and lower dotted orange lines, respectively. Finally, the green dotdash line denotes the counterfactual proposed in Murray and Helm (2023b), which extrapolates the growth in consents in the pre-treatment period (12.6 % p.a.) into the post-treatment period.

We draw four main conclusions from Figure 8. First, circa 2017–18 actual consents per 1000 residents p.a. in Auckland surged above the levels implied by the three simple methods described above (per the

[20] From 1996–2015, Auckland issued an average of 5.9 consents per 1000 residents p.a. In 2016, Auckland issued close to this number: 6.3 units per 1000 residents. Although consents might fluctuate over time, we could expect them to revert to this historical average in the long run and absent any policy changes.

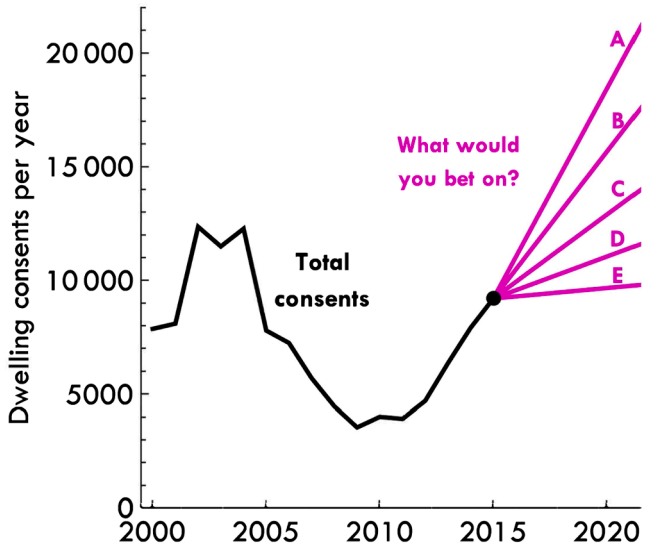

Fig. 7. Murray and Helm's alternative counterfactuals (Source: Murray and Helm 2023b).

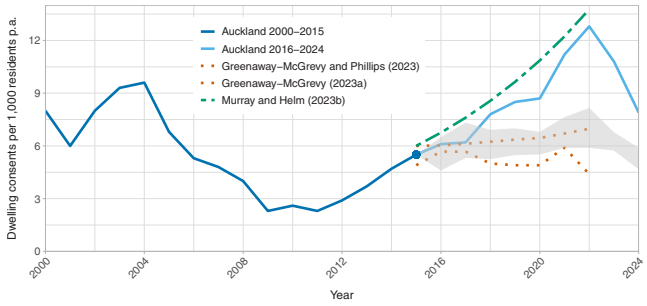

Fig. 8. Comparing observed dwelling consents per 1000 residents in Auckland 2000–2024 to alternative counterfactuals discussed in the text and in Murray and Helm (2023b). *Notes:* The grey shaded area denotes the range in dwelling consents that are defined by the three methods discussed in the preceding paragraph for the period from 2016–2024, specifically 1) mean dwelling consents from 1996–2015, 2) mean dwelling consents in regions of New Zealand that did not upzone, and 3) mean dwelling consents in the Northland, Waikato, Bay of Plenty, Wellington, and Otago regions. We note that Greenaway-McGrevy and Phillips (2023) analyses total consents, rather than consents per 1000 residents. The dwelling consent data for recent years is provisional and subject to revisions.

grey shaded area). Second, these simple methods imply levels of consents that are similar to the counterfactuals that Murray and Helm ascribe to Greenaway-McGrevy and Phillips (2023) and Greenaway-McGrevy (2023a) — per the higher and lower orange dotted lines, respectively. The counterfactuals used in these two quasi-experimental studies are therefore close to what we would reasonably expect based on both historical data for Auckland and outcomes elsewhere in New Zealand.[21] Third, the counterfactuals implied by the simple tests in Figure 8 are close to D in Figure 7.[22] As such, the counterfactual D is, in our view, *ex ante reasonable*. Fourth, when considered in this broader context, the counterfactual proposed by Murray and Helm (per the green dotdash line) is seen to be absurdly high, exceeding both the pre-AUP maximum (by approximately 40 %) *and* the level of consents that are observed in the wake of the AUP.

In short, Figure 8 both dispels Murray and Helm's critiques of the counterfactuals that are used in Greenaway-McGrevy and Phillips (2023) and Greenaway-McGrevy (2023a), *and* illustrates the absurd nature of the alternative counterfactual proposed in Murray and Helm (2023b).[23] Ex ante, we consider it extremely unlikely that Auckland could achieve these levels of consents without a major policy change, such as the AUP.

4.2. D is not, in fact, the counterfactual

We have established that the counterfactual D in Figure 7 — which Murray and Helm ascribe to Greenaway-McGrevy and Phillips (2023) — is not ex ante unreasonable. However, contrary to Murray and Helm's claims, D is not, in fact, the counterfactual that is used in Greenaway-McGrevy and Phillips (2023).

Figure 9 presents a graph from Murray and Helm (2023b), which illustrates their interpretation of the counterfactual used in Greenaway-McGrevy and Phillips (2023) (per the solid pink line). Clearly, if this was the counterfactual used in Greenaway-McGrevy and Phillips (2023), then it would be quite odd — as it is a relatively poor fit for the observed data (per the black line in the pre-treatment period 2010–2015). Murray and Helm are, however, mistaken and their own words reveal the origins of their error:

> "… [the counterfactual] looks reasonable with the sample data, but quite odd when applied to the city-wide total data, as in the chart below."

Here, Murray and Helm tacitly admit that Figure 9 was created by applying the counterfactual from the data in Greenaway-McGrevy and Phillips (2023) (per Section 3.1), to the *full sample* for all of Auckland, including SpHAs, business, and rural areas.

One can readily show the solid pink line in Figure 9 is not the mean counterfactual used in Greenaway-McGrevy and Phillips (2023) by calculating the difference between it and total consents (per the black line). This calculation provides an approximate point estimate for the effects of the AUP of around 34,000 consents, which is significantly higher than the 21,808 that is reported in Greenaway-McGrevy and Phillips (2023).

Murray and Helm's mistake is to extrapolate the growth rate to the *full* sample. Neither Greenaway-McGrevy and Phillips (2023) nor the Extension Paper use this approach. As discussed in detail in Section 3.1, the main paper excludes SpHA and business/rural areas to provide for like-for-like comparisons. By construction, the linear pre-treatment trend in Greenaway-McGrevy and Phillips (2023) will not include growth in these areas. Put simply, it is erroneous for Murray and Helm to compare the counterfactual in Greenaway-McGrevy and Phillips (2023) to the trend in consents for Auckland as a whole.

5. Corroborating evidence

In this section, we now expand the discussion beyond Greenaway-McGrevy and Phillips (2023) to consider corroborating evidence on the impacts of the AUP on housing outcomes in Auckland. Specifically, we discuss the implications of the two other quasi-experimental studies, namely Greenaway-McGrevy (2023a) and Greenaway-McGrevy and So (2024), which consider the impacts of the AUP on

[21] The Auckland "functional urban area" used in Greenaway-McGrevy (2023a) excludes large rural areas, which is why the latter's counterfactual is slightly lower than the other data points. If we scale this counterfactual by the difference in consents, then it shifts up to lie within the grey shaded area.

[22] In Figure 7, Line D implies that Auckland would issue approximately 12,000 consents in 2021. In that year, the mid-point of the grey shaded area implies Auckland would issue approximately 6.75 consents per 1000 residents p.a., while Stats NZ estimates Auckland's resident population was 1.72 million. These simple methods thus imply a counterfactual of around 11,600 consents, which lies between D and E.

[23] Murray and Helm's counterfactual also exceeds the maximum consenting rate that was observed in post-earthquake Canterbury and upzoned Lower Hutt — as illustrated in Figure 12 in Appendix B.

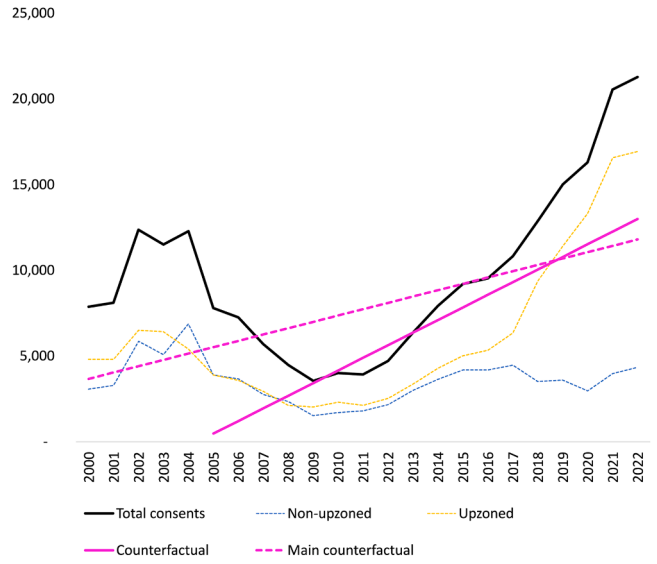

Fig. 9. Murray and Helm's comparison of total consents to their interpretation of the counterfactual in Greenaway-McGrevy and Phillips (2023) (Murray and Helm, 2023b). *Notes:* Murray and Helm claim the solid pink line represents the counterfactual that Greenaway-McGrevy and Phillips (2023) estimate using data for the pre-treatment period from 2010–2015. Murray and Helm go on to note that this counterfactual seems to be an "odd" fit for total consents city-wide denoted by the black line. In practice, the counterfactual in Greenaway-McGrevy and Phillips (2023) excludes consents in business / rural areas and SpHAs, as previously discussed in Section 3.1.

consents and rents, respectively. In doing so, we relate the findings of these studies to aspects of Murray and Helm's critiques.

5.1. Greenaway-McGrevy (2023a)

Whereas Greenaway-McGrevy and Phillips (2023) identifies the impact of the AUP by comparing consents between upzoned and non-upzoned areas within Auckland, Greenaway-McGrevy (2023a) compares consents between Auckland and other similar cities that did not upzone. Crucially, Greenaway-McGrevy (2023a) bypasses the question of linearity entirely — as discussed in Section 3.3.2 — by using another quasi-experimental method known as a "synthetic control".[24] The synthetic control method is non-linear and non-parametric: The counterfactual can go wherever is implied by the data that are used in its construction. To the extent that data on consents is affected by broader property cycles, for example, then this will be captured in the counterfactual. In Figure 10, the red dashed line ("Synthetic Auckland") denotes the counterfactual in Greenaway-McGrevy (2023a) whereas the solid black line ("Actual Auckland") shows observed consents.

The findings of Greenaway-McGrevy (2023a) provide support for several of our earlier comments on Greenaway-McGrevy and Phillips (2023). First, by using data for Auckland's entire urban area Greenaway-McGrevy (2023a) mitigates Murray and Helm's critique of the sample used in Greenaway-McGrevy and Phillips (2023), as discussed in Section 3.1. Second, Figure 10 reveals that actual dwelling consents in Auckland initially diverged around 2013 before then diverging further

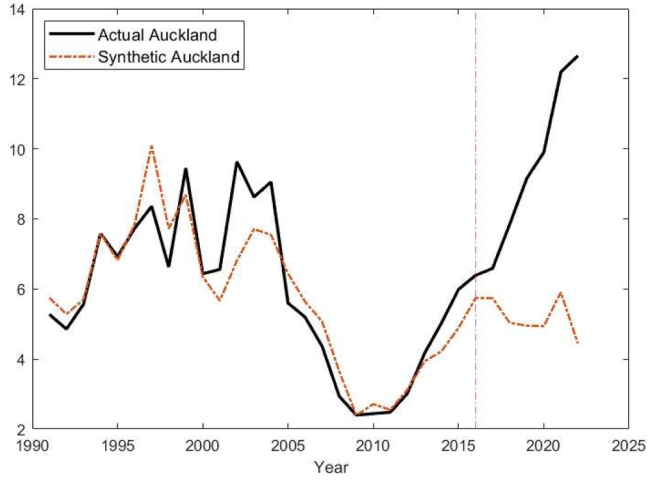

Fig. 10. Dwelling consents per 1000 residents in Auckland 1993–2024 (Source: (Figure 5, Greenaway-McGrevy, 2023a)).

after 2016. These changes coincide with the beginning of SpHA and the full adoption of the AUP, respectively, as discussed in Section 3.1. Third, Figure 10 shows growth in the counterfactual ("Synthetic Auckland") levelled off from 2016 onwards. As discussed in Section 3.3.2, this highlights the importance of external validity and the risks involved in extrapolating non-linear trends into the post-treatment period without considering the broader context, such as outcomes observed in cities elsewhere in New Zealand that are similar to Auckland but that did not upzone.

Compared to Greenaway-McGrevy and Phillips (2023), Greenaway-McGrevy (2023a) finds the AUP had even larger effects (21,808 and 43,500 consents, respectively). While this partly reflects the latter's coverage and timelines, it also suggests that the methods used in Greenaway-McGrevy and Phillips (2023) may understate, rather than overstate, the effects of the AUP. Specifically, the levelling off of the counterfactual in Figure 10 after 2013 implies that extrapolating linear pre-treatment trends forward into the post-treatment period — as done in Greenaway-McGrevy and Phillips (2023) — is likely to overstate consents in the counterfactual and thereby understate the effects of upzoning.

To end this section, we note that comparing Auckland to similar cities that did not upzone, as done in Greenaway-McGrevy (2023a), is *precisely* what Murray and Helm suggest in their second blog post where they write, "What might have been better? Comparison with other cities, for one" (Murray and Helm, 2023b). Despite aligning with the advice to compare Auckland to other cities, Helm has nonetheless taken to social media to criticise Greenaway-McGrevy (2023a) as follows (Helm, 2024a):

> "While the published paper assumed permit growth without upzoning would have halved, this unpublished paper presents an even more pessimistic counterfactual. Again, there is no story for why growth without upzoning would have fallen off a cliff midway through the 2014–2019 migration boom, during which NZ's population growth rate topped the OECD …"

Helm's comment reveals two fundamental misunderstandings of the synthetic control method that is used in Greenaway-McGrevy (2023a). First, the latter's counterfactual flat-lines because that is what happened to consents in cities that are similar to Auckland but that did not upzone. This is the simple story that Helm seems oblivious to. Notably, a similar flat-lining is predicted by the simple counterfactuals that are illustrated in Figure 8. Second, to the extent that population growth in the period from 2014 to 19 also affected the cities that contribute to the synthetic control, then it will be controlled for. In the wake of the AUP, we note

[24] This method constructs a synthetic version of Auckland that provides the counterfactual for what would have happened in the absence of the AUP. The impact of the latter is estimated by comparing observed outcomes in Auckland to predicted outcomes in synthetic Auckland. While sophisticated methods are used to identify the appropriate units and weights for the synthetic control, the latter can be simply understood as a weighted average of building consents in locations with similar characteristics and behaviour to Auckland before the AUP *but that did not implement major zoning reforms in this period.*

that observed population growth in Auckland was close to that predicted by "Synthetic Auckland", which suggests that the cities in the synthetic control grew at a similar rate to Auckland.[25] Put simply, the use of a non-linear synthetic control method in Greenaway-McGrevy (2023a) challenges many of Murray and Helm's critiques and corroborates the findings of Greenaway-McGrevy and Phillips (2023). If anything, we again find the latter study seems likely to understate the impacts of upzoning.

5.2. Greenaway-McGrevy and So (2024)

Whereas Greenaway-McGrevy and Phillips (2023) and Greenaway-McGrevy (2023a) quantify the impacts of upzoning on housing supply, the working paper by Greenaway-McGrevy and So (2024) instead quantifies its impacts on housing prices, specifically rents. Like Greenaway-McGrevy (2023a), Greenaway-McGrevy and So (2024) uses a synthetic control method to compare rents in Auckland to other similar cities in New Zealand that did not upzone. Six years after the AUP was fully adopted, Greenaway-McGrevy and So (2024) estimate rents for comparable properties in Auckland are 28 % lower than they would have been otherwise. By using independent data to estimate a negative effect on prices, Greenaway-McGrevy and So (2024) implicitly corroborates the positive effects on supply documented in Greenaway-McGrevy and Phillips (2023) and Greenaway-McGrevy (2023a). The effects of Auckland's upzoning on housing supply and prices thus seems to accord with the theoretical predictions of conventional economic models.

Although Murray and Helm's two blog posts did not directly engage with the findings of Greenaway-McGrevy and So (2024), Helm has critiqued the latter's findings on social media. On 22 August 2024, for example, Helm commented (Helm, 2024a):

> "Another unpublished paper looks at rents. It claims rents would be 28 % higher without upzoning. The chart below shows what that implies. Does this pass the sniff test? NZ is a small country with easy internal migration. Would people hang on for grim life in Auckland when they could move to another city and reduce housing costs by a third? It's utterly implausible."

We note three problems with Helm's critique of Greenaway-McGrevy and So (2024). First, there are several theoretical reasons why differences in rents — for example, due to changes in housing supply — might not be eliminated by the movement of people between locations. Helm's argument seems to implicitly assume perfect mobility, which is contrary to a large body of economic evidence.[26] Plausible reasons why we would expect to observe imperfect mobility in response to lower rents include moving costs (both monetary and non-monetary) and the imperfect transmission of information between locations (see, e.g., Glaeser et al., 2014; Nenov, 2015).

Second, other empirical evidence finds that housing supply affects rents. At a regional level, Mense (2023) finds a 1 % increase in the flow of housing supply lowers rents by 0.2 %. More locally, Li (2022) finds that a new apartment building decreases rents and prices in nearby areas relative to those further away. Similarly, Asquith et al. (2023) finds that new apartment buildings reduce rents nearby by approximately 6 %. Although the AUP appears to have had relatively large effects on rents compared to the existing literature, this could be explained both by the relatively large size of the upzoning and/or the relatively expensive nature of housing in Auckland before the AUP. The empirical economic evidence thus seems to directly undermine Helm's theoretical critique.

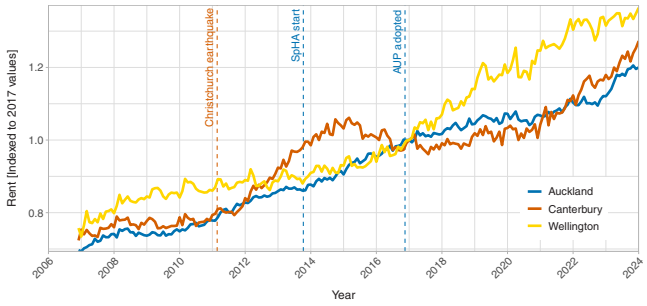

Fig. 11. Rents in Auckland, Canterbury, and Wellington regions 2006–2024.

Third, Helm supports his claim with a chart that compares rents in Auckland to Wellington and Canterbury. This chart suffers from two problems. First, Helm's chart implies Wellington and Canterbury are reasonable counterfactuals for Auckland. As noted in Section 3.3.2, however, upzoning in Lower Hutt means Wellington is a poor counterfactual for Auckland.[27] Meanwhile, Canterbury is also a poor counterfactual for Auckland because the former's largest city, namely Christchurch, suffered an earthquake in 2011.[28] Second, Helm's chart plots rents from 2006. If one instead indexes rents to just after the adoption of the AUP — that is, the start of 2017 — then a different picture emerges, per Figure 11. Of these regions, we see Auckland had the fastest relative growth in rents before 2017 but the slowest growth thereafter — directly contradicting Helm's claim.[29]

Importantly, the negative impact of the AUP on rents documented in Greenaway-McGrevy and So (2024) is also evident in other housing affordability indices for Auckland (see, e.g., Ministry of Housing and Urban Development, 2024). Similarly, the recent study by Greenaway-McGrevy (2025) uses a structural economic model to analyse Auckland's zoning reforms, which are predicted to cause a 15–27 % fall in house prices in the long run. For these reasons, we suggest Greenaway-McGrevy and So (2024) provides robust evidence of the negative impact of the AUP on housing rents and implicitly corroborates the positive impacts on supply that are found in Greenaway-McGrevy and Phillips (2023) and Greenaway-McGrevy (2023a). Contrary to Murray and Helm's claims, there is robust evidence that Auckland's upzoning led to more housing and lower rents.

[25] See Section 3.2 in Greenaway-McGrevy (2023a) for a discussion of the matching variables that are used to identify the units and weights in the counterfactual. We note the counterfactual in Greenaway-McGrevy (2023a) implies that population growth in Auckland would have, in the absence of the AUP, outstripped growth in the housing stock, which is consistent with observed outcomes in the pre-AUP period.

[26] Perfect mobility implies the elasticity of migration, or labour supply, is infinite, which contravenes several empirical studies that report finite elasticities. Per Ahlfeldt et al. (2023), for example, Caliendo et al. (2019) and Caliendo et al. (2021) estimate elasticities of 0.5 for the US and Europe, respectively; Tombe and Zhu (2019) estimate elasticities that range from 1.5–2.5 in China; Bryan and Morten (2019) estimate elasticities of approximately 2.7 for Indonesia; Beaudry et al. (2014) estimate elasticities of around 2.0 for the US; and Morten and Oliveira (2024) estimate elasticities of 4.5 in Brazil.

[27] This is confirmed by the matching exercises that are used to identify the units that contribute to the synthetic control in Greenaway-McGrevy and So (2024), which assign zero weight to Wellington.

[28] The earthquake damaged or destroyed many dwellings and led to a large rise in rents as well as the adoption of zoning reforms (West and Garlick, 2023). The Canterbury region is a poor counterfactual to Auckland in terms of pre-trends, post-trends, and on a theoretical basis. Due to these problems, Greenaway-McGrevy and So (2024) removes the Christchurch metropolitan area from their donor pool.

[29] We are not suggesting it is appropriate to compare rents in Auckland to Christchurch and Wellington nor to index rents to 2017. Rather, we are merely highlighting that shifting the starting point of the data used in Helm's graph contradicts his critiques and supports the finding of Greenaway-McGrevy and So (2024).

6. Conclusions

At a time of growing concern with housing affordability, Auckland's upzoning has provided a rare opportunity to study the impacts of major zoning reforms on housing outcomes and address an important gap in the extant economic literature. Three quasi-experimental papers — specifically, Greenaway-McGrevy and Phillips (2023), Greenaway-McGrevy (2023a), and Greenaway-McGrevy and So (2024) — have found Auckland's upzoning increased housing supply and reduced housing prices. These findings dovetail with a large body of economic evidence and align with the views of a majority of economists.

For these reasons, assigning equal merit to "both sides" of the debate on zoning reforms strikes us as a false equivalency. The quasi-experimental evidence from Auckland simply confirms what is a common finding in the economic literature that is accepted by a large majority of economists. Housing is, in many places, a major policy challenge that warrants urgent action. We suggest it is unreasonable to delay action on the pretence the 'jury is out' on zoning reform. Rather, the jury is in: Auckland's upzoning worked.

Although we find critiques of the Auckland upzoning studies have little to no merit, arriving at this point has produced useful insights and raised interesting questions.

Firstly, there is value in critiques of economic and econometric papers, especially where they introduce new ideas, challenge the prevailing "groupthink", or contest evidence where there exists only a nascent consensus. All empirical work, including these three studies from Auckland, have limitations that are worth probing, testing, and addressing.[30] Such critiques are, however, of most value when they are carefully documented and undertaken by impartial observers who focus on the methods more so than the findings.

Secondly, despite their informal nature and lack of merit, these critiques have managed to influence formal planning and policy processes in New Zealand and Australia with surprising ease. In our view, this raises important questions about how these processes engage with economic evidence. Guidelines and sanctions could, for example, be strengthened to encourage expert witnesses to represent the economic evidence in an accurate and balanced manner. Planners and commissioners, who often lack training in economic and econometric methods, might benefit from additional support to help them assess the credibility of economic arguments. No doubt economists could also do better at summarizing and communicating the weight of evidence in formats that are palatable to wider audiences, as we attempt to do here. Expert surveys from professional organisations that represent economists, like those cited in this paper, might also help to identify the level of consensus that exists on questions that are of importance to policy.

Thirdly, rebutting these critiques has helped to highlight some areas for further research. With the effects of upzoning on housing supply and prices well-established, future work should explore unresolved questions. We see value, for example, in understanding the aspects of Auckland's context that contributed to such large effects, which can, in turn, shed light on their relevance in other contexts ("external validity"). Research could also investigate the impacts of upzoning on a variety of other outcomes that are relevant to policy, such as distributional impacts ("winners" and "losers"), rates of household formation, levels of urban amenities, commuting patterns, firm locations, workforce participation, and labor productivity.[31] Additionally, studying how upzoning interacts with other policies like income support, transport investment, and social housing seems likely to generate further relevant insights.[32] Rather than angsting over the relatively clear and intuitive effects of upzoning on housing supply and housing prices, we suggest our collective attention is better focused on unanswered questions like these.

CRediT authorship contribution statement

Matthew Maltman: Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Stuart Donovan:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Formal analysis, Data curation, Conceptualization.

Declaration of Competing Interest

- The authors declare their affiliations as per the submitted work
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The authors confirm that (1) this work has not been published previously, (2) that it is not under consideration for publication elsewhere, and (3) that its publication is approved by all authors and by the responsible and relevant authorities where the work was carried out.

If accepted, we confirm it will not be published elsewhere in the same form, in English or in any other language, including electronically.

Data availability

Data will be made available on request.

Appendix A. Additional critiques

In addition to their critiques of the Auckland upzoning studies, Murray and Helm hold a range of related views on housing that are worth briefly discussing here.

First, Murray and Phibbs (2023) question the credibility of observational studies of upzoning due to the presence of endogeneity. In response, we simply note that no empirical methods are perfect but there exists a high degree of alignment in the findings of different observational, quasi-experimental, and theoretical economic studies on the effects of planning policies on housing outcomes. For more details, we refer interested readers to studies such as Hilber and Vermeulen (2016); Jackson (2016); Eriksen and Orlando (2022); Molloy et al. (2022); Ahlfeldt et al. (2023);

[30] Here, we feel compelled to note that we do not consider any of the three studies discussed in this paper to be perfect or infallible. For instance, we consider it likely the methods used to estimate spillovers in Greenaway-McGrevy and Phillips (2023) are too conservative, such that the baseline results underestimate the effect of upzoning on housing consents in Auckland, as implied by the results of Greenaway-McGrevy (2023a). Additionally, the paper's broad counterfactual set serves more as a test of statistical significance than a precise estimation of economic effects. Future research should focus on improving econometric methods to better capture spillovers, given their prevalence in most urban economic settings.

[31] Maltman (2024) provides some preliminary evidence of strong construction productivity growth in New Zealand since upzoning, although detailed analysis using firm-level data would be valuable.

[32] Greenaway-McGrevy (2024) analyses the effects of upzoning on "state housing" — that is, public or social housing — and finds supply increased significantly in the wake of the AUP. Indeed, the share of permits for state housing increased from 3 % to 10 % of permits before and after upzoning, respectively.

Asquith et al. (2023); Maltman and Greenaway-McGrevy (2025); Greenaway-McGrevy (2025). For this reason, we do not find the arguments in Murray and Phibbs (2023) to be compelling.

Second, Murray and Helm have — like other supply sceptics — asserted that upzoning will not affect quantities or prices because developers will instead choose to landbank and dripfeed new housing supply into the market at a rate that maintains price levels (see, e.g., Murray, 2020; Helm and Murray, 2024). Most economists tend to dismiss such ideas for a variety of reasons.[33] From a theoretical perspective, developers aim to *maximize profits*, not *maintain prices*. This means developers can be expected to increase supply if the additional revenue from selling more houses exceeds the additional cost of building them. Even if increasing their own-supply did cause house prices to fall, the increase in quantities could nonetheless more than offset the decline in prices such that it was still profitable for the developer to supply more houses. While development decisions are dynamic rather than static, upzoning shifts the supply curve at all points in time.

Central to the landbanking / dripfeeding theory advanced by Murray and Helm is the concept of market power. For a developer to benefit from restricting housing supply, the impact of their own-supply on price — that is, their level of market power — needs to be relatively high. There is, however, little to no evidence that developers have high levels of market power. In 2023, for example, the largest home builder in Australia (Metricon Homes) was responsible for just 4693 of the 171,302 homes started nationally, or 2.7 %. There is also little formal empirical evidence that finds landbanking occurs in practice. The small number of existing studies are limited by a lack of data on developers' cost structures, which prevents them from ruling out that delayed construction or large "land banks" are driven by factors like managing dynamic production costs (e.g., labor constraints, material price fluctuations) or attempts to smooth costs over time. Evidence of "drip-feeding" also tends to stem from exurban markets with highly concentrated land ownership, which seems unlikely to generalise to suburban or urban environments where land ownership is much more dispersed (see, e.g., Murray, 2020; Fitzgerald, 2022). Many of these studies also make poor econometric choices that undermine their findings.[34]

That said, even if developers did have market power and engage in landbanking and dripfeeding, then this strikes us as an argument for upzoning, rather than vice versa. From a theoretical perspective, even neoclassical models of market power — including but not limited to a pure monopoly — predict that reductions in marginal costs will elicit an increase in supply. For this reason, where upzonings serve to lower development costs by reducing the land input required per dwelling, then they can also be expected to increase housing supply even in situations where developers have complete market power. Moreover, in high-demand areas where "price premiums" exist due to the presence of market power, then upzoning might allow new developers to enter the market and supply more housing — thereby eroding the premium. As developers are incentivised to seek out and exploit price premiums when and where they exist, there is competitive pressure to move quickly. This is an important point: If market power is leading to landbanking and dripfeeding, then upzoning to enable more development opportunities and enhance competitive pressure would seem to be the appropriate policy response. From an empirical perspective, Auckland's experience — as documented in the three quasi-experimental studies discussed in the main body of this paper — suggests upzoning can increase supply and mitigate the problems of landbanking and dripfeeding.

B. Additional figures

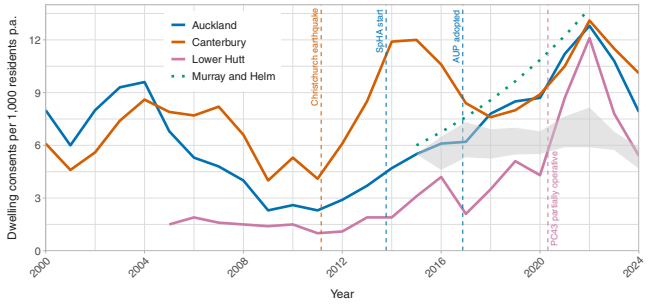

Fig. 12. Comparing observed consents per 1000 residents in Auckland, Canterbury, and Lower Hutt to alternative counterfactuals from Section 4.1 and Murray and Helm (2023b). *Notes:* The grey shaded area denotes the range in dwelling consents that are defined by the three methods discussed in Section 4.1 for the period from 2016–2024, specifically 1) mean dwelling consents from 1996–2015, 2) mean dwelling consents in regions of New Zealand that did not upzone, and 3) mean dwelling consents in the Northland, Waikato, Bay of Plenty, Wellington, and Otago regions. The dwelling consent data for recent years is provisional and subject to revisions.

C. A note on spillovers

In Section 3.1, we discuss treated and control group selection in the framework of a standard DiD study. However, it is important to note that Greenaway-McGrevy and Phillips (2023) is not a standard DiD, as it attempts to account for "spillovers" — or, the displacement of consents — between non-upzoned and upzoned areas.

In typical DiD studies, treatment in one area should not impact outcomes in control areas.[35] Given plausible spillovers between treated and control areas, this assumption does not hold in Greenaway-McGrevy and Phillips (2023). Upzoning one area, for example, may prompt a developer to choose

[33] Tulip (2021); Productivity Commission (2022), for example, conclude there is insufficient economic evidence to support the landbanking / dripfeeding theory.

[34] Murray (2020), for example, argues that developers reduce housing supply during hot markets by letting approvals lapse, stating: "when lot production and approved stocks are high, so too is a delaying behaviour of letting approvals lapse." This conclusion is based on a reported correlation between high housing production and high levels of lapsed approvals within a region. However, the regression specification contains a critical flaw: it does not control for the size of the region. Instead, it simply observes that larger areas have both more housing activity and more lapsed approvals. When council fixed effects are included to control for differences in the size of areas, the positive relationship between lapses and housing market activity becomes *negative*, contradicting the paper's claim. Despite this, the paper asserts: "The positive relationship between approval lapses and new lot production observed here is … consistent with this prediction if developers with approvals who have made irreversible investments increase their supply, whereas those who have not made such investments wait and let their approvals lapse."

[35] Formally, this is referred to as the Stable Unit Treatment Value Assumption (SUTVA).

to develop in the newly-upzoned location rather than in non-upzoned areas, which in turn implies the control area is indirectly affected by the policy. To our knowledge, Greenaway-McGrevy and Phillips (2023) is the first study to formally address these spillovers. To do so, it estimates the maximum spillover effect that would need to occur for the AUP to have a statistically insignificant impact on dwelling consents. While the method is not flawless, and will likely be refined in future research, Murray and Helm's critique overlooks this novel contribution.

One possible interpretation of Murray and Helm's critique regarding Greenaway-McGrevy and Phillips (2023)'s sample is that, by excluding certain areas, the paper might overlook potential spillovers between these excluded and upzoned areas. We make two points on this potential concern. First, any spillovers between excluded and treated areas are likely minor, as these areas are not highly substitutable. Large spillovers are more likely to occur between housing types within similar neighborhoods, such as single-family zones, rather than between rural and single-family urban areas. Second, and more crucially, the Extension Paper includes all data in Auckland, fully accounting for spillovers between all areas. This analysis shows an even larger effect from upzoning.[36]

For these reasons, the impact of the AUP identified in Greenaway-McGrevy and Phillips (2023) appears robust to spillovers between upzoned and non-upzoned areas, even in settings that exclude data from business/rural areas.

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