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The impact of the 2018 Families Package Accommodation Supplement area changes on housing outcomes

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Disclaimer

These results are not official statistics. They have been created for research purposes from the [Integrated Data Infrastructure (IDI) which is carefully managed by Stats NZ. For more information about the IDI please visit https://www.stats.govt.nz/integrated-data/. The opinions, findings, recommendations, and conclusions expressed in this file are those of the authors, not Stats NZ or MSD.
All results presented have been confidentialised in accordance with Statistics New Zealand's requirements. In particular, all sample sizes and counts have been randomly rounded to base 3 (RR3).

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Abstract

In this paper we analyse the effects on rents of the substantial April 2018 changes in the Accommodation Supplement (AS) policy. These policy changes adjusted which geographic locations were assigned to each AS-area, and the AS-maxima were increased to reflect the rising costs of housing in each AS-area. The result of these changes was that the maximum AS-payments for recipients in all locations increased, and the increases varied across geographic locations within redefined AS-areas. We exploit the relative changes in maxima that occurred on either side of such AS-area boundaries to identify the effects of the policy changes on relative rents in these boundary areas. First, we estimate that recipients on the side of boundaries with larger increases in the AS-maxima received on average about $14-19 per week more in accommodation support relative to recipients on the other side. Although we estimate that the relative raw rent increase in the second year after the policy change was about $9 per week on the boundary-sides that received larger increases, once we control for observable and fixed unobservable characteristics of clients, we find negligible differences in rent changes. We conclude that higher-rent new AS-recipients to the treatment areas largely explain the composition changes in these areas, but explain little of the increase in average support. Finally, regression kink analysis shows only weak evidence of stronger rent increases for AS-recipients directly affected by the policy changes.

JEL codes

H22, R21

Keywords

Accommodation supplement, housing demand subsidy

Summary haiku

Rents have been rising
Housing support increases
did not increase rent

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1 Introduction

Housing allowances to low-income individuals and families are an important component of most developed countries' housing policies.[1] By assisting low-income households to access good-quality affordable housing, these policies help facilitate other social policy objectives, including poverty reduction, improved health and education, and equal opportunity and social inclusion (e.g: Eerola, 2021; Gubits et al., 2016; Kennedy, 1980; Mills et al., 2006; Orr et al., 2003; Wood et al., 2008). However, one concern with the efficacy of housing allowances is the extent to which some of the benefits accrue to landlords in the form of higher rents.[2] Although the international literature generally has found that housing allowances result in higher rents, there is substantial variability in the estimates, both across and within countries, as well as by estimation method (e.g.: Brewer et al., 2019; Collinson & Ganong, 2018; Eerola & Lyytikäinen, 2021; Eriksen & Ross, 2015; Fack, 2006; Gibbons & Manning, 2006; Hyslop & Rea, 2019; Kangasharju, 2010; Laferrère & Le Blanc, 2004; Susin, 2002; Viren, 2013). In a recent partial review, Brackertz et al. (2015) conclude that the range of estimated landlord capture varies from 0.3 to 0.8, with more credible methodologies resulting in lower estimates. These estimates imply that between 20% and 70% of the housing assistance is captured by recipients in the form of lower net housing payments.

New Zealand has two major housing allowance support programmes for low-income households. These have broadly similar objectives to provide housing support to recipients. First, the Income Related Rent Subsidy (IRRS) is available for tenants in public housing. As its name suggests, the IRRS that tenants receive is related to their income: it is calculated as the difference between the market rent of the property and an income-determined rent, and is paid directly to the public housing provider. Second, the Accommodation Supplement (AS) is available to low-income private sector rental tenants, as well as boarders and homeowners with high housing costs. A recipient's AS allowance is determined by their accommodation costs, their family structure and where they live,[3] and is abated with income.

The structures of the IRRS and AS policies are quite different. The IRRS is a 'percent of income' subsidy, that directly links a recipient's rent obligation to their income and provides the

[1] Del Pero et al. (2016) report that public spending on housing allowances among a variety of countries for 2012-13. New Zealand is among a group of countries that spends between about 0.5% and 0.8% of GDP, with the UK being the only country spending more than this amount (1.8% of GDP).

[2] Del Pero et al. (2016) discuss how rents may increase with housing assistance. These include an increase in housing demand at the extensive margin (i.e. from more tenants seeking housing), or at the intensive margin (recipients may consume more housing); or landlords may raise rents when they know that tenants receive housing allowances, or general rents may increase when landlords know a significant share of target population receive allowances.

[3] The country is divided into four AS areas, according to the relative rent in geographic areas. Area-1 has the highest rents, and Area-4 the lowest rents. Appendix Table A1 documents the geographic areas allocated to each AS-area, and appendix Figure A1 shows graphically the AS areas before and after the 2018 policy changes.

subsidy to the public housing provider as a top-up to the market rent level. In contrast, the Accommodation Supplement is a 'percent of rent' subsidy, that is linked directly to the recipient's accommodation costs, and paid to the recipient. There are two important implications of these differences. First, while most IRRS recipients' net rent is only 25% of their income, AS recipients typically will pay substantially more of their income in rent. Second, the policies provide different incentives for recipients with respect to rent levels: because IRRS recipients' net rent is entirely determined by their income, they have no incentive to economise on the market value of their housing; in contrast, because AS recipients make co-payment contributions across the full range of housing costs, they have an incentive to economise with respect to their housing costs.

In this paper we analyse the effects on rents of substantial changes in the Accommodation Supplement policy that came into effect in April 2018.[4] These changes consisted of two broad components. First, there was a reallocation of geographic areas across the four AS areas according to the relative cost of housing in each area. Second, the AS-maxima in each AS area were increased to reflect the rising costs of housing in each area. The changes in weekly maxima are documented in Table 1. They vary in magnitude from $5 (5%) for single recipients in Area-2 to $80 (36%) for families of three or more in Area-1 and $75 (47%) for two-person families in Area-1. The result of these policy changes was that the maximum AS-payments increased for recipients in all locations, and the increases varied across geographic locations within redefined AS-areas, as well as across existing AS-areas and family sizes.

The reclassification of geographic locations into different AS-areas meant that the boundaries between the AS-areas shifted. In turn, this resulted in substantially different relative increases in the maximum AS payments for residents in locations that straddled such boundaries. In this paper, we exploit the relative changes in maxima that occurred on either side of such AS-area boundaries to identify the effects of the policy changes on relative rents in these boundary areas. We adopt a similar research design and estimation approach to that used by Hyslop and Rea (2018, 2019) in analysing the effects of a similar set of policy changes in 2005 on changes around the newly created Central Auckland AS Area-1. In particular, we use regression-adjusted difference-in-differences to compare the relative changes in rents from before to after the policy change. We do this for recipients who lived close-to but on either side of a boundary between areas with contrasting AS-maxima increases.

[4] The AS policy changes were originally announced in the previous National coalition Government's 2017 budget. These changes were maintained by the Labour coalition government and included in the Families Package announced in December 2017. Previous AS rebasing exercises occurred in 1997 and 2005, with the 2005 changes resulting in an increase in the number of AS-areas across the country from three to four through the introduction of a new area around central Auckland.

Our analysis uses anonymised administrative data from Statistics New Zealand's Integrated Data Infrastructure (IDI) on Accommodation Supplement receipt since 2010. The AS data contain demographic information on the primary AS-claimant and their family structure, as well as details of their rental payments and AS and other welfare support payments. These data are merged with MSD residential location information. The boundary analysis involves local boundary areas, consisting of statistical meshblocks that lie within 1km of a contrast-boundary. We characterise the "control" group associated with each contrast boundary as the side of the boundary that did not involve an AS-area change, and the "treatment" group as the other side that changed to a higher-maxima AS-area and hence received larger increases in the maxima. Our main analysis focuses on AS-rent recipients over the four-years from April 2016 to March 2020. This spans the two years before and after the April 2018 policy change date.[5]

In addition to the AS data, we also analyse data from the Ministry of Business Innovation and Employment's (MBIE) Tenancy Bonds database in the IDI to document broader rental market trends between April 2010 and December 2020. Using these data, we extend the AS analysis to consider the effects on the broader rental market, and attempt to understand the extent to which any increase in rent paid by AS recipients represent a 'pure' increase in rents to landlords versus an increase in the quantity or quality of accommodation for recipients.

We begin by documenting the longer-term trends in accommodation support and rental payments for the contrast boundary control and treatment groups over the period since April 2010. This shows discrete step-increases in AS-support with the policy change in April 2018 for recipients on both sides of the boundaries, and substantially larger changes for the treatment areas. Across all boundary contrasts, average weekly AS-payments increased 55% in the treatment group (from $73 to $113) compared to 18% in the control group (from $99 to $117). Although these increases are moderated by temporary additional support (TAS) payments,[6] average total accommodation support increased 38% and 17% in the treatment and control areas respectively (from $93 to $128, and $115 to $134). There were corresponding step-decreases in the fraction of clients receiving the maximum-AS payments, with the fraction falling 33 percentage points (pp) (from 66% to 33%) in the treatment areas compared to 11 pp (from 47% to 36%) in the control areas. In contrast, there was no discrete change in average rents on either side of the boundaries, although the average weekly rent increased about $7 per year

[5] All analysis and discussion of dollar-values are in nominal terms. Because inflation was low over the main (2016-20) analysis period (e.g. average annual CPI-inflation was 1.5%, and total inflation 6.5%), reported increases largely reflect real housing cost increases. Also, as our primary focus is on the relative differences between boundary areas, any confounding effects of inflation will be further reduced.

[6] When a client has accommodation costs that are sufficiently high relative to their income, TAS can be used to assist with those costs. Due to the increase in income resulting from increased AS-payments, there was an accompanying fall in TAS payments.

faster in the treatment than the control areas following the policy change. We also document that comparative trends in tenancy bond average and lower quartile (25th percentile, P25) new tenancy rents have been substantially stronger than that of average AS-rents since 2012, with little noticeable change after April 2018.

We use difference-in-difference regression analysis to estimate the effects of the policy changes on rents in the contrast-boundary sample. First, in line with the relative trends in rents, we estimate that, on average, rents increased $6.40 per week more in the treatment group than the control group over the 2-years after the changes compared to the 2-years before the changes; and the relative increase was greater by the second year ($8.90) than in the first year ($6.55) after the changes. Compared to an estimated $18.60 per week average higher relative total accommodation support in the treatment group by the second year, this suggests that landlord capture of the increase was less than 50% ($8.90/$18.60=48%), implying that at least half of the support accrued to AS recipients. However, these estimates include the effect of any uneven changes in the mix of recipients on either side of the boundary. Controlling for observable differences in AS-clients' characteristics, the estimated relative rent increases are roughly halved ($2.40 and $3.90 by the first and second years after the policy changes), while the estimated relative increase in accommodation support was largely unchanged ($16.90 by the second year), suggesting that the maximum landlord capture was less than one-quarter, so that at least three-quarters of the support was thus captured by recipients. Further controlling for fixed unobserved client effects, the estimated relative rent increases decline further to $1.10–$1.40 and are statistically insignificantly different from zero, while there is little change in the estimated relative increase in accommodation support (about $13.70 per week). Based on these results we conclude there is little evidence that the relative increases in accommodation support in the treatment areas led to stronger rent increases in those areas.

We provide some separate analysis of patterns by ethnicity. Māori are about 3 percentage points (6%) more likely to receive the maximum AS payments than all recipients and are slightly (0.6 pp or 2%) more likely to receive TAS payments. We estimate that, for those living on the treatment compared to the control sides of the boundaries, their total accommodation support increased $11.70 – $18.35 per week, while their average relative rent increase was $1.70 – $8.70 per week. These estimates imply that the average after-rent increase in support was $8.30 – $11.70 (or 49–87% of the increase in support).[7] Thus, at most half (and likely only 13-36%) of the

[7] The range of estimates are based on the following. First, the raw average relative increase in total accommodation support was $17 per week, and average relative rent increase was $8.70: implying $8.30 increase in support after rent (or 49% of the $17 relative increase). Second, controlling for recipients' observed differences in location and characteristics on either side of the boundaries and over time, the corresponding estimates are $18.35 and $6.65 respectively by the second

relative increase in support associated with the policy changes that we analyse was spent on higher rent payments, with the remaining support available for other costs.

Pacific peoples are almost 10 percent of the AS recipients in our analysis sample. Pacific peoples are less likely to receive either the maximum AS-payments or TAS payments than all recipients but have higher average accommodation support and rent payments. These differences likely reflect differences in family sizes and location of Pacific peoples compared to all AS recipients – e.g. Pacific peoples are more likely to live in the Auckland areas, where rent and accommodation support is higher. Associated with the policy changes we analyse, we estimate that Pacific peoples' average accommodation support increased $20.75 – $24.50 per week, their average rent increased $0.90 – $11.70 per week, and their after-rent support increased $12.20 – $19.85 per week (or 52-96% of the increase in support).[8] Thus, we find that less than half (and possibly almost none) of the increase in support for Pacific peoples was spent on higher rent payments.

Given that relatively stronger growth in average rents in the treatment group areas is absorbed by observed and unobserved differences in client-characteristics, we next attempt to understand the sources of this growth. To do this, we condition on samples of treatment and control group clients who received AS-payments in the March months of consecutive years, and decompose the annual changes in average rents in each group. We find that the number of AS recipients grew relatively strongly following the policy change, due to both an increase in the inflow of new AS recipients and also a decrease in the likelihood of recipients leaving AS. Furthermore, the net inflow of new or existing AS recipients with relatively high rental costs contributed to a rise in average rents. This effect was stronger on the treatment side of boundaries. In contrast, average rent increases for non-moving recipients who continued receiving AS support were almost the same across boundaries.

Finally, any behavioural response on the part of AS-clients is expected to depend on their rent and AS-payments before the policy change. In particular, clients who previously received the maximum AS-support were directly affected by the changes, while those who received less

year after the changes: implying a net gain after-rent of $11.70 (or 64% of the $18.35 relative increase in AS support). Third, controlling also for constant unobserved differences of AS-clients, the estimated increases in support and rent are $13.10 and $1.70 by the second year after the changes, and imply the after-rent gain was $11.40 per week on average (87% of the $13.10 increase in support).

[8] This range of estimates is based on the following. First, the raw average relative increase in total accommodation support over the two years following the policy change was $22 per week, and average relative rent increase was $9.70: implying $12.80 increase in support after rent (or 56% of the $22 relative increase). Second, controlling for recipients' observed differences in location and characteristics on either side of the boundaries and over time, the corresponding estimates are $24.50 and $11.70 respectively by the second year after the changes: implying a net gain after-rent of $12.80 (or 56% of the $24.50 relative increase in AS support). Third, controlling also for constant unobserved differences of AS-clients, the estimated increases in support and rent are $20.75 and $0.90 by the second year after the changes, and imply the after-rent gain was $19.85 per week on average (96% of the $20.75 increase in support).

than their applicable maximum AS-payment were not directly affected.[9] To address this, we analyse whether there were differences in rent changes for AS-recipients according to how their rent compared to the pre- and post-change AS-maxima.

The dominant pattern that we observe is one of mean reversion – renters with low rental costs tend to have relatively high subsequent growth in rent, whereas renters with high rental costs tend to have relatively low subsequent rent growth. We test whether this underlying pattern varies depending on whether recipients have rental costs above or below the pre-change or post-change thresholds for maximum AS payments. We find no evidence of differential rent growth around these thresholds, or of recipients clustering around the thresholds.

The paper is organised as follows. We review and discuss New Zealand's Accommodation Supplement policy, the 2018 policy changes, and relevant international and New Zealand literature on the effects of housing demand subsidies on rents. In section 3, we outline the research design adopted in the analysis. Section 4 contains a description of the data to be used, the characteristics of the sample, and the longer-term trends in the main variables of interest. We present the main results in section 5, and detailed analysis of compositional effects in section 6. The paper concludes with a summary discussion in section 7.

2 Background, literature and theory

We begin by providing some context as background for the following analysis. This includes a general discussion of the coverage of New Zealand's two main housing support policies (IRRS and AS) which, combined, provide support for over 14% of the population: about 3.5% by IRRS, and over 11% by AS. Following this, we provide a more detailed discussion of the AS policy design and 2018 changes. We then provide a review of international literature on the effects of housing support on rents, and the section concludes with a discussion of the expected effects of the support increases on rents based on a standard neoclassical comparative statics exercise.

Since its introduction in 1993, the Accommodation Supplement (AS) has been New Zealand's largest housing support policy. It currently provides non-taxable cash supplement payments for low-income families with significant accommodation costs in private sector housing, be they renters, boarders, or homeowners. AS also supported state housing tenants

[9] That is, those not directly affected would not expect an increase in AS-payments because of the policy changes, while those directly affected would receive an increase. The directly affected group can also be split according to whether their rent was below or above the level associated with the post-change maximum, which affects whether they would receive less than (or equal to) the full increase in maximum AS-support: furthermore, the former group will be subject to both an income and substitution effect associated with the policy change, while the latter subgroup will be subject to just an income effect.

until 2000, when the then Labour coalition government re-introduced an income-related rent subsidy (IRRS) for state tenants. Since 2014, the IRRS has been available to tenants of Kāinga Ora (formerly Housing New Zealand) and those in registered community housing providers (CHP). Local council tenants are not eligible. The income-related-rent is calculated as 25% of a tenant's assessable income below a threshold, and 50% of their income above that threshold. For most tenants this results in their net-rent being 25% of income, and the IRRS is paid to the housing provider to top-up the difference to the market rent. Eligibility for IRRS is subject to an asset test, but the vast majority (over 90%) of the 73,500 public housing tenants receive IRRS, at a cost of $1.2bn (0.4% of GDP) in the 2020/21 fiscal year.[10]

The Accommodation Supplement covers most other (non-public housing) tenants. In particular, AS covers local council tenants, as well as boarders and other non-main tenants in public housing. Students are not eligible for AS, but those receiving Student Allowance can receive Accommodation Benefit support payments. AS is both asset and income means-tested. AS provides a 70% subsidy for rent above a threshold (determined as 25% of benefit income for renters and boarders, and 30% for homeowners) up to a maximum that varies by family size and across four sets of AS-areas. Since the 2018 policy changes, the maxima are currently set to the 40th percentile of size-adjusted area rent in 2016. As at June 2020, there were 349,000 AS recipients, and the cost of AS was $2.0bn (0.6% of GDP) over the 2020/21 year. From the AS recipient data analysed below, we estimate about 480,000 people were covered by AS-rent (400,000) and board (80,000) claims in any month (and over 65,000 covered by AS-homeowner claims). Compared to 2018 Census estimates, this suggests that AS provides support for about 40% of people living in private sector rental housing,[11] implying AS has broad coverage in the rental market, and likely stronger incidence in the low-end of the market.

In addition to eligibility varying according to the public- versus private-housing tenure of IRRS and AS recipients, there are important differences in the structure of each policy. The IRRS is a "percent-of-income" subsidy, with most recipients' rent determined as 25% of their income, and the difference paid to the housing provider landlord as the subsidy. In contrast, the AS is a "percent-of-rent" subsidy, with most recipients likely paying more than 25% of their income in net-rent. For example, a person on a main benefit is expected to pay 25% of their (benefit) income on rent before they receive any AS support, then 30% of additional rent up to a maximum rent, and 100% of additional rent above that level. For a single adult Jobseeker

[10] Based on the 2018 Census, Stats NZ (2021) report 1.4m people live in rental housing, of which 12.9% are in Housing New Zealand properties (pp. 38-39). This suggests about 180,000 people live in public housing, most of which is subject to IRRS.
[11] That is, from Stats NZ (2021) numbers (pp. 38-39), we estimate about 1.22m are in private (non-public) housing. The extent to which AS-recipients are non-main tenants in public housing implies the 40% coverage will over-estimate the true AS-coverage of those in private rental housing.

Support (JSS) recipient with no other income in Wellington after the policy changes in 2018, their weekly benefit was about $215 (net of tax: gross≈$240), they would pay the first $54 of rent before any support; if their rent (≈$204) just qualified them for the maximum support level, they would receive $105 of AS, leaving them with net rent payments of $99 or 46% of their net income (i.e. 99/215, or 41% of gross income).[12]

Another potentially important background factor is the long-term decline in the rate of homeownership in New Zealand. For example, 64.5% of households owned their own home in 2018, down from 73.8% in 1991 (StatsNZ, 2021). This suggests more renters who may have previously bought a house, and AS-recipients potentially concentrated further down the overall rent distribution. However, these changes are likely to be relatively slow (e.g., the home-ownership rate was 65% in 2013), and thus expected to have minor effect on our analysis period.

2.1 Accommodation Supplement background

The Accommodation Supplement (AS) provides non-taxable cash supplement payments for low-income families in private sector housing, be they renters, boarders, or homeowners. The AS is designed so that eligible low-income families are expected to spend up to 25-30% of their income on housing before they receive any support. They are then responsible for at least 30% of any additional rental cost from that point. An applicant's accommodation supplement entitlement is determined by the following formula,[13]

$$AS = \min\{AS\,max\,, \max\{0, subsidy\ rate * (AccCost - AccCost_0)\}\} - abatement.$$

This formula has four elements: a minimum accommodation cost entry threshold (*AccCost$_0$*), below which AS is zero; a *subsidy rate* (set at 70% since 1997) for accommodation costs above the entry threshold; up to a maximum payment entitlement, *AS max*; and finally, payments are subject to *abatement* that depends on the applicant's non-benefit income and assets.[14] The minimum and maximum parameters vary by family size and beneficiary status, and the AS maximum also varies across geographic areas, with the country divided into four classes of AS-area on the basis of accommodation costs.

In contrast to policy in some countries that provide 100% subsidy rates over a range of housing costs (e.g., the US and the UK), a feature of New Zealand's AS policy is co-payments

[12] If they also earned the abatement free threshold $80 per week, their net-rent would be 31% of gross income.
[13] The AS policy features are very similar to those of housing subsidies provided in several other countries, including Australia (Martin et al., 2016), Finland (Eerola & Lyytikäinen, 2021), and France (Laferrère & Le Blanc, 2004).
[14] Accommodation Supplement is not income tested for beneficiaries. Because of this, the income abatement threshold for non-beneficiaries is set at the income level at which the main benefit applicable to their situation would be fully abated. For non-beneficiaries, AS entitlement is subject to abatement for cash assets above $2,700 (singles) and $5,400 (others); and for beneficiaries and non-beneficiaries, eligibility for AS is subject to a cash-asset limit of $8,100 for single persons, and $16,200 for other recipients (https://www.moneyhub.co.nz/accommodation-supplement.html).

across the full range of accommodation costs (i.e., full payment below the entry threshold and above the maximum, and 30% co-pay in between these levels). This co-pay feature ensures recipients have financial incentives to economise on the cost of housing. Together with other design features,[15] this may have important implications for how the incidence of the subsidy falls on the demand versus supply side (i.e. tenants versus landlords) of the housing market.

The 2018 AS policy change involved two components. First, reflecting different trends in lower-end housing costs across the country, geographic areas were re-classified across four AS-areas which determine the maximum AS entitlement available to recipients.[16] Second, the AS-maxima within each AS-area were adjusted to reflect the changes in prevailing (40th percentile) rents for different family sizes. These changes are summarised in Table 1 and show that increases in the weekly maxima ranged from $5 (1 person, Area-2) to $80 (3+ people, Area-1), or from about 5 to 60% in relative terms. Together with the re-allocation of AS-areas, the actual change in the AS-max faced by recipients was often greater than these figures – e.g., much of the greater Auckland area was re-zoned from Area-2 to Area-1, resulting in substantially larger increases in the effective maxima in those areas ($65-$140 per week, or 65-88%).

It is important to realise that the changes in the maximum AS payment rates have a direct effect only on recipients who would receive the pre-change maximum rate. For those recipients, the changes resulted in an increase in the amount of AS received and enabled them to increase their expenditure on housing or on other goods and services. Other recipients were affected only if they increased their accommodation costs above the pre-change maximum, or if there were indirect effects of the policy change on housing costs. Unfavourable indirect effects may have occurred, for example, if the increase in AS maxima leads to a general increase in rental costs due to increased housing demand. In this case, all tenants (recipients and non-recipients) will face higher accommodation costs than before the policy change. Alternatively, if the AS increase affects the rents only of those directly affected by the policy change (i.e. those with AS payments constrained by the pre-change maxima), then outcomes of other recipients should be unaffected. Such potential adverse spillover effects of the policy change are an important issue that we will consider and attempt to address.

2.1.1 Temporary Additional Support (TAS)

In addition to AS, recipients facing particular hardship may also be eligible to receive a Temporary Additional Support (TAS) hardship benefit for up to 13 weeks per application.

[15] For example, AS is a non-rationed entitlement, determined as a 'percent of rent' rather than a 'percent of income' subsidy, and paid directly to the recipient rather than the landlord.

[16] We document the geographic area changes in appendix Table A1, and appendix Figure A1 shows the re-classification of the four AS-areas resulting from these changes.

Although eligibility for TAS payments is broadly available for any form of hardship, high accommodation costs appear to be a primary factor in the receipt of TAS. In our analysis, more than a quarter of AS recipients also received TAS. The implications of TAS for the analysis are unclear, potentially affecting both treatment intensity (i.e. relative AS changes) and treatment response (i.e. relative demand for housing).

First, in terms of the treatment intensity, for those receiving less than the maximum-TAS payment, TAS payments abate at 100% as income increases, so AS payments associated with the policy change will not increase total accommodation support until TAS is fully abated; while those who are (censored) at the maximum-TAS, may receive an increase in AS. The other consideration is that TAS recipients are more likely to receive the maximum-AS (over 80% in our sample) than those not receiving TAS (one-third), implying that they are more 'at-risk' of being directly affected by the policy change. Empirically we estimate that TAS and non-TAS recipients received very similar increase in accommodation support on average.[17] Second, in terms of treatment response, for those receiving the maximum-AS and less than the maximum-TAS, TAS (hence total support) payments can increase at the same rate as rent, effectively implying no recipient co-payment over that range, and so less sensitivity to rent changes.

These factors appear to have offsetting effects on treatment and possible responses for our boundary area analysis, although the former effects on the policy-change treatment intensity are likely to dominate possible response differences. For these reasons, we treat TAS receipt as an integral component of the accommodation support in our AS analysis, and focus primarily on the total accommodation support, calculated as the sum of AS and TAS payments. In addition, we examine whether TAS has any confounding effects on the results by stratifying the analysis for AS-recipients who did and didn't receive TAS payments.

2.2 Literature review

The research evidence on the impacts of demand-side housing subsidies on rents and other housing outcomes is mixed. Although the international literature generally finds that housing subsidies increase rents and other housing costs, the estimates vary widely, both across and within jurisdictions. Findings appear to depend on design features of the subsidies (full or partial subsidy, relative generosity, eligibility criteria, payment schedules), as well as on the research design.

[17] For those receiving AS at the date of policy change (i.e. in March and April 2018), we estimate the increase in average AS payments on the 'treated' relative to the 'control' sides of boundaries (i.e. the side with the larger relative versus smaller increase in maxima) for TAS recipients is roughly double that of non-TAS recipients ($29.10 compared to $15.70). This difference is balanced by almost the same relative drop in TAS, so that the relative changes in total accommodation support (AS+TAS) are almost the same ($12.70 and $13 on average respectively).

Research based on difference-in-differences type analyses using either area-level repeated cross-sectional or panel data generally estimate positive impacts of subsidies on housing costs. For example, in the US estimates range from small impacts (Eriksen & Ross, 2015) up to a substantial elasticity of about 1.3 (Susin, 2002), meaning that a 1% increase in subsidy payments leads to a 1.3% increase in local rental payments. In the UK, estimates of the proportion of subsidy changes (decreases in this case) absorbed by changes in rents range between 0.6 (Gibbons & Manning, 2006) and 0.1 (Brewer et al., 2019).[18] In continental European countries estimates are up to 0.8 for France (Fack, 2006; Laferrère & Le Blanc, 2004), and in the range 0.3–0.6 for Finland (Kangasharju, 2010; Viren, 2013).

In contrast, research based on more credible micro-level quasi-experimental research designs that exploit specific aspects of the housing policy generally estimate smaller impacts of housing subsidies on rents. For example, Collinson and Ganong (2018) estimate modest effects for the US; and using a regression discontinuity design associated with subsidies being related to apartment size (floor area) in Finland, Eerola and Lyytikäinen (2021) found no evidence of subsidy pass through to rents.

There are three existing New Zealand empirical analyses of the effects of AS. First, using aggregate time series, Stroombergen (2004) analysed the effect of AS payments on weekly rental payments from 1991 to 2003. He estimated very small and statistically insignificant rent elasticities with respect to AS-payments, of 0.001 nationally and 0.002 in Auckland (e.g. a 10% increase in AS payments is associated with a 0.01–0.02% increase in rent). Given that AS was estimated to affect about 13% of the rental market (most likely concentrated in the lower end of the rent distribution), he further analysed the relationship between lower quartile rents and AS, and estimated a small and statistically significant but negative rent elasticity of -0.0025.

Second, using an estimated model of the broader housing market, Grimes et al (2013) simulated the effects of an increase in AS payments on rents and house prices. Simulating a 10% increase in (real) average AS receipt by both renters and homeowners, they estimated short run increases in rents directly as a result of the AS increase, and longer term (4-5 year) increases in both house prices and rents (the latter indirectly because of higher house prices). The estimated long-run rent and house-price elasticities are about 0.7 and 1.5 respectively, although the authors caution that the simulated responses likely overstate the true impacts because of collinearity and endogeneity issues involving AS and housing cost movements.

[18] Brewer et al. (2019) conclude there is substantial heterogeneity in the effects of the policy change and find relatively large responses associated with tenants who had been subsidised in higher-rent properties. They argue that the subgroup of such tenants is likely more similar to the sample analysed by Gibbons and Manning (2006) which may explain at least part of the difference in estimated impacts.

Third, Hyslop and Rea (2018, 2019) conducted a micro-level analysis of a natural experiment provided by the 2005 AS policy changes that created a new higher-subsidy area (Area-1) around central Auckland. They used a difference-in-differences approach to compare changes in rental payments by recipients who lived within 1km inside versus outside the new Area-1 boundary over the two years before to the two years after the policy change. The comparison of changes in local areas inside and outside the boundary mitigated possible confounding effects of other broader area factors, such as stronger rent increases closer to the centre of Auckland. Their analysis found no significant differences in rent changes over the first year following the policy change, but that rents increased relatively more inside the boundary in the second year after the change. By the second year after the policy change, rents were estimated to have increased by $2.44 on average, while total accommodation support (i.e. AS plus Temporary Additional Support, TAS, payments) increased by $6.81 per week for those inside relative to outside the boundary: implying a 36% subsidy pass-through to rents. Furthermore, although the estimated rent increases were stronger at the top end of the distribution where the increase in support was larger, there was a uniform subsidy pass-through to rents.

Finally, Russell (2020) analysed the effects of 2018 increases in Student Allowance and Accommodation Benefit payments to eligible students on rents paid by students in Dunedin, Palmerston North, and Wellington. She uses a difference-in-difference approach to estimate the policy effects on the rents of students in these cities relative to a combined control group of non-students in these cities and tenants in four other low-student cities (Gisborne, Napier, Hastings, and Timaru).[19] Russell finds little evidence of any increase in overall student rent payments in response to the increased support available to students. However, she does estimate statistically significant rent increases in larger student flats, particularly in Wellington where the average rent increase was $25-30 per week for 3-4 and 5+ bedroom flats. The effects for Palmerston North and Dunedin were $19 and $22 per week (respectively) for 5+ bedroom flats, but -$7 and -$18 per week for 3-4 bedroom flats.

Taken on face value Russell's results suggest three relevant conclusions. First, to put the latter estimates in context, they imply about 30-40% pass-through from the increase in total student support to rents. Second, as Russell discusses, because the rent measure used is the total rent associated with a tenancy, these estimated effects should be considered "… liberal estimates of landlord capture …" (p.46); using an alternative, per-bedroom, measure of rent leads to more conservative estimates of rent increases of 6-10.5% of the increase in student

[19] In explaining this combined control group, Russell discusses the rationale being to test for spillovers from student to non-student rents in the 'treatment' cities. However, no estimates of these spillover effects appear in the analysis.

support. Third, she also estimates statistically significant reductions in tenancy crowding in 3-4 and 5+ bedroom flats in these cities (except in 5+ bedroom Wellington student flats, where crowding worsened), which suggests the increase in rents may at least partly reflect an increase in accommodation quality.

2.3 Theoretical framework and predictions

In this section, we address the possible theoretical effects of the 2018 policy changes on rents. To do this, consider Figure 1 (copied from Hyslop and Rea (2019)), which depicts a stylised summary of the effects of the Accommodation Supplement area changes on the supply and demand for rental housing.[20] First, we assume the supply schedule (SS) of rental housing is upward sloping in rent (price), and is unaffected by the policy changes; and, in the absence of any accommodation supplement, the demand curve (DD) is downward sloping in rent. Second, the accommodation supplement provides an ad valorem subsidy for rents between the minimum rent price (Mn) which satisfies the AS-entry threshold of expenditure, and the rent price associated with the maximum AS-payment (Mx), and provides a lump-sum subsidy for rent above this level. Thus, the demand curve becomes less elastic (i.e. steeper) over the rent price range between Mn and Mx: this occurs at Mx0 for AS-areas with a lower maximum, and Mx1 for areas with a higher maximum.[21]

The analysis of the expected effect of the policy change becomes essentially a standard comparative statics exercise. Figure 1 makes clear there will be no effect on the rental demand when the price is low (below Mn). When the price is above this level, AS lowers the elasticity of demand (i.e. the demand curve is steeper over this range) up to the price where the AS-maximum is reached (Mx); while demand becomes more elastic (flatter slope) again above this price. Furthermore, the AS policy changes will differentially affect the demand across boundary areas only of those with rent above Mx0. So long as neither demand nor supply is perfectly inelastic (vertical), we would expect the equilibrium rental price to increase and the quantity to also increase.[22] How the incidence of the subsidy is divided between tenants and landlords

[20] In this stylised depiction, we highlight the differences across area-boundaries and relative to the no-subsidy case, and ignore the nuances associated with AS-changes for all areas. Also, the discussion of this figure is terms of a policy change that increases the AS-maximum more on one side of a boundary than the other side from the same initial level (i.e. a "split" boundary as discussed below). Analogous effects can be deduced for the case where the policy change led to different increases in the maximum on either side of the boundary to the same level (i.e. the "merged" boundary case) For example, suppose one side was unsubsidised initially (facing DD demand) while the other side was subsidised up to rent Mx0, and the policy changes unified both areas with a subsidy up to rent Mx1.

[21] An important implication of this framework is that demand must be inelastic (at least over the relevant range): if demand is elastic, then quantity demanded falls relatively faster than the price increases, in which case expenditure on housing (rental payments) will fall, so that AS support would also fall.

[22] In the housing context, an increase in the quantity may represent either an increase in the size of housing rented by tenants, or an improvement in the quality of their housing.

depends on the relative price elasticities of demand and supply, with the incidence falling more on the less elastic side of the market. In particular, any subsidy increase will result in more landlord capture (higher rent increase) the more elastic demand is relative to supply. Although the relative elasticities of supply and demand, and hence how the incidence of an increase in AS support falls, is an empirical question, it is worth noting that, by reducing the demand elasticity (between the entry and maximum thresholds), the 70% subsidy rate acts to reduce the expected incidence on landlords and increase the proportion of the support captured by recipients.

Within this framework, we consider the two extreme outcomes. First, for landlords to capture the total increase in AS in rent, would require the supply of rental properties is perfectly inelastic (i.e., the supply curve is vertical). This would imply there is a fixed supply of rental housing, that is unresponsive to the market level of rent for properties. Second, for tenants to capture the total increase in AS would conversely require that low-income demand for housing is perfectly inelastic (i.e. the demand curve is vertical), in which case demand for housing is unaffected by the increase in support from the policy changes. This may occur, for example, if low-income households consider some basic level of housing to be absolutely essential, apportion the necessary budget from their income for this, and then allocate any surplus income to other consumption, such as food, clothing, etc. If they initially received the maximum AS-payment for this basic level of housing, the increase in accommodation supplement would then be fully allocated to other consumption.

Finally, to give a sense of the possible magnitudes we may expect using this framework, suppose the price elasticity of supply is 0.37 (Wong & Morrissey, 2016), and the price elasticity of demand is -0.21 (Koning & Ridder, 1997).[23] Given these values, the incidence of an increase in subsidy will fall mainly on tenants, with the rent predicted to increase by about one-third (0.36 = 0.37/(0.21+0.37)) of the increase in AS support. Furthermore, if the estimated demand elasticity is in the absence of ad valorem housing subsidy, the effect of the AS subsidy rate will be to lower the demand elasticity over the rent range between Mx0 and Mx1, in which case the predicted rent increase to tenants will be somewhat lower and the proportion of support captured by tenants will be higher.

[23] In discussing AS in the context of rental market, Wong and Morrissey (2016) report nominal prices (rent) increased 62% from 2005 to 2015, while supply increased 11% from 2006 to 2016. Based on a 25% increase in the Consumer Price Index (CPI) over the same period (2005Q2 to 2015Q2), real rents increased 30%, and suggesting a price elasticity of supply of 0.37 (=0.11/0.30). Koning and Ridder's (1997) demand elasticity estimate is based on a sample of low-income households estimated to be eligible for Rent Assistance in The Netherlands in the mid-1980s. The -0.21 elasticity is similar to the price elasticity of demand for rental housing (-0.18) for the US reported in Houthakker and Taylor (1970).

3 Research design

Our analysis is based on an analogous approach to that used by Hyslop and Rea (2018). We compare the relative changes in rents and AS-payments on either side of area-boundaries that had contrasting AS-maxima increases due to the 2018 policy changes. This extends the previous case-study approach of Auckland changes to analyse effects across the country. We also extend that analysis to examine the relative changes in rents and AS-payments for recipients according to whether their AS-payments were constrained by AS-maxima prior to the 2018 changes.

3.1 Boundary-area difference in difference analysis (BADD)

Our research design approach is a difference-in-difference analysis of the relative changes in housing outcomes of recipients residing in local areas on either side of AS-area boundaries that moved as a part of the 2018 policy changes. We will refer to this as the boundary-area difference-in-differences (BADD) approach. In particular, we will identify recipients living close to the boundary, based on the Stats NZ statistical meshblocks that they live in. The crucial assumptions underlying this approach are: (i) that such finely-defined areas that share a common border are part of the same local housing areas; and (ii) the relative policy changes across the common border are largely driven by average rent changes within the broader AS-areas to which the local border areas belong, and not driven by changes in these local boundary areas.

There are several prospective boundary areas to focus on as a result of the changes. We restrict our focus to boundaries between AS-areas: where the AS-area changes on one side of the boundary and remains the same on the other side. As the policy changes only 'upgraded' areas, in the sense of having higher maxima, we refer to the side of the boundary that changes AS-area as the 'treatment' side, and the side that doesn't change AS-area as the 'control' side. The analysis is based on local areas, defined as being meshblocks that lie within 1km of these boundaries. Appendix 2 contains more detailed discussion of the boundary area definition and construction, and appendix Table A2 describes the 11 boundary areas that we focus on.

Figure 2 shows the boundary changes around the Auckland and Wellington regions. Panel (a) shows the 2005 Area-1 boundary around Central Auckland (in dark red) that was the focus of the Hyslop and Rea (2018, 2019) analysis; the 2018 expansion of AS-area 1 to include the greater Auckland areas (as shown by the cross-hatched area) resulted in substantially larger increases in the AS-maxima outside as compared to inside this boundary. In addition, the outer Auckland boundary between the expanded Area-1 and Area-2 (i.e. between the cross-hatched and lighter-shaded orange shaded areas) also provides a boundary contrast. Similarly, panel (b) shows the

2005 Area-2 Wellington boundary between Tawa and Porirua which was extended in 2018 to include the Porirua area, which provides a boundary contrast. The boundary between the expanded Area-2 and existing Area-3 (i.e. between the cross-hatched and green outer-Wellington shaded areas) also provides a boundary contrast for analysis.

An AS-area change typically creates two different boundaries where a discontinuous change occurs: first, at the boundary where Areas are merged into a common Area, and second, at the boundary where a common Area is split. In Table A2, we characterise the boundary areas according to whether they are a "merged" or a "split" boundary. In addition, there are two "miscellaneous" sets of boundary areas that are neither merged nor split. For example, the merged boundaries include the Central Auckland boundary ("21_on_11", represented by the circled boundary around the red area in Figure 2(a)); and the Tawa-Porirua boundary ("32_on_22") in Figure 2(b). Similarly, the split boundaries include the outer-Auckland boundary ("21_on_22") in Figure 2(a); and the Hutt Valley-Wellington boundary ("32_on_33") in Figure 2(b). The two miscellaneous boundaries that we include consist of areas around Tauranga and Queenstown, that were rezoned from Area-2 to Area-1 and border on areas that remained in Area-4 ("21_on_44"); and areas that were rezoned from Area-3 to Area-2, which border on areas that remained in Area-4 ("32_on_44").

The main strength of this approach is that it relies on large differences in policy changes for relatively localised areas to identify the effects of the policy changes on housing outcomes. Although the policy changes were systematically determined by changes in housing costs over time across geographic areas, the AS-area boundaries were based on existing statistical and legislative areas. This which suggests that the relative changes either side of a boundary contrast are expected to be largely exogenous with respect to the local boundary area. In particular, we expect the relative changes to be determined by relative differences in the growth of housing costs within the respective broader AS-areas, of which the boundary areas should only contribute marginally.

In addition, although other related policy changes occurred over the period, these were nationally focused and expected to be relatively constant across areas (particularly across AS-boundary areas). As discussed above, the main exception to this relates to TAS entitlement, which likely acted to counterbalance the relative increase in AS generosity associated with the policy changes. That is, as found by Hyslop and Rea (2018, 2019), we expect there was relatively greater take-up of TAS on the control side of a contrast boundary after the policy change. For this reason, we will consider the combined AS and TAS received as the effective treatment of the policy around the contrast boundaries.

The main possible weakness of the approach comes from a threat to the so-called 'common trends' assumption, that the difference between outcomes either side of a contrast boundary are constant except for the effect 2018 policy-changes, required for the difference-in-difference approach to be valid. Although we expect the planned analytical approach to be robust to the nature of the changes and to changes in other policies, it remains an empirical question as to whether there were other secular changes in the housing market that differentially affected the contrast boundary areas, and the robustness of the estimated effects. We expect there is a trade-off between restricting the analytical period to a narrow window around the policy change during which the policy-change was the dominant treatment difference between the 'treatment' and 'control' sides of a contrast boundary, and allowing the housing market to adjust, as well as being able to test the common trends assumption. Following Hyslop and Rea (2018, 2019), we focus on the four-year window around the change date (i.e. two years pre- and post-). Observing outcomes over only two years before the change provides limited opportunity to test the common trends. As a robustness test, we will include a longer pre-change period from 2010 until 2021, which will facilitate stronger tests for common trends both before the policy change, and after our analysis window.

3.1.1 Empirical specification

We will analyse the effects of the AS-policy change on an outcome of interest, Y (e.g. weekly rental payments), for recipients located in meshblocks on either side of an AS-area boundary that experienced contrasting 2018 AS-maxima changes. To simplify notation we assume that an observation-*i* relates to an AS-recipient at a point in time. We classify recipients in areas that were rezoned as 'treated', and those in areas not rezoned as 'control', observations. Our BADD analysis will be based on difference-in-difference regressions of the form:

$$Y_i = \beta_0 + \beta_1 A_i^T + \beta_2 Post_i + \beta_3 A_i^T * Post_i + X_i'\gamma + \epsilon_i \quad (1)$$

where Y_i is the outcome for AS-claim observation-*i* (e.g. *i*'s weekly rent payment); A_i^T is an indicator variable for whether *i* is 'treated' (i.e. in a rezoned AS-area); $Post_i$ is an indicator for whether *i* is observed after the 2018 policy changes; X_i' is a vector of additional control variables that may affect Y_i, such as the demographic and geographic characteristics associated with the claim; and ϵ_i captures other unobserved factors. Conditional on X_i', the β coefficients have the following interpretation(s): β_0 is the average 'control' outcome before the 2018 changes; β_1 is the pre-change difference in average 'treated' outcome (i.e. relative to 'control'); β_2 is the post-change difference in the average 'control' outcome (i.e. relative to pre-change); and β_3 is the difference in the post-change average 'treated' outcome relative to the difference in their pre-change outcome. This latter coefficient (β_3) is our primary interest, representing the 'difference-

in-difference' effect of the policy change on outcomes for those in the rezoned AS-area relative the non-rezoned contrasts.

The simple difference-in-difference specification in equation (1) can be relaxed in several directions. For example, Hyslop and Rea's preferred specification relaxed the assumption of time-constant effects after the policy change, to allow annual treatment effects. This would be achieved in equation (1) by replacing the constant $Post_i$ indicator variable with separate post-year indicators (e.g. $Post1_i$ and $Post2_i$) and interacting these with the A_i^T treatment group variable. In addition, the regression can be relaxed to allow the various effects to vary across (e.g.) family size, or geographic areas. We will do this by estimating separate regressions for various subgroups of interest.

4 Data

In this section we describe the data construction and sample characteristics of the analysis sample used in the analysis, together with trends in the primary factors of interest. The main analytical sample is derived from various MSD sources and tables in Stats NZ's Integrated Data Infrastructure (IDI) relating to receipt of Accommodation Supplement. A more detailed description of the data construction is provided in Appendix 1. In addition to the main analysis of the AS data, we also provide some comparative analysis of weekly rental amounts using data from the Ministry of Business Innovation and Employment (MBIE) Tenancy Bonds database.

4.1 Accommodation Supplement related data

The primary data source is a set of MSD data tables provided via an ad hoc load into the IDI. These provide information on AS receipt including the AS area in which recipients live, other supplementary payments (particularly, Temporary Additional Support, TAS), and the main AS client's demographic characteristics, family structure, and benefit and non-benefit incomes.[24] These data are then merged with the client's residential location information (from MSD sources) in the main IDI release environment.[25] The residential location information includes information on the statistical meshblock, which we use to identify recipients who live close to AS area boundaries that changed.

The observation unit in each of the tables is a client AS-spell. Our main analysis sample covers the period two years either side of the policy change (April 2016 – March 2020), although

[24] Because AS payments are not subject to income abatement for beneficiaries, the income information is largely incomplete for main benefit recipients.
[25] The focus on MSD residential location information is in the interest of ensuring greater consistency with the AS-receipt data.

we also extract data over the extended period April 2010 – May 2021 to facilitate longer term trends and robustness analysis. Changes in the relevant characteristics of a client's interaction with MSD will result in a new observed administrative spell. For this reason, we construct a monthly panel data extract by *sampling* information pertaining to any AS-spell that is ongoing on the 15th of the month, over the period between April 2016 and March 2020. This *sampling* means the data extract does not include complete AS-spell information and, for example, short AS spells that do not cover the 15th of a month will not be measured.

Importantly, the data do not directly state the family size associated with an AS-claim. Given this, we construct measures of the AS client's partnership status from whether or not they are listed as being partnered in the data, and the family size from the number of dependent children associated with the main client and their partner. From these, we characterise the family structure according to whether the main client is single or partnered, and whether and how many children are covered in the AS claim.

We make two further comments on the data quality. First, we observe a small fraction of observations receive AS payments that exceed the maximum based on their AS-area and our measure of family size. Approximately half of these observations receive amounts equal to the maximum for the next-larger family size; and we estimate a similar fraction of observations receive AS amounts equal to the maximum for the next-smaller family size. This suggests the family size is not perfectly measured, with errors both above and below. Second, the AS-area information provided in the ad hoc AS tables is not entirely consistent with the meshblock identified from the matched residential location table. This may be either because of matching errors in the IDI, or because of AS-administrative errors in determining the correct AS-area associated with a claim. In both cases, because we can accurately identify errors in one direction only, we have decided to include all observations, and take the information at face value. Although we believe the data are generally accurately measured, a consequence of any measurement error is that it may cause some attenuation bias (towards zero) in the resulting estimates.

4.1.1 Sample characteristics and trends

We next discuss the characteristics of the analytical sample and analyse some descriptive trends in AS receipt and rents over the period.

Table 2 summarises the characteristics of our main analysis sample of boundary contrast observations on renters. The first column pertains to the full sample; the next two columns to the 'treatment' subsamples, pre- and post-policy change respectively; columns (4) and (5) similarly to the 'control' subsamples, pre- and post-; and the final column documents whether

there was a statistically significant relative change in the average characteristic value on either side of the boundary.[26] The full boundary analysis sample includes over 1.23 million monthly observations on nearly 74,500 distinct AS-rent clients. Overall, nearly three-quarters (74%) of the observations reside on the treatment side of boundaries, although this varies across the boundaries – e.g. the merged-boundary contrasts have relatively more control than treatment observations.

We next describe the demographic characteristics of the sample. The average age of AS-clients is 46 years, 63% are female, 31% are Māori and 10% are Pacific peoples.[27] Almost one-half (49%) of clients are single adults, about one-third (34%) are sole parents, 8% are couples, and 9% couples with children. Roughly 20% of observations are in each of Area-1 and Area-4, and 30% in each Area-2 and Area-3.[28] Nearly 80% of AS-recipients were main beneficiaries, including New Zealand Superannuation (NZS) and Veteran's Pension (VP) recipients: about one-quarter (26%) were on Jobseeker Support (JSS), and 15-20% on each of Supported Living Payment (SLP), Sole Parent Support (SPS) and NZS/VP.

Although the demographic characteristics are broadly similar across the treatment and control groups on average, they are not fully balanced. For example, there are some noticeable differences in the gender, ethnicity, family structure and benefit receipt across the groups. However, the relative changes in characteristics from before to after the policy change are generally more similar across the treatment and control groups. Also, characteristic changes that are statistically significant different are typically quite small substantively: e.g., there are statistically significant differences in the increase in the number of children and family size in the treatment groups relative to the control group, but these are only on the order of 2-3%.

We next focus on the characteristics of AS recipients' rental and AS-receipt payments. First, close to half (47%) of all observations received the prevailing AS-maximum payments over the full sample period. Not surprisingly, reflecting the relative imbalance in maxima relative to rents in the broader areas before the policy changes occurred, the fraction was higher in the treatment (66%) than the control (47%) group; while after the policy changes, the treatment group fraction was lower than the control group (33% versus 36%), which perhaps reflects lower prevailing rents in the rezoned areas. The average AS-payment received over the period was

[26] To do this, we estimate the simple difference-in-differences regression for each characteristic (X): $X_i = \beta_0 + \beta_1 * Treatment_i + \beta_2 * Post_i + \beta_3 * (Treatment_i * Post_i) + \epsilon_i$. The null hypothesis of a balanced change in value on both sides of the boundary is H_0: $\beta_3 = 0$. To inform this hypothesis, we present the estimated β_3 coefficient and its p-value.

[27] Ethnicity categories are based on total responses. People can identify with more than one ethnicity and can therefore be included in more than one ethnicity group.

[28] The upgrading of AS-areas means that the shares of observations in each area will change before to after the policy change. For example, there will be fewer Area-4 observations, and more Area-1 observations, after the change; in addition, the DiD estimates in the final column indicate there were also fewer Area-3 observations, and more Area-2 observations, after the change.

$97.45 per week. Reflecting both the increased maxima and the area changes that occurred, there was a substantially larger increase in the average AS-payment in the treatment group (from $72.80 to $113.30 per week) than in the control group (from $98.90 to $116.95 per week): the simple DiD estimate of the relative average increase is $22.44 per week (or 28% relative to the average pre-change AS of $79.70). This amount provides a simple measure of the magnitude of the treatment associated with the policy changes that the analysis will focus on.

In addition to AS support, 29% of AS-observations also receive a TAS payment of $17 per week on average. The incidence of TAS receipt is higher among the treatment group (31%, $20) than among the control group (28%, $16.50) before the policy change. Following the policy change there was a reduction in TAS receipt in the treatment group (26%, $14.50), while the incidence actually increased in the control group (29%, $16.70). Combining the AS and TAS payments shows that the relative increase in total accommodation support payments received by those in the treatment group was $16.50 per week. Although this is somewhat lower than the relative increase in AS, it is still a relatively large increase. Compared to the average total support before the change ($98.90), it represents a 16.7% increase in support for those in the treatment group relative to the control group.

Table 2 also shows that the weekly rental payments were $23.00 (8%) lower on the treatment side of the boundaries than the control side before the changes. On average, rents increased $7.00 per week faster in the treatment group than control group after the policy change, with the difference in average rent between the groups of $16.00 (6%) after the changes.

In Figure 3 we present descriptive trends in AS-support and rent payments for all the boundary area treatment and control observations. To provide a sense of the longer-term trends, we have plotted the trends over the extended period from April 2010 to May 2021. Panel (a) plots the trends in the average weekly AS-payments received by recipients on the treatment and control sides of the boundaries. The trend for the treatment group is essentially flat over the eight years prior to the policy changes and then, following a step-jump in April 2018, shows a gently increasing trend over the following 18 months. In contrast, the average AS support in the control group is about $20 per week higher in 2010 and shows a very gentle trend increase before the change, followed by a smaller step-jump in April 2018 and then essentially no change until mid-2020. As a result, the average AS payments of the two groups had almost converged by 2020. Broadly similar patterns are observed in panel (b) for the average total accommodation support payments received by the two groups, albeit with both groups showing increasing average support levels before 2018, and less overall convergence late in the period.

The treatment and control group trends in the fractions receiving the AS-maximum payment are shown in panel (c). Both groups' trends show steadily increasing fractions receiving the AS-maximum payment and are roughly parallel throughout the period, except for step-down changes when the policy came into effect in April 2018. Consistent with the notion that AS is relatively less generous for those on the treatment side of boundaries before the policy changes which then corrects this imbalance, the treatment group fraction is 18-20 percentage points (pp) higher before the policy change but 3-4 pp lower after April 2018.

Finally in Figure 3, panel (d) shows the trends in average weekly rent for the two groups together with the difference between these averages. There are comparatively steady increasing average rents for both groups, with a noticeable relative increase in the treatment group trend after April 2018. In fact, the trend difference shows some divergence in average rents between 2010 and 2017, followed by relatively strong convergence in the last two years. For example, the average weekly rents in the treatment and control sides diverged at about $0.75 per year between April 2010 and April 2018, and then converged at about $5 per year over the two years after April 2018.[29]

Figure 4 presents the corresponding trends for the Central Auckland boundary contrast area (i.e. 21_on_11) treatment and control observations. The patterns are broadly similar to those in Figure 3. For this case, panel (d) shows the average rents are essentially the same on both sides of the boundary over the eight years before the policy change. This is consistent with the hypothesis that the treatment and control sides of the boundary form a common housing market. However, there is a steady relative increase in rents for treatment group recipients after the change. Average rents are $6-10 per week higher on the treatment side by 2020. Consistent with these rent patterns (and the AS policy parameters), the average AS-payments (with and without TAS) for those in the treatment area change from being substantially ($15-$25 per week) lower before the change to about $5 per week higher after the policy change.

The simple trends in average rents in the treatment and control groups for all boundary contrasts (Figure 3) and the Central Auckland boundary (Figure 4) are suggestive of a policy treatment effect of the increase in AS support resulting in an increase in relative rents. We analyse this hypothesis more seriously in Section 5.

[29] In fact, that the difference trend in panel (d) appears to change after April 2017 suggests there may possibly be announcement effects associated with 2017 Budget announcement of the AS policy changes.

4.2 Comparison of AS and Tenancy Bond rents

Finally, to provide some assessment of how representative AS-recipients' rents are with respect to broader market rents, and to investigate whether the changes in AS policy are related to changes in the broader housing market, we have compared AS-rents with those from MBIE's Tenancy Bonds (TB) database in the IDI. This contains information on tenancies that have bonds lodged with MBIE, including the start date of tenancy, the end date (if the tenancy has been closed and bond returned), the weekly rental amount associated with the tenancy, and some basic documentation on the type of dwelling (house, flat, apartment, etc), the number of bedrooms, and the type of landlord (property management company, private landlord, etc). For our comparative analysis of rents, we have focused on the rental payments of newly lodged tenancy bond agreements as being most representative of the current market rental value. From the monthly numbers of tenancy bonds lodged in the TB table in the IDI, it appears the data are fairly complete until the end of 2020.[30]

Using the TB data, we calculate the average weekly rental amounts on newly lodged tenancies in each calendar month between April 2010 and December 2020. Also, because AS-recipient rents are expected to be lower than average, we also estimate the 25th percentile (P25) rent as a possibly more representative comparison. We summarise the characteristics of the Tenancy Bond sample in appendix Table A4: for all boundaries in panel (a) and the Central Auckland boundary in panel (b).

In appendix Figure 5 we describe the trends in TB rents,[31] and compare the TB-P25 and AS-average rents, for all the AS-boundary areas (panels (a) and (b)) described above, and for just the Central Auckland (21_on_11) boundary (panels (c) and (d)). Panel (a) shows both average and lower-quartile new tenancy rents are higher on the control side of the boundaries: by about $40-$50 per week (10%) for average rents, and $15-$20 per week (8%) for P25-rents. Both measures were increasing throughout the period, with the rates of increase somewhat stronger after 2014. Importantly the only hint of a trend increase in rent around 2018 is for the Auckland treatment area's P25 series.

In contrast, the comparative trends of the P25 new tenancy rents and the average AS-rents shown in panel (b) are quite striking. First, the respective series match very closely over 2010-12, before steadily diverging. In particular, while the relative trend increases on the control

[30] As can be seen in appendix Figure 6, there is some drop in the number newly lodged bonds towards the end of this period. This suggests the last few months are still not complete.

[31] There are quite strong seasonal patterns in both average and P25 rental amounts in the Tenancy Bonds data, with new-tenancy rents peaking in January of each year, and are lower the average between May and October. To adjust for the seasonal variation and emphasise the rent trends, we present 13-month centred moving averages.

and treatment side of the boundaries are broadly similar in each data source, the new tenancy rents steadily increase relative to average AS-rents from 2013 to 2020. There is little change in the trends around the AS-policy changes in April 2018: although the average treatment and control AS-rents converged somewhat (discussed above), the TB P25-rents diverged a little.

The corresponding trends for the Central Auckland (21_on_11) boundary are shown in panels (c) and (d). In contrast to the patterns for all the boundaries in panel (a), new tenancy average and P25-rents are higher on the 'treatment'-side of the Central Auckland boundary than the 'control'-side: by about $20 on average and $50 for the P25. The reason for these differences is unclear but is consistent with the statistics in Table A4 panel (b) that shows new tenancies on the treatment-side are on average slightly larger (about 0.1 more bedrooms), and more likely to be houses than flats, apartment or boarding houses and rooms. These differences aside the relative trends in average AS-rents and P25 TB-rents are broadly similar to those in panels (a) and (b). In particular, comparing the new tenancy P25-rents and AS average rents in Figure 5 panel (b), shows flatter trends for the AS-rents than P25-rents throughout the period. In contrast to the diverging treatment and control AS-rents, some convergence in P25-rents is seen towards the end of the period.

We have plotted the trends in the numbers of new tenancies in the TB data and AS-recipients in appendix Figure 6, for all boundary areas in panel (a) and the Central Auckland boundary in panel (b). There was a noticeable increase in AS-recipients after April 2018 while the number of new tenancies was stable or declining (however, note footnote 30). The number of AS-recipients increased about 10% on both sides of the boundaries, either as a direct result of the policy changes or possibly simply due to the rising housing costs. However, there are no obvious long-term differences in the TB and AS trends.

From this analysis, as expected we infer that AS-rents are more representative of those in the lower part of the rent distribution. More puzzling is why the longer-term growth in average AS-rents is noticeably slower than P25 TB-rents, which suggests AS-recipients' rents have become more selective over the period. One possible explanation is that the erosion of AS-support over time resulted in AS-recipients becoming more concentrated in lower-rent housing units. Another explanation may be that AS-recipients have longer-term tenancies with rents below the new-tenancies market rent. However, given the relatively close correspondence between average AS-rents and P25 TB-rents early in our sample window, this would suggest the tenancy-length effects have been occurring only since about 2013.

5 Boundary area difference-in-difference estimates

We now turn to a more detailed analysis of the effects of the 2018 AS policy changes.

5.1 Boundary analysis of rent effects

We first focus on estimating the effects of the policy changes on rental costs in the boundary contrasts sample. To do this we estimate various difference-in-differences regressions based on equation (1). To calculate the policy effects on rent relative to the average increase in accommodation support received, we estimate equivalent regressions for "total-AS" as for rent. We begin by estimating regressions based on the full sample of boundary contrast areas, and summarise the results in Table 3, panel (a) for rent and panel (b) for total-AS.

The first specification, presented in column (1), includes separate controls for the type of boundary contrast (i.e. "merged", "split" or "miscellaneous") and also for the treatment (versus control) side of each of these types of boundary. However, it restricts the time effects to vary pre- versus post-policy, so there is a single policy treatment effect. The estimated treatment effect (the coefficient on the Treatment*post-2018 interaction) is $6.40, which is statistically significant. This is similar to the simple difference-in-differences estimate of $6.97 shown in Table 2 and suggests that rents increased $6.40 on average more on the treatment side of boundaries than the control side over the 2-years after the April 2018 policy change compared to the 2-years before the change. The corresponding (DiD) estimate of the increase in total-AS in panel (b) is $17.60, suggesting that 36% (=$6.40/$17.60) of the relative increase in accommodation support passed through to higher rent payments on the treated side of the boundaries, with recipients capturing 64% of the increase.

In column (2), we control for year-specific effects, and allow the treatment effects to vary by year.[32] In this specification, the coefficient on the treatment*2017/18 interaction effect provides a test of whether there was a significant relative change in rents on either side of the boundary in the year before the policy change (i.e. the common trends assumption). This coefficient (2.70) is moderate and statistically significant, which questions the validity of the common trends assumption.[33] The coefficients on the 2018/19 and 2019/20 treatment-interactions are both statistically significant, and suggest that the AS-changes led to rents being higher on the treatment side relative to the control side of the boundaries by about $6.60 in the first year after the change and $8.90 by the second year. The corresponding panel (b) estimates

[32] We define March-year effects and exclude the first year (April 2016-March 2017) as the base year.
[33] When other factors are controlled for in the subsequent specifications presented Table 3, the Treatment*2017/18 coefficient is generally small and statistically insignificant: this result also generally holds for subsamples presented in other tables below.

for total-AS ($16.50 and $18.60) suggest the pass through of AS to rent increases were 40% in the first year and 48% by the second year, implying that 52% to 60% of the support was captured by recipients.

The third specification controls for the AS client's demographic and family characteristics, and also specific boundary-contrasts.[34] The estimated rent treatment effects in this specification remain statistically significant but are roughly half those in column (2) – i.e. $2.40 in 2018/19 and $3.90 by 2019/20. In contrast, the estimated AS-increases in panel (b) are more similar to those in column (2), implying much smaller pass through to rent (16% and 23%), and a higher (77% to 84%) capture by recipients. We have also estimated regressions which control for the client characteristics and boundary effects separately. The client characteristics account for more of the variation in rents, although both factors contribute to the reduction in policy effects (e.g. the 2019/20 estimates are $5.80 controlling for client characteristics, and $6.70 controlling for boundary effects).

Next, in column (4) we include client fixed effects to control for fixed unobserved differences that may be correlated with the treatment status. This further weakens the estimated policy effects on rents, with the estimates small ($1.10-$1.30) and statistically insignificant effects in the two years after the policy change, while the estimated AS-increases remain comparatively large (about $13.70 in both years). Finally, in order to examine the effects for tenants who remain in the same dwelling over time, we report results for the (column (3)) specification that controls for observable characteristics, estimated on the subsample of clients who do not change their meshblock over the period.[35] The estimates in column (5) are somewhat smaller and less statistically significant than those in column (3), with the 2018/19 effect of $3.44.

To summarise the results in Table 3, consistent with the trends seen in Figure 3 and Figure 4, we find statistically significant increases in weekly rent associated with the relative increase in AS, without controlling for client characteristics and boundaries. The estimated rent increases in column (2) are about one-third and one-half of the increase in AS-support payments in the first and by the second year after the policy change. However, controlling for observable client characteristics reduce the estimated effects by about 50%; and further after controlling for fixed unobserved effects of clients. These patterns imply changes in both the observed and unobserved characteristics of AS-clients over the period that were correlated with their

[34] The demographic controls include gender, age, ethnicity, benefit type, and family structure; while the boundary controls include both specific boundary-contrast and treatment-side effects, and boundary-specific interactions with each year control.

[35] Strictly speaking, because the data do not identify a person's exact dwelling, this sample may include clients who move dwelling within a meshblock.

treatment status account for a substantial part of the estimated raw increase in rents associated with the policy changes. However, these factors have relatively little effect on the corresponding estimates of the increase in accommodation support.

To explore this further, we narrow our focus to a relatively constant subsample of AS-recipients by selecting those recipients who received AS support both in the first year (2016/17) and final year (2019/20) of the sample period. Appendix Table A5 report the results for the same set of regression specifications. Consistent with the interpretation of the pattern of results in Table 3, the estimated effects are much smaller and generally statistically insignificant. In fact, the only statistically significant estimates are in column (1) ($2.20) and for the 2019/20 effect ($3.00) in the column (2). Again, here are larger and relatively stable estimated increases in accommodation support of $12-15 per week.

5.1.1 Boundary area effects

Next, we examine whether the effects vary across the boundary areas. Table 4 summarises the effects across different boundary samples: the merged boundary contrasts, the split boundary contrasts, the miscellaneous boundary contrasts, and finally the Central Auckland boundary ("21_on_11") which dominates the merged boundary contrasts. For each sample, we present results from two specifications, corresponding to those in columns (3) and (4) of Table 3: i.e. controlling for observable factors, and additionally controlling for AS-client fixed effects. Both the estimated treatment (i.e. AS support in panel (b)) and rent effects (panel (a)) vary quite a lot across these samples. In particular, the estimated rent effects are relatively small and insignificant in the merged and Central Auckland boundary samples. In contrast, the effects in the split boundary sample are somewhat larger, and statistically significant in column (4), with implied 30-50% pass through of the increase in AS support to rent on average. The results for the miscellaneous boundary contrasts are also significantly positive in column (5), implying about 35 and 50% pass through of AS support to rent in the first and second year after the policy change respectively, but the estimates are smaller and statistically insignificant controlling for client fixed effects in column (6).

5.2 Robustness analysis

Before moving on to consider effects for population subgroups, we first consider the robustness of these regression results of the impacts of the AS-policy changes on rent. For this we consider three alternative approaches to test the robustness. First, we extend the period of analysis, in order to better test the common trends assumption associated with the research design. Second, we test for broader market effects using Tenancy Bond data on new lodgements. Third,

we test the robustness of the results to considering a broader characterisation of the local area around a boundary. We consider each of these analyses in turn.

5.2.1 Extended period analysis

Partly to better assess the common trends assumption, we first consider regressions based on a longer observation period. For this, we first extend the sample period back to April 2010, and re-estimate the regressions corresponding to those in columns (1)—(4) in Table 3. We then also extend the period forward to May 2021 and re-estimate the third and fourth specifications. The results are presented in Table 5.

The common trends test rejects the assumption of common trends for the rent specifications in panel (a) column (2) (p-value=0.03), and marginally rejects the assumption when controls are included in columns (3) and (5) (p-values=0.09), but the common trends hypothesis is accepted in columns (4) and (6) when client fixed effects are included (p-values>0.2). Nonetheless, the (post-policy) estimated treatment effects on rents based on these longer samples are generally comparable but a little larger and more precisely estimated (reflecting larger the sample size) than those in Table 3. For example, the 2018/19 and 2019/20 estimates in column (3) are 2.70 and 4.40 compared to 2.40 and 3.90 in Table 3. Furthermore, the estimated policy effects for the 2020/21 and 2021/22 years in columns (5) and (6), are comparable to the 2019/20 estimates, which supports the notion that any policy effect on rents was concentrated over the two years following the policy change.[36] In contrast to the rent results, the common trend results are always accepted for the total accommodation support regressions in panel (b) of Table 5, except for column (4) (p-value=0.09). The estimated 'treatment' effects on accommodation support are also slightly larger than the corresponding estimates in Table 3.

5.2.2 Tenancy bond analysis

Our second robustness check of the boundary analysis results is to estimate analogous regressions using the Tenancy Bond data on new tenancy rents. The advantages of these data are that they include a broader sample of rental properties than just those associated with AS recipients. We again consider a comparable set of regressions, bearing in mind differences in the

[36] This is consistent with the results in Hyslop and Rea (2019). However, there is a caveat associated with results based on the extended post-change period, due to the likely effects of the COVID-19 pandemic and policy responses, which included a six-month rent freeze from April 2020. Also, the 2020/21 effects are based only on two months (April and May).

structure and available information in the TB data. The results from this exercise are summarised in Table 6 for the full boundary-contrasts sample.[37]

The first three specifications correspond to those in Table 3 and are based on the new tenancies over the period April 2016 – March 2020. The results in columns (4) and (5) correspond to the third specification with controls, but estimated over the extended periods April 2010 – March 2020, and April 2010 – December 2020 respectively. There is no evidence of AS-treatment effects on rents following the 2018 policy change in any of these specifications. In fact, the simple difference-in-difference estimate in column (1) is insignificantly negative, as are the year-specific effects in column (2) of -$0.22 in 2018/19 and -$8.08 by 2019/20. When including control variables in columns (3) – (5), the year-effects imply statistically insignificant treatment effects of about $3.50 per week increase in rents in 2018/19, and a -$5.30 per week effect by 2019/20. In addition, each of the column (2) – (5) specifications find significantly higher rents (of $9.20 – $14.60) in 2017/18 relative to 2016/17, although the common trends assumption is not rejected in the final two columns.

5.2.3 Placebo boundary analysis

The third robustness analysis we consider examines and tests the robustness of the main results to allowing for possible placebo effects away from the policy-change boundaries. We first extend the definition of the local area around boundaries to include meshblocks on either side that lie within 2km of the boundary. We then distinguish recipients who live in meshblocks that lie within 2km but not 1km on the treatment side (T2) or on the control side (C2) of the boundary. We re-estimate the main regressions from Table 3, estimating policy treatment effects for all recipients who live on the treatment relative to those who live on the control sides of the boundary. The hypothesis we test is that there are no differential effects within the treatment-side sample or within the control-side sample, which we do by testing whether there are statistically significant differences in outcomes for those in the T2 or C2 areas relative to the overall treatment and control side effects respectively.

The results from this analysis are presented in Table 7: for all boundary areas in the first three columns, and the Central Auckland boundary areas in the final two columns. The column headings again relate to the corresponding specifications in Table 3. First, the main rent-treatment effects in panel (a) are comparable to those in Table 3 for the full sample. Second, and more importantly, once controls for observed and unobserved differences are included, there is

[37] This table summarises the results from standard OLS regressions. Because AS-recipient rents are concentrated in the lower part of the rent distribution, as a further robustness check we have also estimated quantile regressions (for q=0.25, the 25th quantile). The results are qualitatively the same as those presented in Table 6 and are available on request.

no evidence of significantly different effects associated with the T2 or C2 areas. In fact, the F-statistics for the joint hypothesis of no effects have p-values greater than 0.10 for all boundary area regressions, and only less than 0.10 for the Auckland boundary regression with controls.

We re-examine these effects for the Central Auckland (21_on_11) boundary areas. In this case, one possible reason why there may be different effects for the T2 or C2 areas is if there is a Central Auckland 'gravity' effect which leads to increasing rents over time for properties in closer proximity to the centre. If so, we may expect the C2 rent-effects to be positive (and growing) over time, and similarly, the T2 rent-effects to be negative and growing over time. Again, the main policy treatment estimates on rents are similar to those in Table 4, and there is no evidence of significant differences associated with either the T2 or C2 areas. Also, although the C2 effects are positive (consistent with an increasing gravity effect), the estimated T2 effects are typically positive too.

Based on these three sets of results, we believe the main results discussed above are robust to considering a longer period of analysis, to considering a broader sample of rental tenancies, and also to considering an alternative broader characterisation of local areas. Furthermore, these additional regressions provide support for the common trends assumption (at least when controls are included in the regressions) required for the identification.

5.3 Subgroup effects

We now turn to considering the policy effects on various subgroups of interest, using the full sample of boundary contrasts. This will include considering analyses separately for different family-structure groups, based on the number of adults, and the presence of children; and by the AS-client's gender and ethnicity, whether or not they received a main-benefit, and the effects of TAS receipt. Based on the numbers reported in Table 2, the number of monthly AS-recipient observations in the two years post-2018 was 5.5% higher than the number in the preceding two years. This growth was not uniform across demographic groups. Growth was particularly strong for single adults (11.0%), JSS recipients (12.2%), NZS/VP recipients ()11.4%) and non-beneficiaries (9.0%). There was also stronger growth for Pacific peoples (6.8%), Europeans (6.7%), and the 'other ethnicity' group (8.0%), but only average growth for Māori (5.4%). Growth of monthly payments to Asian recipients grew by only 0.8%. The changing composition of recipients clearly contributed to the raw rent and accommodation support effects discussed above. It also highlights the need to estimate separate group-specific treatment effects to test for possible heterogeneous treatment effects.

5.3.1 Family type effects

We consider effects across family types, summarised in Table 8 for the specification that controls for observable factors (i.e. specification 3 above). Again panel (b) shows there are quite variable levels of treatment across the family types, reflecting the variation in increases in the AS-maxima across family sizes and areas. In particular, families with children on the treatment side of boundaries received relatively larger AS-support increases ($15 – $25 per week on average) than singles and couples ($10 – $15), albeit they also generally had higher rent and AS-support levels.

The only statistically significant estimated rent effects are for Sole parent families with at least 2 children. For this group, rents increased about $8.75 in the first year and $11.40 by the second year (implying about 45% pass through of the AS-support increase). Controlling for client fixed effects these effects wash out, with the resulting 2018/19 and 2019/20 estimates being $1.60 and $3.40 respectively. In addition, the estimated rent increases for sole parents with 1 child ($4.90 and $5.70), although not statistically significant, imply AS-support passthrough to rent of one-quarter to one-third. These effects wash out again, however, when client fixed effects are included. These patterns are suggestive of changing characteristics of sole parent families over the period that are correlated with the policy treatment status.

5.3.2 Demographic effects

Our final subgroup analysis is across demographic characteristics and benefit status. For each subgroup, we again present results based on specification 3 in Table 3. First, Table 9 summarises the effects for gender and age subgroups. The first two columns imply the estimated rent effects are broadly similar for male and female clients, with increases by 2019/20 of $2.70 (not significant) and $3.50 (significant) for men and women respectively. Again, when we control for client fixed effects, the estimates wash out. Across age groups, we find relatively strong rent increases for young clients (aged 16-24): the 2019/20 relative increase in rents is estimated to be $13.30 (about three-quarter AS-support passthrough). Although the estimated effects are smaller ($5.90 for 2019/20) and insignificant when we control for fixed effects, they are suggestive of over 50% passthrough to rent for young tenants. For older working age (25-64) clients, the effects are somewhat smaller but significant, implying about 20-25% AS-support passthrough to rents; again, the effects washout with client fixed effects. We find both small and insignificant effects for older (65+) tenants.

Table 10 summarises effects for subgroups based on ethnicity, based on total response categories. People reporting more than one ethnic identity will be included in more than ethnic category. By ethnicity, the most prominent and significant effects are for Pacific peoples tenants, where we estimate relative rent increases of $8.50 and $11.70 (one-third and one-half

passthrough) in 2018/19 and by 2019/20 respectively, although these effects wash out when client fixed effects are included. The client fixed effects estimates in the final column of the table imply that only 4% of the accommodation support changes are reflected in average rents, implying that 96% of the support is captured by tenants rather than landlords. The estimated effects on rents and support for Māori tenants is broadly similar to the estimates across all tenants, as reported in Table 3. For Māori, the results imply that 87% of the accommodation support is captured by tenants. Curiously, the estimated rent effects ($2-3) for European tenants are largely unaffected by controlling for client fixed effects, while the estimated support effects do fall 10-15%, and imply about 15-20% passthrough (83% to 87% capture by tenants). This suggests that European tenants are not driving the compositional changes across the boundaries.

5.3.3 Benefit-related effects

We next consider whether the estimated policy effects vary across recipients who were main beneficiaries or not. The results are summarised in the first four columns of Table 11. The estimates imply very similar effects on rents for beneficiaries and non-beneficiaries, with greater statistical precision for beneficiaries. For example, controlling for observable differences, we estimate the 2019/20 policy effects on rent was about $3.60 (and statistically significant) for beneficiaries, and about $4.20 (insignificant) for non-beneficiaries. Controlling for unobserved client fixed effects, the estimated effects fall by more than 50% to $1.70 and $1.90 respectively and are no longer statistically significant.

However, panel (b) implies the estimated relative increase in AS-support was much larger for non-beneficiaries ($23-24) than beneficiaries ($11-15). Also, these estimates are largely robust to including client fixed effects: the beneficiary estimates fall by less than 25%, while the non-beneficiary estimates increase slightly. Combined with the estimated rent effects (although statistically insignificant), these imply there was greater passthrough of the increase in support to rents for beneficiaries than non-beneficiaries (about 15-25% versus 5-15%). This implies that beneficiary recipients capture 75% to 85% of accommodation support, and that non-beneficiaries capture 85% to 95%.

Finally, we examine whether there are important interaction effects associated with receipt of TAS payments that affect the estimated effects of the policy changes. On one hand, accommodation cost related TAS (hence total support) payments can increase at the same rate as rent over some range, between the maximum-AS level and maximum-TAS level. This means there is effectively no recipient co-payment over that range. On the other hand, TAS payments abate at 100% with marginal income increases, so that the increase in AS payments associated

with the policy change will not increase total accommodation support until TAS is fully abated. These two factors appear to have offsetting effects on support for those receiving TAS, although the latter is likely to dominate, and thus dilute the policy-change treatment, for our boundary area analysis.

In order to examine whether these factors affect the estimated policy effects, we stratify the sample of AS-recipients according to whether or not the recipients ever received TAS payments over the four-year analysis period.[38] The regression results for these subgroups are summarised in the final four columns of Table 11. For those who never-received TAS over the period, we estimate statistically significant rent increases by 2019/20 of about $7.10 per week (controlling for observables) and $3.50 (with client fixed effects). The corresponding estimates for those who ever-received TAS are smaller and not statistically significant. However, in contrast to these rent effects, we find the policy effects on accommodation support were larger for the ever-received TAS ($15-20 per week) than the never-received TAS ($13-17 per week) group. These findings imply that the policy changes that differentially increased the AS-maximum payment levels across local boundary areas benefited TAS recipients relatively more because their support was more constrained by the previous AS-maximum payments. Also, given the absence of any rent increase, the increase in support was effectively an increase in their net income.

5.4 Analysis of AS-boarder and homeowner effects

About two-thirds of AS-recipients are renters, while about 22% are boarders and 11% are homeowners. In this section, we consider these other two groups of recipients, and focus on whether the 2018 policy changes affected their housing costs. For this we present analogous trend and main regression results from parallel analyses to that discussed previously for AS-renter recipients. Clearly there are some important differences in the characteristics of these groups of recipients that explain much of the difference in outcomes across the groups.

5.4.1 Accommodation Supplement Boarder recipients

We begin by considering AS-boarder recipients in our boundary areas. AS-boarder recipients are much more likely to be single adults (over 80%) compared with renters (about 50%). They are more likely to be male (53% compared to 37% of renters), and are also more likely to be younger (average age of 37 years compared to 46 for renters). Boarders are also more likely to be Māori

[38] This stratification results in almost equal numbers of monthly observations associated with those who ever-received TAS (about 619,000 observations) and never-received TAS (about 615,000 observations).

(45% compared to 31% of renters), and Pacific (13% versus 10%), and less likely to be European (57% versus 68%).

We first describe the monthly trends in average outcomes for AS-boarders in the "treatment" and "control" sides of the boundary areas from April 2010 – May 2021, presented in Figure 7. Panel (a) shows the trends in average AS-payments received, panel (b) the trends in total accommodation support (AS+TAS) payments, panel (c) the fractions who receive the maximum AS-payment, and panel (d) the trends in average weekly Board payments. The trends in AS and total support payments in panels (a) and (b) for boarders differ from those for renters in Figure 3 in three respects. First, the average AS and total support amounts are very similar, implying low incidence of TAS receipt among boarders. Second, the average payments are much lower than those received by renters – e.g. in March 2018, boarders received $35-40 per week, compared to $70 or $100 for renters. Third, the average support for boarders increased gradually over the period, with relatively little increase around the April 2018 policy change.

These differences in support patterns for boarders compared to renters can be understood by considering the fraction of recipients receiving the maximum AS payments, shown in panel (c). In contrast to the high fraction of AS-renters who receive the maximum payment, relatively few boarders receive the maximum. For example, even though the fractions are increasing in the period before the policy change, in March 2018 only 13% of boarders on the treatment side and 8% on the control side received the maximum (compared to 55% and 35% respectively for renters). Thus, because AS-support was not constrained by the maximum for most boarders, the average support was able to increase steadily as accommodation costs increased over time.

Panel (d) shows steady increases in the average accommodation costs faced by boarders over the period. Average costs on the control sides increased nearly $60 per week (nearly 40%) from $165 in 2010 to $230 in 2021. Average costs on the treatment sides were about $10 per week lower on average, with the difference increasing a little between 2010 and 2015. After this it appears to be roughly stable, although quite noisy after the policy change in April 2018. Also, although the average board payments are much lower than average rent, the relative increase over the period is broadly similar.

The low rates of boarders receiving the AS-maximum allowance implies that the 2018 policy changes should have had relatively little direct effect for boarders. To test this hypothesis, we estimated the same set of regressions as for renters in Table 3 and summarised the results from this in the first three columns in Table A6. The three specifications correspond to those in the same labelled columns of Table 3: i.e. the first regression column "(2)" is essentially the raw difference-in-difference regression with year-specific effects, controlling for the 'merged', 'split',

and 'miscellaneous' boundary types; the column "(3)" regression includes controls for each boundary and observable characteristics of AS-clients; and column "(4)" includes AS-client fixed effects to control for fixed unobserved differences of clients. Panel (a) presents the results for boarders' accommodation costs, and panel (b) for their total accommodation support (AS+TAS) payments.

In line with the no policy-effect hypothesis, the results in both panels provide no evidence of any significant differences in either accommodation cost or accommodation support differences between the treatment and control sides of the boundaries. In fact, the post-policy treatment estimates are consistently negative, but statistically insignificant. From this we conclude, as expected, that the 2018 area policy-changes had no noticeable effects on either the costs or support payments of boarders in the boundary areas.

5.4.2 Accommodation Supplement Homeowner recipients

We next focus on AS-homeowner recipients. Homeowner AS-recipients are less likely to be single adults (43%) than are renters, and more likely to be couples with or without children (about one-third compared to one-sixth of renters). They are also more likely to be European (three-quarters versus two-thirds), slightly more female (66% compared to 63% of renters), and older (average age of 54 years compared to 46 for renters).

Figure 8 presents monthly trends in the same set of outcomes for homeowners as discussed for boarders and renters. These patterns are much more similar to those of renters than boarders, both in terms of the levels and the trends of the outcomes. For example, the average AS-receipt (panel (a)) was roughly constant over the period before the policy change: $65-70 per week for those on the control sides, and about $90 per week for those on the treatment sides of boundaries. There are roughly $20 (control) and $30 (treatment) step increases at the April 2018 policy change date, followed by some gradual increase in payments until the end of observation period in May 2021. The total accommodation supplement trends in panel (b) are similar, although about $15-20 higher, reflecting the effects of additional TAS payments. There are also noticeable increases in AS and total accommodation support payments associated with the COVID-19 lockdown in April 2020, which is immediately after the end of our main analysis period.

Panel (c) presents the trends in the fractions of homeowners on either side of the boundaries receiving the maximum AS-payments. Similar to the trends observed for renters in Figure 3, this shows trend increases of about 10 percentage points over the period before April 2018. About 45% of homeowners in the control areas and 55% in the treatment areas are at the maximum in March 2018. There are large drops in the fraction following the policy change

(nearly 30pp in the treatment areas and about 15pp in the control areas), before the fractions again trend upwards until the end of the observation period.

Trends in average homeownership payments are shown in panel (d) for AS-recipients in the treatment and control boundary areas. These suggest average costs were roughly constant from 2010 until 2014, before increasing (particularly for those in the control areas) until about March 2018, followed by a sharper increase after that (especially for those in the treatment areas). The difference trend shows diverging average payments between those in the treatment and control areas from 2014 to 2018, followed by convergence after the policy change in 2018.

The patterns of support, in particular the higher rates of homeowners receiving the AS-maximum allowance and the large relative difference in the changes in April 2018, suggest there was a significant treatment associated with the 2018 policy changes. The final three columns in Table A6 summarise regression results from analogous specifications to those discussed for boarders. The results for specifications in columns "(2)" and "(3)" in panel (a) suggest large policy effects on homeowner costs, and about as large as the increase in support estimated in panel (b). In fact, the raw estimates in column "(2)" $20-23 per week are larger than the estimated increase in support of $17-19. While regression-adjusted estimates in specification (3) are somewhat moderated, they remain relatively large. In addition, the estimates for the year before the change are also positive ($5.80-7.10 per week), and statistically significant in specification (3). However, once we control for client fixed effects in specification (4), the estimated effects on costs disappear (in fact, the point estimates are negative), while the estimated treatment in terms of total support is about $13 per week and significant.

We are not sure quite what to make of these results. First, they imply there were significant relative composition changes in recipients on either side of the boundaries that can explain the raw estimated increases in costs of homeowners in the treatment relative to the control areas. This is similar to the patterns for renters discussed earlier. Second, the estimated relative increase in treatment area costs in the year prior to the policy change seen in columns "(2)" and "(3)" suggests the common trends assumption may not hold for the analysis of homeowners. This is supported by the patterns observed in panel (d) of Figure 8, which suggest the relative trends for homeowners on either side of the boundaries are less stable; in which case, the estimated effects would be contaminated by spurious differences. Third, we wonder if the estimated effects may reflect something about the reporting of homeowner costs by recipients when applying for AS over time. For example, recipients only have an incentive to report costs up to the level at which support is constrained by the maximum AS-payments. Given this, perhaps homeowners have more discretion in reporting their accommodation costs than do

renters (and boarders): if so, those receiving the maximum support may legitimately report additional costs when this constraint was relaxed by the policy changes.

5.5 Contributions to recipients' income support

We now consider the effect of the 2018 area changes on the income support of recipients. Our analysis of this is limited because we only observe AS-recipients' income reported in the AS-related tables. In particular, this excludes any tax credit related income, and includes only the benefit income of beneficiaries (no earned income is recorded for beneficiaries, as their AS does not abate until their benefit has fully abated). As a result, the income measured will tend to understate recipients' true incomes and thus overstate any relative increase in income associated with the AS changes.

Table 12 summarises the contributions of the changes for the full population of AS-recipients, and various subgroups analysed above. The first two columns show the average income of those on the 'treatment' sides of the AS boundary areas over the two years before the policy change, and the regression-adjusted estimated policy-increase in total accommodation support by 2019/20 controlling for observable characteristics (i.e. based on specification (3) in Table 3). The next two columns show the corresponding average pre-policy rent and estimated 2019/20 rent increase. Because the estimated rent increases shown here are generally larger than when we also control for unobserved client fixed effects, this exercise can be viewed as giving a relatively conservative view of the relative contribution of the policy change to recipients' net income (after housing). The final column reports the ratio of the estimated rent change to accommodation support increase (i.e. column (4) / column (2)), which is an estimate of the marginal propensity to spend on housing out of income associated with the policy changes.

The full-population effects, reported in the first row, imply that the estimated effect of the policy on recipient incomes (i.e. the increase in total accommodation support) was comparatively modest, contributing an estimated 3.1% increase on average. However, the policy effect on rent was even lower, with an estimated 1.5% relative increase in rents. More importantly for this analysis, the final column shows that, on average, recipients spent 23% of the increase in income on rent. Put another way, recipients' net (of accommodation costs) income increased by 77% of the increase in accommodation support.

The following rows in Table 12 report the corresponding estimates for different subgroups of the population. The results show variation in the extent of support (e.g. the relative income increase ranges from 1.6% for over-65 recipients to 4.1% for Pacific peoples recipients); the

effects on rent (the relative rent effects range from marginally negative for single adults, couples, and those over-65 to 5.5% for 16-24 year olds); and the implied MPS (from -10% for couples and over-65s to 77% for 16-24 year olds). Although the MPS is quite strongly correlated with the relative increase in income (correlation coefficient=0.68), this suggests that even population groups that experienced larger rent increases also had relatively large net income gains from the policy changes.

6 The composition of AS effects

In the estimates presented above, we found that the AS treatment effect was considerably reduced when we controlled for changes in the characteristics of AS recipients. These were generally washed out when client fixed effects were included in the regressions. This implies that the relative rent growth in treatment areas compared with control areas in large part reflects changing composition of who was receiving AS support. In this section, we provide a complementary analysis of compositional change, focusing on patterns of entry on to and exit from AS support.

For this section, we use an annual panel extracted from the monthly dataset described in section 4.1. For each AS-recipient, we retain one observation per year – specifically, for the 15th of March. To focus on the pattern of year-to-year transitions, we analyse a series of one-year periods, from one March to the next. The Families Package AS policy change took effect on 1 April 2018, so the March 2018 observation is immediately prior to the change, and the effects of the policy change will first be evident in the year to March 2019. We extract data for all AS recipients but also present a range of analyses restricted to boundary areas only, or to the Central Auckland boundary.

6.1 AS entry and exit

We begin by documenting changes in AS-recipiency over the period 2011–2020. For each year, we express entry and exit rates as a proportion of the average number of clients across two mid-March dates.

$$Entry\ rate = \frac{\#Entrants}{(N_0 + N_1)/2}$$

where N_0 is the number of clients the start of the year, and N_1 is the number at the end of the year. Similarly, the exit rate is a count of clients who received AS support at the start of the year but not at the end, as a proportion of the average number of clients.

Table 13 summarises the patterns for all AS renter recipients. It shows the number of AS clients declined from the year ending March 2012 to the year ending March 2018. This came before a sharp rise to 197,124 in the year to March 2020 – two years after the Families Package changes came into effect. The rise was particularly strong between 2019 and 2020. The number of clients in the 2020 year was the highest it had been since at least 2012. The entry rate – from people starting to receive AS support, dropped between 2012 and 2018, from 30% down to 24%. After the AS policy change, the entry rate rose to 27% in 2019 and 2020, consistent with AS supporting more people after the Families Package changes. The 6% net change in the number of clients in 2019 and 2020 also reflected a sharp drop in the exit rate, to 22%, as a result of fewer people ending a period of AS support. The net effect was that the number of clients who were receiving support in both March 2018 and March 2019 or in both March 2019 and March 2020 was over three-quarters of the average number of clients each period. Even with this historically high degree of retention, there is still considerable scope for changing client composition to contribute to the observed growth in rents over time, or between treatment and control boundaries.

A similar pattern of a rise in the entry rate, lower exits, and overall growth in the number of AS clients is also evident in both boundary and treatment areas, including those on the Auckland boundary.

Whether these patterns of inflow and outflow rates contributed to differential rent growth between treatment and control areas depends on the composition of flows as well as their size. If entering recipients enter with above average rents, they raise the rent growth rate in an area. Similarly, exits of relatively low-rent recipients would also contribute to average rent growth. To quantify the role of inflows and outflows to rent growth differences between treatment and control areas, we decompose, in the next section, the annual changes in average rent and average total accommodation support. We decompose these changes into contributions from entry and exit, and mobility within and between treatment and control boundary areas.

6.2 Decomposing the effect of mobility on treatment effects

We next consider the effects of inflows, outflows, and mobility of AS-recipiency on the relative change in average rents, total accommodation support, and probability of receiving a benefit, in the treatment versus the control areas. For each year, we calculate the annual change in each of these outcomes. We calculate these separately for treatment boundary areas (denoted X=T) and control boundary areas (denoted X=C) and identify contributions of various entry and exit groups to the change in the average rent, based on the following formula:

$$\Delta\bar{Y}^X = \bar{Y}_1^X - \bar{Y}_0^X = \sum_g \lambda_{g1}^X\left(\bar{Y}_{g1}^X - \bar{Y}^X\right) - \lambda_{g0}^X\left(\bar{Y}_{g0}^X - \bar{Y}^X\right) \quad (2)$$

where $\bar{Y}^X = \frac{\left(N_1^X * \bar{Y}_1^X + N_0^X * \bar{Y}_0^X\right)}{\left(N_1^X + N_0^X\right)}$ is the average across the pair of years, and $\lambda_{gt}^X = \frac{\left(N_{gt}^X\right)}{N_t^X}$ is the group share in year-t (t=0 or 1). We distinguish six distinct groups (*g*) of people who receive AS support in one or both years, so that they contribute to either the first year mean ($\bar{Y}_0^X$) for the boundary area, or the second year mean ($\bar{Y}_1^X$). First, we identify two groups that contribute to net inflows – clients who were receiving AS support in only one of the years. Second, we identify 'stayers' – clients who received AS in both years and remained in the same meshblock. Third, we identify three remaining groups that contribute to 'net transfers'. This includes clients who moved into the area, clients who moved out of the area, and the small number of clients who changed meshblock within the boundary area.

We decompose the changes in average rent, accommodation support, and benefit shares separately from treatment and control boundary areas. For each of the groups identified in the previous paragraph, we calculate their contribution to difference-in-difference treatment effects as the difference between their contribution in treatment areas and their contribution in control areas.

Table 14 summarises the pattern of contributions between March 2016 and March 2020. The table reports cumulative contributions to average changes in rents, accommodation support and benefit shares since 2016. The top half of the table shows the contributions across all boundary areas. As shown in the final 2 columns of panel (a), mean rents increased by $53.69 in treatment areas, and by $43.13 in control areas, resulting in an estimated treatment effect of $10.55, as shown in the preceding column. This change is decomposed into contributions from net AS entry, net transfers, and stayers.

Net AS entry combines the annual contributions of people who started receiving AS support in the second year of each period and those who stopped receiving AS support between the first and second year. If entry and exit were balanced (net entry=0) and entrants and exiters were randomly selected from each year's recipients, the difference between the mean rents of entrants and the mean rent of exiters would equal the change in average rents in the period. Consequently, the contribution of net entry to average rent changes would be proportional to their share of recipients (25%), as shown in the first column of the table. However, we find that net entry contributes a disproportionately large $7.64 to the $10.55 treatment effect for average rents between 2016 and 2020 (72%).

In contrast, the contribution from rent increases for year-on-year stayers accounted for almost none ($0.20 or 2%), well below their recipient share of 60%. Also, although net transfers

(inflows and outflows of people receiving AS support in both years) accounts for a relatively small proportion (15%) of the sample, the cumulative contribution to treatment effects is 26% ($2.70) over the 2016-2020 period. The net inflow of relatively high-rent new AS recipients into treatment areas and, to a lesser extent, the net transfer of relatively high-rent continuing recipients account for almost the entire treatment effect over the 2016-2020 period. The effects are most pronounced in the year in which the policy change occurred, although there is some evidence of a positive net entry effect in the prior and following years.

In contrast to the patterns for rent changes, stayers account for a sizeable proportion of the treatment effect for total accommodation support. As shown in panel (b) in the top half of Table 14, the cumulative treatment effect for average support jumped to a substantial $15.71 in the year to March 2019, when the accommodation supplement policy changes were introduced, and further to $18.86 by 2020. The contribution from net entry is roughly proportional to the group's mean share. Stayers contribute around $7.77 (41%) to the AS support treatment effect between 2016 and 2020, below their mean share of around 60% but considerably larger than their contribution to the rent effect. The contribution to treatment effects of AS recipients transferring into and out of treatment or control areas is disproportionately large ($4.95 or 26%), indicating that the net effect of client relocations was to raise average support payments more in treatment areas than in control areas. In addition, net entry also made a disproportionate contribution of $6.14 (33%) to total support.

The final panel in the upper half of Table 14 decomposes the treatment effect on the share of recipients who received a benefit. The most notable change in the year of the policy change was the negative contribution of net entry. As shown in the final two columns, net entry makes a strong positive contribution to the growth in the beneficiary share in both treatment (3.15 pp contribution) and control (3.60 pp contribution) areas. This contribution was smaller in treatment areas, indicating that the net entry of non-beneficiaries was relatively stronger in treatment areas than in control areas.

6.2.1 A closer look at the Central Auckland boundary

To further investigate patterns of mobility for new and existing clients, the lower half of Table 14 repeats the analysis in the upper half of the table but focused on the Central Auckland (21_on_11) boundary. The table provides a more disaggregated set of inflows and outflows. In particular, it disaggregates transfers into 'Cross-border' transfers and other transfers. Cross

border transfers are flows between the Treatment (outer) boundary area and inner Auckland, or between the Control (inner) Boundary area and outer Auckland.[39]

The overall treatment effect on rents on the Central Auckland boundary led to a relative rise of $8.92 between 2016 and 2020, with the cumulative effect peaking in 2019, at $10.91. Net AS entry (46%) and net flows across the merge boundary (48%) in Auckland contributed disproportionately to the rent treatment effect. In contrast, the contribution from stayers was negligible. The cross-border contribution reflects increased flows of high-rent recipients moving from inner-Auckland to the outer (treatment) boundary and reduced flows of high-rent recipients from outer Auckland into the inner (control) boundary. This is consistent with a prior sorting of high-rent recipient households into inner Auckland where they had previously been able to receive higher maximum AS payments. After the merging of the Auckland AS areas, the incentive for this sorting is reduced. Consistent with the patterns for all rental boundary areas, stayers make a considerably larger contribution to the accommodation support treatment effect (61%), roughly equal to their 64% share, reflecting the policy design which raised maximum AS support more in the treatment area.

Finally, in contrast to the 'all boundary' patterns, net entry in Auckland contributed to a stronger rise in benefit proportion in the treatment area than in the control area. Net transfers, however, contributed to a decline in the benefit proportion in the treatment area, and a rise in the control area, and accounted for a high proportion of the overall negative treatment effect.

6.2.2 Rent and AS changes, for clients who changed location, family size, and benefit type

The previous section has clearly shown that entry of high-rent and high-support AS clients disproportionately raised the average level of rents and accommodation support in treatment areas. In this section, we examine the possible role of other changes experienced by clients who remained on AS in consecutive years. We analyse balanced subpanels (clients present in both years) of clients in boundary areas, classified as either control or treatment clients according to where they lived in the initial year of each period. We record whether they changed meshblock location between the initial and subsequent year ("movers"), whether their family size changed, or whether they changed the benefit they were on (including movements to and from non-beneficiary status). We estimate a stratified difference-in-difference regression, which is an extension of equation (1).

[39] There is a small number of clients observed transferring between treatment boundary areas and contiguous control boundary areas, but these are, in most cases, too small to report, so have been omitted from the table.

$$Y_i = \beta_0 + \beta_1 A_i^T + \beta_2 Post_i + \beta_3 A_i^T * Post_i + \sum_c (c = 1) * (\beta_0^c + \beta_1^c A_i^T + \beta_2^c Post_i + \beta_3^c A_i^T * Post_i) + \epsilon_i \quad (3)$$

In this extended specification, *c* identifies each of three different changes the clients may have experienced (relocation, family change, or benefit change) and the β_3^c coefficients reported in Table 15 capture the treatment effect for clients experiencing each type of change. The coefficients show the difference in the growth of rent or accommodation assistance in treatment areas relative to changes in the control areas. We do not include the set of controls ($X_i'\gamma$) that are shown in equation (1) as they would absorb the effect of the changes that we wish to investigate in the current section. Recipients who made any of the three types of moves (location, family, benefit) generally had higher average rents and support before the change compared with non-movers. Location changes were generally associated with rent and support increases.

The first row of Table 15 presents a simple baseline difference-in-difference specification for each year (constraining β_3^c=0 for all c), which highlights whether these patterns differed across treatment and control boundaries. It shows a significant positive treatment effect ($14.45) for the amount of accommodation support received in 2019, which coincides with the policy changes. In contrast, there is not a significant treatment effect on rents in any year. The estimated rent treatment effect in 2019 is a statistically insignificant $0.56. Given the results in Table 14, we expect the difference from our main estimates in Table 3 reflects the fact that by focusing on balanced subpanels in the current section, we have removed the influence of net entry.

In the second (Stratified DiD) panel of the table, the main treatment effect captures the treatment effect for clients who did not change location, family size, or benefit. This main effect shows a pattern similar to the simple effect. The most significant estimate is again on the 2019-year accommodation support changes ($12.18). Coefficients on the other types of change (location, family, or benefit receipt) are statistically insignificant or weakly significant. There is some indication that treatment area recipients who move have smaller increases in support than movers from control areas in the final 2 years, reflecting the policy change that raised support more in treatment areas.

The analysis presented here is consistent with that in section 5, and finds little evidence that the AS policy changes resulted in relatively higher rents in the more-affected areas. We conclude that the raw increases in average rents are largely driven by compositional changes in the samples associated with net entry of higher-rent new AS-recipients into 'treatment' boundary areas.

6.3 Heterogeneous effects across initial rent ranges

Finally, despite the absence of any policy effects on average rents, in this section we consider whether there were any relative behavioural differences for recipients at different points of the rent distribution. In particular, we consider how recipients' rent and accommodation support change depending on whether they were not directly affected (i.e. their initial rent was below the implied pre-change AS-max level), or directly affected with initial rent between the pre-change and post-change AS-max levels or above the post-change AS-max level.

The changes in AS maximum payments had different effects for clients on different pre-change levels of rent. The non-linearity of the effects is illustrated in Figure 9, for the case of a three-person household in AS Area 1. First, for clients with low initial rents (in range 'A'), the policy change did not affect the amount of AS support they received or the degree of subsidy they faced for increased rental expenditure.[40] Second, for clients initially paying rents between the pre-change maximum and the raised maximum (range 'B'), they received not only an increase in their AS entitlement, but also an increase in the subsidy that they received for additional rental expenditure – they would pay only 30% of the extra rental costs, up to the rent level at which the new AS maximum applied. In range B, they therefore experienced both an income effect (additional income without changing their rental expenditure) as well as a substitution effect (the cost of increased housing expenditure was reduced by the subsidy). The substitution effect provides an incentive to increase housing expenditure relative to other forms of expenditure, whereas the income effect enables recipients to spend more on all forms of expenditure, including housing. Third, for recipients with initial rents above the post-change maximum threshold (range 'C'), clients received a pure income effect. They are able to increase all forms of expenditure due to their higher incomes, but they still face the full cost of any additional housing expenditure.

In this section, we report annual changes in rents paid and in accommodation support received, for each of these three rent ranges, for four March-to-March years. We also examine the patterns of changes in rents and in accommodation support around the boundaries between the three rent ranges. By focusing on these 'boundary-kinks', we aim to identify whether the mix of income and substitution effects differentially affected the behaviour of clients on either side of the kinks. We also examine whether the rents that clients pay are clustered around the level where the AS maximum applies – i.e. where they no longer receive a 70% subsidy for additional housing expenditure.

[40] For this analysis, we ignore the minor increases associated with annual CPI-adjustments to the entry thresholds.

6.3.1 Rent and accommodation support changes, by initial level of rent

Table 16 shows the rent and accommodation support changes in each period for all AS-rent recipients, by the initial rent range. In the two pre-policy change years, there is clearly a greater increase in rents for those on relatively low rents ($16.49 and $17.24 for range A in the 2017 and 2018 years respectively) than for those on higher rents ($6.11 and $7.13 for range C). This general pattern of mean reversion is evident in all periods. The amount of accommodation support received follows a similar pattern, reflecting the rent changes, though the magnitudes are more muted. Even before the policy change, low-rent clients had average increases in support of $5.17 and $6.14, whereas high-rent clients on average received around $2 less in support.

When the policy changes took effect in the year to March 2019, the effect of the policy on support is clearly evident. Clients with rents in the lower range (A) received an extra $10.31 in support on average, while those in the intermediate range (B) received $26.11 more, and those with higher rents (range C) $44.66 more. These increases are each noticeably higher than the increases in the previous two years, especially for range (B) and (C) recipients.[41] The relatively higher average rent increases in 2018/19, compared to the earlier years, for recipients in range (A) and (B) is consistent with predictions of a policy increase on rent, but the average rent increase for those in range (C) is smaller in 2018/19 than the previous years, so not in line with such predictions.[42] Presumably the effects of mean reversion in rents outweighed any income effects that would be expected to enable higher rental (and other) expenditure. The accommodation support changes across the rent ranges in the period two years after the policy change (2020) were small, and similar to the pre-change 2018 changes.

There is no evidence of strong income or substitution effects in 2019 or 2020. The final year rent changes still appear to be dominated by mean reversion – similar to the pre-policy-change period, but with smaller rent increases for range B, which is the range for which the substitution effect would have been expected to encourage greater housing expenditure.

6.3.2 Discontinuity analysis at kink points

To provide a more focused analysis of the differential effects of the policy change across different rent ranges, we analyse whether clients are initially concentrated around the policy kink points, as shown in Figure 9, and whether there is a discontinuous jump in the size of the subsequent rent change at the points where the kinks occur. By focusing on local variation at the

[41] Although range (A) recipients were not treated at their initial rent levels, the 'mean-reverting' increases they on average experience will move some of them into the treated range (B) or (C). The larger increase in support in the 2018/19 year will reflect the policy increase for these recipients.

[42] That is, while the $19.85 2018/19 average increase in range (A) was $2.60 higher, and the $11.42 range (B) increase was $8.16 higher, than in 2017/18, the $2.87 average rent increase in range (C) was $4.26 lower than in 2017/18.

levels of rent associated with the kinks, we abstract from variation that occurred within the ranges, and control for the overall pattern of mean reversion.

We investigate possible discontinuities at two different kink points. The first (K1 in Figure 9) is at the point where AS support reached its maximum prior to the 2018 policy changes. To the left of this point (range A in Figure 9), recipients would have faced no change in the level of support they received before and after the policy changes. To the right of this point (range B in Figure 9), they received not only an increase in the level of assistance, but also faced a lower marginal cost of housing as a result of the extended range of the 70% AS subsidy. The second 'post-change' kink (K2 in Figure 9), is at the point where the post-2018 AS maximum was reached. To the left of this point (range B in Figure 9), the policy change raised assistance and lowered the marginal price of housing. To the right (range C in Figure 9), recipients received additional support but no change in the marginal cost of housing.

Because the rent level associated with the kinks varies by AS Area, benefit type, family type, and year, we measure each client's level of rent relative to the kink associated with their specific circumstances. A value of 0 thus indicates the rent was that associated with the kink in AS support. The top row of panel (a) of Figure 10 uses data on all AS renter recipients in March 2018 who were still receiving AS support a year later. It shows how the change in rent varies with the level of rent paid in March 2018 (relative to the relevant kink). The fitted line is a local third-order polynomial regression line (Calonico et al., 2017). The figure also shows confidence intervals for the mean rent change at various levels of initial rent (kernel-weighted means at selected evaluation points). The left figure shows a larger change in rent to the left of K1 – for recipients who were not affected by the policy change. We interpret this pattern as reflecting mean reversion, as described above. There is no evidence of higher changes in rent to the right of K1, where recipients received not only higher assistance (income effect) but also a lowering of the marginal cost of housing (substitution effect).

The bottom row of graphs in panel (a) of Figure 10 shows rent change in the year to March 2020, aligned to the pre-policy change kink (K1) on the left or the post-policy change kink (K2) on the right. Puzzlingly, there is a slightly higher rent change to the left of pre-policy kink in the year to March 2020 – two years after the change, despite recipients with rents in that range being unaffected by the policy change.

The top right graph in panel (a) of Figure 10 shows the pattern of rent changes around the post-change kink (K2). Rent changes are insignificantly higher for recipients immediately to the left of the kink, who faced both an income and a substitution effect (range B) than for recipients immediately to the right of the kink who faced an increase in support but no substitution effect

(range C). Such a pattern would be expected if recipients were increasing their housing expenditure to the point where housing costs are no longer subsidised by the Accommodation Supplement, although the size of the discontinuity is relatively small (-$2.70) as well as statistically insignificant. Even two years after the policy change, a discontinuity is not evident, as shown in the bottom right graph in panel (a).

Panel (b) of Figure 10 shows the density of AS recipients according to the level of their rent relative to the pre-change and post-change policy kinks. The first row shows the density prior to the policy change and the second row shows the density in March 2019, roughly a year after the policy change. The graphs provide indirect evidence of how the AS policy might affect housing choices. The density estimation is based on a 3rd degree local polynomial, allowing for a discontinuity at the kink point (Cattaneo et al., 2018). The top-left graph shows a hollowing out of the density around the pre-policy change kink (K1), and a heightened density of recipients at rent levels above the point where their housing expenditure is no longer subsidised. The level at which this kink occurred had been relatively stable since 2005, so the density pattern could reflect a gradual shifting of density to just above the kink, as recipients found the most housing they could afford at subsidised rates. The density pattern is persistent over time – the concentration of recipients at rent levels above the pre-policy change kink is evident one year after the policy change (bottom left graph), when that kink had no particular policy relevance.

The right-hand graphs in panel (b) Figure 10 show the density around the post-change kink (K2). Although the level of rent associated with the post-change kink had no special significance before the change, there happened to be a slightly higher density just above that point. Following the policy change, the further shifting of density to above the post-change kink (K2) is evident in the lower right-hand graph. The increase in density above K2 is even greater in March 2020 (not shown).

Figure 11 and Figure 12 present analogous evidence on discontinuities in rent changes and in rent densities at policy kink points, but for boundary treatment and control areas. The overall lack of clear patterns of differential rent change at kink point that was observed in Figure 10 is also reflected in Figure 11, for both treatment and control areas. Admittedly, the smaller number of observations in treatment and control boundary areas compared with the overall renter sample does lessen our ability to detect any discontinuities. Similarly, the analysis of density discontinuities at kink points in treatment and control areas, shown in Figure 12, is unable to identify any significant patterns.[43]

[43] Appendix Figure A2 and Figure A3 provide versions of Figure 11 and Figure 12 restricted to the Central Auckland boundary ('21_on_11'). The smaller samples lead to even more imprecise estimates. However, in contrast to the other

7 Concluding discussion

7.1 Summary of results

This paper has analysed the impact of the 2018 area and maxima changes to Accommodation Supplement policy as part of the Families Package on rent. Our analysis has focused on boundary areas that received different policy changes on either side, comparing the relative changes in weekly rents paid, and accommodation support received, by AS-recipients on either side of these boundaries. We characterise the sides that received the larger increase in AS-maxima as the 'treatment' areas, and the other sides as the 'control' areas.

First, our raw estimates are that the policy changes led to $16.50 higher weekly accommodation assistance on average for recipients on the treatment sides of the boundaries in the first year (2018/19), and a cumulative $18.60 by the second year (2019/20); while the corresponding estimated changes in average weekly rents were $6.60 and $8.90. Taken at face value, these estimates suggest about 40-50% of the increase in accommodation assistance passed through to rents on average. However, the relative changes in average rents reflect differential changes in the characteristics of recipients receiving support on either side of the boundaries.

Second, controlling for observable characteristics associated with AS-claims and boundary locations, the estimated accommodation assistance effects are similar ($15.30 and $16.90). However the estimated rent effects fall by more than half, to $2.40 and $3.90 in the first and second years respectively. These estimates suggest the relative rent increases account for only 15-25% of the increased accommodation support. Furthermore, when we also control for constant unobserved client effects, the resulting estimated rent effects are small and statistically insignificant ($1.10 and $1.30), while the estimated assistance effects remain sizeable ($13.70 and $13.80). Based on these fixed effects estimates, the relative rent increases were only 9% of the relative change in accommodation support. They also imply that over 90% of the increase in assistance was captured by AS recipients in terms of after-rent support.

This pattern of results is broadly consistent over most area, socio-demographic, family-type, and benefit-status subgroups. The results are also robust to several robustness tests, including: using an extended sample period (2010-21); using tenancy bond data to analyse wider rent market effects of the policy (which shows even weaker effects); and considering placebo treatments around pseudo-boundaries.

density results presented, there is a heightened density concentration above the post-change kink (K2) in both treatment and control boundary areas. This concentration is larger in the year after the policy change than before, and even greater in the second year after (not shown).

Third, the results imply there were relative changes in the composition of AS-recipients on either side of the boundaries that were correlated with the treatment status. This conclusion is supported by analysis of a more balanced sample of recipients over the four year period, which shows similar increases in accommodation support but only small increases in raw rent ($2.90-$3.00 per week) and negligible effects when controls are added.

To further understand the source of this compositional change, we analysed the contributions to the relative growth in treatment-side average rent from annual inflows and outflows of AS-recipients, and continuing recipients who move and stay (within a meshblock). This analysis shows that new AS-recipients into treatment-areas account for the bulk of the relative increase in rents: almost 85% of the first year change, and 65% of the change by the second year. That is, new recipients with higher average rents selectively moved into the treatment side of boundaries, but it is not clear why this happened.

We conclude there was little evidence of a policy treatment effect on rents in these boundary areas, recognising that the policy changes directly affected only AS-recipients who were previously constrained by the AS-maxima. However, we also examined whether there was any evidence of behavioural changes across the rent range. To do this, we compared changes for (unaffected) recipients below the pre-change implied rent maxima, with those (affected) between the pre- and post-change maxima, and those above the post-change maxima. As expected, we find that the assistance increases were monotonically increasing across these three groups. In contrast, we find relatively smaller rent increases in the second (and third) group than the first group after 2018 than before 2018. Regression kink analysis of effects around the two kink-points in the rent range between these groups also shows only weak evidence of the predicted stronger rent increases for those in the second group near either the lower or the upper kink point.

7.2 Interpretation and implications

The main implication of our results is that the increased accommodation support associated with the 2018 AS-area policy changes did not lead to substantial strong landlord capture via an increase in rents charged to AS-recipient tenants. In the worst case, the raw changes suggest the average rent increase was at most half of the average increase in support. However, as discussed above, the vast majority of this effect is attributable to changes in the composition of AS-recipients over the period that are correlated with the policy changes. Controlling for such changing composition, we conclude the policy effect on rent increases was negligible. In

particular, we conclude there was no evidence of any effect on the rent of recipients who didn't move location.

Furthermore, the theoretical considerations in section 2.3 provide little reason to expect strong rent effects associated with landlord capture. That is, given the demand for housing by low-income families appears to be quite inelastic, for strong rent increases and landlord capture to occur would require the supply of rental housing to be extremely inelastic (i.e. almost in fixed supply). In fact, the design of AS policy acts to lessen the incidence of increasing support on rent increases.

Given the absence of both theoretical and empirical evidence for landlord capture, it is perhaps worth considering where the perception and fear of landlord capture comes from. Although we don't have a clear or strong view on this, one possibility is that it is due to confounding effects of secular increases in rents over the recent past. For example, the trends in average weekly rents for our sample in Figure 3 shows that nominal rents for AS-recipients have been increasing steadily at around 3% per year since 2010 (and, in fact, much longer), and at a slightly higher rate of 3-4% ($10-15 increase per year) since 2015; and Figure 5 shows that wider market rents have been increasing even faster. As a result, tenants and market commentators perhaps conflate secular increases in rent as due to increasing support during periods around significant policy changes.

We also find no strong behavioural responses to the differential financial incentives of the AS policy on recipients according to whether their support was constrained by the pre-change maxima. That is, we found no consistent evidence of stronger rent increases for those who were constrained and thus directly affected ('treated') by the policy changes, compared to those were not constrained and thus unaffected by the changes. Given the absence of noticeable increases in rent attributable to the AS-area changes this is perhaps not surprising.

The AS-area policy changes provided a relative incentive for policy-affected high-rent tenants to locate on the treatment sides of the boundaries, and (if rents responded) for low-rent tenants to locate on the control sides. Such 'sorting' is one explanation of the changing composition of recipients across the boundaries that accounts for a substantial component of the raw policy effect on average rent. However, that such sorting appears to largely explain the raw rent increases but has little effect on the estimated accommodation sort remains a puzzle.

Another implication of our results is that the relative changes are associated with points of housing transition. That is, there are relatively strong rent increases for tenants who move, and little effect for those who stay. We are unable to identify whether these differences represent pure price-increases associated with landlord capture, housing-quality or quantity upgrading

choices on the part of tenants, or simply reflect the fact that trend increases in rents tend to take effect when tenancies change. This remains an important issue for future research.

Finally, two other puzzling issues remain from our analysis and results. First, understanding the source and motivation of the new AS-recipients following the 2018 policy changes. For example, was the increase driven by the ongoing increases in housing costs for many tenants, or by increased take-up by eligible claimants, perhaps triggered by announcements around the policy changes that increased awareness of accommodation support? Second, further analysis of reasons behind AS-recipient movements between AS-areas, to try to understand the extent to which that was a direct result of the policy changes. This is a potentially important issue for consideration in the design and implementation of policy changes.

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Appendix 1: Data description

The data used in the analysis is derived from various sources and tables in Statistics New Zealand's Integrated Data Infrastructure (IDI). The observation unit in each of the tables is an MSD client-time spell.[44] These include the following set of *ad hoc* data tables that we sourced information from are from the "clean_read_MSD" schema in the IDI:

- ASTYPEO: this contains the main AS-claim and weekly payment details, including the assessed accommodation costs (e.g. rent), the AS payment, the AS type (rent, board, or home owner support) and AS area.
- PRNTSUPO: this contains information on other supplementary support, including the assessed cost and weekly payments. We have kept only information on TAS or Special Benefit spells.
- FAMILY: this contains information on a client's family characteristics, including their marital status and (if they are partnered) their partner's identity, and the numbers of their own and their partner's children.
- CLINT: this contains the client's age, sex, ethnicity and marital status.
- INCOME: this contains the assessable non-benefit income and assets of clients, and (separately) their partners. Note that, because AS is not subject to abatement for main beneficiaries, this income information is only available for non-benefit AS recipients.
- PSRTEO: the contains main benefit and other supplementary support information for clients who are main beneficiaries, and (separately) their partners.

In addition to these *ad hoc* tables, we also sourced residential location information from the IDI main release "msd_clean" schema:

- MSD_RESIDENTIAL_LOCATION table: this contains statistical meshblock (and other geographic level) identifiers, which we use to identify which AS recipients are located near AS-area boundaries.

Based on this data structure we constructed a monthly panel data extract by *sampling* information pertaining to any client AS-spell that is ongoing on the 15th of the month, over the period between April 2010 and May 2021 (our main analysis is restricted to the four year period April 2016 – March 2020).[45] In particular, for each monthly AS-claim observation (from the ASTYPEO table), we merge information from the other tables using the AS-client's Social Welfare

[44] Any change in relevant characteristics of a client's interaction with MSD results in a new observed administrative spell.
[45] As a result, this data construction does not collect complete AS-spell information. For example, because the characteristics of an AS client can change over time, means that we are 'sampling' the available information on the 15th of the month. Also, short spells that do not cover the 15th will not be measured.

Number (SWN).[46] The resulting merge provides a 98.4% match rate of monthly AS observations to residential location information, and 98.9% match rate of AS-clients, for the 2016-2020 analysis sample.

[46] We obtain a single observation match from each of the FAMILY and CLINT tables for each AS monthly observation. The INCOME and PSRTEO tables have a relatively small number of overlapping spells, which appear to largely be the result of some spells not having been 'closed-off'. We have cleaned these up, resulting in nearly a complete match of benefit income to AS observations. Because of the incomplete coverage of the INCOME data, and only partial receipt of TAS, the match rates from these sources is, as expected, substantially lower.

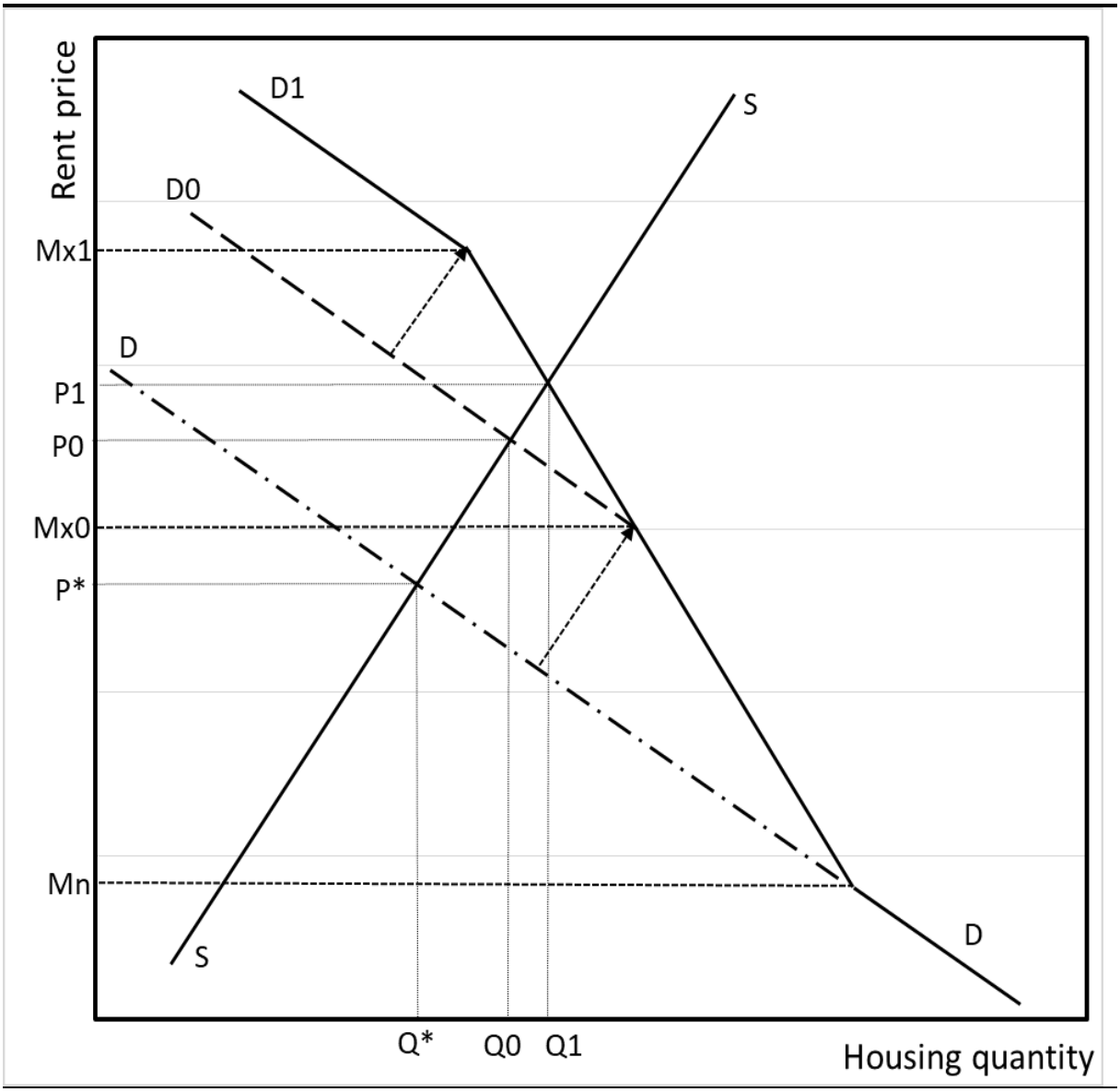
Appendix 2: Description of boundary contrasts

The main sample of boundary contrast areas consists of AS-recipients who live in a meshblock that lies within 1km of a boundary between AS areas, of which at least one side of the boundary changed AS-area designation. In addition to the main (1km) boundary contrast sample, we have constructed analogous samples based on 2km, 500m and 25m buffer distances. We use the 2km-based sample to provide robustness analysis of the importance of the actual boundary versus placebo boundaries between the 1km and 2km meshblocks on either side of the actual boundary. In this appendix, we describe in more detail the various boundary contrast areas.

In the data we focus on 11 distinct types of AS boundaries that have differing contrasts on either side. These are summarised in Table A2, and we distinguish between (1) Merged-area boundary contrasts; (2) Split-area boundary contrasts; or (3) Miscellaneous-area boundary contrasts. Merged-area boundaries are those between what were distinct AS areas before the 2018 policy changes, and became a common area following the changes – e.g. the boundary around the pre-change Auckland Area 1. Split boundaries are those between distinct areas following the changes, that were a common area before the changes – e.g. the boundary around outer Auckland, that split Area 2 into areas that moved into Area 1 and those that stayed in Area 2. We also include two miscellaneous boundaries, between areas on one side that remained in the same AS-area after the 2018 changes, and areas on the other side that changed AS-area.

Boundaries are determined by relative changes in AS maxima on either side of a boundary. Given this, the terminology we use to describe a boundary-contrast reflects the AS area of each side of the before and after the policy change. In particular, we use “ij_on_kk” to denote the boundary between an area that was rezoned from Area-i to Area-j, and an area that remained in Area-k. Furthermore, we denote “ij” side the policy “treatment” group (this side was rezoned and so received larger increases in the AS maxima), and we denote “kk” the “control” group. For example, “21_on_11” refers to the pre-change central Auckland Area 1 boundary, where “21” is the outside “treated” areas that were rezoned from Area-2 to Area-1 and “11” is the inside control areas in Area-1 before and after the change; similarly, “21_on_22” refers to the outer Auckland boundary, where the “21” side areas are treated and the “22” side areas are controls. Because the AS-maxima increased across all AS-Areas and for all family sizes, the treatments of interest are the relative treatments. Also, the level of the policy ‘treatment’ will vary across the boundary contrasts, because the policy changes involved a variety of changes in both Area designation and AS maxima.

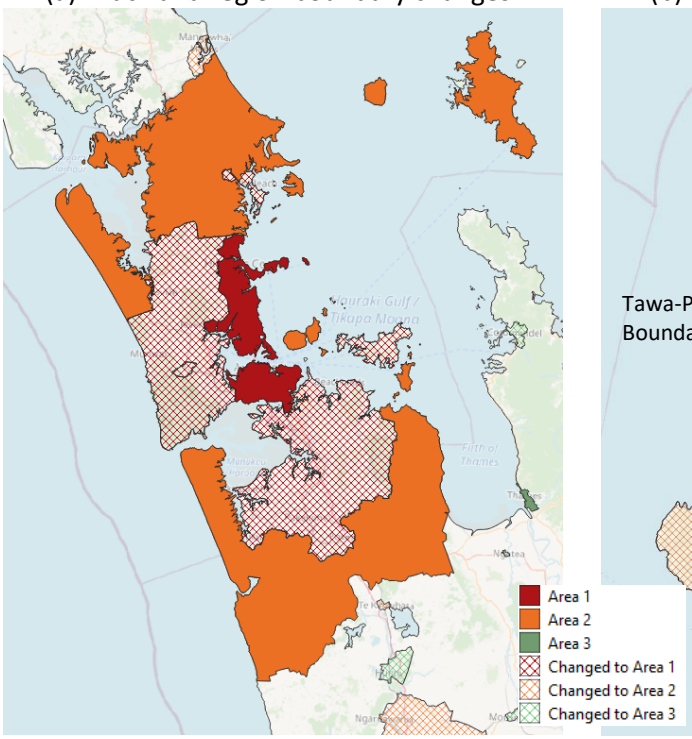
Figure 1: Depiction of an increase in Accommodation Supplement on rental housing



Source: Figure 3 in Hyslop and Rea (2019).

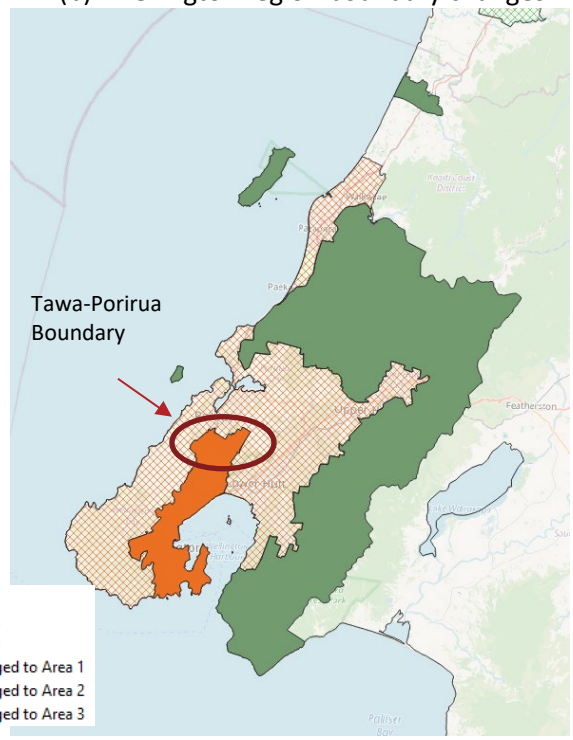
Figure 2: 2018 Auckland and Wellington region boundary areas

(a) Auckland region boundary changes



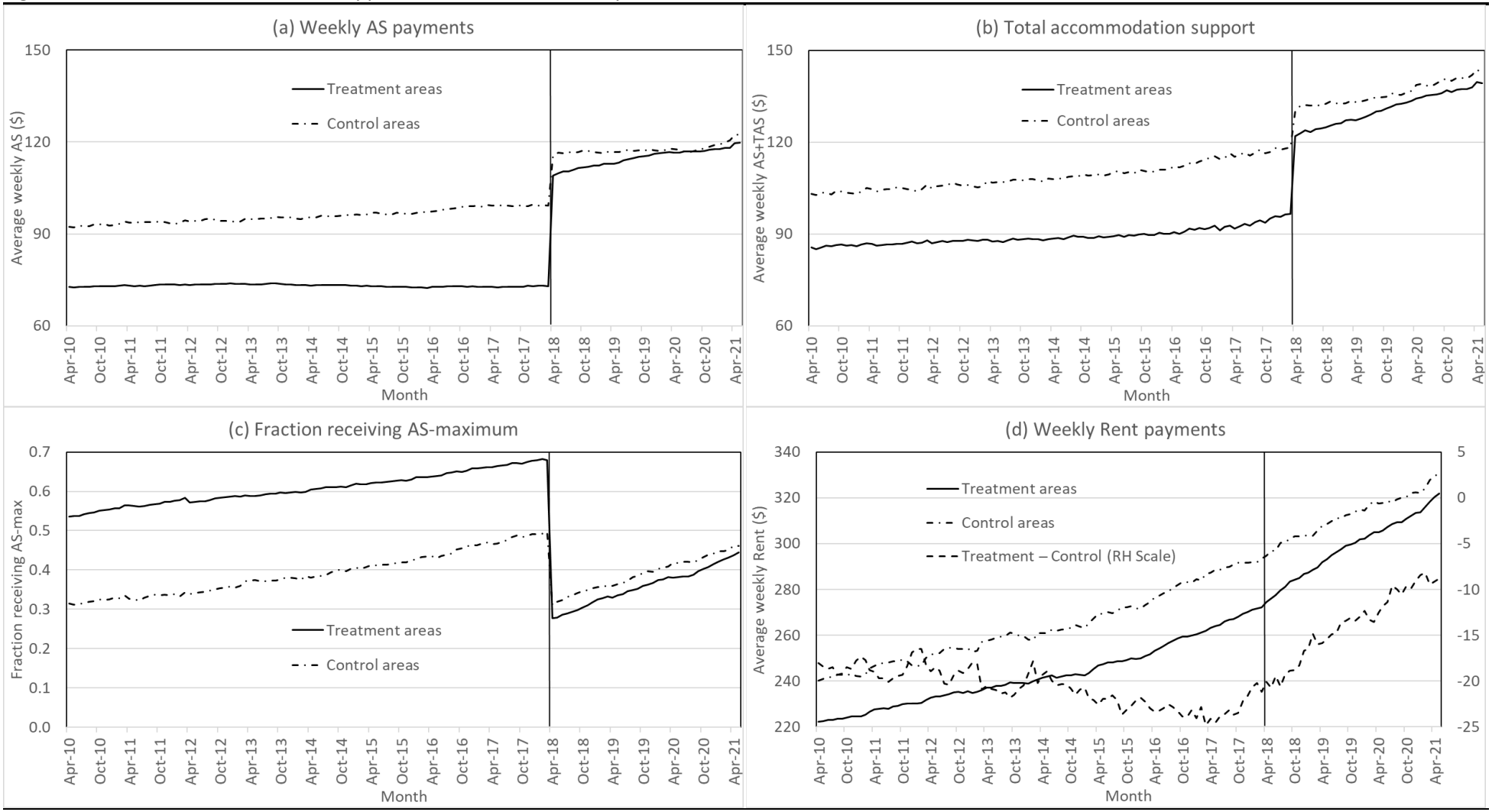
Central cross-hatched area changed from area 2 to area 1

(b) Wellington region boundary changes



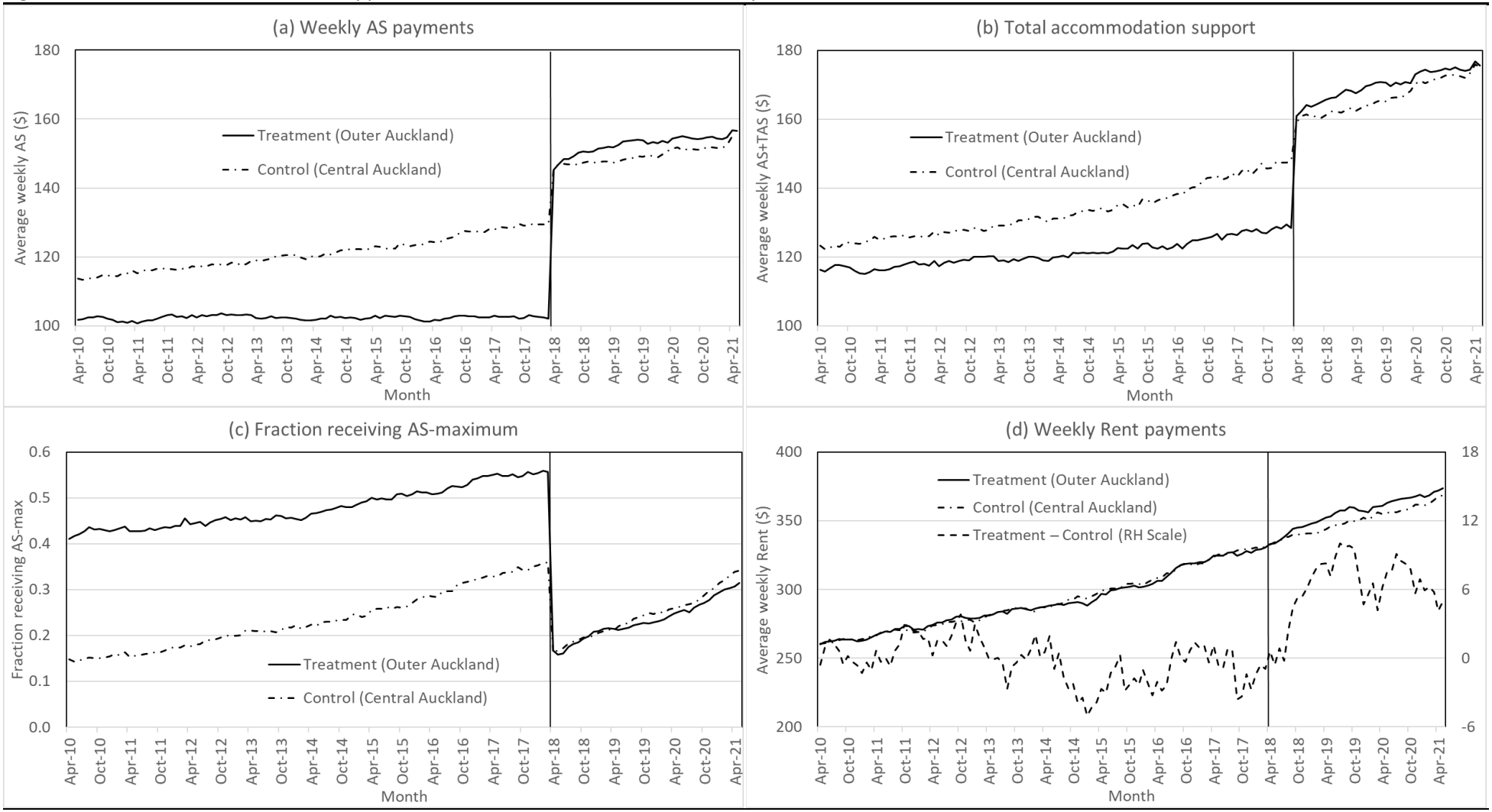
Cross-hatched area changed from area 3 to area 2

Figure 3: Trends in accommodation support and rent – all boundary areas



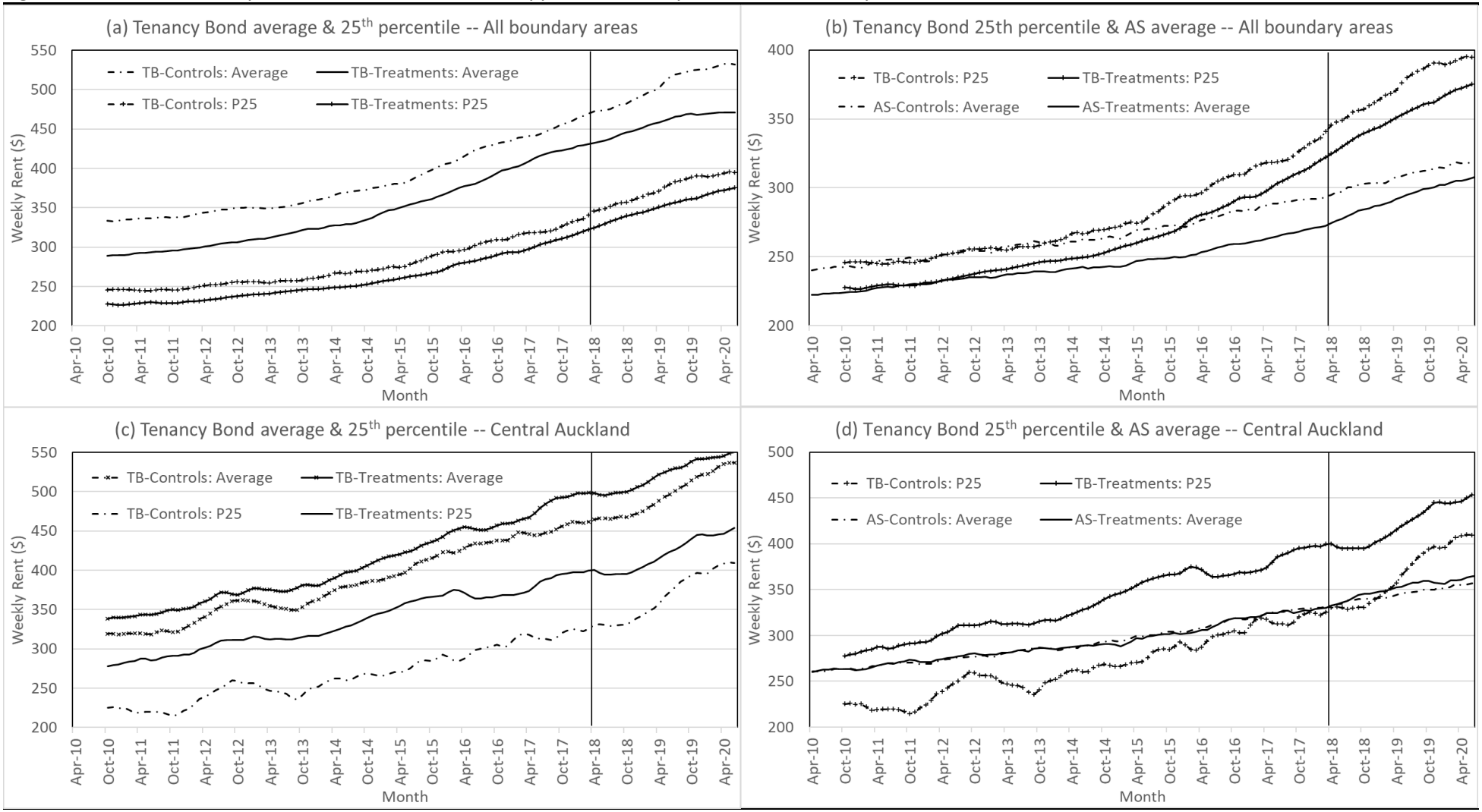
Notes: The trends are based on all AS-rent clients that resided in the geographic boundary areas over the period April 2010 – May 2021.

Figure 4: Trends in accommodation support and rent – Central-Auckland boundary



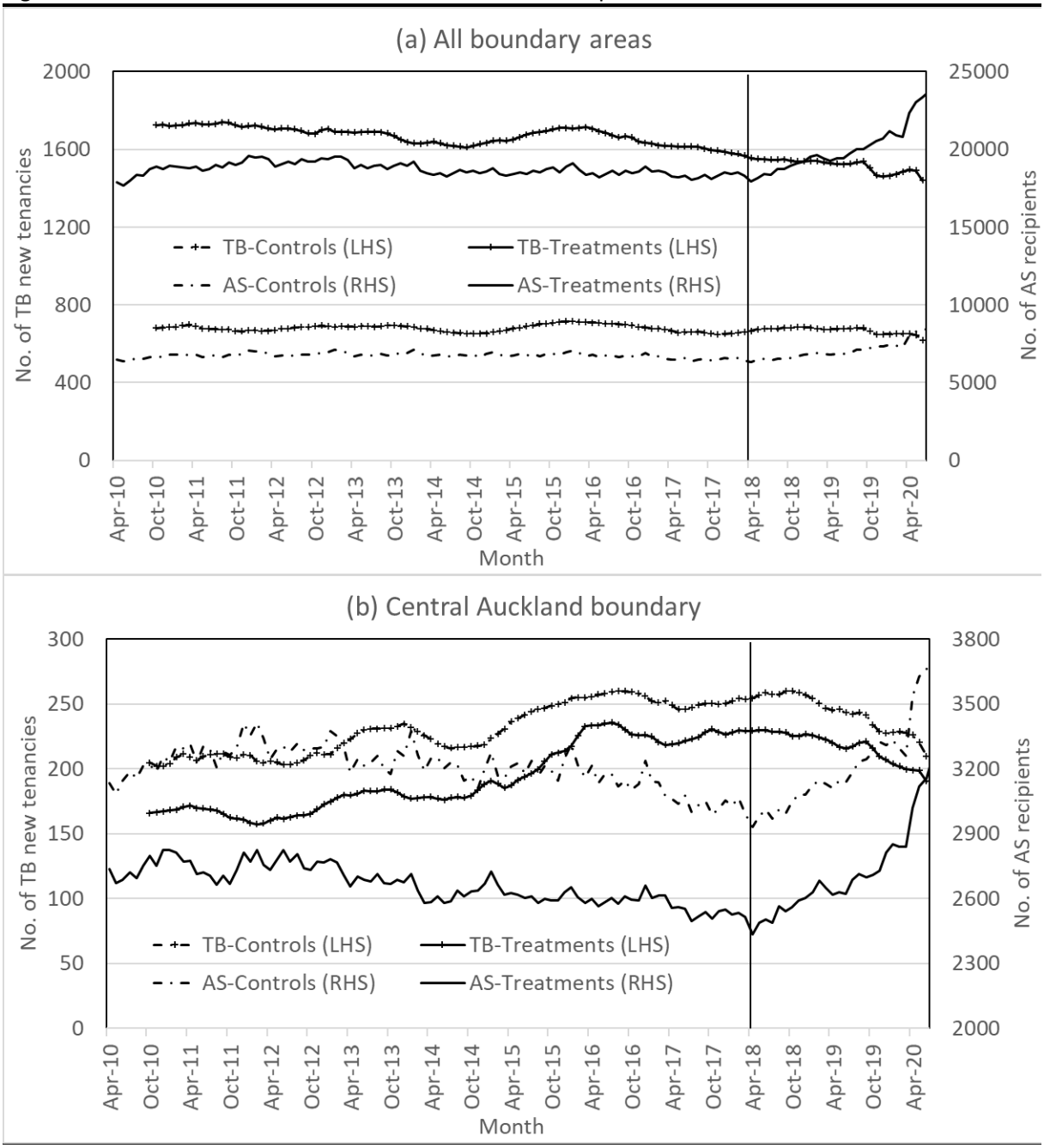
Notes: The trends are based on AS-clients that resided in geographic areas near the Central Auckland boundary (i.e. 21_on_11).

Figure 5: Trends in Tenancy Bond and Accommodation Supplement weekly rents – All boundary areas and Central Auckland



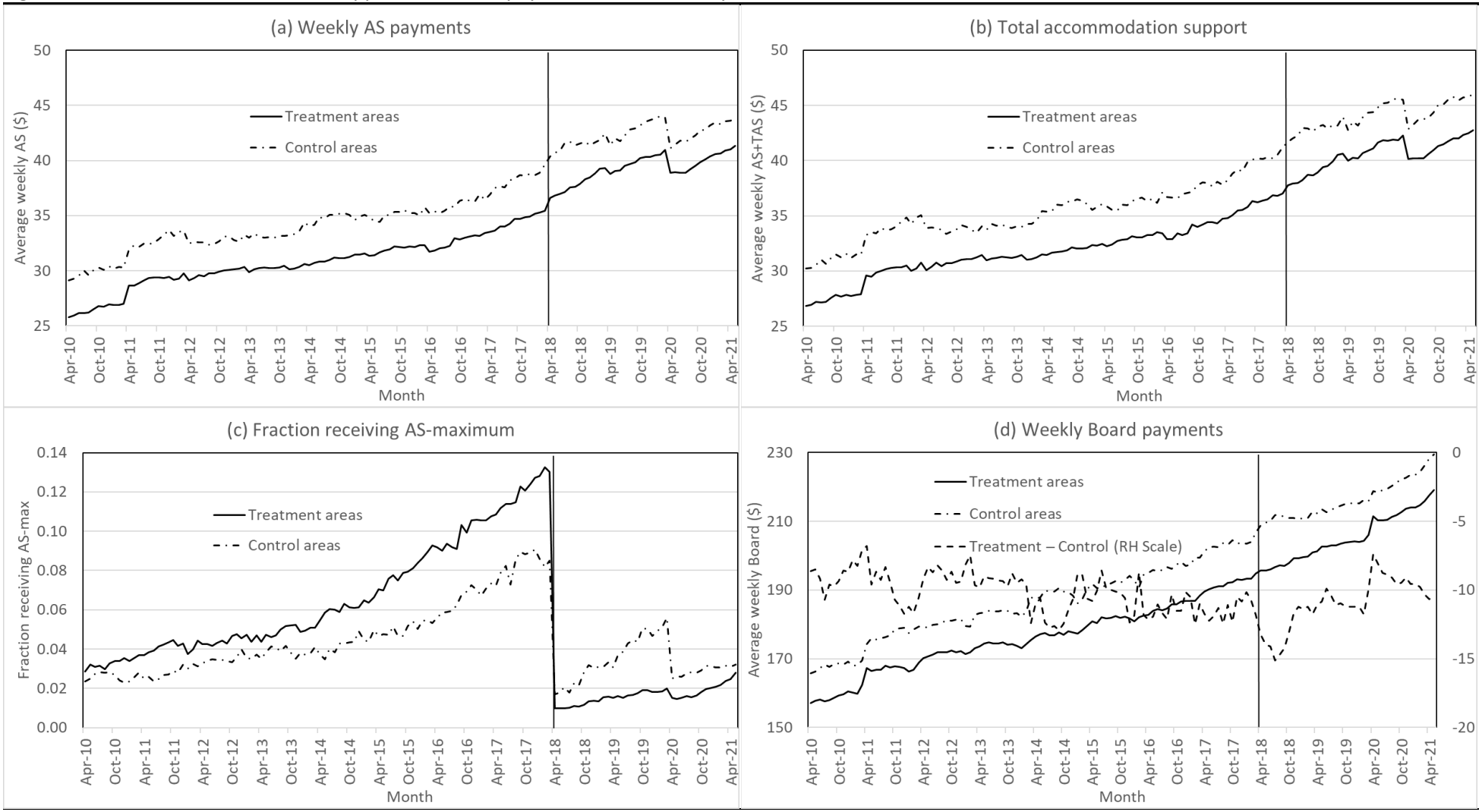
Notes: The Tenancy Bond trends are based on all new tenancies in the geographic boundary areas over the sample period.

Figure 6: Trends in numbers of new-tenancies and AS-recipients



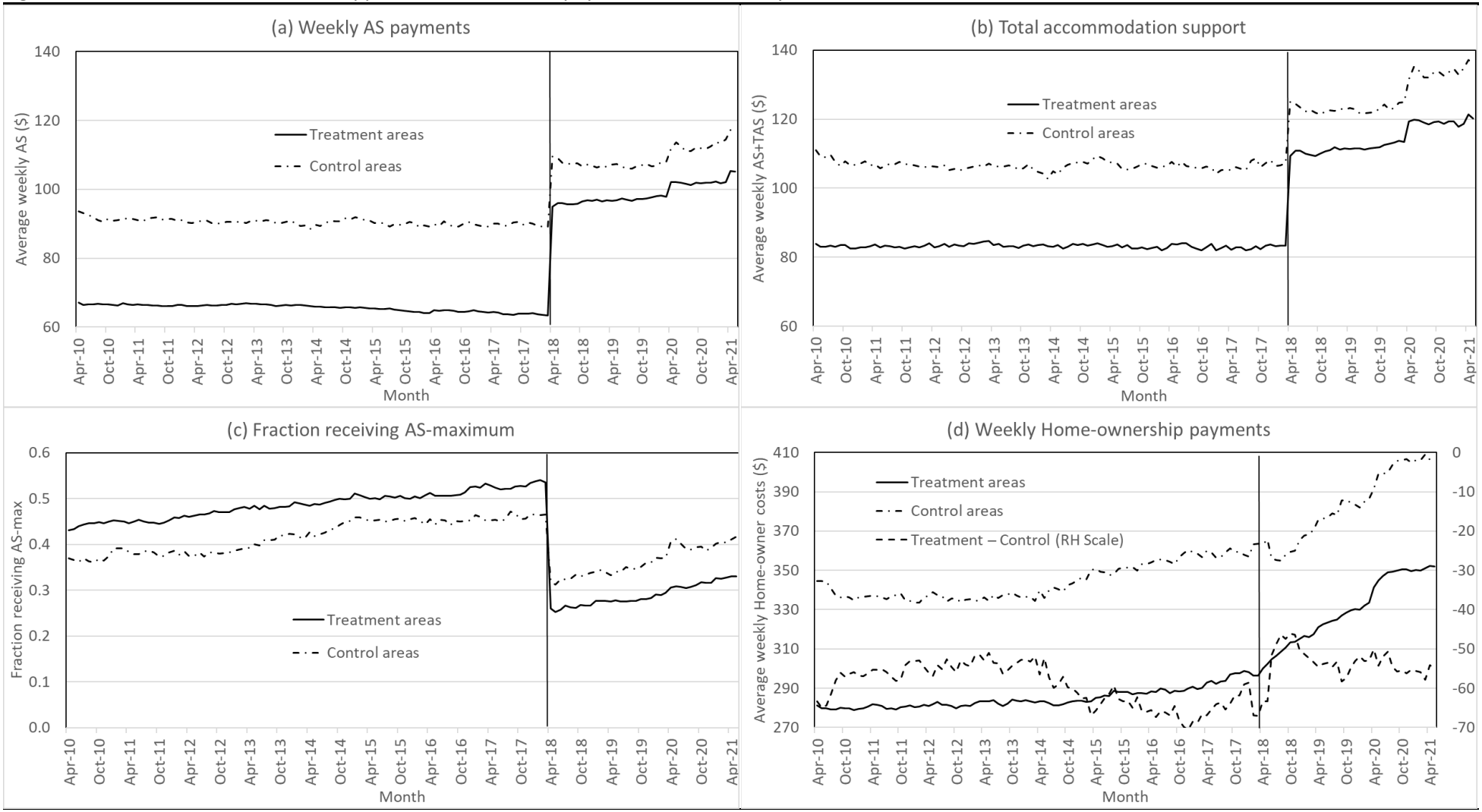
Notes: The Tenancy Bond trends are based on all new tenancies in the geographic boundary areas over the sample period.

Figure 7: Trends in accommodation support and board payments – all boundary areas



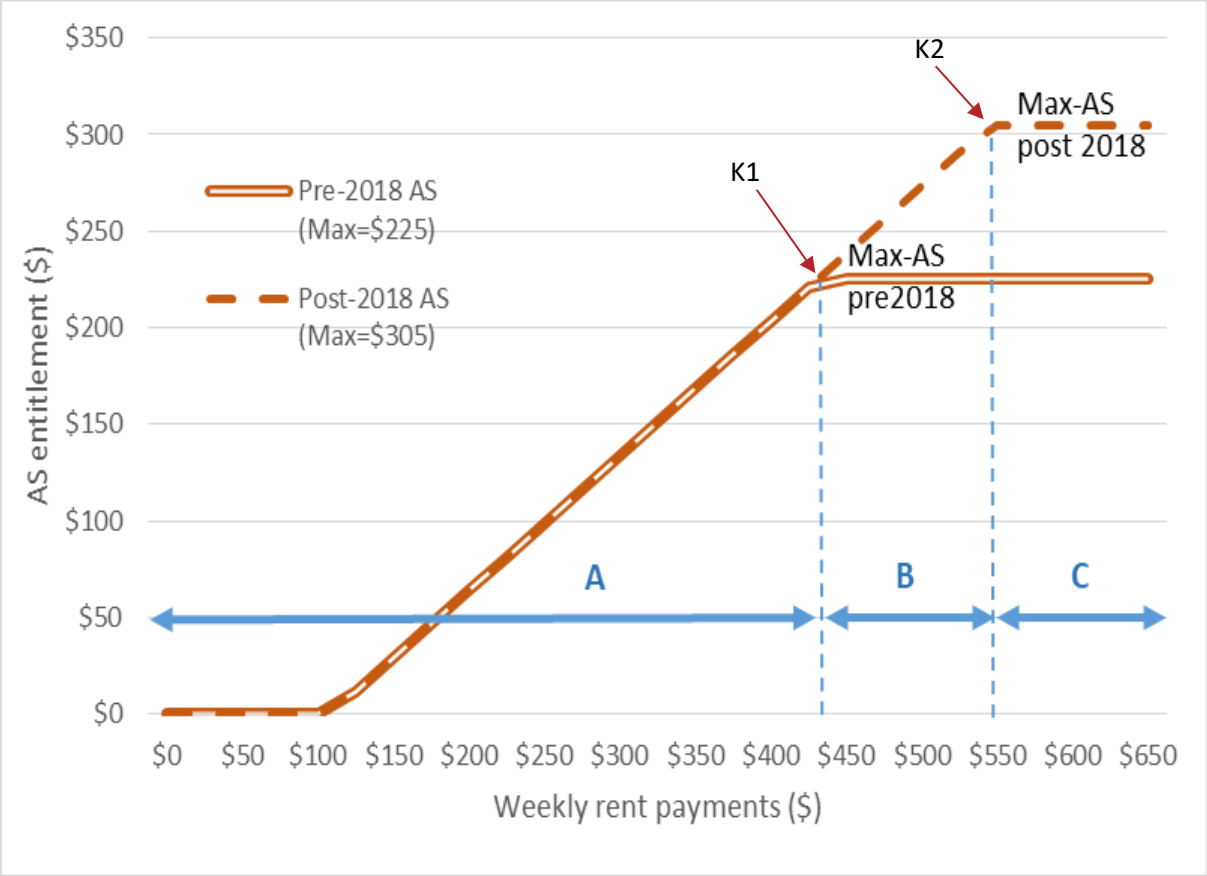
Notes: The trends are based on all AS-board clients that resided in the geographic boundary areas over the period April 2010 – May 2021.

Figure 8: Trends in accommodation support and homeowner payments – all boundary areas



Notes: The trends are based on all AS-home-owner clients that resided in the geographic boundary areas over the period April 2010 – May 2021.

Figure 9: 'Kinks' in the Accommodation Supplement entitlement schedule

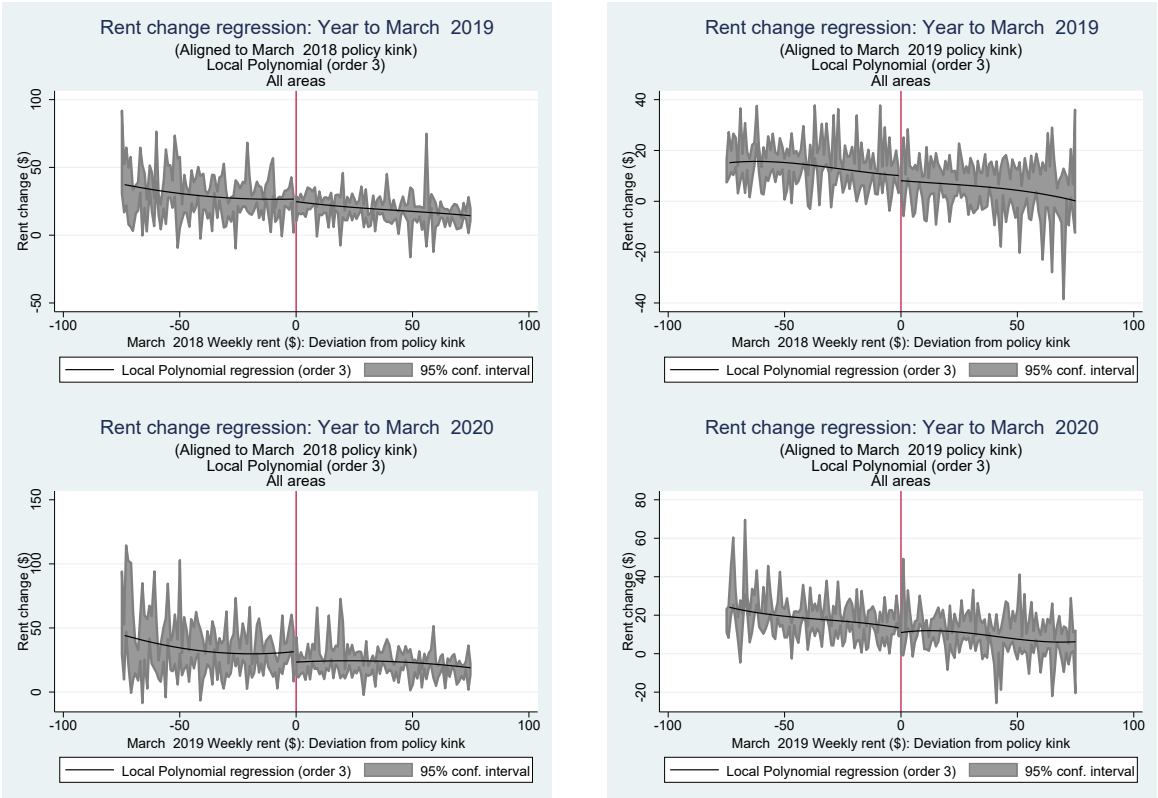


Note: Entitlements shown are for a 3-person household in Area 1

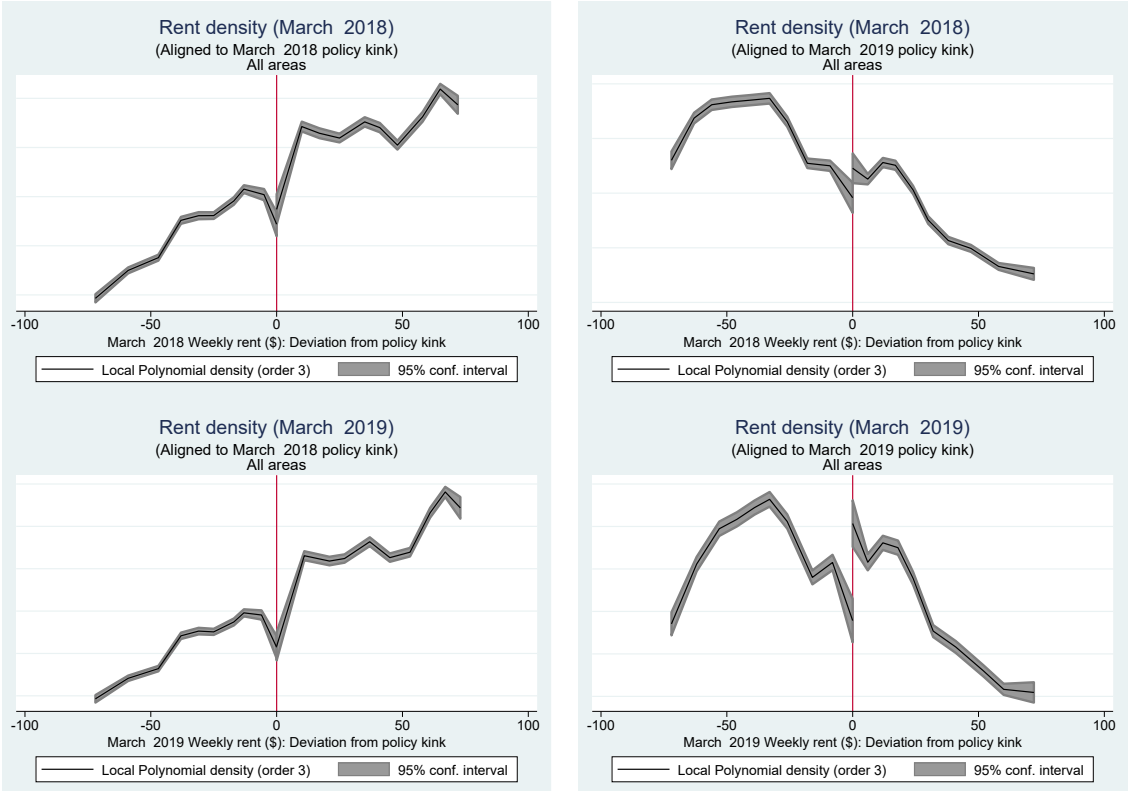
Figure 10: Rent change and rent density at policy kink points – All AS recipients (renters)

Kink pre-policy change (K1)	Kink at post-change max (K2)

a) Rent Change: Year to March 2019 and year to March 2020



b) Rent Density: March 2018 and March 2019

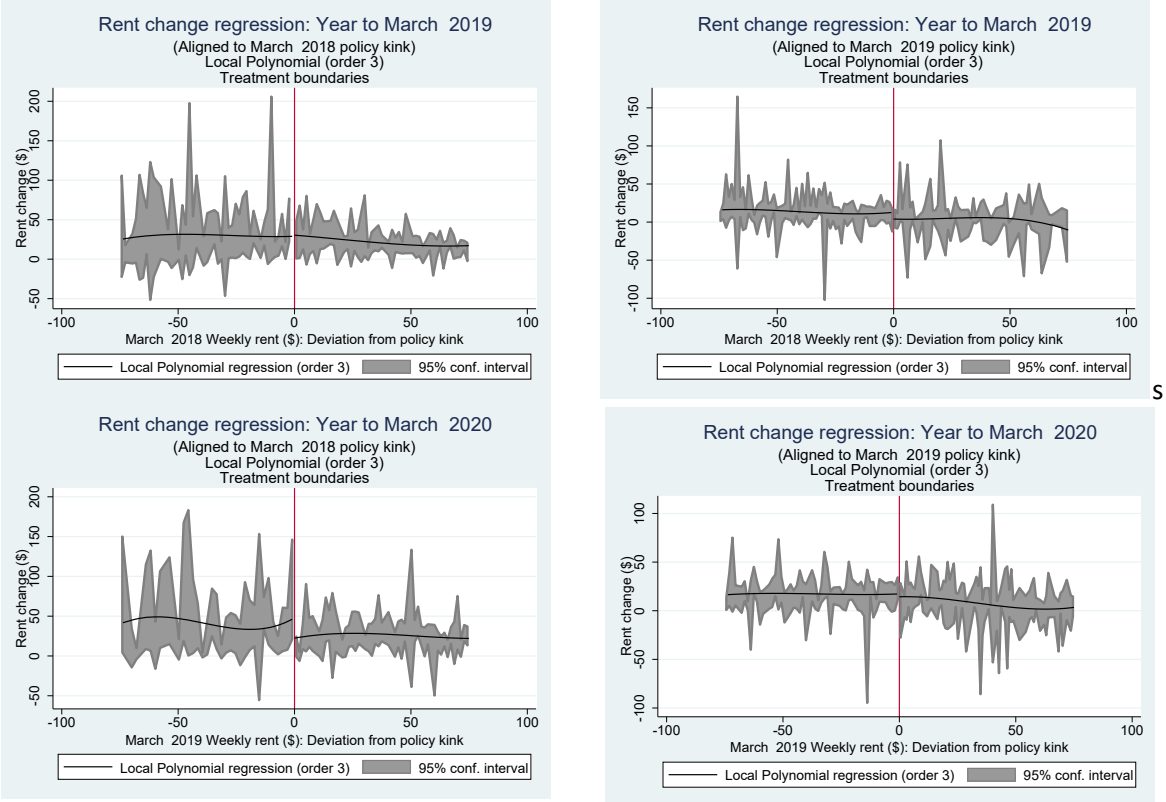


Note: Figures are based on data for people receiving AS support in March of the year for which the density is drawn (or in two consecutive March months for change figures) and also in the following March. Graphs are displayed for a rent range of $75 above or below the policy kink.

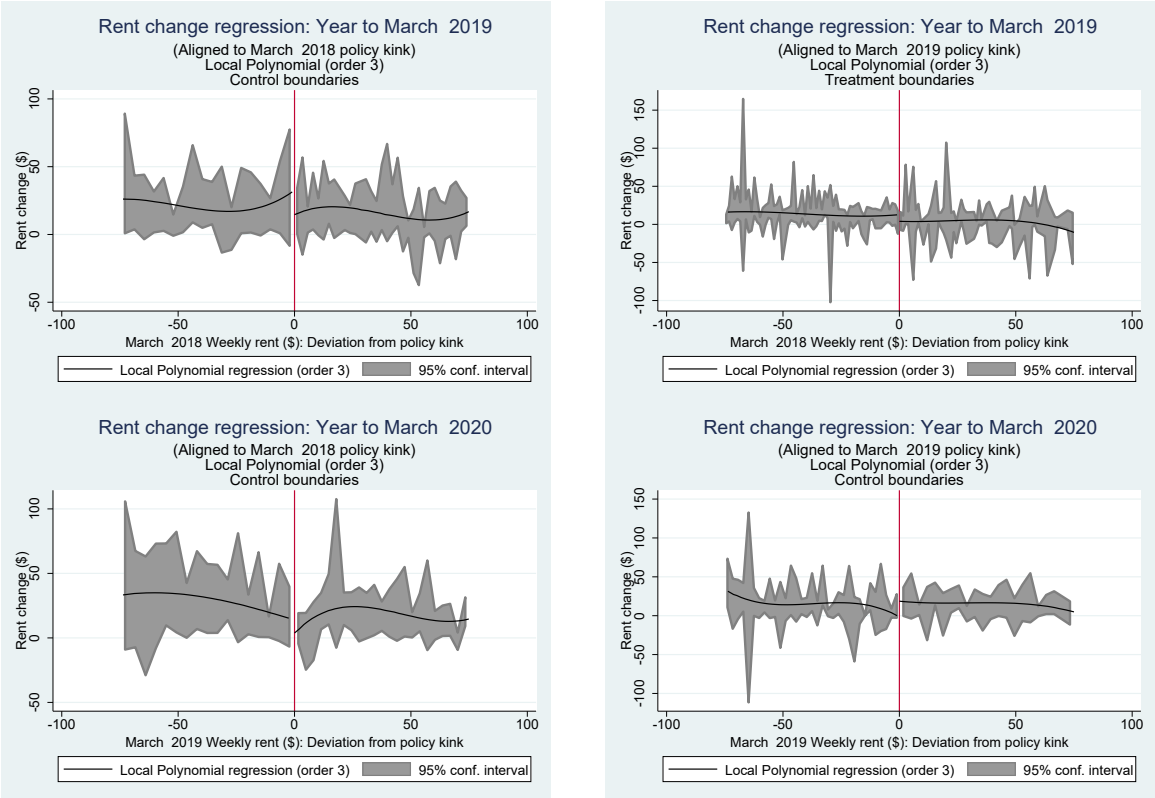
Figure 11: Rent change at policy kink points – Boundary sample

Kink pre-policy change (K1)	Kink at post-change max (K2)

a) Rent Change: Treatment areas: Year to March 2019 and year to March 2020



b) Rent Change: Control areas: Year to March 2019 and year to March 2020

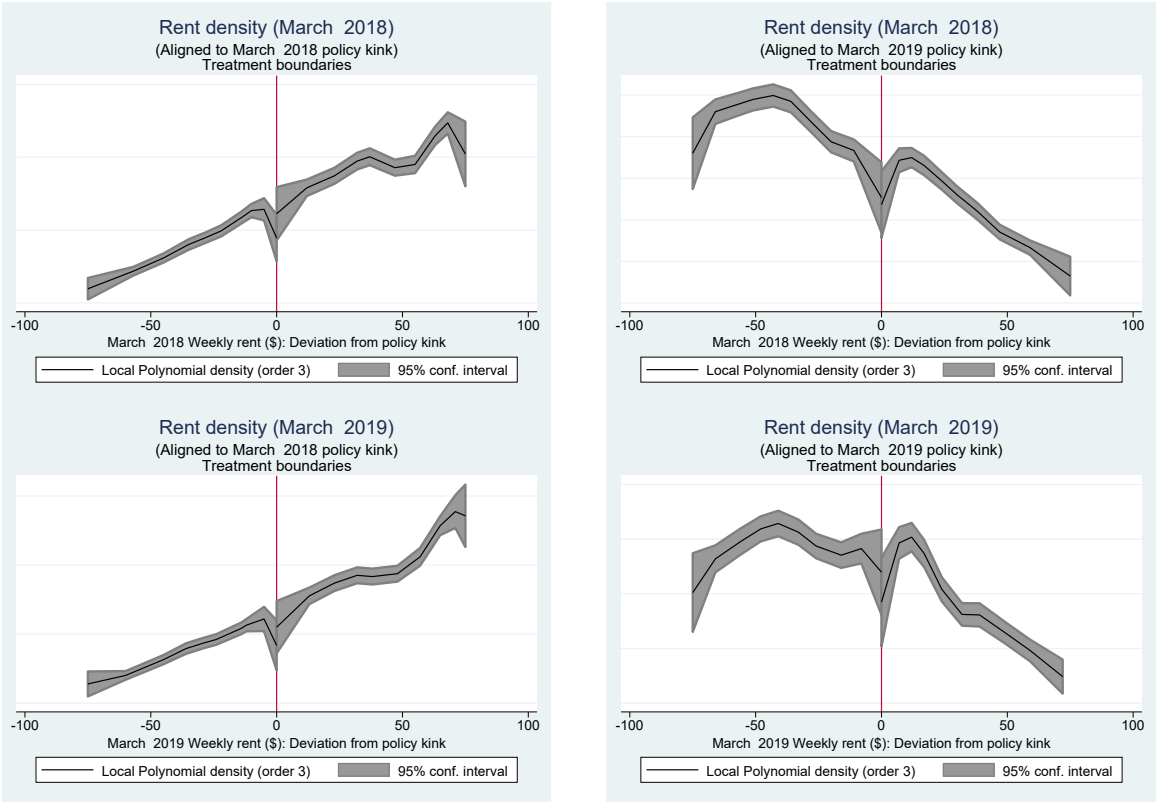


Note: Figures are based on data for people receiving AS support in two consecutive March months. Graphs are displayed for a rent range of $75 above or below the policy kink.

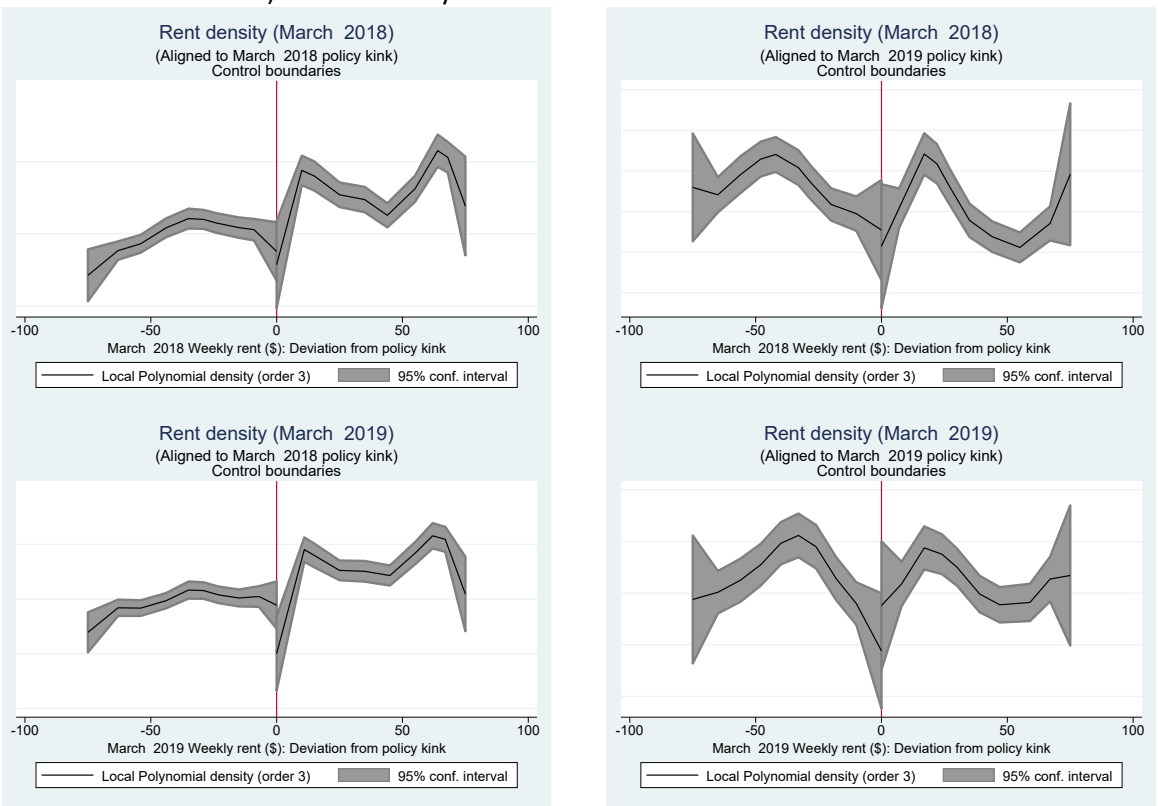
Figure 12: Rent Density at policy kink points – Boundary sample

Kink pre-policy change (K1)	Kink at post-change max (K2)

a) Rent Density: Treatment areas: March 2018 and March 2019



a) Rent Density: Control areas: March 2018 and March 2019



Note: Figures are based on data for people receiving AS support in March of the year for which the density is drawn and also in the following March. Graphs are displayed for a rent range of $75 above or below the policy kink.

Table 1: 2018 AS-maxima and changes, across areas and family size

	Weekly maxima (increase, %change)			
Family size:	Area-1	Area-2	Area-3	Area-4
1 person	$165	$105	$80	$70
	($20, 14%)	($5, 5%)	($15, 23%)	($25, 56%)
2 person	$235	$155	$105	$80
	($75, 47%)	($30, 24%)	($30, 40%)	($25, 45%)
3+ person	$305	$220	$160	$120
	($80, 36%)	($55, 33%)	($40, 33%)	($45, 60%)

Notes: The entries show the current AS maxima (i.e., from April 2018), (with actual and relative increases in parentheses). The effective AS-maximum increase for recipients may differ as the geographic area allocation to AS-areas also changed, meaning that a recipient's dwelling may have moved from one AS-area to another.

Table 2: Sample characteristics – Full boundary sample (renters)

	All Obs	Treatment-side		Control-side		DiD (p-value)[(1)]
		Pre-2018	Post-2018	Pre-2018	Post-2018	
No. Monthly Obs	1,233,906	441,804	468,423	158,511	165,168	
No. Individuals	74,496	36,813	38,700	14,433	15,009	
Female	0.632	0.651	0.647	0.584	0.580	0.000 (0.952)
Age	45.8 (17.0)	45.7 (17.2)	46.0 (17.3)	45.4 (16.3)	45.8 (16.6)	-0.166 (0.304)
European	0.682	0.713	0.721	0.581	0.584	0.004 (0.375)
Māori	0.313	0.338	0.334	0.243	0.251	-0.013 (0.004)
Pacific	0.097	0.074	0.076	0.159	0.159	0.003 (0.388)
Asian	0.081	0.053	0.052	0.164	0.154	0.009 (0.005)
Other ethnicity	0.030	0.026	0.027	0.039	0.039	0.001 (0.682)
Family size	1.98 (1.3)	2.02 (1.3)	1.98 (1.3)	1.96 (1.3)	1.88 (1.2)	0.037 (0.004)
Partnered	0.174	0.164	0.160	0.215	0.201	0.010 (0.012)
No. children	0.80 (1.1)	0.85 (1.2)	0.82 (1.2)	0.74 (1.1)	0.68 (1.1)	0.027 (0.021)
Single adult	0.490	0.470	0.491	0.498	0.534	-0.014 (0.005)
Couple adults	0.081	0.077	0.075	0.096	0.094	0.000 (0.926)
Sole parent, 1 child	0.161	0.173	0.162	0.148	0.138	-0.001 (0.792)
Sole parent, 2+ children	0.175	0.193	0.186	0.139	0.127	0.005 (0.173)
Couple with children	0.093	0.087	0.085	0.119	0.107	0.010 (0.002)
Area-1	0.219	0.001	0.256	0.466	0.457	0.263 (0.000)
Area-2	0.323	0.251	0.442	0.246	0.248	0.190 (0.000)
Area-3	0.273	0.411	0.296	0.052	0.049	-0.112 (0.000)
Area-4	0.186	0.336	0.006	0.235	0.246	-0.341 (0.000)
On main benefit:	0.785	0.800	0.790	0.756	0.757	-0.011 (0.018)
JSS (Job-seeker)	0.259	0.237	0.253	0.288	0.304	-0.004 (0.408)
SLP (Supported Living)	0.151	0.158	0.151	0.148	0.137	0.003 (0.326)
SPS (Single Parent)	0.192	0.218	0.195	0.161	0.140	-0.002 (0.655)
NZS/VP (NZS/ Veterans)	0.167	0.172	0.178	0.134	0.151	-0.011 (0.001)
Non-beneficiary	0.215	0.200	0.210	0.244	0.243	0.011 (0.018)
Financial variables:						
At AS-max	0.471	0.661	0.331	0.467	0.364	-0.228 (0.000)
Received TAS	0.286	0.309	0.264	0.278	0.292	-0.058 (0.000)
Has non-benefit income	0.322	0.321	0.316	0.338	0.327	0.005 (0.290)
Rent	282.56 (111.6)	262.95 (98.0)	291.20 (113.7)	285.94 (114.8)	307.23 (127.6)	6.97 (0.000)
AS received	97.45 (54.3)	72.82 (34.5)	113.33 (58.7)	98.87 (51.3)	116.95 (61.6)	22.44 (0.000)
TAS received	17.06 (32.1)	20.14 (34.6)	14.47 (29.4)	16.47 (32.1)	16.70 (31.9)	-5.91 (0.000)
Total Acc Support	114.51 (67.0)	92.96 (52.9)	127.80 (70.8)	115.34 (65.7)	133.65 (75.1)	16.53 (0.000)
Non-benefit income	151.20 (280.7)	134.72 (253.1)	152.74 (286.4)	168.00 (295.9)	174.77 (315.1)	11.24 (0.000)
Benefit income	430.96 (232.5)	411.20 (218.0)	460.25 (240.4)	397.69 (224.8)	432.62 (244.3)	14.12 (0.000)
Total income	582.15 (223.5)	545.92 (194.9)	612.99 (230.6)	565.69 (226.4)	607.40 (254.0)	2.36 (0.000)

Table 2 (Continued)

	All Obs	Treatment-side		Control-side	
		Pre-2018	Post-2018	Pre-2018	Post-2018
Treatment-side of Bdy	0.738	1.000	1.000	0.000	0.000
Merged boundary:	0.296	0.155	0.150	0.708	0.692
21_on_11	0.223	0.139	0.136	0.468	0.456
32_on_22	0.062	0.014	0.013	0.200	0.196
42_on_22	0.001	0.000	0.000	0.002	0.002
43_on_33	0.011	0.001	0.001	0.038	0.038
Split boundary:	0.429	0.521	0.524	0.163	0.173
21_on_22	0.055	0.056	0.059	0.043	0.048
32_on_33	0.098	0.130	0.130	0.008	0.010
41_on_44	0.003	0.003	0.005	0.000	0.000
42_on_44	0.030	0.028	0.035	0.025	0.027
43_on_44	0.244	0.304	0.296	0.086	0.087
Misc. boundary:	0.275	0.324	0.326	0.129	0.135
21_on_44	0.047	0.055	0.059	0.017	0.018
32_on_44	0.228	0.269	0.267	0.112	0.117

Notes: Entries in parentheses are standard deviations (except p-values after DiD estimates). The 'treatment-side' of a boundary is defined as the side which involved a change in AS-area. See the text for a detailed discussion of the boundary contrasts. Estimates over the four year period April 2016–March 2020.

(1) DiD (p-value) is the estimated coefficient (β_3, p-value) in the simple difference-in-differences regression:

$$X_i = \beta_0 + \beta_1 * Treatment_i + \beta_2 * Post_i + \beta_3 * (Treatment_i * Post_i) + \epsilon_i.$$

For each characteristic (X), this measures whether there was a statistically significant relative change in its average value on either side on the boundary (the standard errors are adjusted for clustering at the AS-client level in these regressions).

Table 3: Regression results – full boundary samples

Policy treatment effect:	Simple-DiD (1)	Year-effects (2)	Controls (3)	Client-FE (4)	'Stayers' (5)
		(a) Weekly rent regressions			
*Post2018	6.42***				
	(1.43)				
*2017/18		2.68**	-0.015	0.21	-0.38
		(1.25)	(0.96)	(0.56)	(1.04)
*2018/19		6.55***	2.42*	1.14	1.59
		(1.69)	(1.31)	(0.84)	(1.44)
*2019/20		8.93***	3.93***	1.28	3.44**
		(1.92)	(1.50)	(1.03)	(1.66)
R-squared	0.072	0.075	0.483	0.947	0.485
		(b) Weekly total accommodation support regressions			
*Post2018	17.61***				
	(0.84)				
*2017/18		-0.04	-0.62	-0.84*	-0.88
		(0.67)	(0.57)	(0.46)	(0.62)
*2018/19		16.53***	15.26***	13.74***	14.72***
		(0.99)	(0.87)	(0.75)	(0.96)
*2019/20		18.62***	16.93***	13.76***	16.47***
		(1.08)	(0.95)	(0.87)	(1.05)
Year effects	X	Y	Y	Y	Y
Controls	X	X	Y	Y	Y
Client fixed effects	X	X	X	Y	X
R-squared	0.129	0.130	0.395	0.874	0.393
No. Observations	1,233,906	1,233,906	1,233,906	1,233,906	931,743

Notes: The specification in column (1) includes dummy variable controls for 'merged', 'split' and 'miscellaneous' boundary types, treatment-groups for each type, and Post2018; column (2) replaces the Post2018 dummy with year-specific controls for 2017/18, 2018/19, and 2019/20. Controls in columns (3)-(5) include boundary-specific controls and treatment-side effects, boundary-specific interactions with each year control; and controls for gender, age, ethnicity, benefit type, and family structure. Column (4) also includes Client fixed effects to control for constant unobserved characteristics; and column (5) sample controls for observable differences and is restricted to clients who do not change meshblock location during their time on AS over the period. Standard errors are in parentheses, adjusted for clustering at the AS-client level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 4: Regression results – separate boundary samples

	Merged boundaries		Split boundaries		Misc boundaries		Central Auckland	
Policy treatment effect	Controls (3)	Client-FE (4)	Controls (3)	Client-FE (4)	Controls (3)	Client-FE (4)	Controls (3)	Client-FE (4)
			(a) Weekly rent regressions					
*2017/18	-1.44	-0.781	-0.083	1.19	2.72	0.20	-2.26	-0.98
	(1.46)	(0.85)	(1.68)	(0.92)	(1.81)	(1.05)	(1.61)	(0.93)
*2018/19	1.61	-1.18	2.45	3.93***	3.82	1.94	1.53	-0.98
	(2.02)	(1.29)	(2.23)	(1.36)	(2.48)	(1.58)	(2.22)	(1.41)
*2019/20	2.10	-2.40	3.31	7.22***	6.90**	2.44	1.87	-2.48
	(2.28)	(1.54)	(2.58)	(1.74)	(2.85)	(1.99)	(2.52)	(1.68)
R-squared	0.471	0.958	0.462	0.937	0.462	0.947	0.440	0.957
			(b) Weekly total accommodation support regressions					
*2017/18	-1.61*	-1.52**	-0.20	-0.39	0.86	-0.54	-1.63	-1.57**
	(0.89)	(0.70)	(0.96)	(0.76)	(1.02)	(0.85)	(0.10)	(0.78)
*2018/19	18.23***	16.59***	14.06***	12.82***	10.43***	7.90***	19.27***	16.93***
	(1.42)	(1.22)	(1.36)	(1.15)	(1.46)	(1.31)	(1.59)	(1.36)
*2019/20	18.43***	15.44***	16.02***	14.09***	14.22***	9.29***	18.32***	15.00***
	(1.55)	(1.38)	(1.50)	(1.33)	(1.60)	(1.62)	(1.73)	(1.53)
R-squared	0.338	0.886	0.394	0.858	0.358	0.859	0.297	0.880
No. Observations	365,346	365,346	529,833	529,833	338,727	338,727	274,656	274,656

Notes: The specifications in columns labelled "(3)" and "(4)" correspond to those in columns (3) (controlling for observable characteristics) and (4) (also controlling for AS-client fixed effects) of Table 3 respectively. Standard errors are in parentheses, adjusted for clustering at the AS-client level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 5: Regression results – full boundary samples, observed 2010-2021

Policy treatment	2010 – 2020 Sample				2010 – 2021 Sample	
effect:	(1)	(2)	(3)	(4)	(3)	(4)
		(a) Weekly rent regressions				
*Post2018	9.24***					
	(1.50)					
*2010/11		-3.19*	-1.41	-1.79	-1.28	-1.89
		(1.88)	(1.44)	(1.31)	(1.44)	(1.33)
*2011/12		-1.66	-0.55	-1.99	-0.45	-2.12*
		(1.87)	(1.46)	(1.21)	(1.46)	(1.24)
*2012/13		-1.34	-1.09	-1.83*	-1.03	-1.89*
		(1.79)	(1.37)	(1.09)	(1.37)	(1.11)
*2013/14		-2.15	-3.31**	-1.16	-3.33**	-1.21
		(1.74)	(1.32)	(0.99)	(1.32)	(1.02)
*2014/15		-0.26	-1.88	-1.51*	-1.92	-1.65**
		(1.59)	(1.20)	(0.82)	(1.20)	(0.84)
*2015/16		-0.01	-1.52*	-1.60***	-1.53*	-1.65***
		(1.27)	(0.90)	(0.58)	(0.90)	(0.60)
*2017/18		3.27**	0.12	-0.12	0.07	-0.085
		(1.33)	(0.96)	(0.61)	(0.96)	(0.63)
*2018/19		7.14***	2.73**	1.43	2.62**	1.58*
		(1.74)	(1.32)	(0.92)	(1.31)	(0.93)
*2019/20		9.51***	4.40***	1.53	4.25***	1.58
		(1.95)	(1.50)	(1.12)	(1.50)	(1.13)
*2020/21					4.62***	1.85
					(1.58)	(1.29)
*2021/22					4.72***	2.16
					(1.74)	(1.45)
Year effects	X	Y	Y	Y	Y	Y
Controls	X	X	Y	Y	Y	Y
Client fixed effects	X	X	X	Y	X	Y
R-squared	0.091	0.109	0.496	0.908	0.503	0.904
F-statistic		2.26	1.75	1.35	1.78	1.38
(p-value)	–	(0.03)	(0.09)	(0.22)	(0.09)	(0.21)
No. Observations	3,077,412	3,077,412	3,077,412	3,077,412	3,538,596	3,538,596

Notes: The sample includes all AS-clients over the extended period (April 2010–May 2021). Standard errors are in parentheses, adjusted for clustering at the AS-client level. See notes to Appendix Table A5 for more details of the regression specifications. For consistency with earlier results, we maintain the omitted base year as 2016/17. The F-statistic (and p-value) relate to the common trends hypothesis, H0: the pre-2018 policy 'treatment' effects (i.e. *2010/11, …, *2017/18) are each zero.
*** p<0.01, ** p<0.05, * p<0.1.

Table 5 (Continued)

Policy treatment	2010 – 2020 Sample				2010 – 2021 Sample	
effect:	(1)	(2)	(3)	(4)	(3)	(4)
	(b) Weekly Total accommodation support regressions					
*Post2018	16.30***					
	(0.88)					
*2010/11		2.06**	1.51*	1.93**	1.46	1.66*
		(1.03)	(0.90)	(0.92)	(0.91)	(0.93)
*2011/12		2.00**	1.12	1.44*	1.03	1.09
		(1.01)	(0.88)	(0.87)	(0.89)	(0.89)
*2012/13		1.95**	0.67	0.89	0.53	0.58
		(0.98)	(0.86)	(0.82)	(0.87)	(0.83)
*2013/14		0.70	0.61	1.24*	0.53	1.03
		(0.93)	(0.81)	(0.73)	(0.81)	(0.74)
*2014/15		0.89	0.98	0.60	0.93	0.40
		(0.85)	(0.73)	(0.62)	(0.73)	(0.63)
*2015/16		0.81	0.63	0.17	0.62	0.06
		(0.67)	(0.55)	(0.45)	(0.55)	(0.46)
*2017/18		-0.41	-0.56	-0.91*	-0.58	-0.84*
		(0.70)	(0.57)	(0.47)	(0.57)	(0.48)
*2018/19		16.16***	15.45***	14.15***	15.39***	14.16***
		(1.00)	(0.88)	(0.76)	(0.87)	(0.77)
*2019/20		18.27***	17.31***	14.44***	17.21***	14.59***
		(1.09)	(0.96)	(0.88)	(0.96)	(0.88)
*2020/21					15.41***	14.16***
					(0.99)	(0.99)
*2021/22					14.51***	13.77***
					(1.09)	(1.09)
Year effects	X	Y	Y	Y	Y	Y
Controls	X	X	Y	Y	Y	Y
Client fixed effects	X	X	X	Y	X	Y
R-squared	0.145	0.148	0.385	0.823	0.393	0.823
F-statistic		1.31	0.87	1.75	0.88	1.54
(p-value)	–	(0.24)	(0.53)	(0.09)	(0.53)	(0.15)
No. Observations	3,077,412	3,077,412	3,077,412	3,077,412	3,538,596	3,538,596

Notes: The samples in the first four columns includes all AS-clients over the extended period (April 2010–March 2020); and in the final two columns over the period April 2010 – May 2021. Standard errors are in parentheses, adjusted for clustering at the AS-client level. See notes to Appendix Table A5 for more details of the regression specifications. For consistency with earlier results, we maintain the omitted base year as 2016/17. The F-statistic (and p-value) relate to the common trends hypothesis, H0: the pre-2018 policy 'treatment' effects (i.e. *2010/11, …, *2017/18) are each zero.

*** p<0.01, ** p<0.05, * p<0.1.

Table 6: Tenancy Bond regression results – full boundary samples

	2016-20			2010-20	2010-21[(1)]
Policy treatment effect	Simple-DiD (1)	Year-effects (2)	Controls (3)	Controls (4)	Controls (5)
*Post2018	-11.00				
	(8.70)				
*2010/11				-1.02	-1.22
				(5.02)	(5.00)
*2011/12				-0.76	-0.96
				(5.02)	(5.01)
*2012/13				-1.83	-2.03
				(5.16)	(5.15)
*2013/14				4.36	4.13
				(5.22)	(5.20)
*2014/15				1.94	1.83
				(5.06)	(5.06)
*2015/16				3.60	3.44
				(4.85)	(4.84)
*2017/18		13.40*	9.32*	9.97**	9.93**
		(6.87)	(5.00)	(5.05)	(5.04)
*2018/19		-0.22	3.73	3.56	3.65
		(9.35)	(5.96)	(6.54)	(6.55)
*2019/20		-8.08	-5.30	-4.93	-4.81
		(11.41)	(7.15)	(8.14)	(8.10)
*2020/21					3.644
					(6.72)
Year effects	X	Y	Y	Y	Y
Controls	X	X	Y	Y	Y
Observations	108,876	108,876	108,876	279,225	295,923
R-squared	0.097	0.103	0.456	0.483	0.487
F-statistic				1.13	1.13
(p-value)				(0.34)	(0.34)

Notes: The sample includes all new-tenancy rents observed over the respective periods April 2016–March 2020 (columns 1–3), April 2010–March 2020 (column 4), and April 2010–December 2020 (column 5). The specification in column (1) includes dummy variable controls for 'merged', 'split' and 'miscellaneous' boundary types, treatment-groups for each type, and Post2018; column (2) replaces the Post2018 dummy with year-specific controls for each year (April-March). Controls in columns (3)-(5) include boundary-specific controls and treatment-side effects, boundary-specific interactions with each year control; and controls for the number of bedrooms, the type of dwelling (house, apartment, flat, boarding house or room), and the type of landlord (property management company, private, public). Standard errors are in parentheses, adjusted for clustering at the meshblock level. For consistency with earlier results, we maintain the omitted base year as 2016/17. The F-statistic (and p-value) relate to the common trends hypothesis, H0: the pre-2018 policy 'treatment' effects (i.e. *2010/11, …, *2017/18) are each zero.

[(1)] Sample period is April 2010–December 2020.

*** p<0.01, ** p<0.05, * p<0.1.

Table 7: Regression results – 2km placebo and robustness checks

Policy treatment	All boundary-areas			Central Auckland boundary	
effect:	Year-effects	Controls	Client-FE	Controls	Client-FE
		(a) Weekly rent regressions			
*2017/18	2.26*	0.17	0.23	-2.39	-0.20
	(1.17)	(0.89)	(0.54)	(1.61)	(0.98)
*2018/19	6.14***	2.85**	1.60*	1.37	0.31
	(1.59)	(1.22)	(0.82)	(2.23)	(1.48)
*2019/20	9.44***	5.09***	1.98*	1.70	-0.87
	(1.81)	(1.39)	(1.02)	(2.52)	(1.78)
T2*2017/18	-1.96	-1.32	-0.05	0.45	-0.34
	(1.61)	(1.20)	(0.73)	(2.19)	(1.36)
T2*2018/19	-5.68**	-2.22	-0.91	-1.26	0.02
	(2.22)	(1.66)	(1.10)	(3.04)	(2.11)
T2*2019/20	-2.58	-0.40	0.067	2.97	2.06
	(2.53)	(1.90)	(1.36)	(3.44)	(2.52)
C2*2017/18	1.69	1.37	-0.41	1.76	-0.34
	(1.42)	(1.06)	(0.63)	(1.60)	(0.96)
C2*2018/19	4.59**	1.62	-0.033	3.16	0.16
	(1.96)	(1.45)	(0.94)	(2.19)	(1.42)
C2*2019/20	2.88	0.59	-0.12	2.69	0.24
	(2.23)	(1.66)	(1.15)	(2.48)	(1.71)
F-statistic	1.62	0.75	1.48	1.93	1.23
(p-value)	(0.14)	(0.61)	(0.18)	(0.07)	(0.29)
R-squared	0.073	0.491	0.938	0.444	0.940
		(b) Weekly total accommodation support regressions			
*2017/18	-0.65	-1.06**	-1.11***	-1.65*	-1.47*
	(0.63)	(0.53)	(0.43)	(1.00)	(0.79)
*2018/19	17.06***	15.57***	14.63***	19.22***	17.50***
	(0.94)	(0.81)	(0.69)	(1.59)	(1.35)
*2019/20	19.55***	17.68***	15.42***	18.25***	15.77***
	(1.03)	(0.88)	(0.79)	(1.72)	(1.53)
T2*2017/18	-1.16	-0.74	0.01	-1.54	-0.45
	(0.86)	(0.72)	(0.57)	(1.34)	(1.06)
T2*2018/19	-1.24	-0.93	0.10	-3.81*	-0.80
	(1.29)	(1.09)	(0.90)	(2.12)	(1.80)
T2*2019/20	-0.41	-0.49	1.01	0.06	2.74
	(1.42)	(1.20)	(1.05)	(2.31)	(2.05)
C2*2017/18	0.71	0.17	-0.53	0.29	-1.08
	(0.76)	(0.64)	(0.49)	(1.00)	(0.76)
C2*2018/19	1.23	0.21	-0.31	4.12***	1.80
	(1.12)	(0.94)	(0.76)	(1.49)	(1.20)
C2*2019/20	0.99	0.12	-0.59	2.25	0.03
	(1.25)	(1.04)	(0.89)	(1.66)	(1.40)
F-statistic	0.71	0.51	1.44	3.10	4.99
(p-value)	(0.64)	(0.80)	(0.20)	(0.005)	(0.000)
R-squared	0.123	0.398	0.865	0.310	0.863
No. Observations	2,244,981	2,244,981	2,244,981	656,994	656,994

Notes: The specification in columns correspond to those in Table 3. Standard errors are in parentheses, adjusted for clustering at the AS-client level. T2 (C2) recipients live in meshblocks on the Treatment (Control) areas within 2km but not 1km of the boundary. The F-statistic relates to the joint hypothesis that all of the T2 and C2 interactions are equal to zero. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Regression results – by family type

	Sole parent with 1 child		Sole parent with 2+ children		Couple w/ children	Single adult	Couple adults
Policy effect:	Controls (3)	Client-FE (4)	Controls (3)	Client-FE (4)	Controls (3)	Controls (3)	Controls (3)
			(a) Weekly rent regressions				
*	0.83	0.28	4.49	1.37	0.35	-0.64	-4.82
2017/18	(2.70)	(1.51)	(2.84)	(1.74)	(3.59)	(1.17)	(3.10)
*	4.86	1.46	8.75**	1.62	1.11	-0.10	-4.35
2018/19	(3.76)	(2.47)	(3.91)	(2.67)	(4.94)	(1.57)	(4.19)
*	5.67	0.43	11.43**	3.36	2.11	-0.52	-1.50
2019/20	(4.22)	(3.00)	(4.46)	(3.43)	(5.46)	(1.80)	(4.90)
R-squared	0.280	0.923	0.385	0.925	0.367	0.178	0.265
			(b) Weekly total accommodation support				
*	-1.07	-1.80	0.31	-1.07	-0.90	-0.38	-0.18
2017/18	(1.66)	(1.32)	(1.71)	(1.37)	(2.18)	(0.65)	(1.88)
*	18.55***	17.18***	19.59***	15.36***	20.51***	10.20***	13.24***
2018/19	(2.53)	(2.20)	(2.63)	(2.30)	(3.35)	(0.98)	(2.90)
*	20.37***	16.59***	25.26***	16.97***	24.10***	9.75***	14.85***
2019/20	(2.73)	(2.51)	(2.87)	(2.72)	(3.60)	(1.08)	(3.32)
R-squared	0.361	0.861	0.455	0.873	0.344	0.295	0.342
No. Obs	198,798	198,798	215,652	215,652	114,879	604,401	100,176

Notes: The specifications in columns labelled "(3)" and "(4)" correspond to those in columns (3) (controlling for observable characteristics) and (4) (also controlling for AS-client fixed effects) of Table 3 respectively. Standard errors are in parentheses, adjusted for clustering at the AS-client level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 9: Regression results – by gender and age

Policy effect:	Males	Females	Age 16-24	Age 25-64	Age 65+
		(a) Weekly rent regressions			
*2017/18	-0.24	-0.19	3.27	0.46	-1.90
	(1.39)	(1.30)	(3.66)	(1.16)	(2.03)
*2018/19	1.11	2.60	5.97	3.42**	-1.34
	(1.91)	(1.76)	(4.49)	(1.59)	(2.85)
*2019/20	2.70	3.51*	13.27***	4.15**	-0.95
	(2.17)	(2.01)	(4.88)	(1.80)	(3.39)
R-squared	0.502	0.450	0.491	0.490	0.307
		(b) Weekly total accommodation support			
*2017/18	-1.26	-0.41	-0.17	-0.35	-1.24
	(0.82)	(0.77)	(2.20)	(0.68)	(1.21)
*2018/19	11.48***	16.73***	15.46***	17.79***	6.65***
	(1.28)	(1.16)	(3.07)	(1.04)	(1.91)
*2019/20	12.38***	18.80***	17.13***	18.99***	9.44***
	(1.39)	(1.27)	(3.30)	(1.13)	(2.20)
R-squared	0.357	0.411	0.357	0.395	0.283
No. Observations	454,497	779,409	105,144	886,794	214,431

Notes: All specifications correspond to that in column (3) of Table 3 (controlling for observable characteristics). Standard errors are in parentheses, adjusted for clustering at the AS-client level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 10: Regression results – by ethnicity

	European		Māori		Pacific	
Policy effect:	Controls (3)	Client-FE (4)	Controls (3)	Client-FE (4)	Controls (3)	Client-FE (4)
			(a) Weekly rent regressions			
*2017/18	1.06	0.69	0.74	0.98	2.87	0.57
	(1.19)	(0.69)	(1.87)	(1.09)	(2.81)	(1.83)
*2018/19	3.10*	2.47**	2.74	-0.12	8.47**	1.87
	(1.62)	(1.02)	(2.52)	(1.74)	(3.86)	(2.66)
*2019/20	2.02	2.21*	6.65**	1.68	11.72***	0.88
	(1.85)	(1.28)	(2.84)	(2.14)	(4.34)	(3.16)
R-squared	0.488	0.944	0.452	0.934	0.463	0.950
			(b) Weekly total accommodation support			
*2017/18	-0.59	-0.68	-0.47	-1.52*	0.40	-1.22
	(0.69)	(0.56)	(1.12)	(0.92)	(1.75)	(1.45)
*2018/19	14.45***	13.29***	14.87***	11.75***	24.51***	21.59***
	(1.04)	(0.90)	(1.64)	(1.50)	(2.72)	(2.44)
*2019/20	15.62***	13.30***	18.35***	13.08***	24.52***	20.75***
	(1.13)	(1.05)	(1.77)	(1.73)	(2.90)	(2.69)
R-squared	0.407	0.870	0.393	0.861	0.350	0.872
No. Obs	841,281	841,281	385,854	385,854	119,493	119,493

Notes: The specifications in columns labelled "(3)" and "(4)" correspond to those in columns (3) (controlling for observable characteristics) and (4) (also controlling for AS-client fixed effects) of Table 3 respectively. Standard errors are in parentheses, adjusted for clustering at the AS-client level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 11: Regression results – by benefit and TAS receipt status

	On main benefit		Not on benefit		Never received TAS		Ever received TAS	
Policy effect:	Controls (3)	Client-FE (4)	Controls (3)	Client-FE (4)	Controls (3)	Client-FE (4)	Controls (3)	Client-FE (4)
				(a) Weekly rent regressions				
*2017/18	0.37	0.52	-1.44	0.16	-1.26	0.46	0.99	-0.13
	(1.07)	(0.63)	(2.22)	(1.25)	(1.19)	(0.63)	(1.31)	(0.93)
*2018/19	2.24	1.61*	2.76	1.56	2.50	1.65*	3.21*	0.51
	(1.45)	(0.93)	(3.05)	(1.87)	(1.69)	(0.95)	(1.75)	(1.36)
*2019/20	3.57**	1.66	4.17	1.88	7.14***	3.35***	2.74	-0.69
	(1.65)	(1.16)	(3.47)	(2.24)	(1.96)	(1.19)	(1.97)	(1.63)
R-squared	0.437	0.941	0.455	0.963	0.591	0.971	0.508	0.914
				(b) Weekly total accommodation support				
*2017/18	-0.29	-0.17	-1.02	-1.46	-0.85	-0.53	-0.30	-1.05
	(0.64)	(0.48)	(1.21)	(1.02)	(0.53)	(0.41)	(0.81)	(0.81)
*2018/19	12.60***	10.88***	23.76***	24.46***	12.82***	11.77***	18.75***	15.76***
	(0.98)	(0.80)	(1.88)	(1.85)	(0.95)	(0.89)	(1.18)	(1.19)
*2019/20	14.81***	11.12***	23.78***	23.89***	16.61***	12.96***	19.58***	14.93***
	(1.07)	(0.93)	(2.01)	(2.06)	(1.02)	(0.98)	(1.28)	(1.38)
R-squared	0.409	0.894	0.371	0.864	0.461	0.904	0.458	0.812
No. Obs	968,217	968,217	265,692	265,692	615,255	615,255	618,654	618,654

Notes: The specifications in columns labelled "(3)" and "(4)" correspond to those in columns (3) (controlling for observable characteristics) and (4) (also controlling for AS-client fixed effects) of Table 3 respectively. Standard errors are in parentheses, adjusted for clustering at the AS-client level.
*** p<0.01, ** p<0.05, * p<0.1.

Table 12: Summary of relative income and rent changes

Population	Income		Rent		
Subgroup	Income	ΔTotal AS	Rent	ΔRent	MPS
Full	546	16.93	263	3.93	23.2%
Family-type:					
Single adult	417	9.75	206	-0.52	-5.3%
Sole parent +1	588	20.37	293	5.67	27.8%
Sole parent +2	661	25.26	327	11.43	45.2%
Couple adult	706	14.85	288	-1.50	-10.1%
Couple +children	762	24.10	347	2.11	8.8%
Gender:					
Male	502	12.38	234	2.70	21.8%
Female	570	18.80	279	3.51	18.7%
Age group:					
Aged 16-24	484	17.13	242	13.27	77.5%
Aged 25-64	543	18.99	275	4.15	21.9%
Aged 65 plus	588	9.44	223	-0.95	-10.1%
Ethnicity:					
European	542	15.62	261	2.02	12.9%
Māori	534	18.35	258	6.65	36.2%
Pacific	605	24.52	305	11.72	47.8%
Other ethnicity	581	16.39	284	2.92	17.8%
Benefit-status:					
On-benefit	521	14.81	249	3.57	24.1%
Off-benefit	644	23.78	320	4.17	17.5%
TAS receipt:					
Never-TAS	563	16.61	239	7.14	43.0%
Ever-TAS	529	19.58	287	2.74	14.0%

Notes: Reported income (rent) is average treatment-side income (rent) before the policy change; ΔTotalAS (ΔRent) is the regression-adjusted estimate of the relative treatment-side increase in total accommodation support (rent) in 2019/20, controlling for observable characteristics (based on specification (3) in Table 3) for all subgroups; in the final column, the marginal propensity to spend on housing (MPS) is estimated as the ratio ΔRent/ΔTotalAS.

Table 13: Inflows, outflows, and changes, by year (All AS rental recipients)

	Year ending March:								
	2012	2013	2014	2015	2016	2017	2018	2019	2020
# AS $\frac{(N_1+N_0)}{2}$	195,999	195,141	189,675	184,743	184,545	184,596	182,703	186,426	197,124
Of which:									
Entry rate	30%	28%	26%	28%	26%	25%	24%	27%	27%
Exit rate	29%	29%	31%	28%	26%	26%	26%	22%	22%
Net change	0%	-1%	-4%	-1%	1%	-1%	-2%	6%	6%
% both years	70%	71%	71%	72%	74%	75%	75%	76%	76%
Of which:									
Δ location	27%	27%	26%	23%	23%	21%	20%	20%	20%
Δ fam size	12%	12%	11%	11%	10%	10%	9%	9%	9%
Δ ben type	13%	13%	47%	12%	11%	10%	10%	10%	9%

Note: All rates, and net change, are expressed as a proportion of the mean number of rental clients. Counts are for March 15th .

Table 14: Contributions to cumulative (difference in difference) treatment effects

Subgroup	Mean share	Contribution to cumulative treatment effect (since March 2016)				2016-2020 cumulative change contributions	
		To 2017	To 2018	To 2019	To 2020	Treatment	Control
All Rental Boundary areas							
				(a) Rent			
net AS entry	25%	-$1.13	$2.34	$5.53	$7.64	$17.72	$10.07
net transfer	15%	$0.47	$1.14	$3.17	$2.70	$20.38	$17.68
Change for stayers	60%	-$0.80	-$1.28	-$0.80	$0.20	$15.59	$15.39
Total		**-$1.46**	**$2.20**	**$7.90**	**$10.55**	**$53.69**	**$43.13**
				(b) Total accommodation support			
net AS entry	25%	-$1.49	-$0.22	$4.15	$6.14	$15.79	$9.65
net transfer	15%	$0.36	$1.20	$4.91	$4.95	$12.43	$7.48
Change for stayers	60%	-$0.98	-$1.22	$6.65	$7.77	$15.52	$7.75
Total		**-$2.11**	**-$0.24**	**$15.71**	**$18.86**	**$43.74**	**$24.88**
				(c) Benefit share (Percentage point change)			
net AS entry	25%	-0.24%	0.15%	-0.52%	-0.45%	3.15%	3.60%
net transfer	15%	0.18%	0.29%	0.23%	0.22%	-0.62%	-0.84%
Change for stayers	60%	-0.09%	-0.17%	-0.12%	-0.16%	-0.69%	-0.53%
Total		**-0.15%**	**0.28%**	**-0.41%**	**-0.39%**	**1.84%**	**2.23%**
Central Auckland (2-1 on 1-1 boundary)							
				(a) Rent			
net AS entry	24%	-$1.20	$1.73	$5.58	$4.10	$17.36	$13.26
Net Cross-border	6%	$3.01	$1.14	$4.21	$4.29	$11.52	$7.22
net transfer	6%	$0.09	$0.34	$1.84	$0.90	$10.89	$9.99
Change for stayers	64%	-$0.42	-$1.16	-$0.71	-$0.37	$19.75	$20.12
Total		**$1.48**	**$2.04**	**$10.91**	**$8.92**	**$59.52**	**$50.59**
				(b) Total accommodation support			
net AS entry	24%	-$2.75	-$2.06	$6.39	$5.34	$16.05	$10.71
Net Cross-border	6%	$1.00	$0.20	$3.77	$3.19	$6.31	$3.12
net transfer	6%	-$0.52	$0.07	$1.50	-$0.84	$5.45	$6.29
Change for stayers	64%	-$0.30	-$1.04	$11.12	$11.97	$22.01	$10.04
Total		**-$2.57**	**-$2.83**	**$22.78**	**$19.66**	**$49.82**	**$30.16**
				(c) Benefit share (Percentage point change)			
net AS entry	24%	-0.68%	-0.50%	0.43%	0.91%	2.2%	1.2%
Net Cross-border	6%	-0.36%	-0.55%	-0.72%	-0.35%	-0.3%	0.1%
net transfer	6%	-0.37%	-0.65%	-1.26%	-0.80%	-0.4%	0.4%
Change for stayers	64%	-0.08%	-0.60%	-0.33%	-0.28%	-1.0%	-0.7%
Total		**-1.49%**	**-2.30%**	**-1.88%**	**-0.52%**	**0.5%**	**1.0%**

Note: Mean share is averaged across all years and boundary areas.

Table 15: Treatment effects and changes in location, family, or benefit

	Rent				Total accommodation support			
Year ending March	2017	2018	2019	2020	2017	2018	2019	2020
Simple DiD								
Trtmt effect (β_3)	-0.77	-0.73	0.56	0.02	-0.76	-0.25	14.45***	0.15
	(0.60)	(0.72)	(0.78)	(0.71)	(0.70)	(0.70)	(0.89)	(0.72)
Observations	31,362	31,539	32,118	34,182	31,362	31,539	32,118	34,182
Adj R^2	0.094	0.090	0.073	0.061	0.133	0.122	0.112	0.065
Stratified DiD								
Main effect (β_3)	2.45*	-0.62	0.11	1.79	0.14	-0.76	12.18***	0.76
	(1.02)	(1.13)	(1.11)	(1.16)	(0.77)	(0.81)	(1.01)	(0.81)
*Mover	-3.53	2.83	0.29	-8.00	-1.50	2.25	-8.77*	-9.67*
	(5.44)	(5.78)	(5.84)	(6.46)	(3.59)	(3.75)	(4.19)	(4.63)
*Family-size change	-10.67**	-2.30	-1.10	-4.83	-6.44	5.61	4.24	0.59
	(3.25)	(3.45)	(3.80)	(3.60)	(3.32)	(3.32)	(3.81)	(3.77)
*Benefit change	-0.46	2.95	-0.40	1.26	-2.40	-4.68	8.80*	2.75
	(2.56)	(2.66)	(3.21)	(3.45)	(3.36)	(3.51)	(3.92)	(4.07)
Observations	31,362	31,539	32,118	34,182	31,362	31,539	32,118	34,182
Adj R^2	0.195	0.190	0.182	0.163	0.237	0.228	0.219	0.173

Note: The regression sample is restricted balanced sub-samples of boundary areas clients who receive AS support in consecutive years. Standard errors are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Table 16: Change in rent and total accommodation support, by initial rent range

	A: Below initial maximum	B: Between initial and subsequent maximum	C: Above subsequent maximum
Raw change in Rent			
• Year to March 2017	16.49***	3.47***	6.11***
	(0.216)	(0.192)	(0.211)
• Year to March 2018	17.24***	3.26***	7.13***
	(0.23)	(0.20)	(0.22)
• Year to March 2019	19.85***	11.42***	2.87***
	(0.26)	(0.17)	(0.39)
• Year to March 2020	18.78***	4.43***	5.49***
	(0.18)	(0.257)	(0.37)
Raw change in (AS+TAS)			
• Year to March 2017	5.17***	-0.23	-1.99***
	(0.136)	(0.150)	(0.192)
• Year to March 2018	6.14***	1.23***	-2.23***
	(0.15)	(0.15)	(0.20)
• Year to March 2019	10.31***	26.11***	44.66***
	(0.19)	(0.15)	(0.45)
• Year to March 2020	7.54***	-0.51*	-5.27***
	(0.14)	(0.20)	(0.34)

Note: Data are for a balanced panel of clients who received AS support in consecutive years. Allocation to rent ranges is based on location and circumstances at the start of the year. Standard errors are in parentheses.

*** p<0.01, ** p<0.05, * p<0.1.

Table A1: Description of AS-areas after 2018 policy change

Area 1:
Area 1 from 2005:
Central Auckland urban zone; Northern Auckland urban zone
Extended Area 1 (Area 2 from 2005):
Bombay area unit; Clevedon area unit; Glenbrook area unit;Helensville urban area; Helensville South area unit; Hunua area unit; Karekare area unit; Kaukapakapa area unit; Kaukapakapa rural area unit (previously Kaukapakapa area unit); Kingseat area unit Riverhead area unit; Mahurangi area unit (previously Algies Bay – Mahurangi area unit); Muriwai Valley area unit; Muriwai Beach area unit; Parakai Urban area unit (previously Parakai area unit); Patumahoe area unit; Pokeno area unit; Pukekohe urban area; Rewiti area unit; Snells Beach urban area; Southern Auckland urban zone; Tahekeroa area unit; Waiheke Island urban area; Waitakere West area unit; Waiuku urban area; Warkworth urban area; Western Auckland urban zone Tauranga urban area
Arrowtown urban area; Queenstown urban area; Wanaka urban area

Area 2:
Area 2 from 2005:
Awhitu area unit; Great Barrier Island area unit; Islands–Motutapu Rangitoto Rakino area unit; Kaiaua area unit (previously Mangatawhiri area unit); Kawakawa-Orere area unit (Previously Clevedon area unit); Kawau area unit; Leigh area unit; Little Barrier Island area unit; Mangatawhiri area unit; Maramarua area unit; Matakana area unit (previously Cape Rodney area unit); Matheson Bay area unit; Meremere area unit; Moonshine Valley area unit; Nelson urban area; Omaha area unit (Previously Cape Rodney area unit); Onewhero area unit; Otaua area unit; Paparata area unit (Previously Mangatawhiri area unit); Parakai Rural area unit (previously Parakai area unit); Point Wells area unit (previously Cape Rodney area unit); South Head area unit; Tauhoa–Puhoi area unit; Wakefield urban area; Wellington urban zone; Wellsford urban area
Extended Area 2 (Area 3 or 4 from 2005):
Amberley urban area; Ashburton urban area; Blenheim urban area; Brightwater urban area; Cambridge urban zone; Cape Rodney area unit; Cape Rodney South area unit (previously Cape Rodney area unit); Christchurch urban area; Cromwell urban area; Darfield urban area; Hamilton urban zone; Hastings urban zone; Kapiti urban area; Katikati Community urban area; Kerikeri urban area; Leeston urban area; Lower Hutt urban zone; Makara–Ohariu area unit; Mangawhai area unit; Mangawhai Heads urban area; Mapua urban area; Matamata urban area; Motueka urban area; Napier urban zone; New Plymouth urban area; Ngunguru urban area; Oxford urban area; Porirua urban zone; Raglan urban area; Rakaia urban area; Rangiora urban area; Rapaura area unit; Rolleston urban area; Taupo urban area; Te Awamutu urban zone; Te Kauwhata urban area; Te Puke Community urban area; Upper Hutt urban zone; Waihi Beach urban area; Whangamata urban area; Whangarei urban area; Whitianga urban area; Woodend urban area

Area 3:
Area 3 from 2005:
Alexandra urban area; Cloustonville area unit; Dunedin urban area; Feilding urban area; Hanmer Springs area unit (previously called Hanmer Springs urban area); Kaitaia urban area; Kapiti Island area unit; Mana Island area unit; Mangaroa area unit; Maungakotukutuku area unit; Opiki area unit; Otaki urban area; Paekakariki Hill area unit; Paihia urban area; Palmerston North urban area; Pencarrow area unit; Rotorua urban area; Russell area unit (previously called Russell urban area); Taipa Bay-Mangonui urban area; Tairua urban area; Takaka urban area; Thames urban area; Tokomaru area unit; Waihi urban area; Whakatane urban area
Extended Area 3 (Area 4 from 2005):
Carterton urban area; Coromandel urban area; Dargaville urban area; Edgecumbe urban area; Geraldine urban area; Gisborne urban area; Greymouth urban area; Greytown urban area; Hawera urban area; Hokitika urban area; Huntly urban area; Inglewood urban area; Kaikohe urban area; Kaikoura urban area; Kauwhata area unit; Kawakawa urban area; Levin urban area; Martinborough urban area; Masterton urban area; Methven urban area; Morrinsville urban area; Ngatea urban area; Oamaru urban area; Otorohanga urban area; Paeroa urban area; Picton urban area; Pleasant Point urban area; Shannon urban area; Te Aroha urban area; Temuka urban area; Timaru urban area; Tokorangi-hiwinui area unit; Twizel Community urban area; Waikouaiti urban area; Waipawa urban area; Waitara urban area; Winton urban area; Te Kauwhata urban area; Te Puke Community urban area; Upper Hutt urban zone; Waihi Beach urban area; Whangamata urban area; Whangarei urban area; Whitianga urban area; Woodend urban area

Area 4: All other areas

Notes:

Table A2: Description of Boundary contrasts used in the analysis

Boundary contrast:	Description:
Merged boundary contrasts	
21_on_11: areas in Area 2 that merged with Area 1, bordering the previous Area 1	Boundary around previous Central Auckland Area 1
32_on_22: areas in Area 3 that merged with Area 2, bordering the previous Area 2	Includes areas near the Tawa / Porirua boundary;
42_on_22: areas in Area 4 that merged with Area 2, bordering the previous Area 2	Areas on the outskirts of Auckland: around Mangawhai Heads and Kawakawa Bay
43_on_33: areas in Area 3 that merged with Area 3, bordering the previous Area 3	Includes areas around Feilding and Palmerston North
Split boundary contrasts	
21_on_22: areas in Area 2 that split into Area 1, bordering those that stayed in Area 2	Consists of areas around the greater Auckland Area 2 boundary;
32_on_33: areas in Area 3 that split into Area 2, bordering those that stayed in Area 3	Includes areas on the outskirts of the Wellington urban area – from the edges of Hutt Valley, and West to Pukerua Bay
41_on_44: areas in Area 4 that split into Area 1, bordering those that stayed in Area 4	Consists of areas around Tauranga and Queenstown - around The Lakes and Arthur's Point
42_on_44: areas in Area 4 that split into Area 2, bordering those that stayed in Area 4	Includes settlements in Canterbury (Ashburton, Rakaia, Rolleston, Lincoln, Rangiora, Pegasus, Amberley, Darfield, Leeston) and areas around Blenheim, Taupo, Cambridge, Matamata, Mapua, Te Kauwhata, Mangawhai Heads, Ngunguru, and Tutukaka.
43_on_33: areas in Area 4 that split into Area 3, bordering those that stayed in Area 4	Includes areas including or around Winton, Waikouaiti, Oamaru, Twizel, Timaru, Temuka, Pleasant Point, Geraldine, Methven, Hokitika, Greymouth, Kaikoura, Picton, Martinborough, Greytown, Carterton, Masterton, Levin, Shannon, Palmerston North, Feilding, Waipawa, Hawera, Ingelwood, Waitara, Gisborne, Edgecumbe, Otorohanga, Huntly, Morrinsville, Te Aroha, Paeroa, Waihi, Coromandel, Dargaville, Kaikohe, and Moerewa.
Miscellaneous boundary contrasts	
21_on_44: areas in Area 2 that split into Area 1, bordering those in Area 4that stayed in Area 4	Includes Tauranga; and areas around Queenstown, Arrowtown, and Wanaka
32_on_44: areas in Area 3 that split into Area 2, bordering those in Area 4that stayed in Area 4	Includes areas around Christchurch, Leeston, Lincoln, Darfield, Rolleston, Rangiora, Motueka, Blenheim, Waikanae, Napier, Hastings, New Plymouth, Taupo, Hamilton, Te Puke, Matamata, Katikati, Waihi Beach, Raglan, Whangamata, Whitianga, Whangarei, and Kerikeri.

Notes:

Table A3: Sample characteristics – Central Auckland (21_on_11) boundary

	All	Treatment-side		Control-side		DiD
	Obs	Pre-2018	Post-2018	Pre-2018	Post-2018	(p-value)
No. Monthly Obs	274,656	61,614	63,489	74,187	75,372	
No. Individuals	17,130	5,262	5,388	6,405	6,489	
Female	0.573	0.598	0.590	0.562	0.549	0.004 (0.64)
Age	46.7 (16.7)	46.4 (17.1)	46.8 (17.0)	46.4 (16.2)	47.0 (16.5)	-0.163 (0.60)
European	0.405	0.437	0.438	0.376	0.380	-0.003 (0.72)
Māori	0.215	0.209	0.213	0.215	0.223	-0.003 (0.70)
Pacific	0.277	0.258	0.267	0.289	0.291	0.007 (0.44)
Asian	0.247	0.238	0.236	0.260	0.250	0.008 (0.28)
Other ethnicity	0.039	0.040	0.037	0.040	0.039	-0.002 (0.58)
Family size	1.98 (1.3)	2.04 (1.3)	2.03 (1.3)	1.98 (1.3)	1.90 (1.3)	0.062 (0.02)
Partnered	0.259	0.268	0.267	0.259	0.246	0.012 (0.14)
No. children	0.72 (1.1)	0.78 (1.1)	0.76 (1.1)	0.72 (1.1)	0.66 (1.1)	0.050 (0.03)
Single adult	0.489	0.450	0.466	0.497	0.532	-0.020 (0.04)
Couple adults	0.122	0.131	0.131	0.116	0.114	0.002 (0.79)
Sole parent, 1 child	0.130	0.141	0.135	0.127	0.121	0.000 (0.98)
Sole parent, 2+children	0.121	0.141	0.132	0.117	0.101	0.007 (0.28)
Couple with children	0.137	0.137	0.136	0.143	0.132	0.011 (0.13)
On main benefit:	0.721	0.718	0.712	0.731	0.721	0.004 (0.64)
JSS	0.274	0.237	0.258	0.287	0.304	0.005 (0.55)
SLP	0.129	0.132	0.119	0.139	0.127	-0.001 (0.86)
SPS	0.135	0.159	0.141	0.133	0.111	0.005 (0.46)
NZS/VP	0.153	0.156	0.162	0.140	0.155	-0.010 (0.13)
Non-beneficiary	0.279	0.282	0.288	0.269	0.279	-0.004 (0.64)
Treatment-side of Bdy	0.455	1	1	0	0	
Financial variables:						
At AS-max	0.316	0.540	0.207	0.325	0.215	-0.222 (0.00)
Received TAS	0.314	0.338	0.310	0.297	0.316	-0.047 (0.00)
Has non-benefit income	0.353	0.370	0.370	0.339	0.340	-0.001 (0.88)
Rent	334.28 (126.7)	320.99 (117.8)	350.14 (133.4)	321.57 (119.4)	344.25 (132.2)	6.47 (0.01)
AS received	133.13 (60.5)	102.49 (39.2)	151.50 (69.8)	127.67 (50.1)	148.05 (65.0)	28.63 (0.00)
TAS received	17.65 (32.5)	23.96 (38.5)	16.16 (31.21)	16.00 (31.2)	15.35 (29.2)	-7.16 (0.00)
Total Acc support	150.77 (76.2)	126.45 (63.3)	167.66 (84.9)	143.67 (67.7)	163.40 (79.7)	21.47 (0.00)
Non-benefit income	204.57 (340.4)	195.78 (316.6)	217.88 (354.7)	194.80 (330.3)	210.14 (355.9)	6.75 (0.33)
Benefit income	433.00 (249.8)	406.50 (241.2)	461.44 (262.4)	416.46 (236.0)	446.94 255.5)	24.46 (0.00)
Total income	637.56 (255.4)	602.28 (221.9)	679.31 (268.4)	611.26 (243.4)	657.08 (273.9)	31.20 (0.00)

Notes: see text and notes to Table 2 for details.

Table A4: Sample characteristics – Tenancy bond new tenancies, April 2010 - December 2020

	All	Treatment-side		Control-side	
		Pre2018	Post2018	Pre2018	Post2018
		(a) All Boundary contrasts			
No. Monthly Observations	125,577	39,219	49,017	16,278	21,063
No. bedrooms	2.50	2.47	2.56	2.44	2.49
	(1.2)	(1.2)	(1.2)	(1.3)	(1.3)
0-1 bedroom	0.205	0.204	0.179	0.247	0.234
2 bedrooms	0.210	0.207	0.211	0.208	0.214
3 bedrooms	0.394	0.413	0.419	0.338	0.342
4+ bedrooms	0.191	0.175	0.191	0.208	0.209
Property Management Co.	0.523	0.528	0.542	0.474	0.507
Private landlord	0.450	0.439	0.440	0.480	0.470
Public landlord	0.025	0.031	0.015	0.045	0.022
House	0.677	0.689	0.701	0.629	0.638
Flat	0.132	0.131	0.104	0.193	0.149
Apartment	0.055	0.053	0.060	0.039	0.059
Boarding house	0.037	0.031	0.027	0.053	0.057
Treatment-side	0.703	1	1	0	0
Average rent	450.36	407.30	460.50	445.17	510.94
	(203.1)	(195.2)	(193.2)	(201.2)	(222.4)
		(b) Central Auckland boundary			
No. Monthly Observations	26,079	5,496	6,840	6,123	7,623
No. bedrooms	2.23	2.20	2.35	2.12	2.22
	(1.3)	(1.3)	(1.3)	(1.3)	(1.3)
0-1 bedroom	0.303	0.298	0.250	0.344	0.319
2 bedrooms	0.239	0.243	0.254	0.228	0.233
3 bedrooms	0.297	0.307	0.322	0.279	0.281
4+ bedrooms	0.161	0.151	0.175	0.148	0.167
Property Management Co.	0.544	0.515	0.533	0.539	0.579
Private landlord	0.399	0.417	0.426	0.374	0.380
Public landlord	0.055	0.062	0.039	0.086	0.041
House	0.565	0.575	0.625	0.515	0.546
Flat	0.175	0.176	0.151	0.218	0.159
Apartment	0.046	0.048	0.054	0.033	0.050
Boarding house	0.090	0.077	0.050	0.111	0.117
Treatment-side	0.473	1	1	0	0
Average rent	488.16	474.40	524.76	446.20	498.76
	(212.8)	(206.9)	(200.6)	(211.8)	(221.6)

Notes: The sample consists of new tenancies in each calendar month between April 2010 and December 2020.

Table A5: Regression results – sample of clients in first and final years

Policy-effect:	(1)	(2)	(3)	(4)	(5)
		(a) Weekly rent regressions			
Post2018	2.19*				
	(1.33)				
2017/18		1.48	-0.76	0.33	-0.15
		(1.33)	(0.97)	(0.67)	(0.59)
2018/19		2.98*	0.20	1.15	0.56
		(1.68)	(1.33)	(0.97)	(0.85)
2019/20		2.92	0.61	0.70	0.77
		(1.93)	(1.59)	(1.19)	(1.05)
R-squared	0.078	0.080	0.475	0.931	0.967
		(b) Weekly total accommodation support			
Post2018	14.72***				
	(0.94)				
2017/18		-1.10	-1.66***	-1.00*	-1.15**
		(0.77)	(0.64)	(0.55)	(0.57)
2018/19		13.64***	12.41***	13.33***	12.54***
		(1.12)	(1.00)	(0.88)	(0.95)
2019/20		14.71***	13.59***	13.55***	13.24***
		(1.26)	(1.14)	(0.10)	(1.05)
Year effects	X	Y	Y	Y	Y
Controls	X	X	Y	Y	Y
Client fixed effects	X	X	X	Y	Y
R-squared	0.121	0.121	0.389	0.850	0.884
No. Observations	647,670	647,670	647,670	647,670	459,141

Notes: The sample is restricted to AS-clients who appear in both the first year (2016/17) and final year (2019/20). The specification in column (1) includes dummy variable controls for 'merged', 'split' and 'miscellaneous' boundary types, treatment-groups for each type, and Post2018; column (2) replaces the Post2018 dummy with year-specific controls for 2017/18, 2018/19, and 2019/20. Controls in columns (3)-(5) include boundary-specific controls and treatment-side effects, boundary-specific interactions with each year control; and controls for gender, age, ethnicity, benefit type, and family structure. Standard errors are in parentheses, adjusted for clustering at the AS-client level.

*** p<0.01, ** p<0.05, * p<0.1.

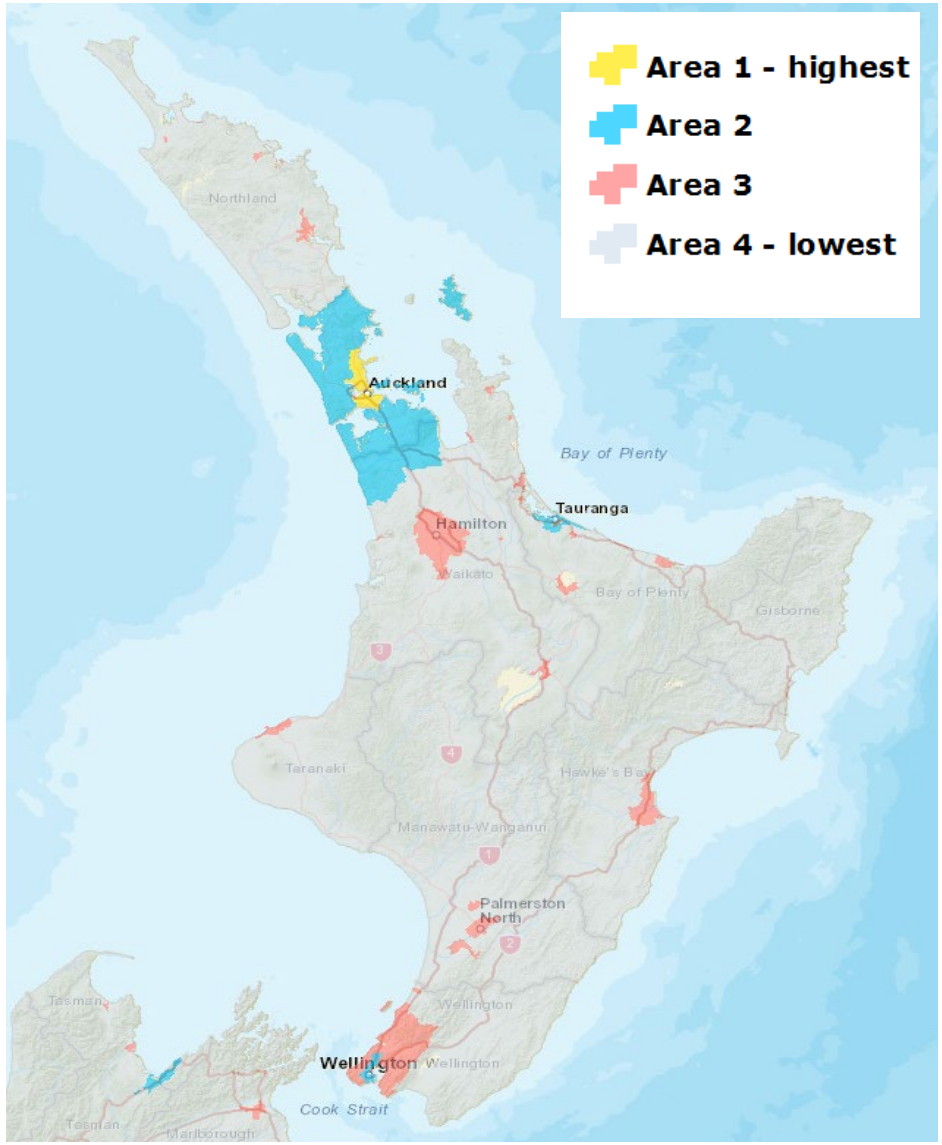
Table A6: Regression results – samples of AS-boarder and AS-homeowner recipients

	AS-Boarders			AS-Homeowners		
Policy-effect:	(2)	(3)	(4)	(2)	(3)	(4)
	(a) Weekly accommodation cost regressions					
2017/18	0.50	1.49	-0.08	5.78	7.11**	0.49
	(1.34)	(1.11)	(0.66)	(3.52)	(3.15)	(1.46)
2018/19	-1.85	-0.55	-0.77	20.11***	18.35***	-1.80
	(1.76)	(1.47)	(1.04)	(5.15)	(4.75)	(2.54)
2019/20	-1.54	-1.47	-0.94	23.01***	16.61***	-2.65
	(1.88)	(1.61)	(1.31)	(6.14)	(5.62)	(2.79)
R-squared	0.034	0.320	0.938	0.083	0.327	0.909
	(b) Weekly total accommodation support					
2017/18	0.28	0.47	-0.05	0.19	-0.51	-0.39
	(0.51)	(0.49)	(0.32)	(1.40)	(1.30)	(0.98)
2018/19	-0.41	-0.27	-0.24	17.23***	15.81***	12.87***
	(0.69)	(0.67)	(0.51)	(2.36)	(2.22)	(1.78)
2019/20	-0.40	-0.52	-0.42	19.22***	17.28***	13.14***
	(0.75)	(0.72)	(0.63)	(2.55)	(2.43)	(2.05)
R-squared	0.029	0.094	0.900	0.103	0.279	0.872
No. Observations	406,059	406,059	406,059	226,209	226,209	226,209

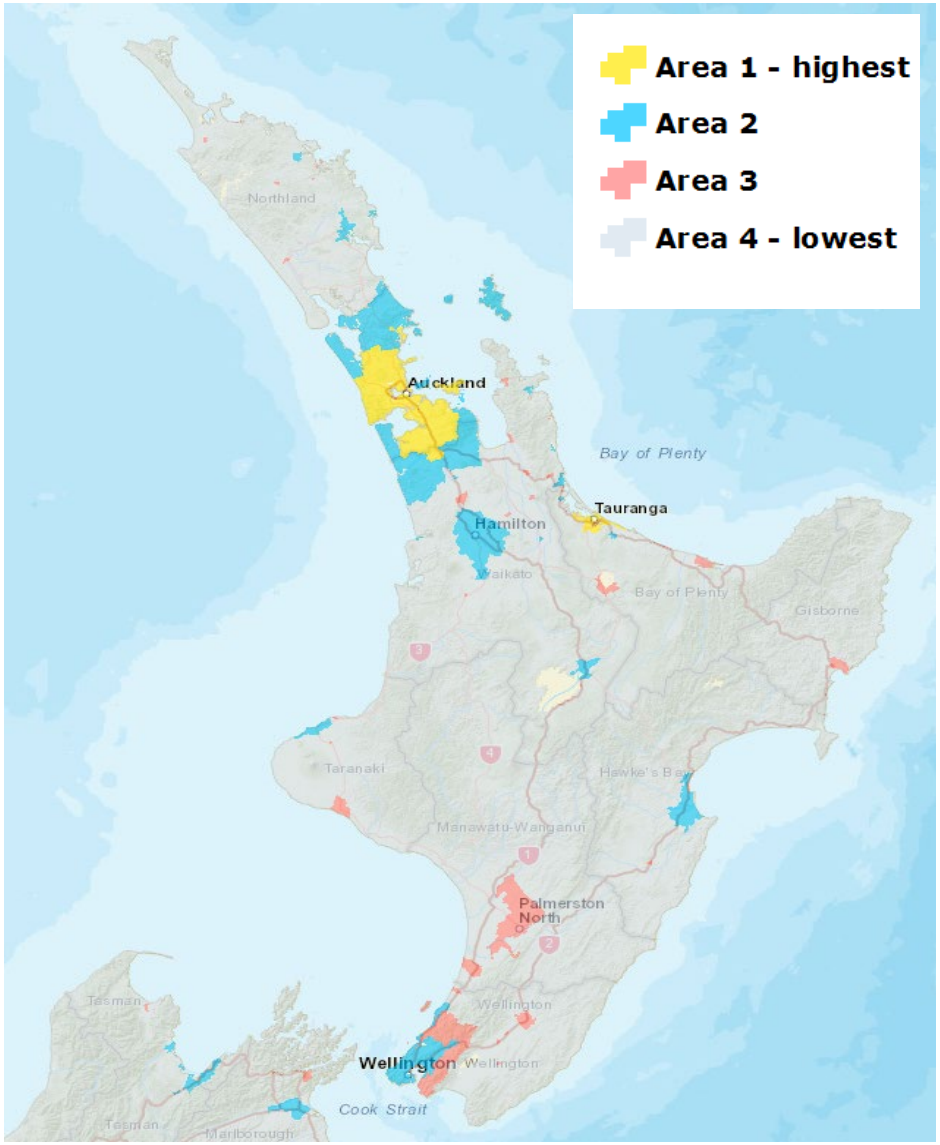
Notes: The samples in the first three columns as AS-Boarder clients; and in the final three columns AS-Homeowner clients. Column labels "(2)", "(3)" and "(4)" correspond to specifications in those columns in Table 3. Standard errors are in parentheses, adjusted for clustering at the AS-client level.
*** p<0.01, ** p<0.05, * p<0.1.

Figure A1: Accommodation Supplement areas – pre- and post-2018

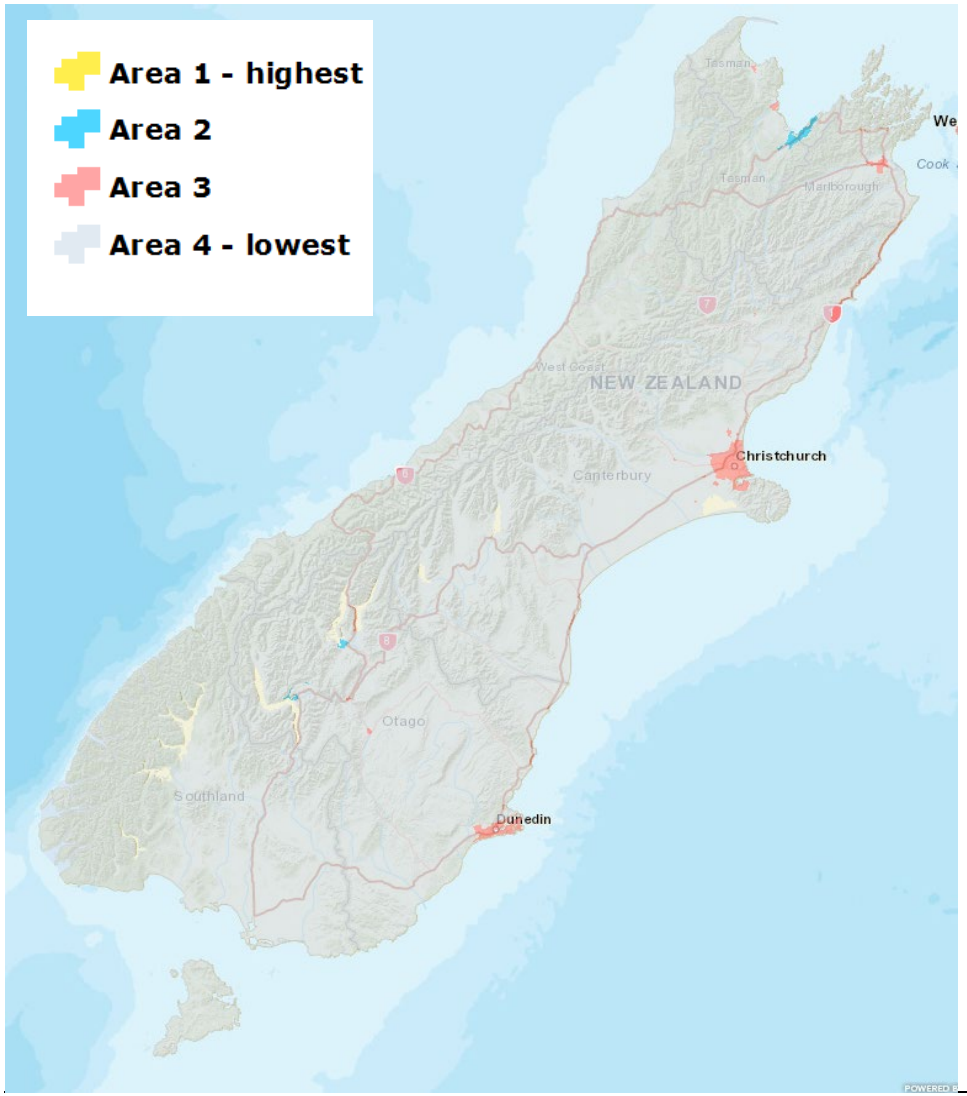
North Island, Pre-2018



North Island, Post-2018



South Island, Pre-2018



South Island, Post-2018

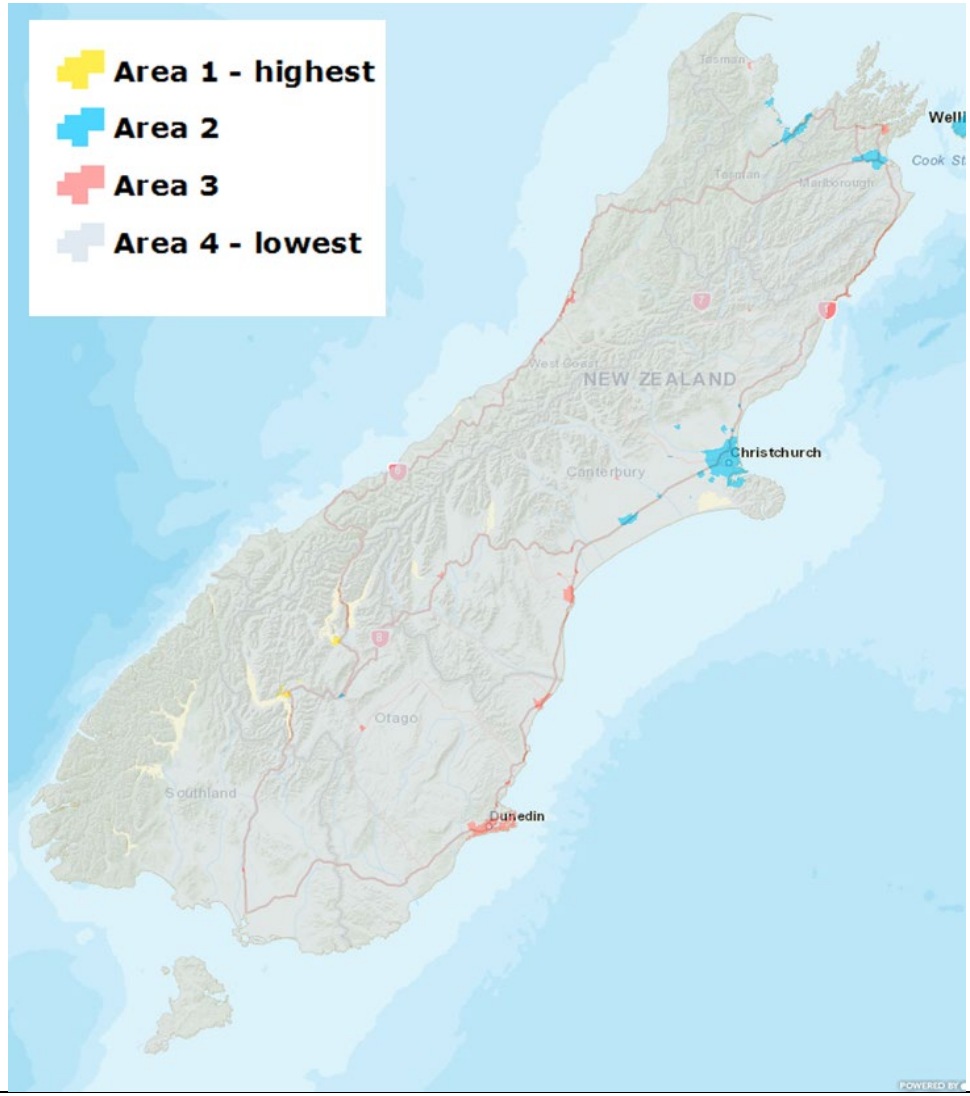
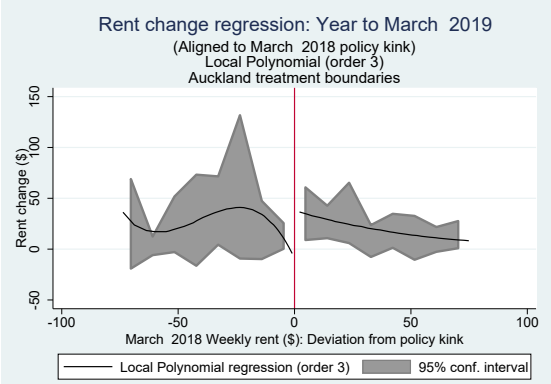
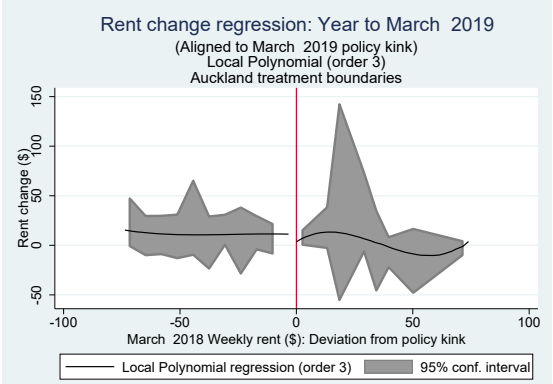


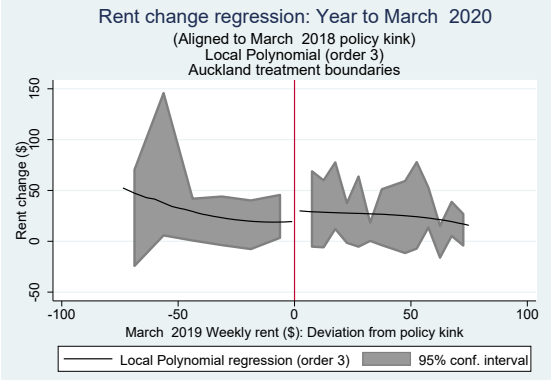
Figure A2: Rent change at policy kink points – Auckland boundary sample

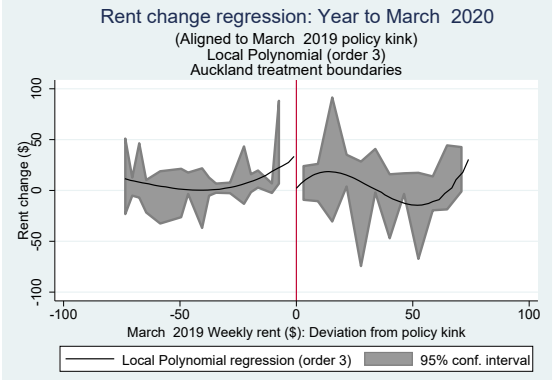
Kink pre-policy change (K1)	Kink at post-change max (K2)

a) Rent change in Treatment areas: Year to March 2019 and year to March 2020

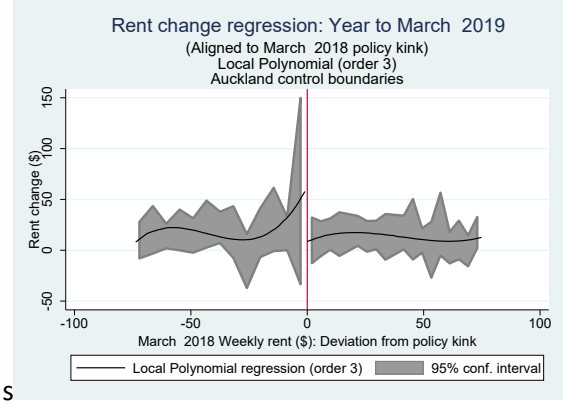


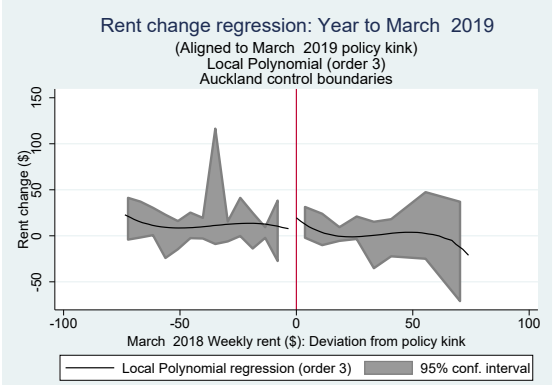






b) Rent change in Control areas: Year to March 2019 and year to March 2020





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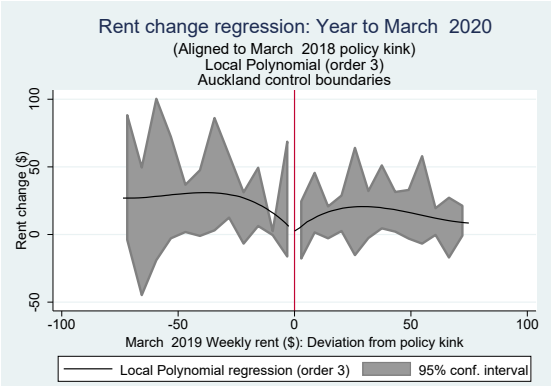
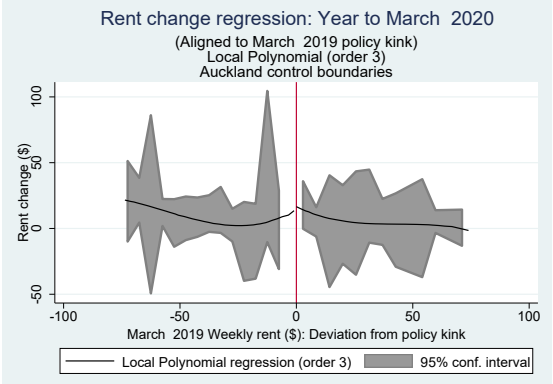


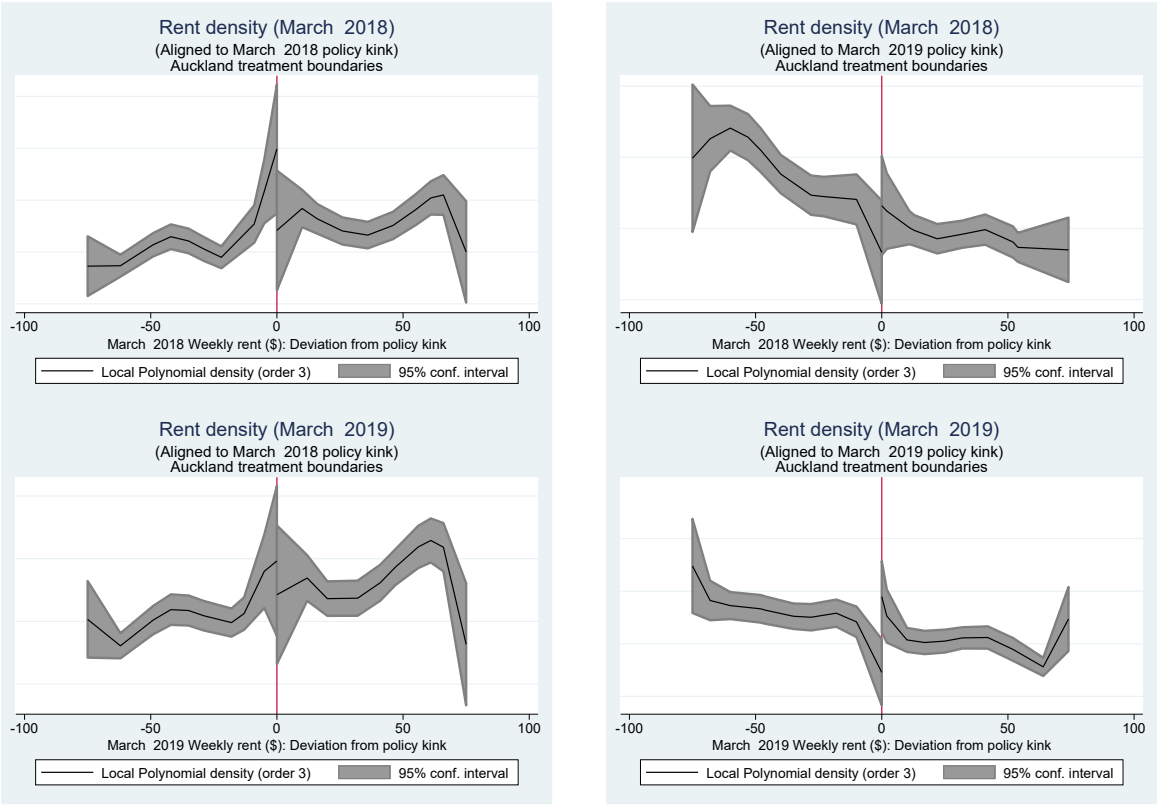


Note: Figures are based on data for people receiving AS support in two consecutive March months. Graphs are displayed for a rent range of $75 above or below the policy kink.

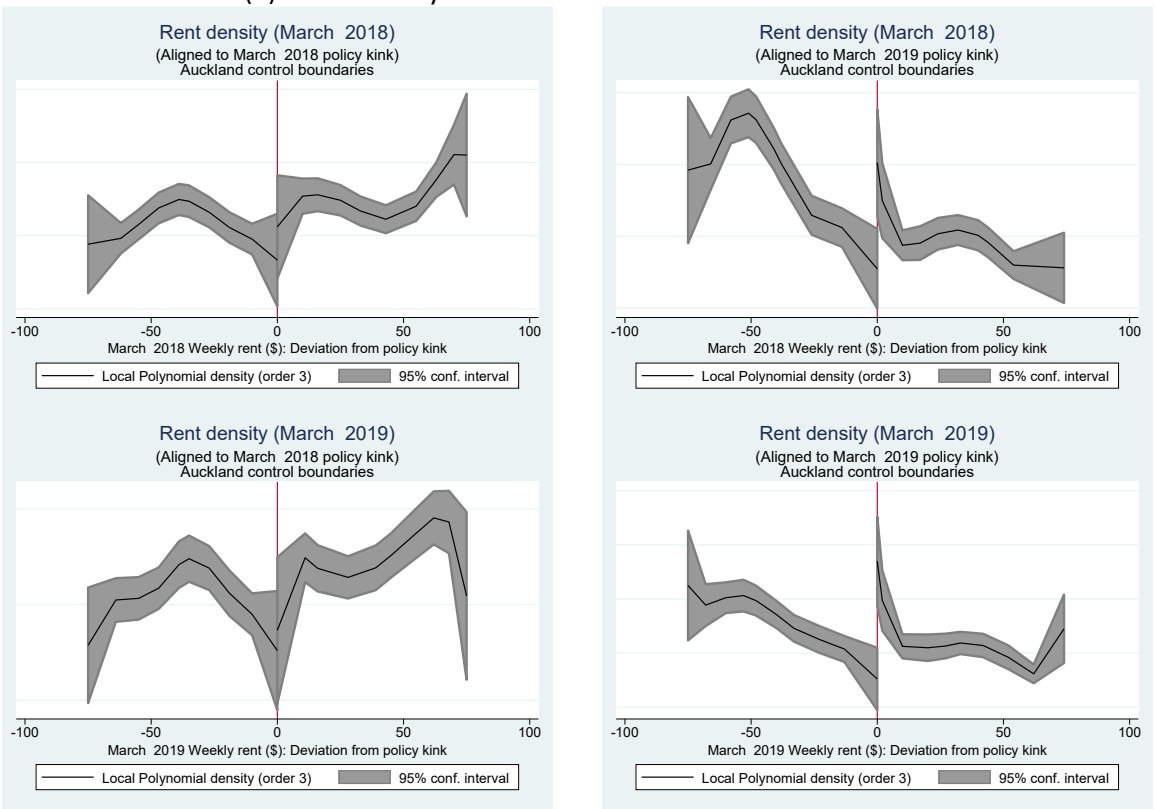
Figure A3: Rent density at policy kink points – Auckland Boundary sample

Kink pre-policy change (K1)	Kink at post-change max (K2)

(a) Rent density in Treatment areas: March 2018 and March 2019



(b) Rent density in Control areas: March 2018 and March 2019



Note: Figures are based on data for people receiving AS support in March of the year for which the density is drawn and also in the following March. Graphs are displayed for a rent range of $75 above or below the policy kink.