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The impact of the 2018 Families Package Winter Energy Payment policy

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Abstract

This paper analyses the effects of the Winter Energy Payment (WEP), that was introduced as part of the 2018 Families Package. The WEP amounts to a relatively small fraction of receiving households' income and total expenditure (nearly 7% of main benefit support on average, 5% of total income support, and about 4% of total household income and expenditure); but is a substantial fraction of energy expenditures (120% on average, and 60% median). We focus on four sets of analyses: the WEP effects on recipient expenditure patterns (particularly on power) and self-report measures of wellbeing; whether WEP affected health outcomes, as measured by hospitalisations; the financial incentive of WEP to be on a main benefit during the winter months; and whether WEP had any effect on the receipt of hardship grants. Our analyses find predominantly statistically insignificant effects of the WEP across each of these outcomes, either because the effect sizes or the samples are relatively small, making it difficult to draw definite conclusions. However, the direction of estimated effects are generally suggestive that the WEP caused recipient households to increase their expenditures on electricity and power, alleviated material hardship and improved wellbeing, and positively affected health outcomes. We find little evidence of any increase in benefit receipt in response to the increased financial incentives of the WEP to be on-benefit.

JEL codes

H24, H53, I14, I38

Keywords

Heating, benefits, energy expenditures, health, hardship, employment

Summary haiku

WEP has mild effects
on power, health, wellbeing
weakly positive

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1 Introduction

The Winter Energy Payment (WEP) was introduced as part of the 2018 Families Package, to help older New Zealanders and poorer working-age beneficiary families to heat their homes during the winter months, with the aim to improve their health outcomes. The WEP was first available in 2018 for 13 weeks from 1 July; and in subsequent years it was available for 22 weeks from 1 May. It is universally available to New Zealand Superannuation and Veteran's Pension (NZS/VP) recipients and to working age main benefit (WAB) recipients.[1] Other policy changes in the Families Package included increases in the maximum Accommodation Support (AS) payments from 1 April; increases to the Working For Families Family Tax Credit (FTC) amounts, abatement thresholds and rates from 1 July; and the introduction of the Best Start Tax Credit (BSTC) for families with new born children after 1 July.[2]

In this paper, we analyse the effects of the WEP along several distinct dimensions. To the extent that the WEP achieves its purpose of improving the health and wellbeing of recipient families, the policy's expected intervention logic is that it increases the home heating and energy expenditures of recipients. Our first focus is to analyse the effects of the WEP on expenditure patterns, and in particular energy expenditure, using data from the Household Economic Survey (HES). Although the policy has an "energy payment" label, it is an unconditional cash transfer to recipients. In the absence of any labelling effect, standard economic theory implies the only effect of the WEP on expenditure patterns should be via its increase in income. As energy accounts for about 5% of households' total expenditure, only a small increase in spending on heating would be expected, and would perhaps be moderated further by an income effect if the energy share of expenditure diminishes with income. Thus, an important question is whether the WEP has a labelling effect that increases the share of income spent on energy beyond this predicted amount.[3] In order to assess the effects on subjective wellbeing, we also analyse the effects of the WEP on self-reported material wellbeing using HES data.

Our second analysis focuses on whether the WEP had substantive effects on improving the health of recipient families. This analysis uses data on hospitalisations from the Ministry of Health matched to survey data from the Household Labour Force Survey (HLFS), to identify the

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[1] Since the NZ Superannuation and Veteran's Pension eligibility requirements are very similar, we will combine these into a single group.

[2] See Arnesen and Wilson (2019) for more background information on the Families Package. Related research papers provide more detailed analyses of the effects of the Families Package on family incomes (Wilson & McLeod, 2021), the effects of Accommodation Supplement policy changes (D. R. Hyslop & Maré, 2022), and the financial incentive effects associated with the Family Tax Credit policy changes (Riggs et al., forthcoming).

[3] Analysing the labelling effect of a similar cash transfer payment, the UK Winter Fuel Payment (WFP), Beatty et al. (2014) estimate that recipient households spend nearly half of the WFP on fuel in contrast to a predicted 3% expenditure share.

demographic and family characteristics, and welfare benefit receipt data from the Ministry of Social Development (MSD).

Because the WEP eligibility requires receipt of either a main working age benefit or NZS/VP, the policy increases the financial incentives to be on-benefit. To the extent this occurs, it creates an unintended consequence of the policy, and may adversely affect the health outcomes of the recipients. Our third analysis focuses on whether there is any evidence of responses to these financial incentives. As the incentives are to be on-benefit rather than out-of-employment *per se*, we primarily focus on the effects on the main-benefit status of working age recipients, but also consider whether there is any evidence of a drop in earnings in order to maintain benefit eligibility. This analysis uses administrative data from the IRD's Employer Monthly Schedules (EMS) matched to data from the Household Labour Force Survey (HLFS).

Finally, we examine whether the WEP had any effect on the receipt of hardship grant payments. Although the receipt of WEP is excluded from assessable income for additional supplementary welfare assistance (The Treasury, 2018), such receipt nonetheless may reduce the need for additional hardship support by recipients. Alternatively, if potential recipients responded to the financial incentives of the WEP to be on-benefit, thus reducing their incomes, the policy may have had the unintended effect of increasing the incidence of hardship receipt.

The research primarily uses alternative difference-in-difference (DiD) type analyses of WEP recipients versus non-recipient groups, allowing separate effects for WAB and NZS recipients. In particular, the analyses of the WEP effects on expenditure patterns, wellbeing, health outcomes, and receipt of hardship grants will use triple-difference (DDD) approaches, in which we compare the outcomes of eligible and non-eligible recipients, in the WEP winter versus non-winter months, before and after the WEP policy was introduced. Such DDD approaches allow for the outcomes to vary across each of these three dimensions, as well as between the three two-way interactions, and attributes any remaining differences to the treatment effects of the WEP policy. For the analysis of the financial incentives of the WEP to be on-benefit, we adopt a standard DiD approach, and compare the outcomes in winter versus non-winter months, before and after the introduction of the WEP policy. Because the WEP policy is available only during certain months of the year (July to September in 2018, May to September in later years), in contrast to other support policies, the triple-difference methodology is robust to other policy changes over the period (in particular, to the other Families Package changes).

We begin by documenting the magnitude of the WEP relative to recipients' incomes and expenditures in the HES expenditure sample. WEP amounts to nearly 11% of working age beneficiaries' main benefit, and 6% of NZS/VP payments, and somewhat less relative to recipients' total income support (5% across all recipients) and total income (4%). Furthermore, weekly WEP amount received by recipients during the winter months is about 4% of their total weekly expenditure on average, although 120% of their energy expenditure (median of 60%). This suggests the WEP provides a relatively small unconditional cash transfer treatment, but substantial when viewed as an energy subsidy. This suggests any policy effects may be relatively small and difficult to detect statistically given the sample sizes available for the analyses.

Our results across each of these dimensions are generally statistically relatively imprecise, due to a combination of small effect sizes and small samples. Nonetheless, the point estimates are generally consistent with the WEP increasing spending on power, and having positive, if weak, effects on health and wellbeing outcomes. We also find similar suggestive evidence of an increase in working-age benefit receipt and lower earnings in the winter months in response to the WEP financial incentives, and some evidence of a reduction in hardship assistance to working age benefit recipients. The imprecise full sample effects, together with the small subsamples for population subgroups of interest, in particular for Māori and Pacific peoples, precludes the value of presenting results for these populations.[4]

The rest of the paper is organised as follows. In the next section, we discuss the background to the WEP policy and related international and domestic research literature. We then present each of our analyses in turn: section 3 contains our analysis of the effects of the WEP on energy expenditures and material wellbeing from the HES; in section 4 we discuss the effects on health outcomes; section 5 focuses on the financial incentives for working-age people to be on-benefit; and in section 6 we analyse the effects on receipt of hardship grants. The paper concludes with a summary discussion in section 7.

2 Background and literature

In this section we provide some background to the Winter Energy Payment policy, and then discuss relevant international and New Zealand literature.

2.1 Background

The Winter Energy Payment was introduced as part of the Families Package and was first available in July 2018. In 2018 payments were made for the three months until the end of September; since 2019, payments are made for the five months from the start of May until the end of September. The WEP is a non-taxable, non-income tested benefit available to working

[4] For example, as Māori typically make up 15-20% of our samples, the sampling errors more than double. For Pacific peoples, making up less than 10% of the samples, the loss of precision is even greater.

age individuals on main benefits (WAB), and New Zealand Superannuation or Veteran's Pension (NZS/VP) recipients, to help recipients meet their household heating costs during the winter months. Eligibility lapses for those who leave NZ for more than 28 days during the winter payment period. Payments to eligible participants vary by family type. In 2018 and 2019, single participants without dependent children were paid $20.46 a week ($450 over 5 winter months from 2019), whereas coupled participants (married, in a civil union or a de facto relationship with or without dependent children) and sole parents received $31.82 a week ($700 over 5 winter months). The annual cost of the WEP in 2019 was approximately $450m.[5]

The purpose of the WEP is to help with the cost of heating homes over the winter, with the objectives being to improve home heating and related health outcomes of recipients. Previous research has shown the link between cold homes and poor health outcomes, with much of the health burden from these cold indoor temperatures falling on the elderly (World Health Organization, 2018). Based on systematic reviews of the scientific evidence, the *WHO Housing and health guidelines* recommend that 18°C is a minimum safe indoor temperature during cold seasons to protect health. Many New Zealand homes do not appear to comply with this standard. According to the 2014/2015 NZ General Social Survey, 21% of individuals, on average, live in houses that are often or always cold in winter, and these results varied greatly depending on the demographics of the household (e.g., 35% of renters, 43% of Pacific peoples, and 33% of single-parent families reported that their homes are often or always cold in winter) (Statistics New Zealand, 2015). While these estimates are based on self-reports, they likely underestimate the true extent of the problem. Results from the BRANZ 5th Housing Condition Survey (White & Jones, 2017) showed that 46% of households did not heat any occupied bedrooms at night and only 15% of bedrooms were heated overnight.[6] BRANZ also measured temperatures in bedrooms across the country and found that 84% of bedrooms were below 18°C – the temperature recommended by the *WHO Housing and health guidelines* – between 11pm and 9am (Plagmann, 2018).

Homes being too cold may also lead to damp or mould, which have both been linked to additional health problems (World Health Organization, 2018). In fact, one BRANZ report suggests that many homes would experience far fewer periods of high relative humidity if they were heated to a minimum of 18°C throughout (Pollard, 2018). Hence, it is important to recognise that heating homes can reduce health problems associated with cold as well as

[5] As part of the Government's Covid-19 response package announced in March 2020, the 2020 rates were doubled to $900 for singles and $1,400 for couples over the winter months (May – September). In May 2021, the rates returned to the 2019 levels.
[6] Heating bedrooms overnight is important for preventing health problems, especially for children.

reducing health problems associated with damp and mould. In New Zealand, damp and mouldy homes are a problem – assessors for the BRANZ 5th Housing Condition Survey reported visible mould in 49% of houses (White et al., 2015).

2.2 Literature review

We now provide a review of relevant research for the effects of the WEP on various outcomes of interest. These include, first, whether such payments may lead to increases in expenditure on home heating by recipient households, and the subsequent effects on home heating; and then on more substantive health, and wellbeing and hardship outcomes of interest.

The UK has a similar winter energy payment policy, called the Winter Fuel Payment (WFP). The WFP was introduced in 1997 for households including elderly people. It was initially a means-tested policy targeting low-income households with an individual aged over 60. In 2000 the WFP dropped means-testing and provided universal entitlement for all households with anyone over 60, with a £250 lump-sum payment made in November or December to households with the oldest person aged 60-80, and £400 for those with a person aged 80 or over. Subsequent changes reduced the annual payments rates to £200 and £300 from 2011; and from 2010, the eligibility-age was tied to that of the female state pension age, which is scheduled to gradually increase from 60 in 2010 to 68 in 2046.

There have been several research papers analysing the effects of the UK WFP on home fuel expenditure, indoor temperatures, and health-related outcomes. We briefly discuss three here, each of which uses a regression discontinuity design (RDD), with eligibility for the support being based on the age of the eldest person in the household – i.e., below/above 60 up until 2010. First, Beatty et al. (2014) focused on the effect of the WFP on household fuel expenditures, using data from a household-level expenditure survey (the Living Costs and Food Survey, LCFS), which is comparable to Statistics NZ's Household Economic Survey (HES). Their primary research focus was to test whether such a fuel-labelled cash payment had behavioural effects that increased spending on fuel relative to a simple 'unlabelled' cash payment. Beatty et al. estimate that nearly half (47%) of the WFP was spent on home-fuel expenditures on average; which compares to an expected 3% associated with 'unlabelled' income.

Second, Crossley and Zilio (2018) examined the effects of WFP on alternative health outcomes of recipients, using data from the Health Surveys for England (HSE), the Scottish Health Surveys (SHeS), and the English Longitudinal Study on Ageing (ELSA). They estimated that eligibility for WFP at age 60 led to a large and statistically significant 6 percentage point (pp) drop in the incidence of serum fibrinogen (considered to be a marker of current infection and associated with chronic pulmonary disease), from a 12% base (implying a 50% reduction). Crossley and Zilio also found evidence of positive health effects on other disease markers (including self-reported chest infections and hypertension), although with less statistical significance and robustness. They also find significant declines in an aggregate 'poor health index' associated with the WFP, with larger effects associated during colder months and for those with lower education, but not for those with lower incomes.

Third, Angelini et al. (2019) analysed the effects of the WFP on home indoor temperatures and health outcomes, using data from the ELSA for individuals aged over 50. Their analysis found no consistent evidence that receipt of the WFP resulted in higher indoor temperatures or health outcomes, although they caveat that the sample sizes were perhaps too small to detect statistically significant small effects.

Results from the Warm-Up New Zealand: Heat Smart programme indicated that households in the programme used much of the energy savings from their newly installed heating and insulation to increase the temperatures in their homes rather than reduce their energy payments (Grimes et al., 2011). This indicates that people wanted warmer homes but were unable to afford them. Receiving insulation as part of the programme was also found to lower hospitalisation costs, but especially for respiratory illness and asthma, as well as costs for pharmaceutical dispensations. Moreover, the results were even stronger for households with Community Services card holders (Telfar Barnard et al., 2011). The programme was also found to reduce mortality for people aged over 64 who had been hospitalised previously with circulatory illness (Telfar Barnard et al., 2011).

Other research of relevance for effects of the WEP on material wellbeing includes an extensive literature on the relationship between income and material hardship. From a detailed study, Mayer and Jencks (1989) concluded there is a complicated relationship between income and hardship. On one hand they estimate that family income explains a relatively small fraction (14%) of the variance in material hardship, and broader measures of economic resources explain little more. However, they also find that both "permanent" and transitory income affect hardship[7]; and further, that current income shocks have persistent effects on hardship, while future (transitory) income has no direct effect on current hardship.[8]

Using panel data, both Iceland and Bauman (2007) and Sullivan et al. (2008) conclude there is a stronger relationship between hardship and "persistent" income current (annual) income. For example, Iceland and Bauman (2007) conclude the number of spells, length of time

[7] Mayer and Jencks measure permanent income as the average across two years, and transitory income as the current year's income.

[8] The latter suggests families have little ability to avoid hardship by borrowing against future income.

and depth of poverty are strongly correlated with hardship. Similarly, Sullivan et al. (2008) estimate the elasticity of hardship with respect to "permanent" income is -0.34;[9] while, controlling for persistent income, there is only a statistically weak relationships between transitory income changes and most hardship domains, except food insufficiency (elasticity = -0.11). In a more recent study, Loopstra (2018) concludes that cash transfer and food subsidy programmes would likely be effective in reducing food insecurity and improving health outcomes.

Finally, we are aware of one preliminary analysis of the effects of the WEP on Special Needs Grants (SNG), conducted by MSD staff. In this study, Weatherall et al (2019) find that the number of weekly SNGs relative to the number of main beneficiaries fell by between 0.2 and 0.4 percentage points during the 2018 WEP period relative to the previous year.[10]

2.3 Focus of analysis

Our analysis of the effects of the Winter Energy Payment focuses on four distinct sets of outcomes. These analyses are briefly described here and covered in depth in each of the following sections. For the various outcomes, we use a combination of survey and administrative data in Statistics NZ's Integrated Data Infrastructure (IDI). A limitation of using survey data for this analysis is the generally small sample sizes available and the timing of the surveys in relation to the introduction of WEP. For each of the analyses outlined below, we produced descriptive results and applied the research design where feasible.

First, we analyse the impacts of WEP on energy expenditures and material wellbeing using data from the Household Economic Survey (HES) to see if households appear to be spending on the additional income from WEP on warming their homes. If this is not the case, it makes the link between any potential health effects and WEP payments via warmer homes more tenuous. Of course, the additional income could be spent in other health-improving ways (e.g., purchasing more efficient heaters, installing insulation, purchasing healthier foods).

Second, we analyse the WEP impacts on hospitalisations and other measurable wellbeing outcomes for a sample of households from the Household Labour Force Survey (HLFS) matched to health and hospitalisation data from the Ministry of Health (MOH). Our primary outcome of interest is whether individuals were hospitalised for housing related reasons. In addition, we

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[9] Sullivan et al.'s (2008) analysis is based on a 6-year panel of income and material hardship from the Women's Employment Study (WES), and they measure permanent income as the average household income over the period.

[10] Based on a main benefit population of 300,000, this translates to 600–1,100 fewer weekly grants, or about 4-8% of the total number of weekly grants. They also show there is a sharp drop in the number of SNGs at the start of the WEP period, and a sharp increase in SNGs at the end of the WEP period, on the order of 10-15%.

consider the length of stay in hospital associated with such events, and also the cost of the hospitalisation event.

Third, we analyse whether there is any evidence of behavioural responses associated with the incentive effects of WEP, in terms of being on- versus off-benefit, using data from the HLFS matched to administrative records of employment and benefit receipt from IR and MSD. Finally, we examine whether WEP had any effect on the receipt of hardship assistance from MSD, in the form of Temporary Assistance Support (TAS) or lump-sum Hardship Grants (HG).

Our research focuses mainly on the effects during the 2018 and 2019 years, partly because of the Covid-19 disruptions in 2020 and partly due to data availability.[11] In 2018, there were on average approximately 774,000 WEP recipients, and the first two months of 2019 had slightly more recipients (Arnesen & Wilson, 2019). Beneficiaries are able to opt out of the programme, but less than 1,000 individuals in 2019 had chosen to do so (Arnesen & Wilson, 2019). As at the end of June 2019, nearly two-thirds (64%) of WEP recipients were NZS recipients; while WAB recipients included jobseeker support (17% of WEP recipients), supported living payments (10%), and sole parent support (7%); and most (close to 90%) of WEP recipients do not have dependent children.

Most of the literature on the effects of the UK Winter Fuel Payment that used a regression discontinuity design (RDD), exploiting the eligibility at age-60. Although an RDD approach focused on the age-65 eligibility for NZS and the WEP may be fruitful, the sample sizes available for our analyses are insufficient to support an RDD analysis. In addition, such an approach is uninformative with respect to the effects of the WEP on working-age benefit recipients – i.e. the RDD approach identifies effects of the policy only near to the point of discontinuity (age 65 in this case) and, arguably, the main benefits of the WEP may be within either older NZS recipients or the working-age benefit WEP recipients. For these reasons, our analyses are based on difference-in-differences approaches: in particular, difference-difference-in-differences (DDD) methods in which we compare differences in outcomes of eligible non-eligible groups, between the (WEP) winter and non-winter months, from before to after the 2018 policy change.

[11] The effects of COVID-19 would be problematic on a number of dimensions related to our employment and health outcomes. For example, during the pandemic, people were less likely to seek treatment for health concerns, so health outcomes could improve for this reason and be unrelated to WEP. Exposure to seasonal influenza was also reduced by the lockdown.

3 Effects on energy expenditures and wellbeing

We begin our analysis by examining whether the WEP had any effects on household expenditure patterns, in particular home energy expenditures. For this analysis, we use Household Economic Survey (HES) data on recipients' energy expenditure to examine to what extent the WEP is being used to heat homes during winter months. Secondarily, we examine the effects of the WEP on self-reported material and overall wellbeing, using information reported in the HES. In particular, we focus on three sets of questions that may be directly affected by the WEP: heating related measures of wellbeing (e.g., heating or keeping accommodation warm in winter; dampness or mould); measures related to financial hardship (e.g., problems paying for energy); and a general subjective wellbeing measure of overall life satisfaction.

These analyses will help inform changes in intermediate effects of the WEP that indicative of other substantive outcome of interest; in particular, health related outcomes, such as housing related hospitalisations. For example, if there is no change in recipients' expenditures on energy or in their ability to keep their homes warm, we may not expect to see changes in their health outcomes.

The analysis is based on samples from the 2015/16 and 2018/19 Household Economic Surveys (HES). Persons in households in these HES samples are matched to Ministry of Social Development (MSD) data in the IDI to identify whether they received main (first-tier) and supplementary (second-tier) benefit receipt. The HES conducts an annual Income survey, which includes household income, housing costs and material wellbeing, as the core HES. The HES also includes detailed Expenditure, and Net Worth (assets), surveys that each run on a three-year rotating basis (in separate years). Prior to 2018/19 the HES sample consisted of about 3,500 households. Associated with the introduction of The Child Poverty Reduction Act (2018) which required annual reporting of child poverty measures, from 2018/19 onwards the HES sample for the core Income survey was increased to about 20,000 households, while the Expenditure and Net Worth surveys were based on subsamples intended to achieve about 3,500 households. In addition to the increase in core sample, the sample design also included a booster sample from the electoral role to ensure adequate representation of Māori, and was also stratified by geographical area to provide better regional representation. The descriptive statistics in Table 1 and Table 6 suggest these changes affected the composition of the HES samples for our analysis. However, our research design is expected to neutralise the effects of these composition changes on the analysis.

3.1 Methodology

We first describe the methodology used to estimate the effects of the WEP policy on expenditure patterns and wellbeing measures, and then discuss the results of these analyses.

Together with WEP eligibility status (i.e., receipt of a WAB or NZS/VP), there are two salient dimensions of variation to exploit: first, time variation between HES years (i.e., 2015/16 vs 2018/19); and second, within year seasonality (i.e., WEP-winter months or non-WEP months). Given these three sources of variation, we adopt a triple-difference (DDD, or difference-in-difference-in-differences) approach to identify the WEP effects of interest. In particular, we estimate the treatment effects of the WEP policy allowing for differences in outcomes between beneficiaries and non-beneficiaries, differences between the (WEP) winter and non-winter months, and differences over time (i.e. 2015/16 and 2018/19 periods), as well as household characteristics observed in the HES data.

An important detail to note is that we adopt an "intention-to-treat" (ITT) approach. That is, we focus on the intended treatment group of households that include someone who receives either a working-age main benefit or NZS/VP who are eligible for the WEP, rather than the subset of those who actually receive WEP. This is partly because the WEP-receipt rate among those eligible is very high (nearly 98%), so the scope for bias is low. This also simplifies having to model the selectivity associated with WEP take-up in order to construct a comparable sample of potential recipients in the pre-WEP 2015/16 period.

Our analysis considers effects of WEP on WAB and NZS/VP recipients combined, as well as separate WEP effects for each of these recipient sub-populations. In addition, to the extent that age is an important dimension along which energy expenditures vary, given the effective absence of non-NZS/VP recipients among the population age 65 and over, we will also consider two restricted older-age subsamples (aged 55 and over, and aged 55-74) to provide robustness checks on the estimated effects for the NZS/VP population.

We adopt a so-called triple difference (DDD) approach to identify and estimate the 'intention-to-treat' effects of WEP on households' expenditure patterns and material wellbeing. Controlling for household demographic and monthly seasonal effects, the DDD approach allows for differences in outcomes over time (i.e. between 2015/16 and 2018/19), between WEP-eligible and non-eligible households, and between WEP-winter and non-winter months, and ascribes any remaining difference in outcomes to the WEP treatment effects. The basic DDD regression specification, with controls, is:

$$
\begin{aligned}
Y_i = \beta_0 + \beta_1 WEP_i + \beta_2 Wint_i + \beta_3 Post_i \\
+\beta_4 (WEP_i * Wint_i) + \beta_5 (WEP_i * Post_i) + \beta_6 (Wint_i * Post_i) \\
+\beta_7 (WEP_i * Wint_i * Post_i) + X_i' \gamma + \epsilon_i
\end{aligned} \quad (1)
$$

where Y_i is a measure of household-i's energy expenditure (or other components of expenditure) or wellbeing outcome; WEP_i is an indicator variable for whether household-*i* was eligible to receive the WEP (i.e. received a WAB or NZS/VP during the May – September WEP-winter months); $Wint_i$ is an indicator for whether the household was surveyed during the May-September WEP-winter months; $Post_i$ is an indicator for whether household-*i* is observed in the post-WEP period (i.e. in the 2018/19 versus the 2015/16 HES); X_i' is a vector of relevant control variables; and ϵ_i captures other idiosyncratic unobserved factors. The control variables include total household expenditure in the expenditure analysis, and total household income in the wellbeing analysis.

As we control for household expenditure (or income), the DDD treatment effect of the WEP in equation (1), measured by the coefficient (β_7) on the interaction ($WEP_i * Wint_i * Post_i$), reflects the labelling effect of the policy on energy expenditure (or wellbeing), beyond the standard effect associated with WEP's increase in income. That is, in the absence of any labelling effect, household energy expenditure is expected to increase with the increase in income.[12] In identifying the labelling effect, equation (1) allows outcomes (Y_i) to vary across those receiving a main benefit (or NZS/VP) or not (β_1), across winter and non-winter months (β_2), in 2018/19 compared to 2015/16 (β_3), as well as across the three two-way interactions ($WEP_i * Wint_i$, $WEP_i * Post_i$ and $Wint_i * Post_i$), that allow for different variation in winter months and over time for beneficiaries and non-beneficiaries, and also over time for the winter and non-winter months; and controlling for differences in household characteristics.

A central assumption in equation (1) is that the selected non-beneficiary households provide a valid comparison group for WEP households, in the sense that the coefficients $\beta_0 - \beta_6$ (and $X_i'\gamma$) fully capture the expected expenditure differences between the two groups in the absence of WEP. If so, the coefficient β_7 can be interpreted as capturing the causal treatment effect of the WEP policy on expenditure. Because we only observe one pre- and one post-period, it is not possible to directly test the implications of this assumption in this context. However, we will assess the robustness of the estimates to various specifications.

[12] Our analysis below is in terms of an expenditure-share specification, and controls for a quadratic in log(total expenditure). This implies energy expenditure will increase with WEP income according to the energy expenditure share, and may be moderated if the energy share declines as household total income (and expenditure) increases. To the extent that the WEP provides a relatively small increase in income, we expect the latter effect to be of second order importance, but will provide some assessment of this below.

3.2 HES expenditure analysis

We begin by analysing the effects of the WEP on household expenditure patterns. In this section we first discuss the HES data, and describe the main characteristics of the sample that we analyse. We then present and discuss the triple-difference (DDD) estimation results.

3.2.1 HES expenditure survey data

The HES conducts a detailed expenditure survey every third year. We use data from the latest pre-WEP policy period (July 2015 – June 2016) and the first post-policy year (July 2018 – June 2019), consisting of about 3,500 households in 2015/16 and 3,900 households in 2018/19. Households are surveyed throughout the year, and expenditures recorded in relation to their survey reference date. The 2018/19 HES covers the three-month period when the WEP was first introduced in 2018 (July – September), as well as the first two months of its availability in 2019 (May and June).

The HES expenditures are derived from various sources, including a two-week expenditure diary, records of most recent payment (e.g. for household energy expenditure), and survey questions designed to prompt respondents' recall of various purchases. The detailed expenditure component of the HES asks respondents about household energy expenditures which include mains gas, electricity, bottled (LPG) gas, home-heating oil, firewood, coal, and other types of domestic fuels. The expenditures are annualised; however, expenditures for electricity and mains gas are generally based on the respondent's last bill (including information about the time period covered by the last bill).[13] Hence, using the survey month, we can differentiate spending in WEP months from spending during non-WEP months, and divide the annual expenditures by 52 to express these in weekly amounts.

For each adult (aged 18 and over) in the HES samples, we matched their MSD first-tier and second-tier benefit receipt over the respective annual survey period (i.e. July 2015-June 2016 or July 2018-June 2019), to identify if and when they received main and supplementary benefits during the year. In particular, we construct indicator variables for whether a person received main (supplementary) benefit at all during the year, as well as during July-to-September, October-to-April, and May-to-June. We also estimate the number of days in receipt of benefits, and the total amount of the benefit received, in each of these periods. We then aggregate these

[13] There are possible issues around how closely aligned the household energy use is with the timing of the reported expenditures. For example, if a household chooses to smooth its energy bill payments over the year, its monthly expenditures may be uncorrelated with its current energy use; similarly, households that rely on firewood for heating may make their main expenditures in warmer months. Although, there are clear and consistent seasonal patterns in household energy expenditures, we expect these factors may cause attenuation bias in the estimation results. In addition, if households self-select into low- versus high-cost energy plans according to their expected energy use, this may lead to heterogeneous policy effects on energy use.

measures across adults in the household, in order to identify if the household received a benefit and how much they received in each period of the year. In our analysis, we focus on the intention-to-treat effect of the WEP, and so focus primarily on whether or not anyone in the household received either a working-age benefit (WAB) or New Zealand Superannuation or Veteran's Pension (NZS/VP).

Table 1 contains summary statistics for the HES-Expenditure sample analysis. We present statistics for the full 2015/16 and 2018/19 HES samples in columns (1) and (3) respectively, and for the subsamples of WEP-eligible households surveyed during the winter months in columns (2) and (4).[14] Comparing columns (1) and (3) indicates there were some noticeable changes in household demographics between the 2015/16 and 2018/19 HES samples. For example, there was a 3.8 percentage point (pp) drop in the fraction of households with any European ethnicity (from 80.7% in 2015/16 to 76.9% in 2018/19), and a 7.5 pp increase in the fraction of households with other ethnicities (from 42.4% in 2015/16 to 49.9% in 2018/19). The latter includes a 1 pp increase in Māori (16.7% to 17.7%), and a 2.1 pp increase in Pacific peoples (5.0% to 7.1%). In addition, Table 1 shows a relatively strong 5 pp increase in households receiving working-age benefits (from 13.3% in 2015/16 to 18.3% in 2018/19).[15] As discussed above, we believe these changes largely reflect changes associated with the HES sample design rather than changes in the underlying population.[16] To the extent such differences are stable across the seasons within the respective sample years, the DDD research design is expected to neutralise any possible adverse effects by comparing seasonal (Winter versus non-Winter) as well as time changes.

We next discuss the expenditure, income and WEP characteristics of the HES samples, which are summarised in Table 2. The top part of the table summarises the weekly amounts received; and the lower part summarises the energy shares of expenditure and WEP amounts relative to the various expenditure and income amounts.[17] First, as expected, total expenditures and incomes are substantially lower in WEP-eligible households than in other households (e.g.

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[14] That is, households who receive either working-age benefits or NZ Superannuation or Veteran Pension payments during the year. Although we classify these subsamples as "WEP-eligible", they include small fractions (1-2%) of households that receive working-age benefits only in the non-winter month periods, and so are not eligible for the WEP.

[15] The table also shows a strong increase in receipt of supplementary benefits from 23% to 47%, which is largely driven by the availability of WEP in 2018/19.

[16] For example, trends in the number of working-age benefit recipients has been relatively stable over the period, at around 300,000 recipients.

[17] In the full samples in columns (1) and (3), the average expenditures and incomes are calculated over the full annual periods, while the column (2) and (4) are calculated over the winter months. This disguises some seasonal variation, mainly in energy expenditures, that we will document in the discussion. Also, the 2018/19 average WEP amounts are calculated only over the winter period – i.e. the 22 weeks corresponding to July-September 2018 and May-June 2019. We have calculated the WEP shares of household expenditures and total income using the average weekly WEP amount relative to the respective weekly expenditures and income; and calculated the WEP shares of income support payments as the ratio of the total WEP received over the winter months to the respective total income support payments received over the same period.

the average for WEP-eligible households are 60-62% of the average for other households). In contrast, the average household energy spending is more similar across such groups e.g. the table shows the average winter energy expenditure by WEP-households is about the same as the average energy expenditure across the year by all households ($46-47 per week in 2015/16 and $43-44 in 2018/19).[18] Second, while average incomes and expenditures increased 3-4% between the 2015/16 and 2018/19 surveys, expenditure on household energy actually fell 5%. This implies the energy expenditure share fell slightly: from 4.9% to 4.8% for the full samples, and from 6.8% to 6.5% for WEP-eligible households winter subsamples.

Third, the WEP-shares in the final column document the relative size of the WEP 'treatment', compared to alternative expenditure and income measures of recipient households. WEP amounted to 3.7% of total household income on average, 4.4% of total expenditure, and 120% of energy expenditure.[19] Furthermore, the WEP received was 6.6% of the combined main (benefit and NZS/VP) income support payments, and 5.3% of total support payments (including non-WEP tier-2 supplementary benefits). WEP was relatively more generous for working age benefit recipients (10.7% of main benefit) than for NZS/VP recipients (5.7% of main payments).

The relatively stable energy expenditure statistics shown in Table 2 disguise some relatively strong seasonal and geographic variation. To provide a clearer sense of this variation, Figure 1 documents the monthly and regional variation in household energy expenditure shares using the combined HES samples. The monthly seasonal variation, described in panel (a), shows energy expenditure is relatively higher in winter than summer months, albeit not perfectly synchronised with the WEP winter month period. That is, average energy shares are around 4.5% between November and June, rising to about 5.5% in July and peak at almost 6% in August, before dropping to 5.5% in September and 5% in October.

The geographic variation in energy expenditure shares is shown in panel (b) of Figure 1, for regions ordered broadly from north to south. From this figure it appears that geographic variation in average energy expenditure shares depend on the region's latitude (e.g. shares are higher in the southern regions of Otago and Southland, although also relatively high in Northland); whether the region is urban versus rural (e.g. shares appear to be relatively low in Auckland, Wellington and Canterbury), reflecting higher income and expenditure levels in urban areas (perhaps explaining the relatively high energy share in Northland); and perhaps climate

[18] More detailed statistics show that the average energy expenditure of WEP-households is 86-88% that of other households in winter months, and 92-95% in the non-winter months). This suggests WEP-eligible households economise relatively more on energy use during the winter months.

[19] Caution is suggested in interpreting some of these ratios. In particular, compared to the average share of 120%, the median WEP/energy expenditure share is 60%. Also, the average WEP to expenditure and to income shares are noticeably higher than the ratio of the corresponding averages (2.4% and 1.8% respectively).

(e.g. shares are relatively low in Tasman and Nelson regions). We have also explored how the regional variation average energy varies in winter versus non-winter months: broadly speaking, this shows substantively and statistically greater seasonal variation in the lower South Island areas (Canterbury, Otago and Southland) than elsewhere.[20]

3.2.2 Results: WEP effects on household expenditure

We now discuss the results from our DDD regression analysis of the effects of the introduction of the WEP policy on household power. We discuss the results for expenditure patterns first, and then consider the effects on wellbeing.

Our primary focus here is to examine whether the introduction of the WEP policy in July 2018 led to an increase in household energy expenditures in households that were eligible to receive the WEP. One issue in implementing equation (1) is choosing an appropriate specification for the outcome variable. For this purpose, we mainly analyse a household's energy expenditure as a share of its total expenditure, and will present results using this specification here.[21] To allow the expenditure shares to vary systematically with households' total expenditure (possibly reflecting wealth differences), we include log(total expenditure) as a control variable.[22]

We begin with a selection of alternative regressions based on equation (1), and present the estimated DDD WEP policy effects (i.e. variants of β_7 in equation (1)) in Table 3. We first present the estimates from the simple DDD regression corresponding to equation (1) with no additional controls, and in which the treatment group is defined as any household that received either any working-age benefit or NZS/VP payments during the winter months. The estimated coefficient (1.12) is statistically significant and suggests eligible recipients increased the share of expenditure allocated to energy by 1.12 percentage points (pp) during the WEP winter month period after the introduction of the policy. In fact, it may be helpful to consider the full set of simple-DDD regression estimates for equation (1) (with **bold** font to indicate statistically significant coefficients):[23]

[20] However, there are also significant seasonal energy share effects in the Bay of Plenty and Marlborough regions.

[21] We have also estimated regressions using both log(household energy expenditure) and the $-level of household energy expenditures. The results based on the log(expenditure) specification are broadly consistent with the expenditure share results discussed, while the results for the expenditure in $-levels, are more variable.

[22] We also examined whether expenditure shares vary with log(total income) and concluded that this was not the case. Additional control variables include indicators for total ethnicity at the household level, quadratics in household size, the number of rooms in the house and the oldest age of persons in the household, the adult share and female share of adults, indicators for whether the dwelling is a house (versus apartment), whether it is a public or private rental (vs owner occupied), and family type (Single vs Couple adults, with vs without children, and miscellaneous households).

[23] The estimated relative effects are broadly similar across other regression specifications that control for seasonality and other covariates, and allow policy effects to vary between working-age beneficiaries and NZS/VP recipients.

$$POW\_share_i = \mathbf{3.53} + \mathbf{2.81}\, WEP_i + \mathbf{0.94}\, Wint_i - 0.01\, Post_i$$
$$-\mathbf{0.65}\,(WEP_i * Wint_i) - 0.35\,(WEP_i * Post_i) - \mathbf{0.71}\,(Wint_i * Post_i)$$
$$+1.12\,(WEP_i * Wint_i * Post_i)$$

The positive WEP_i coefficient (2.81) implies the share of expenditure on household energy was substantially higher for those eligible for the WEP (WAB and NZS/VP recipients) than non-eligible households in non-winter months in 2015/16. Similarly, the coefficient on $Wint_i$ (0.94) implies the energy expenditure shares were significantly higher in the winter months in 2015/16. On the other hand, the negative coefficient (-0.65) on $(WEP_i * Wint_i)$ indicates the relatively higher winter energy-share is somewhat reduced for WEP-eligible households. Also, the negative coefficients on the $Post_i$ variables (including interactions with WEP_i and $Wint_i$) imply the energy expenditure shares in 2018/19 were marginally lower for non-eligible households (-0.01), somewhat lower for eligible households (-0.35), and significantly lower in the winter months (-0.71), perhaps suggesting 2018/19 had relatively milder weather particularly in winter.

Because of strong seasonal patterns of energy expenditure shares (seen in Figure 1), we re-estimate the simple regression including monthly seasonal controls. The estimated WEP policy effect (1.16), in the second column, is almost the same as the simple DDD estimate. We next include controls for households' log(total expenditure) and other characteristics. The coefficient of interest (0.51) is substantially reduced, and not statistically significant, with the point estimate suggesting household energy expenditure shares increased by 0.5 pp for WEP-eligible households during the winter months.

Although not statistically significant, it is useful to interpret the magnitude of this policy effect estimate. First, relative to the pre-change (2015/16) estimated average winter energy share for WEP-eligible households of 6.6% (=3.53+2.81+0.94-0.65, based on the simple DDD estimates reported above), this suggests that the WEP policy increased the energy share of expenditure for these households by about 8% (from 6.6% to 7.11% of expenditure). Second, using the sample averages in the final column of Table 1, the total policy effect of the WEP is estimated to increase weekly energy expenditure of recipient households by $5.98,[24] or about 24% of the $24.60 average WEP. Although lower than the 47% estimated by Beatty et al. (2014) for the UK WFP, this fraction is higher than the (approximately) 6.6% that would be expected in

[24] This effect has three components – a labelling effect, the effect of increased income at the initial expenditure share, and an interaction between income and the energy expenditure share. The labelling effect implies recipients are estimated to spend a higher share of a higher expenditure on energy: $73.87 (=(0.066 + 0.0051)*($1014.40 + $24.60)). This is $6.92 more than the $66.95 (= 0.066*$1014.4) energy expenditure in the absence of WEP. There is a small offsetting effect due to an expected drop in energy expenditure shares as income rises, of -$0.94 (implied by the quadratic expenditure terms included in the regression), resulting in the overall effect of $5.98 (=$6.92–$0.94). The estimated labelling effect of the policy is $5.30 (=0.0051*$1,039) or 22% of the average WEP. Thus, the estimated average total increase in energy expenditure of $5.98, consists of the $5.30 labelling effect, $1.62 increase from WEP income associated with the base energy-share, and the $0.94 decrease associated with a fall in energy share as total expenditure increased.

the absence of a policy response. Also, given the sampling variation in the data, a labelling effect size of about 0.7 pp would be required to be statistically detectable, corresponding to an increase in energy expenditure of about $7.30 or 30% of the average WEP.

Because receipt of NZS is not means tested in contrast to working age benefits, and the level of income support is higher for NZS recipients than WAB recipients, the degree of need may vary between these WEP eligible subgroups, leading to different policy effects. One way to think about the intention to treat effect is as the fraction of the eligible group who respond to the policy multiplied by their average response. For example, if relatively fewer NZS recipients are financially constrained, the fraction who respond to the WEP policy by increasing their energy expenditure may be lower than for WAB recipients. Then, assuming the average response is the same among subgroups of responders, the expected treatment effect would be lower for NZS recipients.

To assess this, in the final specification we report in Table 3 we allow for separate WEP-eligibility effects for working-age benefit and NZS/VP receiving households.[25] Although neither of these estimated policy effects is statistically significant, both coefficients are positive, consistent with the hypothesis that energy expenditure shares increased in response to the WEP-policy. Also, the estimated effect is somewhat larger for NZS/VP eligible households (0.54) than working-age benefit eligible households (0.24). This is at odds with the simple hypothesis of fewer NZS responders leading to smaller effects for NZS recipients. One possible explanation is that the labelling response to WEP is greater for NZS than WAB recipients: perhaps because NZS recipients experience less overall financial hardship and are able to optimally increase their expenditure on home heating. But we emphasise the evidence is weak and no clear conclusion can be drawn.

In order to test the robustness of these estimates based on the full sample of households, we next consider three sets of estimates based on alternative sub-samples. We present the results of this exercise are presented in Table 4, using regressions corresponding to those in the second and third columns of Table 3.

First, as NZS is essentially universally available for those aged over 65, there will be less selectivity on unobserved differences in the under-65 comparison group than for non-WAB. Given this, we examine the robustness of the estimates for the NZS/VP sub-population focusing

[25] A relatively smaller number of households receive both working-age benefits and NZS/VP payments – e.g. Table 1 shows 40.5% of households received at least one of these payments, while the sum of the separate incidences (42.4%) implies about 1.9% of households receive both over a HES year. Such households will be classified as both WAB and NZS/VP treated households in this specification. In addition to the results presented here, to allow for possible time lag between energy use and reported spending, we have estimated regressions with a one-month lead in the timing of the winter months (i.e. aligning to June – October). This has no substantive effect on the estimated results.

on an older (aged 55 and over) sub-population that excludes working age beneficiaries. The results of this exercise are presented in the first two columns of Table 4. We estimate a large and statistically significant WEP-policy effect (1.43 pp) controlling just for monthly seasonal effects, and a smaller insignificant effect (0.74 pp) when other control variables are included: although the latter point estimate is larger than the corresponding NZS/VP effect in Table 3.

Second, we further narrow the focus on the effects for the NZS/VP population, by further sub-setting the older (non-WAB) population to those households whose eldest person is aged 55-74. The results of this exercise are shown in the next two columns. We again estimate statistically significant (and larger) effects (1.68 pp) controlling only for seasonal effects, but insignificant (and smaller) effects (0.21 pp) controlling for household covariates.

Our third robustness check focuses on the effects for working-age benefit households. For this exercise, we narrow the population focus to households with no one aged over 64, and whose total expenditure is less than the median expenditure or includes a working age beneficiary. The results of this analysis are presented in the final two columns of Table 4.The estimates in both columns are similar (0.65 and 0.74). Although statistically insignificant, these suggest possibly higher WEP-policy effects for households receiving working-age benefit (0.65-0.75 pp) than that shown in Table 3 (0.24 pp).

We next consider whether the WEP resulted in increased expenditure on other than household energy items, and estimate similar regressions for other expenditure shares. The results are presented in Table 5 for HES diary items,[26] energy and housing related expenditure – i.e. expenditures on household electricity, household operations, home maintenance, housing cost, and mortgages.[27] The results for each expenditure group share are based on the specification in the fourth column of Table 3, that allows separate WAB and NZS/VP effects and controls for household characteristics.

The first outcome in Table 5 is expenditure on household electricity, which is a subset of energy expenditures and may perhaps be more directly related to heating. Neither of the WEP-policy estimates is statistically significant and each is smaller than the energy estimates in Table 3. In fact, the estimated effect for households with working-age benefits is negative. We next report the estimates for the diary items. These are again imprecisely estimated but are both positive. The estimates in the other columns of Table 5 are also statistically insignificant, except for weakly significant working-age benefit effects on housing costs shares, and the coefficient

[26] The HES diary collects information on food and clothing expenditures, as well as alcohol, tobacco and illicit drugs.

[27] In Table A1 we report results for other expenditure groups – i.e. expenditures on other properties, recreation and culture, medical and health, miscellaneous items, telecommunications, transportation, travel, credit and debit accounts, contributions to savings, fees and subscriptions, insurance policies, and loans.

signs are more often negative than positive. Thus, little can be taken from these results. There is also no evidence of clear patterns for the other expenditure groups reported in Table A1. In particular, there are few statistically significant effects, and the signs of the estimated effects are not consistent.

Based on this analysis, we conclude there is no compelling evidence either for or against the hypothesis that WEP-eligibility affected household expenditure patterns, although the signs of the estimated coefficients are weakly suggestive of an increase in household expenditure on electricity and power more broadly. It is not clear whether such a null result is due to the policy having no effect, or whether it had an effect that is too small to detect given the size of the policy treatment and the size of the HES sample available for analysis.

3.3 HES material wellbeing analysis

Our second analysis using the HES data focuses on the effects of the WEP on self-reported material wellbeing outcomes, which we analyse in this section. We first describe the material wellbeing data and sample to be analysed, and then present our results.

3.3.1 HES material wellbeing survey data

As part of the income survey, HES also collects responses to subjective material wellbeing questions. We again focus on the 2015/16 (pre-WEP) and 2018/19 (post-WEP) surveys.

In HES, material wellbeing questions were asked for the household generally with additional questions specifically about the wellbeing of children in the household. Some of the questions apply specifically to heating the home. For example, respondents were asked if they were forced to keep costs down in the last 12 months by putting up with cold. Respondents were also asked about problems in heating and/or keeping their home warm in winter, problems with dampness or mould, the affordability of keeping their accommodation adequately warm, and their ability to pay utility (electricity, gas, rates, water) bills on time due to a shortage of money, as well as a question about overall life satisfaction. Some of these questions may be used as outcome measures in their own right (e.g., WEP may affect the percentage of respondents who report having to put up with cold) or as intermediate outcomes that may be correlated with other outcome measures (e.g., WEP may reduce the need to put off doctor visits which could in turn affect hospitalisations).

Given this information, our analysis focuses on three dimensions of wellbeing. The survey questions for each measure allows for a scaled response, and we aggregate these to binary summary measures. The resulting binary variables are each ordered to indicate worse outcomes. First, we consider three measures related to heating that may be alleviated by the introduction of the WEP: whether the respondents' put up with feeling cold, whether there is damp or mould in the house, and whether the house is cold.[28] Second, we consider three measures related to financial hardship that may be alleviated by the introduction of the WEP: whether the respondent has difficulty paying for Utilities, whether they would struggle to handle an unexpected $500 bill, and whether they have insufficient income.[29] Third, we consider the respondent's general subjective wellbeing measure of their overall life (dis)satisfaction.[30]

Table 6 summarises the characteristics of the sample used for the wellbeing analysis. The demographic, benefit receipt and income characteristics are comparable to those for the expenditure sample in Table 1. The incidence of material hardship is higher across each of the wellbeing indicators in 2018/19 than 2015/16, consistent with the rise in the incidence in benefit receipt. For example, the incidence of the three heating-related measures increased 1-4 percentage points (5-20%) between 2015/16 and 2018/19; the incidence in the financial hardship measures 1-2 pp (about 10%); and the increase in overall life dissatisfaction increased about 8% from 6.0% in 2015/16 to 6.5% in 2018/19.

3.3.2 WEP effects on material wellbeing results

We now discuss the DDD regression analysis results of the effects of the introduction of the WEP policy on self-reported measures of wellbeing. This analysis adopts a linear probability specification for these outcomes as shown in equation (1).

Table 7 summarises the results of this analysis. Again, we present specifications corresponding to that in column (4) of Table 3 (allowing separate WAB and NZS/VP effects). The first three columns relate to the heating-related material wellbeing outcomes (feeling cold, damp and mould, and whether the house is cold). The estimated coefficients for households with working age benefit recipients are each positive and statistically significant for two of these outcomes, suggesting worse outcomes associated with the policy. The estimated effects are quite large, ranging from 4 pp (insignificant) for feeling cold, to 7 and 10 pp for damp and mould and whether the house is cold respectively (30-40% relative to the 2015/16 winter averages for

[28] The first question asks whether "you put up with feeling cold to keep costs down": we distinguish responses of "a little" (12) and "a lot" (13) versus "no problem" (11). The second and third questions ask "does your accommodation, have no problem, a minor problem or a major problem with": "dampness and mould?", or "heating and/or keeping it warm in winter?" Again, we distinguish responses of "a minor problem" (12) or "a major problem" (13) versus "no problem" (11).
[29] The first question asks "in the last 12 months, … you could not pay electricity, gas, rates or water bills on time because of a shortage of money?": we distinguish "once only" (12) or "more than once" (13) versus "not at all" (11). The second question asks "if you (or your partner) had an unexpected and unavoidable expense of $500 in the next week, could you pay it within a month without borrowing?": we distinguish "no" (2) from "yes" (1). The third question asks "how well does your (and partner's) total income meet your everyday needs for such things as accommodation, food, clothing and other necessities?": we distinguish "not enough" (11) versus "only just enough" (12), "enough" (13) or "more than enough" (14). We also check the robustness to the alternative of distinguishing 11 or 12 versus 13 or 14.
[30] The question asks a "very general question about your life … how do you feel about your life right now?": we distinguish "dissatisfied" (14) or "very dissatisfied" (15) versus " very satisfied" (11), "satisfied" (12) or "neither satisfied nor dissatisfied" (13).

WEP-eligible households). We don't have an explanation for these puzzling results, but they may reflect some problem with the model identification. In contrast, the estimated effects for households with NZS/VP recipients are each statistically insignificant and substantively small, implying essentially no effects on heating related wellbeing.

The next three columns relate to the financial hardship outcomes (whether the respondent has difficulty paying for Utilities, whether they would struggle to handle an unexpected $500 bill, and whether they have insufficient income). While the estimated policy effect coefficient for Utility payments for households with working-age benefit recipients is essentially zero, the coefficients for handling an unexpected $500 bill and insufficient income are both negative and statistically significant, suggesting that the policy led to a reduction in financial hardship among these households during the winter months. The estimated coefficients suggest the WEP caused the incidence of hardship to fall by 5-9 pp (30-40% relative to 2015/16 winter averages for WEP-eligible households). The estimated policy effects for NZS/VP are again small and statistically insignificant across each of these wellbeing measures. The relatively larger WAB effects here are suggestive that the WEP policy had a stronger effect alleviating financial hardship on working age benefit recipients.

The final column reports the results for respondents' overall life dissatisfaction measure. The estimated policy effects for households receiving working-age benefits and for households receiving NZS/VP are small and statistically insignificant, suggesting that the WEP had little effect on overall life dissatisfaction.

In summary, the results of this analysis are largely inconclusive regarding the effects of the WEP on these measures of wellbeing. Although there is some evidence that WEP has alleviated financial hardship-related material wellbeing for households receiving working age benefits, we find the reverse effects on heating related wellbeing measures. The evidence for households receiving NZS/VP payments is that the effects are small and statistically insignificant.

4 Effects on health outcomes

4.1 Methodology

The primary focus on health impacts is on hospitalisations using Ministry of Health (MOH) data in the IDI for publicly funded hospitalisations, matched to a pooled sample of individuals and households from the Household Labour Force Survey (HLFS). In particular, we examine the effect of WEP on housing-related hospitalisations,[31] and consider both the likelihood of a hospitalisation event beginning in a given month,[32] and the severity of the hospitalisation event (i.e., number of nights in hospital, total cost).

For this analysis, we used a similar DDD model to that described in equation (1) in Section 3.1. However, for this analysis we have monthly panel data for individuals during the time they are found in the HLFS sample. Although the HLFS surveys the same dwellings over a 2-year period, the panel is unbalanced due to attrition either because of non-response or because of individuals moving out of the surveyed dwelling. The base specification for this model is as follows:

$$\begin{aligned} Y_{it} = {} & \beta_0 + \beta_1 WEP_{it} + \beta_2 Wint_{it} + \beta_3 Post_{it} \\ & + \beta_4 (Bft_{it} * Wint_{it}) + \beta_5 (WEP_{it} * Post_{it}) + \beta_6 (Wint_{it} * Post_{it}) \\ & + \beta_7 (WEP_{it} * Wint_{it} * Post_{it}) + X'_{it}\gamma + \epsilon_{it} \end{aligned} \quad (2)$$

where Y_{it} is individual *i*'s hospitalisation outcome in month *t*, WEP_{it} is an indicator variable for whether individual *i* lives in a household which receives a WEP-eligible benefit (i.e. received a WAB or NZS/VP) in month *t*, $Wint_{it}$ is an indicator for whether the month is a WEP-eligible "winter" month (May-September) $Post_{it}$, is an indicator for whether the month is after the implementation of WEP (July 2018 and beyond), and X'_{it} is a vector of relevant control variables. In equation (2), we measure the DDD treatment effect of WEP by the coefficient (β_7) on the interaction ($WEP_{it} * Wint_{it} * Post_{it}$), which allows outcomes (Y_{it}) to vary across those receiving a main benefit (or NZS/VP) or not (β_1), across winter and non-winter months (β_2), after WEP began in July 2018 compared to before (β_3), as well as across the three two-way interactions ($WEP_{it} * Wint_{it}$, $Bft_i * WEP_{it}$ and $Wint_{it} * Post_{it}$), that allow for different variation in winter months and over time for beneficiaries and non-beneficiaries, and also over time for the winter and non-winter months; and controlling for differences in individual characteristics.[33]

We analysed the effects for all household members on a monthly basis over the time period in which they were observed in the HLFS. We estimate effects for households with NZS recipients only, WAB recipients only, and both NZS and WAB recipients relative to households with no recipients. Estimating changes in health effects over time is complicated by the fact that

[31] To determine which hospitalizations are housing-related, we used the diagnosis codes for health outcomes for which there is sufficient evidence of an association between the housing condition and the health outcome in the literature. For more detail on this methodology, see Riggs et al. (2021).

[32] We also examined the count of hospitalisations in a month; however, more than one hospitalisation event rarely occurred in a given month for the same person. Hence, the results were very similar to the results for the binary dependent variable. For brevity, we only report the results for the binary dependent variable.

[33] We control for age, gender and region. Note that we estimated specifications with year and calendar month controls as well as with month-year controls.

susceptibility to health outcomes changes with age (e.g., children become less susceptible as they age, the elderly become more susceptible). As with the HES analysis described in the previous section, a central assumption of this analysis is that the selected non-beneficiary households provide a valid comparison group for WEP households. In addition, weather effects could also cause deviations in these outcomes, and we control for these to the extent possible using month and year controls as well as regional controls.

4.2 Data

We use individual-level hospitalisation data for publicly funded hospitalisation discharges from MOH to estimate the number of cases for housing-related health conditions. Specifically, we use all publicly funded hospitalisation discharges from 2013-2019 for residents of New Zealand whose primary diagnosis for the hospitalisation is associated with the health conditions outlined in Table A2. Diagnoses for each hospitalisation are coded using the International Statistical Classification of Diseases and Related Health Problems, Tenth Revision, Australian Modification (ICD-10-AM). Hospitalisation admissions within 7 days of another hospitalisation discharge are considered part of the same event. For these events with multiple admissions, the primary diagnosis from one of the first three hospitalisations was used to classify the case – the primary diagnosis from the first hospitalisation was used first to assign a specific health condition, and if that failed, the second hospitalisation diagnosis was used, and finally the third hospitalisation diagnosis. All events are assigned to a unique health condition using this methodology (i.e., health conditions are mutually exclusive and exhaustive). For each case, we then used all related hospitalisations (i.e., those within 7 days of each other) to determine the total length of stay, the severity, and the total cost of the case. Hospitalisations (number, length of stay, and cost) were aggregated for each patient to the monthly level based on the start date of the hospitalisation event. We examined hospitalisations for full sample, as well as for children under 18; however, the hospitalisation incidence among children was too small for meaningful analysis.[34]

The administrative hospitalisation data covers the vast majority of the population but lacks information about households. The administrative benefit data includes some information about household members (though the information is generally limited to family members) but only for recipients and only during the time that they are on benefit, making construction of households over time difficult especially for controls. To help with this, we constructed households from the HLFS which allows us to consistently identify whether a household had

[34] Previous research in New Zealand found that an insulation retrofit reduced the risk of mortality for individuals who had previously been hospitalised for circulatory or respiratory issues (Preval et al., 2017). This is similar to the types of health outcomes found to be affected in Crossley and Zilio (2018) by the UK's WFP.

WEP-eligible recipients or not.[35] We start with individuals in the HLFS for at least one quarter after the beginning of 2016, matched to MSD data on their main (first-tier) and supplementary (second-tier) benefit receipt information in the IDI. We aggregate benefit receipt for households on a monthly basis creating flags for household receipt of a working-age main benefit and NZS/VP in a given month. The monthly hospitalisation data are merged to these data at the individual-month level. Hence, for each individual in the HLFS sample, we have monthly hospitalisation information as well as monthly benefit receipt for the household by type of benefit. From the HLFS, we have gender and region as well as birth month and year. We calculate age in years using birth month and year for each month.

For our analyses, our measures for each hospitalisation event (i.e., length of stay, cost) are attributed to the month of the first admission date. We do this in order to better align the hospitalisation outcomes with the receipt of WEP. For example, if an individual in a WEP household was hospitalised for 15 days on June 28, 2018, the hospitalisation event would be counted in June 2018 (pre-WEP) and all 15 days of hospitalisation would also be attributed to June 2018. If we had attributed 3 days to June and 12 days to July, these latter days would be attributed to the WEP period when in fact the event began pre-WEP. Hence, any WEP effects would not have been experienced by the individual as they were in hospital.

Summary statistics for the sample are provided in Table 8 with the top half of the table pertaining to all individual-month observations regardless of hospitalisation and the bottom half pertaining to only those observations in which there was at least one hospitalisation (i.e., conditional on hospitalisation). This table shows the mean number of monthly hospitalisation events, days in hospital, and hospitalisation costs both pre- and post-WEP as well as in winter and non-winter months for our different groups of interest: individuals in WAB households and individuals in NZS/VP households (separately and combined). In table 7, we can see that, on average, hospitalisation outcomes are worse in winter months (i.e., greater frequency, longer duration, and higher cost) when averaging over the entire sample. Hospitalisation outcomes are also, on average, generally worse for individuals in households receiving a WAB or NZS/VP compared to individuals in households receiving neither benefit, and individuals in households receiving NZS/VP had worse hospitalisation outcomes on average than individuals in WAB households. For example, the average monthly hospitalisation costs for individuals receiving neither WAB nor NZS/VP ranged between \$1 and \$6, whereas for those in households receiving WAB this average cost ranged from \$8 to \$13, and for those in households receiving NZS/VP the

[35] We do this for two reasons. First, it allows us to include children in the analysis. Second, without this, we would have household members receiving the treatment classified as controls which would muddy our results.

average ranged between $18 and $23. The conditional averages (i.e., conditional on the individual having a hospitalisation in the month) follow similar patterns but less strictly so. For example, the highest average conditional monthly hospitalisation cost post-WEP in winter ($8,824) was for individuals in households receiving neither WAB nor NZS/VP. For individuals in households receiving WAB, the average was $4,729, and for those in NZS/VP households, the average was $7,386.

4.3 Results

4.3.1 WEP effects on likelihood of hospitalisation

Our first analysis uses logistic regression to analyse the likelihood of a hospitalisation event starting in given month shown in columns 1 and 3 of Table 9, though we also estimate the regression using OLS (shown in columns 2 and 4).[36] For these regressions, we also include age (and age squared), gender, and region as explanatory variables.[37] The first two columns of Table 9 show the results when we estimate the results for WAB and NZS/VP recipients separately, and the next two columns show the results for the combined group. For both analyses, we clustered the standard errors at the household level. The treatment group includes all the individuals in a household receiving either a working-age benefit (WAB) or NZS/VP payments during the month. We had planned to split the sample to analyse hospitalisations for those under 18 years of age, but the number of hospitalisations for the under 18 age group was too small for reliable estimates. We were, however, able to analyse this for those aged 55+ and for those aged 55-74, but this did not change the results. For brevity, we present the results only for the main sample.

For our main variables of interest, the estimated odds ratio for those living in households receiving NZS/VP in a WEP month is 0.85 which indicates that WEP is associated with a reduction in hospitalisations for this group,[38] but it is not statistically significant. The estimated odds ratio for those living in households receiving a WAB in a WEP month is 0.97, which is also not statistically significant, but suggests a weaker effect than for NZS/VP recipient households. The second column of Table 9 shows the results from the OLS model which also indicate that WEP receipt is associated with a reduction in hospitalisations, but the coefficients are insignificant. We also examined the effects of WEP on the combined eligible population (in either NZS/VP households or WAB households) using a single variable (results shown in columns 3 and 4 of

[36] We have also estimated the regressions using fixed and random effects; however, due to the unbalanced nature of the panel especially post-WEP, the basic logit and OLS were preferred. The results were similar across the different models.
[37] In the specifications shown, the regional breakdown was Auckland and Northland, rest of the North Island, the South Island. In other specifications, we used the full breakdown of regions, but the results were similar.
[38]An odds ratio less than one indicates that the treatment (i.e., WEP) reduces the likelihood of the health outcome (i.e., hospitalisation).

Table 9), and the results were very similar to those shown in columns 1 and 2. Overall, these results suggest that the WEP caused a weak reduction in the incidence of hospitalisations.

4.3.2 WEP effects on severity of hospitalisation

To assess the potential effects on the severity of hospitalisations, we used two dependent variables in our regression models: the length of stay and the hospitalisation costs. We ran the analysis using separate variables for the NZS/VP and WAB households as well as using a single variable for the combined group. The pattern of results was similar, so we present the results using the separate variables.

Since the length of stay variable is also a count variable, we used a variety of different estimation methods including Poisson, quasi-Poisson, and negative binomial, but we also used OLS for contrast (shown in column 1). These results are shown in Table 10. For all models, standard errors were clustered at the household level.[39] Using a Poisson model (column 2) did not yield significant coefficients on our variables of interest. Since we found overdispersion in the Poisson Model, we also used a quasi-Poisson and a Negative Binomial model (columns 3 and 4, respectively). Regardless of the specification used, we did not find significance on our variables of interest. In the first three columns, the results are consistent with WEP receipt for NZS/VP households being associated with an increased length of stay, but the coefficients are not statistically significant. For households receiving WAB, these results are consistent with WEP receipt being associated with a decreased length of stay, but again, the coefficients are not statistically significant. The results for the Negative Binomial model (shown in the column 4) are consistent with WEP receipt being associated with an increased length of stay for both groups, though the coefficients are still statistically insignificant.

For hospitalisation costs, we used OLS with clustering of standard errors at the household level as the preferred model, so we only present these results (shown in Table 11).[40] Column 1 shows the results for the NZS/VP and WAB households separately, and column 2 shows the results for the combined group. In column 1, both coefficients on our variables of interest are insignificant; however, the coefficient for NZS/VP households is positive while the coefficient for WAB households is negative. When looking at the combined effect (NZS/VP and WAB households), the coefficient is positive but insignificant, though closer in magnitude to the coefficient for NZS/VP households than for WAB households, which indicates that the NZS/VP households may be driving these results.

[39] We also ran the model without fixed or random effects but found similar results.
[40] We also ran the model without fixed or random effects but found similar results.

Overall, we find different results for our two recipient groups when using both severity outcome measures. For both measures, WEP receipt appears to be associated with increased severity for NZS/VP recipients but with reduced severity for WAB recipients. However, our results are not statistically significant.

In summary, our results for our hospitalisation measures are generally in a direction that would indicate beneficial effects from WEP for the treated groups; however, these effects are also generally statistically insignificant. Despite having a fairly large sample (about 50,000 unique individuals both pre-WEP and post-WEP), the number of individuals with hospitalisations is much smaller. Adding a longer time series to the post-WEP period could help, but our time period ends just before the Global Pandemic which creates its own estimation issues. Given this, we conclude our results are inconclusive as to the effect of WEP on hospitalisation outcomes.

5 Financial incentives of the WEP

Because WEP eligibility is conditional on receiving either a working-age benefit (WAB) or NZS/VP, it provides an additional financial disincentive to be off the benefit and in fulltime work. In this section, we examine the possible financial incentive effects of the WEP on receiving a main benefit during the winter months.

There are two important issues to consider with the financial incentives from the WEP policy. First, because WEP eligibility requires receipt of a main benefit (or NZS/VP), the financial incentives of WEP are to be on-benefit, rather than to be non-employed per se. Second, the WEP treatment is primarily at the family or household level: i.e., it is paid to eligible individuals on the basis of their family structure (single versus other). Given these issues, we first adopt the household as a proxy for the family unit, and focus primarily on household behaviour. The incentive response to WEP may occur either in terms of an increase in the incidence of households receiving benefit (the extensive-margin response), or in terms of a decrease in earnings in the household so as to maintain benefit eligibility (the intensive-margin response). We will examine both the extensive and intensive margin responses in our analysis.

5.1 Methodology

In this section, we describe the methods we use to analyse whether the WEP affects labour market outcomes of recipients. As NZS eligibility is not work tested, we concentrate on the effects for WAB recipients. If there is a response to these incentives, we would expect to see relatively more people transition onto and/or relatively fewer people transition off main benefits around the WEP start date each year.[41] Similarly, we may expect to see more people transition off main benefits around the WEP end date each year as those who might normally transition off main benefits during the winter months may wait until after the WEP end date. Hence, we may not necessarily see more people transitioning on or off main benefits in total, but we may see clustering of these transitions around the WEP start and end dates.

To address this issue, we propose to use a difference in differences (DiD) approach to compare outcomes, before and after the introduction of WEP during the winter and non-winter months. The outcomes we consider include labour market outcomes such as benefit status, employment, or earnings. In particular, consider the following equation for an outcome of interest (Y_{it}) for an individual-*i* (or their household) in month-*t*:

$$Y_{it} = \beta_0 + \beta_1 Mth_{it} + \beta_2 Post_{it} + \beta_3 (Post_{it} * Mth_{it}) + X'_{it}\gamma + \epsilon_{it} \qquad (3)$$

where Mth_{it} is an indicator for whether the observation-month is a WEP-month (May – September),[42] $Post_{it}$ is an indicator for whether the observation occurred after the WEP policy came into effect (i.e. in July 2018 or later), and X'_{it} is a vector of control variables. In equation (3), the DiD treatment effect (β_3) is identified from the interaction of the Mth_{it} and $Post_{it}$.

The main assumption underlying equation (3) is that, in the absence of the WEP policy, the outcome has a stable pattern of seasonal variation over the year (in particular between the winter and non-winter months), captured by the Mth_{it} variable.[43] The specification allows a common-shift effect across the seasons in the post-WEP period (captured by the $Post_{it}$ variable), and identifies the DiD treatment effect (β_3) as any additional effect from the interaction of the Mth_{it} and $Post_{it}$ variables. To the extent that there is a stable policy response over time, the different WEP start dates in 2018 (July) and 2019 (May) should enable more robust identification of effects: that is, spurious effects would have to occur over different periods each year.

As in our previous analyses, the analysis here focuses on the intention-to-treat effects of WEP associated with working-age main benefit recipients. Because WEP eligibility requires receiving either a main benefit (or NZS/VP), rather than not being employed, we focus primarily on benefit status. We treat this as the extent margin response to the WEP financial incentives. As

[41] One caveat for this is that if the WEP improves health outcomes, employment outcomes may improve and the need for future hardship assistance may fall. If so, the probability of leaving benefits and entering employment may increase; and working individuals in these families may take less time off work due to illness (and their children less time out of school). Given the recent initiation of the WEP programme, there may not be a sufficiently long follow-up period available to assess these effects. Also, we are not aware of data that would enable a reliable assessment of the effects on work absences. Moreover, the impact of the pandemic in 2020 may make it difficult to distinguish its effects from those of the WEP programme for this component of the project.

[42] Alternatively, Mth_i may represent a vector of seasonal variables, such as monthly indicator variables. If so, the DiD effect may be either a simple homogeneous effect (i.e., β_3 is a scalar) or vary seasonally (i.e., β_3 is a vector).

[43] For example, there may be seasonal variation in benefit receipt or employment and earnings due to variation in work availability.

a second focus, we also consider the intensive margin response of the earnings: that is, whether a person or family may lower its earnings in order to maintain its benefit status and so be eligible for the WEP. Finally, as WEP payments are made to individuals on the basis of their family structure (i.e. single versus two of more), it is not clear to what extent any response occurs at the individual or household level. We will focus primarily on the household level (i.e. whether or not someone in the household is eligible for WEP, and household earnings), but will also examine the equivalent individual-level measures.

5.2 Data

We first construct a sample using data from the Household Labour Force Survey (HLFS) over the period 2016 – 2021. This sample includes socio-demographic and family status information collected in the HLFS. For our analysis of the financial incentives to receive benefits, we link this sample to data from IRD's Employer Monthly Schedule (EMS) tables in the IDI to measure monthly employment and main benefit-receipt status, and earnings when employed.

For the analysis of benefit take up, we match the HLFS sample to monthly employment and earnings data from the IRD's Employer Monthly Schedule (EMS) tables in the IDI. As well as PAYE (pay-as-you-earn) tax withheld wage and salary earnings and tier-1 main benefits, the EMS also contains information on some other tax-withheld payments.[44] We focus primarily on the (wage and salary) earnings and benefit payments, and keep information on the monthly receipt of these payments for any calendar months during a quarter that a person appears in the HLFS. As the HLFS typically includes households in the sampling frame for eight quarters, this means individuals may appear in the sample for up to 8 quarters with up to 24 months of EMS data.[45]

As discussed above, we focus primarily on the WEP incentives at the household-level, but also consider equivalent parallel individual-level analyses. For this, we measure each individual's monthly benefit status according to whether or not they receive any benefit in that month; and we measure a household's benefit status according to whether or not any individual was on benefit in that month. Similarly, our intensive-margin measure contains each individual's monthly EMS wage and salary earnings, and the household measure aggregates these earnings across individuals in the household.

[44] These include paid parental leave (PPL), student allowances (SA), earnings related ACC payments, New Zealand Superannuation (NZS), and self-employment earnings that have tax-withheld at source. Throughout this analysis (and in the next section's analysis of hardship assistance), we use nominal income amounts and don't adjust for inflation. As the DiD (and DDD) analyses control for any inflationary effects from the comparison groups, the estimated policy effects are robust to this concern (albeit in nominal values); in any case, inflation was comparatively muted over the sample period (e.g. CPI inflation was about 6.5% over the four year period from second quarter 2016 to 2020).

[45] Due to attrition and other factors, individuals often have fewer observations, especially for those in households at the start (nearing the end of their HLFS-period) and end (starting their HLFS-period) of the sample period. Individuals may also miss one or more quarters and are excluded during these periods.

For the benefit analysis, we focus on all working age (18-64 year old) individuals in the HLFS, and include demographic, earnings and benefit receipt information for all individuals in their households. Table 12 presents summary statistics of our WEP benefit analysis sample.

5.3 Results

We now discuss the results of our analysis of the financial incentive effects of the WEP on benefit receipt. We focus first on the extensive margin of benefit receipt, and then examine whether there are effects on the intensive margin of earnings. The analysis and discussion concentrate on results estimated over the pre-COVID-19 period, from April 2016 – March 2020; results estimated over the full period until June 2021 are broadly comparable.

Table 13 summarises the extensive-margin results on household benefit receipt from linear probability regressions. The first three columns are based on static models of benefit receipt. In column (1), we present the results for the simple DiD model, which includes only controls for monthly seasonal effects that, in the absence of WEP, are assumed to be stable before and after the WEP policy was introduced. The estimated DiD effect of the WEP in this specification is a statistically insignificant <u>reduction</u> in the incidence of benefit receipt by about 0.4 pp in the winter months. Next, we control for household and demographic characteristics:[46] the results in column (2) are again statistically insignificant, and suggest the policy led to about a 0.1 pp lower benefit receipt. The third static specification we present allows for separate effects by the type of main benefit received by the household:[47] the results are again generally statistically insignificant and negative. The exception is Supported Living Payments (SLP), which has a statistically significant <u>positive</u> coefficient, suggesting households who received SLP had about 2.5 pp higher rate of benefit receipt in winter months as a result of the WEP. To the extent that the financial incentives are (more) likely to affect households with other benefit types – in particular, Jobseeker Support (JSS) and Sole Parent Support (SPS) with job search obligations – we suspect this latter result is largely spurious. Otherwise, these static regression analyses provide no evidence of any (positive) response of the WEP financial incentives to be on-benefit.

One issue with the static regressions for monthly benefit receipt is that because benefit spells typically span months this may lead to time-aggregation bias in the results. To address this concern, we have estimated various dynamic regressions for benefit receipt, in which we control

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[46] These include a quadratic in age, and controls for gender, qualifications, total ethnicity (Māori, Pacific peoples, Asian, and other non-European), and household size and number of children in the household.

[47] The benefit type effects are identified from MSD main benefit receipt information over the HLFS-period for each person and then aggregated within the household. Because of this, the benefit type indicators are not mutually exclusive for a household, either because a person may receive more than one benefit over the period or because different people in the household may receive different benefits.

for the (monthly) first lag of benefit receipt. The results from this analysis are presented in columns (4) – (7) of Table 13. The first specification (in column (4)) is the simplest model that includes the lagged benefit receipt in the simple static regression from column (1). In this specification, we estimated a positive but relatively small and statistically insignificant effect of the WEP on benefit receipt in the winter months of about 0.08 percent.

In column (5), we control for demographic and household characteristics, as well as interactions between the lagged benefit status and each of the post- and winter-month effects. The estimated effect of the WEP is somewhat higher than in column (4), but still statistically insignificant and again suggesting the WEP led to about 0.11 pp higher benefit receipt during the winter months. In column (6), we further allow for dynamic treatment effects by interacting the treatment interaction with lagged benefit status. In this specification, both the main effect (0.07) and the interaction effect (0.25) are positive but statistically insignificant. In this specification, the main effect captures the WEP entry effect onto benefit (i.e. the entry rate is 0.07 pp higher in the winter months as a result of the WEP); and the combined main plus interaction effect captures the WEP on benefit retention effect (i.e. those on benefit are 0.32 pp more likely to stay on benefit as a result of the WEP).

Finally, in column (7), we allow both the main and lagged benefit effects to vary by benefit type, by including additional interactions with the benefit types. There are again no statistically significant coefficients of interest, but the point estimates suggest the (main) benefit-entry effects of WEP are stronger for households with JSS and SPS recipients than those with SLP and other benefits. The lagged interaction effects are somewhat more complicated to interpret, but they suggest positive benefit retention effects for JSS (0.6 pp) and SLP (1.5 pp), and negative effects for SPS (-0.6 pp) and other benefits (-0.4 pp).

In summary, there is almost no evidence of any significant benefit response to the financial incentives associated with the WEP. However, the dynamic model results in Table 13 are suggestive of possible effects on both entry-to and exit-from benefits.

We next consider possible intensive-margin effects of the WEP policy on households' earnings. For this analysis, we focus on log(household monthly earnings) from the EMS, and summarise the results in Table 14. Column (1) presents the estimate for the simple DiD regression (with monthly controls), which suggests the WEP lowered winter month earnings by about 0.6 percent and is statistically insignificant. Controlling for demographic and household characteristics, column (2) shows this estimate is larger and statistically significant, with earnings estimated to be 1.1 percent with WEP. In column (3), we interact the WEP treatment with the household's benefit status, and find the main effect (-0.9) is statistically significant while the on-benefit interaction (-1.6) is not. The former suggests earnings of non-beneficiary households were 0.9 percent lower during WEP periods, although it is not obvious how this would be related to the WEP, and may reflect spurious effects; while the earnings of beneficiary households were about 2.5 percent lower.

In the final two columns of Table 14, we extend the regressions to allow for benefit-type effects. Although the main and on-benefit interaction effects remain negative, none of the estimates is individually significant. In addition, the signs on the benefit type interaction coefficients suggest weaker earnings effects for JSS and SPS, and stronger effects for SLP and other benefit types. As discussed above, the signs of these interactions seem at odds with the expectation of greater responsiveness to financial incentives among JSS and SPS recipients.

In addition to the household-level extensive (benefit receipt) and intensive (earnings) margin results presented here, we have estimated analogous regressions for individual benefit receipt and earnings. The results from those regressions are broadly consistent with the household results discussed here.

We emphasise the results from this analysis are generally statistically weak, so we do not believe any strong inference or conclusions should be drawn. However, one possible rationalisation for the apparently perverse results of strong (e.g.) effects for SLP households may be that WEP has the effect of reducing financial hardship in these households which, in turn, reduces the need to supplement their income with additional employment earnings. This observation provides some motivation for our final analysis of possible effects of the WEP on receipt of hardship support payments.

6 Effects of the WEP on hardship assistance

The final analysis we present is to examine whether the WEP policy had any effect on the receipt of hardship assistance.[48] The WEP is not treated as assessable income for Temporary Additional Support (TAS) and for most types of Special Needs Grants (SNG), however it is included in SNGs for power and possibly food, which are generally considered generic cash payments.

As at the end of March 2018, there were approximately 63,000 TAS recipients, and in the first quarter of March 2018, there were 186,000 SNGs, 118,000 Benefit Advance Payments (BAP), and 15,000 Recoverable Assistance Payments (RAP) (Welfare Expert Advisory Group, 2019). Not all of these recipients would have received WEP in its first year.

[48] Hardship assistance covers Temporary Additional Support (TAS) payments, and one-off payments, such as Special Needs Grants (SNG), Benefit Advance Payments (BAP) and Recoverable Assistance Payments (RAP).

The granting of hardship assistance is the result of a potentially complicated set of demand- and supply-side factors. For example, on the demand side, because WEP provides additional income, WEP recipients may feel less need for hardship assistance, and result in fewer client-initiated applications and less subsequent hardship assistance. On the other hand, the assessment of hardship applications may also be affected by WEP payments which would affect the determination of applications. In addition, from discussions with MSD staff, we understand operational factors largely independent of the policy parameters, may also affect both clients' applications for assistance and also the assessment of any application.

To examine hardship assistance receipt, we examine the likelihood that WEP recipients' incidence of assistance changed during the winter months after WEP came into effect.

6.1 Methodology

Our analysis of the effects of the WEP on receipt of hardship support adopts the same DDD approach used to analyse the effects on expenditure, discussed in section 4.1 and described in equation (2). The interpretation of the DDD approach in this context is that it allows the incidence (and amount) of hardship receipt to vary systematically for those who are WEP-eligible (i.e. receiving WAB or NZS/VP) or not, in the WEP-winter versus non-winter months, and before and after the policy was introduced. As well as variation along each of these primary dimensions, hardship receipt may vary along the three two-way interactions (between eligibility, time of year, and before and after policy). Any remaining difference in hardship receipt for recipients in winter after the WEP introduction is ascribed to the effects of the policy, controlling for household and demographic characteristics.

6.2 Hardship-receipt analysis sample

For our analysis of hardship receipt, we use the same HLFS-EMS sample described in section 5.2. This sample is further matched to MSD tables on second tier (supplementary) and third tier (hardship) benefit receipt to identify receipt of hardship assistance. The hardship benefit grants we include are second-tier Temporary Additional Support (TAS) payments that are available for up to 13 weeks at a time, and one-off third-tier Special Needs Grants (SNG), Benefit Advance Payments (BAP) and Recoverable Assistance Payments (RAP).

For individuals in the HLFS sample, we construct monthly indicators of working-age benefit receipt and NZS/VP from the EMS, and indicators of hardship receipt and the amount of hardship payment received.[49] Again, we aggregate the monthly individual receipt and amounts

[49] Note, this combines the flow receipt of TAS payments with the lump-sum receipt of the tier-3 payments.

received to the household-level, and our analysis focuses on individuals aged 18 and over, and the households they are in. Our primary analysis focuses on the pre-COVID-19 period, April 2016 – March 2020, which includes 27 months before the introduction of the WEP in July 2018 and 21 months following that date.

Table 15 presents summary statistics of our WEP hardship-receipt analysis sample. The sociodemographic and household characteristics of the sample are similar to those for the benefit analysis sample in Table 12. Of note for the current analysis, in any month a little over 5% of adults lived in households that received some hardship payment, and over 14% received some hardship payment at some stage over their sample period. For our main period of interest, the incidence of monthly hardship receipt was somewhat higher after the introduction of the WEP policy (5.7% compared to 4.9% before the WEP). The average monthly household hardship amounts received during this analysis period was almost $2,200, and the average total household hardship received was about $2,700. These amounts were noticeably higher after the introduction of the WEP: average monthly payment of $2,560 compared to $1,850 pre-WEP, and $3,220 compared to $2,220 total household payments.

6.3 Results

The results of our analysis on the effects of the WEP on hardship take-up are summarised in Table 16 for the incidence of hardship receipt, and in Table 17 for hardship grant amounts.

We first discuss the estimated effects of the WEP on whether households received hardship payments, in Table 16. Column (1) presents the simple DDD estimate, just controlling for monthly seasonal and annual differences in hardship receipt. This specification finds a statistically significant effect, suggesting the WEP reduced the incidence of hardship payments by 0.8 pp during winter months. Controlling for demographic and household characteristics in column (2), the estimated effect (-0.67) is reduced but still statistically significant. In column (3) we allow the WEP effects to vary between the WAB or NZS/VP populations: this suggests a much larger (1.8 pp) reduction in the incidence of hardship receipt for WAB households and a statistically insignificant and relatively smaller (0.2 pp) reduction for NZS/VP households. In the final specification in column (4), we control for whether the household ever received the various benefit type categories over its HLFS sample period. The results from this specification are relatively uninformative, with the benefit type effects imprecisely estimated; however, the main result from column (3) that the WEP reduced the incidence of hardship receipt in winter month for households receiving WAB still holds.

In Table 17 we report results for analogous specifications of the amount the hardship assistance received (among those receiving hardship). The results in the first two columns suggest the WEP had a small (1.8-1.9 percent) and imprecisely estimated combined effect on hardship amounts received in winter months.[50] In column (3), we allow for separate effects for WAB and NZS/VP recipients, and estimate the WEP had little effect on the amount of hardship grants of WAB-receiving households, but reduced the average hardship grant of NZS/VP households by about 22 percent. The results from the final specification including benefit type effects are again relatively uninformative due to collinearity between the NZS/VP and ever-NZS/VP and imprecision in the benefit type effects, but there again is net negative effect of the WEP on NZS/VP households' hardship amounts in the winter months.

In summary, the results of the DDD analysis of the effects of the WEP on hardship receipt suggest there was a lower incidence of hardship receipt by WAB households, and a reduction in hardship amounts received by NZS/VP households. The former effect is consistent with the results in Weatherall et al. (2019), and suggests that the additional financial support provided by WEP is sufficient to reduce the incidence of hardship payment by WAB recipients. However, given the absence of an effect on the incidence of hardship receipt by NZS/VP households, it's not clear how the latter effect can be interpreted as a causal effect of the policy. We suspect it is more likely a spurious result. For example, as a robustness check, we restrict the sample to individuals aged 55 and older who are not on working age benefits, which is arguably a better comparison group for NZS/VP recipients: in this case, the estimated effects of WEP on the incidence and amount of hardship receipt for NZS/VP are small and statistically insignificant.

7 Concluding discussion

We analysed the effects of the WEP policy along several distinct dimensions using a Difference-in-Difference-in-Difference approach: home heating and energy expenditure, other household expenditures, self-reported material wellbeing, hospitalisation frequency and severity, main-benefit status and earnings of working age recipients, and hardship grant payments. In general, our analysis finds little evidence of statistically insignificant effects of the policy on outcomes, but the results are broadly consistent with small favourable impacts.

First, using the HES Expenditure survey we document that the WEP amounts to a substantial fraction of recipient households' reported energy expenditures in winter (120% on average and 60% at the median), although a relatively small fraction of households benefit

[50] In subgroup analysis, we find that this effect is driven by a reduction in the incidence of SNG during the WEP period.

support, and total income and expenditure (4-7% on average). The latter suggests any policy effects may be modest which, because of relatively small analysis samples, may be difficult to detect statistically.

Our analysis of household energy expenditures finds no statistically significant effects of the policy, although the point estimates suggest positive effects on energy expenditures in WEP-recipient households. The estimated combined (for WAB and NZS/VP recipient households) of a 0.5 pp increase in energy expenditure shares suggests about an 8% increase in share, or that recipient households spent on average almost $6 more per week on energy (about 24% of the WEP received). Estimating separate WEP-effects for NZS/VP and WAB recipients, suggests somewhat stronger effects for NZS/VP recipients. Moreover, restricting the sample to more age-relevant comparison households suggests potentially stronger effects for both NZS/VP households and household with working age benefit recipients, but these results are statistically weak. To the extent that the universal entitlement and higher income support of NZS compared to WAB results in relatively fewer NZS recipients being financially constrained and responding to the WEP policy, these results suggest they have substantially larger response to the WEP labelling.

The results of our analyses of the effects on self-reported well-being measures are mixed. On one hand, we estimate that WEP caused statistically significant lower financial hardship for households with working age benefit recipients on two of the three measures we examined; however, we find perverse effects on all three heating-related wellbeing measures. We estimate consistently small and insignificant effects for households with NZS/VP recipients. The relative effects for NZS and WAB recipients, particularly on financial hardship measures, are consistent with the hypothesis that NZS/VP recipients face relatively fewer financial constraints.

The analysis of housing-related hospitalisation outcomes again yield statistically insignificant results related to the WEP policy effects. However, the estimates generally suggest improved outcomes for individuals in WEP-eligible households, with the larger effects for those in NZS/VP households. Moreover, our results indicate that the WEP is targeted toward the correct groups, with individuals in NZS/VP households being associated with an increased likelihood of being hospitalised in general but also particularly so in winter (May-September). As expected, these months were also consistently associated with worse hospitalisation outcomes (increased likelihood, frequency, length of stay, and cost).

Given the lack of robust results found in the analysis of household expenditures, the weak results found for the hospitalisation outcomes is perhaps not surprising. The lack of significance could be due to small sample sizes or size of the policy treatment. A recent study found that the expected cost to heat one child's bedroom to the World Health Organisation (WHO) recommended temperature was NZ$58 per month – 46% of a family's WEP amount (Shorter et al., 2022). It is also possible that public health messaging around WEP about the benefits of increasing temperatures in homes may have induced behaviour changes even in households which did not receive WEP, affecting both treated and control households, making our reliance on a differencing measure problematic.

We also find only weak statistical evidence that the increased financial incentives of WEP to be on-benefit led to an increase in winter benefit receipt or a reduction in earnings. Finally, our analysis finds there was a reduction in hardship receipt by working age beneficiary households associated with the introduction of the WEP. This reduction was concentrated in Special Needs Grants, which is consistent with preliminary analysis conducted by Weatherall et al. (2019) that found the WEP was associated with significant reductions in the incidence of food related SNGs.

There are several possibly fruitful future research areas. These include, first, more detailed analysis of the differences in WEP policy effects on the NZS/VP and WAB recipients, focusing on the relative needs and responses across and within the two subgroups. Second, a more detailed study of the timing between energy use and expenditure, using energy provider data newly added to the IDI. Third, although our approach of assuming WEP is a household level treatment has led to the need for somewhat restricted sample analysis based on identified households, it may be possible to gain greater leverage on the effects on health outcomes from analysis based on the full IDI population.

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Table 1: HES Expenditure analysis sample – 2015/16 & 2018/19

	2015/16		2018/19	
Household characteristics:	Full sample	WEP eligible	Full sample	WEP eligible
No. observations (households)	3,498	651	3,930	672
Population size (households)	1,690,000	281,000	1,758,000	321,000
European ethnicity	0.807	0.786	0.769	0.782
Māori ethnicity	0.167	0.210	0.177	0.243
Pacific ethnicity	0.050	0.064	0.071	0.069
Asian ethnicity	0.146	0.085	0.152	0.075
Miscellaneous ethnicity	0.062	0.089	0.098	0.100
Working-age Benefit or NZS/VP	0.391	1	0.432	1
Working-age Benefit	0.133	0.363	0.183	0.452
NZS / VP	0.275	0.690	0.271	0.604
Supplementary Benefit	0.228	0.516	0.268	0.539
Winter Energy Payment (WEP)	0	0	0.420	0.978
House	0.825	0.801	0.807	0.782
Public rental	0.038	0.082	0.054	0.109
Private rental	0.238	0.203	0.243	0.231
Household size	2.71	2.38	2.73	2.36
	(1.45)	(1.50)	(1.42)	(1.40)
No. aged 15+	2.15	2.04	2.18	2.02
	(0.99)	(1.03)	(1.00)	(1.01)
No. aged <15	0.54	0.32	0.54	0.32
	(0.97)	(0.83)	(0.96)	(0.82)
Max age in household	53.45	65.45	53.78	63.65
	(16.88)	(15.74)	(16.69)	(16.41)
Adult share:				
Female	0.531	0.580	0.532	0.579
	(0.28)	(0.32)	(0.28)	(0.33)
No qualifications	0.211	0.367	0.209	0.318
	(0.35)	(0.41)	(0.34)	(0.41)
School quals	0.260	0.246	0.253	0.237
	(0.34)	(0.34)	(0.34)	(0.35)
Post-school quals	0.266	0.256	0.271	0.277
	(0.34)	(0.35)	(0.36)	(0.38)
University quals	0.246	0.107	0.238	0.128
	(0.36)	(0.26)	(0.36)	(0.28)
Employed	0.617	0.331	0.594	0.299
	(0.40)	(0.39)	(0.41)	(0.38)
Unemployed	0.032	0.050	0.030	0.040
	(0.13)	(0.17)	(0.13)	(0.16)
NILF	0.352	0.619	0.353	0.632
	(0.40)	(0.41)	(0.41)	(0.41)

Notes: All summary statistics are weighted by HES sample weights. Standard deviations in parentheses. Household level ethnicity is defined by whether there is any adult with that ethnicity in the household.

Table 2: HES Expenditure sample – summary of expenditure, income and WEP

Household Expenditures and incomes:	2015/16 Full sample	2015/16 WEP eligible	2018/19 Full sample	2018/19 WEP eligible
Weekly amounts ($)				
Total expenditure	1,347	943	1,405	1,039
	(1174)	(754)	(1125)	(1052)
Energy expenditure	46.4	46.7	44.2	43.4
	(30)	(35)	(28)	(28)
Total income	1,900	1,315	1,948	1,448
	(1778)	(1075)	(1928)	(1557)
Main benefit(1)	161	413	190	433
	(236)	(198)	(260)	(213)
Working-age Benefit	31.0	86.0	49.0	120.2
	(97)	(150)	(128)	(174)
NZS/VP	130	327	141	313
	(226)	(253)	(248)	(284)
Total support(1)	180	460	220	496
	(257)	(218)	(289)	(231)
WEP	0	0	10.7	24.6
			(14)	(11)
Shares:				
Energy / expenditure	0.049	0.068	0.048	0.065
	(0.04)	(0.06)	(0.05)	(0.06)
WEP / expenditure	0	0	0.019	0.044
			(0.05)	(0.04)
WEP / income	0	0	0.014	0.037
			(0.05)	(0.09)
WEP / Energy exp [Median]	0	0	0.51 [0.00]	1.21 [0.60]
			(3.82)	(7.72)
WEP / Main benefit(1)	0	0	0.067	0.066
			(0.05)	(0.05)
WEP / WAB	0	0	0.123	0.107
			(0.25)	(0.10)
WEP / NZS/VP	0	0	0.057	0.057
			(0.04)	(0.03)
WEP / Total support(1)	0	0	0.049	0.053
			(0.03)	(0.02)

Notes: All summary statistics are weighted by HES sample weights. Standard deviations in parentheses. The income amounts are calculated as unconditional averages (i.e. including households with zero amounts).
(1) "Main benefit" includes tier-1 working-age main benefit and NZS/VP amounts; "Total support" also includes tier-2 supplementary benefit income support (except WEP).

Table 3: Effects on Household energy expenditure shares

Treatment effects	Simple DDD	+Seasonal controls	+Covariate Controls	Separate WAB, NZS/VP
Combined	1.116**	1.164***	0.511	
WAB/NZS-VP:	(0.44)	(0.34)	(0.34)	
Working-age benefit:				0.238
				(0.48)
NZS/VP:				0.540
				(0.38)
Seasonal controls	N	Y	Y	Y
Covariate controls	N	N	Y	Y
R-squared	0.075	0.084	0.462	0.462
Observations	7,431	7,431	7,431	7,431

Notes: All coefficients have been multiplied by 100, so represent percentage point effects. All regressions are weighted using the HES sample weights (expenditure sample weights in 2018/19); standard errors are in parentheses. Seasonal controls include calendar month differences in seasonal expenditure patterns Each regression includes the main effects relevant to the specification: i.e. columns 1–3 include main and interaction effects for HES-sample 2018/19 vs 2015/16, winter vs non-winter, working-age benefit or NZS/VP recipient, and the two-way interactions between each of these); column (4) includes separate WAB and NZS/VP effects and interactions. Covariate controls in columns 3 and 4 include indicators for geographic region, total ethnicity, quadratics in household size, the number of rooms in the house and the oldest age of persons in the household, the adult share and female share of adults, indicators for whether the dwelling is a house (versus apartment), whether it is a public or private rental (vs owner occupied), family type (Single vs Couple, with vs without children, and miscellaneous households), and log(total expenditure).
*** p<0.01, ** p<0.05, * p<0.1

Table 4: Effects on Household energy expenditure shares – Robustness checks

	Aged 55+ Non-WAB		Aged 55-74 Non-WAB		Aged <65 <Median total expenditure	
	Seasonal	+Controls	Seasonal	+Controls	Seasonal	+Controls
NZS/VP effects:	1.425**	0.742	1.678**	0.211		
	(0.69)	(0.53)	(0.70)	(0.52)		
WAB effects:					0.648	0.739
					(0.93)	(0.72)
R-squared	0.049	0.445	0.044	0.489	0.048	0.439
Observations	3,288	3,288	2,418	2,418	2,679	2,679

Notes: All coefficients have been multiplied by 100, so represent percentage point effects. Standard errors are in parentheses. Samples with someone aged at least 55 restricted to households with no working age benefit receipt. Control variables are included in each regression. See Table 1 for additional notes.
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Effects on expenditures – other HES expenditure groups

	HH Electricity	Diary	HH Operations	Home Maintenance	Housing costs	Mortgages
WAB	-0.199	2.463	-0.455	-0.517	2.438*	-0.835
	(0.47)	(2.32)	(0.45)	(0.94)	(1.37)	(1.67)
NZS/VP	0.276	0.576	0.582	-1.080	-0.979	-0.315
	(0.38)	(1.89)	(0.36)	(0.77)	(1.11)	(1.36)
R-squared	7,431	0.110	0.027	0.127	0.664	0.292
Observations	7,431	7,431	7,431	7,431	7,431	7,431

Notes: All coefficients have been multiplied by 100, so represent percentage point effects. All regressions are weighted using the HES sample weights (expenditure sample weights in 2018/19), based on the specification in column (6) of Table 3; standard errors are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 6: HES Wellbeing analysis sample – 2015/16 & 2018/19

	Full	2015/16	2018/19
Nobs (RR3)	24,156	3,498	20,655
Npop (R1000)	3,446,000	1,690,000	1,755,000
Binary count variables:			
European ethnicity	0.785	0.807	0.765
Māori ethnicity	0.179	0.167	0.191
Pacific ethnicity	0.060	0.050	0.070
Asian ethnicity	0.152	0.146	0.158
Miscellaneous ethnicity	0.071	0.062	0.079
WA Benefit or NZS/VP	0.406	0.391	0.421
WA Benefit	0.154	0.131	0.174
NZS / VP	0.270	0.273	0.268
Supplementary Benefit	0.347	0.228	0.461
WEP	0.206	0	0.405
Continuous variables:			
house	0.819	0.825	0.814
public rental	0.044	0.038	0.050
private rental	0.244	0.238	0.250
Max age in HH	53.6	53.5	53.7
	(16.85)	(16.88)	(16.81)
HH size	2.743	2.711	2.775
	(1.45)	(1.45)	(1.46)
No. aged 15+	2.190	2.154	2.227
	(1.02)	(0.99)	(1.04)
No. aged <15	0.540	0.541	0.540
	(0.96)	(0.97)	(0.96)
Adult share:			
Female	0.527	0.531	0.525
	(0.28)	(0.28)	(0.28)
No qualifications	0.213	0.211	0.214
	(0.34)	(0.35)	(0.34)
School quals	0.260	0.260	0.259
	(0.34)	(0.34)	(0.34)
Post-school quals	0.265	0.266	0.263
	(0.34)	(0.34)	(0.34)
University quals	0.244	0.246	0.243
	(0.36)	(0.36)	(0.36)
Employed	0.611	0.617	0.606
	(0.41)	(0.40)	(0.41)
FT employed	0.481	0.488	0.474
	(0.40)	(0.39)	(0.40)
Unemployed	0.032	0.032	0.031
	(0.13)	(0.13)	(0.13)
NILF	0.350	0.352	0.349
	(0.40)	(0.40)	(0.40)

(continues)

Table 6 (continued)

	Full	2015/16	2018/19
Wellbeing indicators:			
Feel cold	0.185	0.168	0.201
Damp & mould	0.224	0.202	0.244
House is cold	0.228	0.221	0.234
Utilities	0.087	0.079	0.095
Unexpected $500	0.192	0.182	0.202
Insufficient income	0.104	0.099	0.108
Life dissatisfaction	0.063	0.060	0.065
Annual incomes:			
HH total income	101,585	98,818	104,250
	(100,882)	(92,433)	(108,342)
HH total WA Benefit & NZS/VP	9,084	8,352	9,789
	(13,032)	(12,231)	(13,724)
HH total WA benefit	2,033	1,604	2,445
	(5,839)	(5,028)	(6,499)
HH total NZS/VP	7,051	6,748	7,343
	(12,348)	(11,736)	(12,905)
HH total supplementary benefit	1,383	995	1,758
	(3,850)	(3,100)	(4,422)
HH total WEP	117	0	230
	(254)	0	(317)

Notes: All summary statistics are weighted by HES sample weights. Standard deviations in parentheses. Household level ethnicity is defined by whether there is any adult with that ethnicity in the household. The annual benefit income amounts are calculated as unconditional averages (i.e. including households with zero amounts).

Table 7: Effects on HES Material Wellbeing

	Heating related:			Financial hardship:			Life
	Feel cold	Damp & mould	House cold	Utilities	Unexp 500	Insuff income	Dis-satisfied
WAB receipt	0.042	0.068***	0.096***	-0.003	-0.092***	-0.049***	0.016
	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)	(0.02)
NZS/VP receipt	-0.014	-0.026	0.002	0.008	-0.003	-0.021	-0.021
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
Observations	24,135	24,087	23,961	24,096	24,084	24,120	24,075
R-squared	0.133	0.112	0.103	0.146	0.227	0.122	0.050

Notes: All regressions are weighted using the HES sample weights (material wellbeing sample weights in 2018/19), based on the specification in column (6) of Table 3; standard errors are in parentheses. The control variables are the same as those listed in the notes to Table 3, except that we control for a quadratic in log(total income) instead of log(total expenditure).
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 8: Summary Statistics for HLFS Hospitalisation Sample by Type of Beneficiary

	In household receiving WEP-eligible payments				Not in household receiving WEP-eligible payments			
	Pre-WEP		Post-WEP		Pre-WEP		Post-WEP	
WAB or NZS/VP	Non-Winter	Winter	Non-Winter	Winter	Non-Winter	Winter	Non-Winter	Winter
Number of Hospitalisations	0.0020	0.0029	0.0024	0.0031	0.0005	0.0006	0.0005	0.0006
Number of Hospitalisation Days	0.0150	0.0191	0.0136	0.0185	0.0014	0.0021	0.0018	0.0032
Hospitalisation Costs ($)	15.26	18.72	16.28	19.45	1.74	2.23	3.34	5.59
No. observations	22,845	22,380	21,765	19,845	32,250	32,514	29,898	27,921
WAB								
Number of Hospitalisations	0.0017	0.0024	0.0020	0.0025	0.0010	0.0014	0.0011	0.0015
Number of Hospitalisation Days	0.0070	0.0116	0.0079	0.0102	0.0069	0.0085	0.0065	0.0094
Hospitalisation Costs ($)	8.22	12.62	11.52	11.30	7.05	8.19	8.22	12.62
No. observations	11,046	10,509	9,462	8,193	43,500	43,749	41,991	39,351
NZS/VP								
Number of Hospitalisations	0.0021	0.0033	0.0025	0.0035	0.0007	0.0009	0.0008	0.0010
Number of Hospitalisation Days	0.0193	0.0240	0.0165	0.0224	0.0024	0.0035	0.0029	0.0045
Hospitalisation Costs ($)	19.13	23.05	18.47	23.12	2.91	3.72	4.93	6.70
No. observations	13,539	13,536	13,794	12,915	37,275	37,806	35,013	32,994
WAB or NZS/VP w/Hospitalisation								
Number of Hospitalisations	1.0202	1.0154	1.0246	1.0252	1.0080	1.0098	1.0148	1.0094
Number of Hospitalisation Days	7.8025	6.7022	5.8750	6.0756	2.6720	3.5441	3.5481	5.0283
Hospitalisation Costs ($)	7961.46	6565.84	7025.34	6371.49	3396.87	3677.95	6729.07	8823.83
No. observations	501	525	375	327	225	183	123	105
WAB w/Hospitalisation								
Number of Hospitalisations	1.0184	1.0243	1.0567	1.0288	1.0163	1.0108	1.0113	1.0195
Number of Hospitalisation Days	4.2258	4.9223	4.1135	4.2500	7.0533	6.2724	5.7262	6.2953
Hospitalisation Costs ($)	4965.14	5339.85	6015.47	4728.64	7235.21	6044.83	7257.00	7571.50
No. observations	162	171	114	90	567	543	384	342
NZS/VP w/Hospitalisation								
Number of Hospitalisations	1.0197	1.0103	1.0154	1.0222	1.0138	1.0191	1.0310	1.0207
Number of Hospitalisation Days	9.3042	7.4474	6.5785	6.5852	3.2867	3.9619	3.7713	4.7876
Hospitalisation Costs ($)	9205.23	7144.02	7386.15	6808.10	4040.47	4196.51	6415.81	7107.57
No. observations	372	393	279	252	354	315	219	177

Table 9: Regressions of any Monthly Hospitalisations

	(1) Logit model: Odds ratio *(SE)*	(2) OLS: Coefficient β *(SE)*	(3) Logit model: Odds ratio *(SE)*	(4) OLS: Coefficient β *(SE)*
Bft*Wint*PostWEP				
NZS/VP*Wint*PostWEP	0.846 (0.142)	-0.0319 (0.035)		
WAB*Wint*PostWEP	0.974 (0.191)	-0.00465 (0.039)		
ALL*Wint*PostWEP			0.754 (0.147)	-0.0303 (0.026)
Bft*Wint				
NZS/VP*Wint	1.220** (0.117)	0.110*** (0.020)		
WAB*Wint	1.031 (0.112)	0.0437** (0.021)		
ALL*Wint			1.210* (0.127)	0.0909*** (0.016)
Observations	1,906,557	1,906,557	1,906,557	1,906,557
Psuedo-R^2	0.0578	0.0015	0.0545	0.0014

Notes: The OLS coefficients have been multiplied by 100, and represent percentage point effects. Standard errors in parentheses, clustered at household level. All models include controls for age, gender, region (Auckland and Northland, the rest of the North Island, and the South Island), and year and month.
*** p<0.01, ** p<0.05, * p<0.1

Table 10: Regressions of hospitalisation length of stay

	(1) Coefficient β (SE)	(2)	(3) Odds ratio (SE)	(4)
Bft*Wint*PostWEP				
NZS/VP*Wint*PostWEP	0.00363 (0.0068)	1.055 (0.419)	1.055 (0.364)	1.017 (0.404)
WAB*Wint*PostWEP	-0.00302 (0.0029)	0.700 (0.247)	0.700 (0.300)	1.044 (0.453)
Bft*Wint				
NZS/VP*Wint	0.000260 (0.0048)	0.711 (0.161)	0.711* (0.144)	0.973 (0.183)
WAB*Wint	0.00485** (0.0020)	1.404 (0.318)	1.404 (0.330)	1.227 (0.325)
Model	OLS	Poisson	Quasi-P	NB
Observations	1,906,557	1,906,557	1,906,557	1,906,557
R-squared	0.0003	0.124		0.0381

Notes: Standard errors in parentheses, clustered at the household level. All models include controls for age, gender, region (Auckland and Northland, the rest of the North Island, and the South Island), and year and month.
*** p<0.01, ** p<0.05, * p<0.1

Table 11: Regressions of Monthly Hospitalisation Cost

	(1)	(2)
	Coefficient β *(SE)*	
Bft*Wint*PostWEP		
NZS/VP*Wint*PostWEP	2.512 (7.309)	
WAB*Wint*PostWEP	-3.637 (4.059)	
ALL*Wint*PostWEP		1.473 (5.687)
Bft*Wint		
NZS/VP*Wint	0.167 (4.594)	
WAB*Wint	5.146** (2.299)	
ALL*Wint		1.131 (3.181)
Model	OLS	OLS
Observations	1,906,557	1,906,557
R-squared	0.0002	0.0002

Notes: Standard errors in parentheses, clustered at the household level. All models include controls for age, gender, region (Auckland and Northland, the rest of the North Island, and the South Island), and year and month.
*** p<0.01, ** p<0.05, * p<0.1

Table 12: HLFS Benefit analysis sample – 2016 – 2021

	Full WAP	April 2016 -- March 2020			April 2020-
	Sample	Full sample	Pre-WEP	Post-WEP	June 2021
No. Observations	1,073,412	803,583	438,486	365,097	269,832
No. Individuals	98,721	77,811	49,647	28,164	20,910
Female	0.497	0.498	0.497	0.499	0.495
Partner	0.625	0.627	0.629	0.624	0.621
Age	40.0	40.1	40.1	40.0	39.9
	(13.2)	(13.2)	(13.2)	(13.2)	(13.2)
HH size	3.47	3.47	3.47	3.46	3.49
	(1.7)	(1.7)	(1.7)	(1.7)	(1.7)
No. children	0.86	0.86	0.86	0.86	0.85
	(1.2)	(1.2)	(1.2)	(1.2)	(1.2)
No. WAP	2.52	2.51	2.52	2.50	2.54
	(1.2)	(1.2)	(1.2)	(1.2)	(1.3)
No. Over-65	0.10	0.09	0.09	0.10	0.11
	(0.3)	(0.3)	(0.3)	(0.4)	(0.4)
No qualifications	0.129	0.129	0.134	0.123	0.128
School qualifications	0.292	0.296	0.299	0.292	0.281
Post-school qualifications	0.243	0.244	0.240	0.248	0.241
Degree qualifications	0.315	0.310	0.305	0.317	0.331
European	0.685	0.691	0.702	0.677	0.669
Māori	0.161	0.160	0.160	0.161	0.161
Pacific peoples	0.072	0.071	0.069	0.074	0.075
Asian	0.149	0.144	0.135	0.154	0.165
Miscellaneous ethnicity	0.028	0.029	0.027	0.031	0.027
W&S Employed	0.864	0.867	0.866	0.868	0.856
HH with Employment	0.929	0.930	0.929	0.931	0.926
On-benefit	0.111	0.107	0.109	0.106	0.121
HH with benefit	0.167	0.161	0.162	0.160	0.185
Monthly earnings	5,428	5,302	5,139	5,499	5,817
	(4911)	(4779)	(4702)	(4863)	(5280)
Monthly Benefits	1,226	1,187	1,172	1,207	1,331
	(483)	(478)	(451)	(508)	(484)
HH monthly earnings	10,651	10,374	10,003	10,824	11,501
	(7918)	(7647)	(7484)	(7816)	(8642)
HH monthly benefits	1,615	1,558	1,545	1,575	1,765
	(898)	(866)	(856)	(878)	(962)

Notes: All sample statistics are weighted by HLFS sample weights; standard deviations are in parentheses. The samples consists of working-age (aged 18-64) individuals in the HLFS samples over the period April 2016 – June 2021.

Table 13: Household benefit-receipt regressions

	Static models			Dynamic models			
	Simple (1)	Controls (2)	Benefit-types (3)	Simple (4)	Controls (5)	LDV effects (6)	Benefit-types (7)
Post*Winter-month	-0.368	-0.088	-0.058	0.077	0.106	0.068	0.028
	(0.30)	(0.28)	(0.11)	(0.08)	(0.08)	(0.07)	(0.03)
*JSS			-1.15				0.502
			(1.13)				(0.76)
*SPS			-1.07				0.077
			(1.52)				(1.26)
*SLP			2.60**				-1.55
			(1.24)				(1.72)
*Miscellaneous benefit			-2.32				-0.021
			(2.56)				(2.01)
*lag(Benefit)						0.249	0.660
						(0.38)	(1.00)
*lag(Benefit)*JSS							-0.072
							(1.11)
*lag(Benefit)*SPS							-1.25
							(1.56)
*lag(Benefit)*SLP							0.872
							(1.94)
*lag(Benefit)*Misc benefit							-1.09
							(2.83)
No. Observations	803,583	785,493	785,481	702,819	687,681	687,681	687,672
R-squared	0.000	0.176	0.593	0.912	0.912	0.912	0.916

Notes: All coefficients have been multiplied by 100, so represent percentage point effects. All regressions are weighted using the HLFS sample weights; standard errors in parentheses, controlling for household clustering. The benefit types are indicator variables for whether anyone in the household received that type of benefit during the period they were in the HLFS sample. In specifications that include interaction effects, the relevant main interactions (with Winter-month and Post) are also included as controls.
*** p<0.01, ** p<0.05, * p<0.1

Table 14: Intensive margin regressions – log(household earnings)

	Simple (1)	Controls (2)	Benefit-effect (3)	Benefit-types (4)	Benefit & types (5)
Post*Winter-month	-0.563	-1.11**	-0.889*	-0.764	-0.736
	(0.62)	(0.55)	(0.51)	(0.53)	(0.53)
*On-benefit			-1.58		-2.37
			(2.42)		(3.15)
*JSS				1.14	1.32
				(1.96)	(2.07)
*SPS				0.657	0.91
				(3.09)	(3.22)
*SLP				-3.82	-0.20
				(3.74)	(4.12)
*Misc benefit				-1.79	-2.54
				(4.93)	(4.94)
No. observations	740,217	723,915	723,915	723,915	723,915
R-squared	0.006	0.189	0.253	0.246	0.262

Notes: All coefficients have been multiplied by 100, so represent percentage effects. All regressions are weighted using the HLFS sample weights; standard errors in parentheses, controlling for household clustering. The benefit types are indicator variables for whether anyone in the household received that type of benefit during the period they were in the HLFS sample. In specifications that include either NZS/VP vs benefit or benefit-type effects, the relevant main interactions are also included as controls (e.g. WAB*Winter-month and WAB*Post, etc).
*** p<0.01, ** p<0.05, * p<0.1

Table 15: HLFS hardship analysis sample – 2016 – 2021

	Full Sample	April 2016 -- March 2020			April 2020-June 2021
		Full sample	Pre-WEP	Post-WEP	
No. Observations	1,411,530	1,057,539	582,498	475,041	353,991
No. Individuals	121,965	96,093	62,160	33,936	25,872
Female	0.505	0.505	0.506	0.504	0.503
Partner	0.648	0.649	0.651	0.647	0.644
Age	40.4	40.4	40.4	40.4	40.4
	(13.4)	(13.4)	(13.4)	(13.4)	(13.4)
HH size	3.51	3.51	3.52	3.50	3.53
	(1.7)	(1.7)	(1.7)	(1.7)	(1.7)
No. children	0.89	0.89	0.90	0.89	0.87
	(1.2)	(1.2)	(1.2)	(1.2)	(1.2)
No. WAP	2.53	2.52	2.53	2.51	2.55
	(1.2)	(1.2)	(1.2)	(1.2)	(1.3)
No. Over-65	0.10	0.10	0.09	0.10	0.11
	(0.3)	(0.3)	(0.3)	(0.4)	(0.4)
No qualifications	0.127	0.128	0.132	0.122	0.126
School qualifications	0.298	0.301	0.306	0.296	0.288
Post-school quals	0.244	0.244	0.242	0.247	0.242
Degree qualifications	0.311	0.306	0.299	0.315	0.325
European	0.680	0.685	0.696	0.671	0.666
Māori	0.149	0.149	0.148	0.149	0.151
Pacific peoples	0.070	0.069	0.066	0.072	0.072
Asian	0.162	0.158	0.149	0.168	0.175
Misc. ethnicity	0.029	0.030	0.028	0.032	0.028
Any HH Hardship	0.143	0.144	0.135	0.155	0.140
Monthly HH Hardship	0.056	0.053	0.049	0.057	0.066
Total HH hardship	2,777	2,703	2,220	3,223	3,011
	(4,598)	(4,392)	(3,319)	(5,261)	(5,186)
HH monthly hardship	2,214	2,192	1,852	2,558	2,285
	(3,402)	(3,371)	(2,732)	(3,912)	(3,497)

Notes: All sample statistics are weighted by HLFS sample weights; standard deviations are in parentheses. The sample consists of all individuals aged 18 and over in the HLFS sample over the period April 2016 – June 2021.

Table 16: Household receipt of hardship grants

WEP Treatment effects	Simple	Controls	NZS/VP vs WAB	Benefit types
Combined	-0.799**	-0.668*		
WAB / NZS-VP	(0.38)	(0.35)		
NZS-VP			-0.176	1.72
			(0.25)	(2.58)
WAB			-1.81**	-2.67**
			(0.91)	(1.34)
Ever rec JSS				0.797
				(0.87)
Ever rec SPS				1.06
				(1.77)
Ever rec SLP				0.797
				(1.71)
Ever rec Misc Benefit				0.388
				(1.95)
Ever rec NZS/VP				-1.85
				(2.57)
Observations	1,344,489	1,315,632	1,315,632	1,315,623
R-squared	0.078	0.165	0.252	0.270

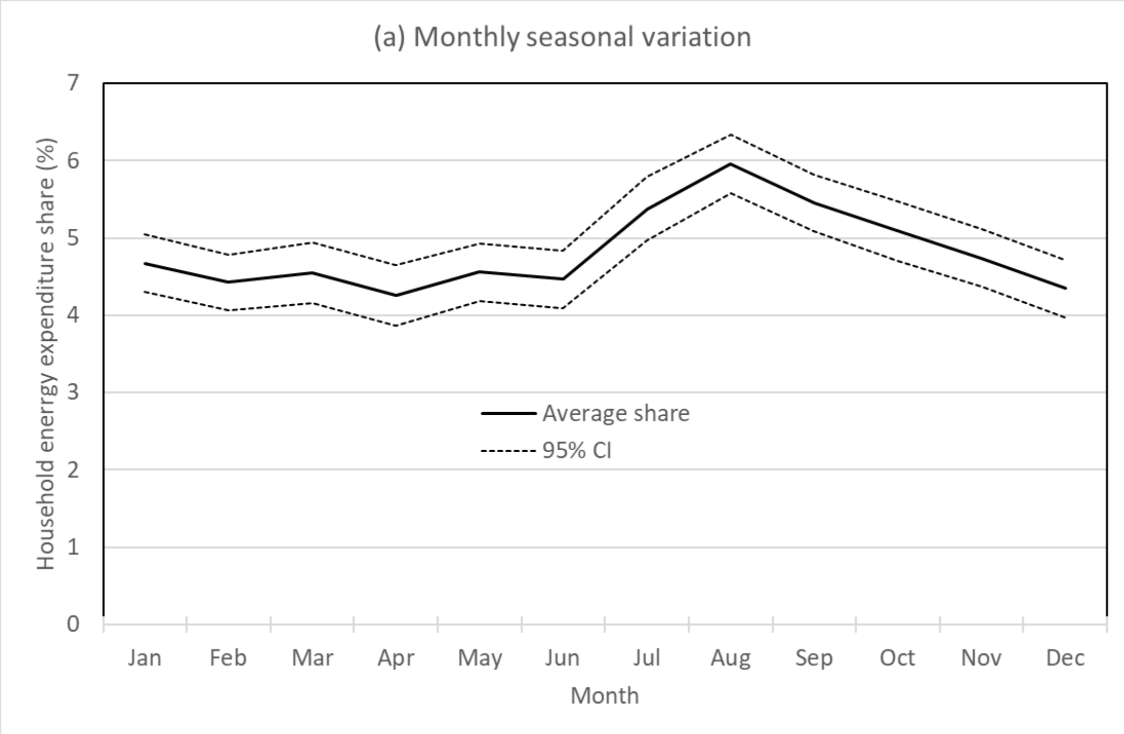
Notes: All coefficients have been multiplied by 100, so represent percentage point effects. All regressions are weighted using the HLFS sample weights; standard errors in parentheses, controlling for household clustering. The benefit types are indicator variables for whether anyone in the household received that type of benefit during the period they were in the HLFS sample. In specifications that include either NZS/VP vs benefit or benefit-type effects, the relevant main interactions are also included as controls (e.g. WAB*Winter-month and WAB*Post, etc).
*** p<0.01, ** p<0.05, * p<0.1

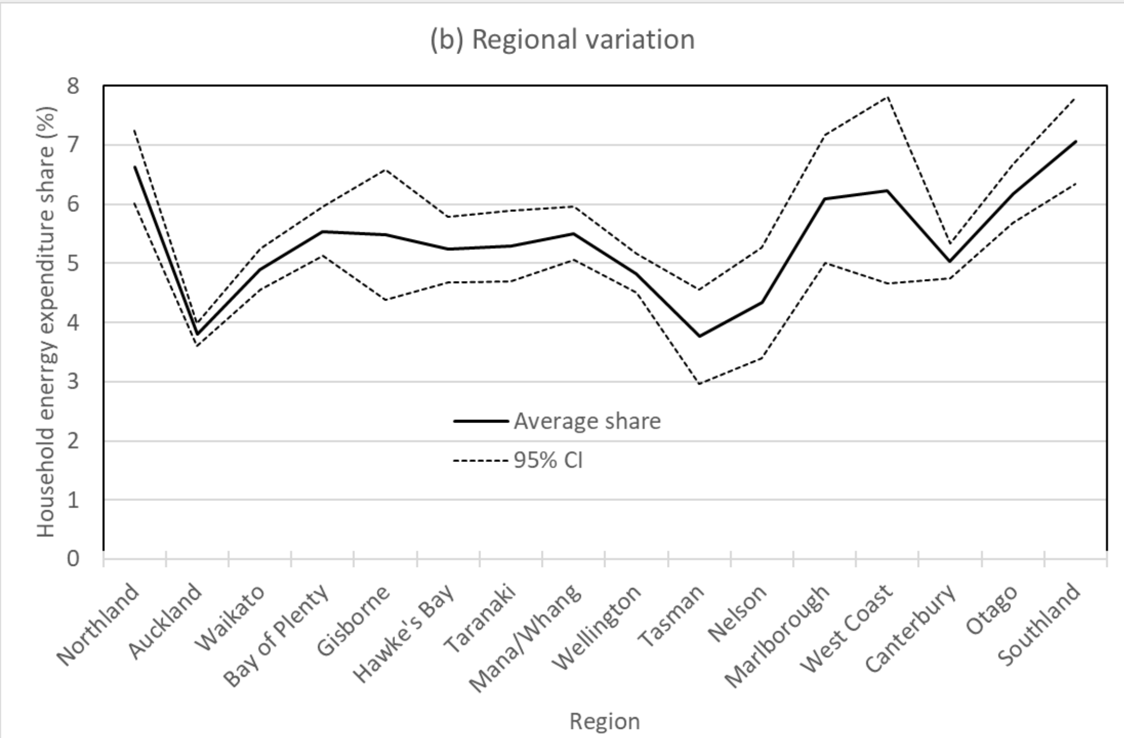
Table 17: Amount of household Hardship grants received – log(household hardship grant)

WEP Treatment effects	Simple	Controls	NZS/VP vs WAB	Benefit types
Combined	1.84	1.89		
WAB / NZS-VP	(13.1)	(12.8)		
NZS-VP			-22.4**	82.1*
			(10.3)	(42.1)
WAB			-2.38	-3.19
			(10.1)	(12.0)
Ever rec JSS				22.7
				(8.00)
Ever rec SPS				2.00
				(8.92)
Ever rec SLP				1.39
				(9.11)
Ever rec Misc Benefit				14.3
				(14.3)
Ever rec NZS/VP				-104.1**
				(41.8)
Observations	66,315	63,486	63,486	63,486
R-squared	0.002	0.032	0.032	0.043

Notes: All coefficients have been multiplied by 100, so represent percentage effects. All regressions are weighted using the HLFS sample weights; standard errors in parentheses, controlling for household clustering. The benefit types are indicator variables for whether anyone in the household received that type of benefit during the period they were in the HLFS sample. In specifications that include either benefit-receipt or benefit-type effects, the relevant main interactions are also included as controls (e.g. On-benefit*Winter-month and On-benefit*Post).
*** p<0.01, ** p<0.05, * p<0.1

Figure 1: Seasonal and Regional variation in household energy expenditure shares





Notes: Authors calculations from the HES 2015/16 and 2018/19 Expenditure survey samples.

Table A1: HES Expenditure Regressions – other expenditure groups

	Other Prop's	Rec & Cult	Med & Health	Misc payments	Tele-comms	Trans-portation	Travel	Credit & Debit accounts	Cont to Saving	Fees & Subs	Insuran ce policies	Loans
WAB	0.082	-0.393	-0.641	0.241	-0.514	-2.241	0.901	-0.119	0.556	-0.065	-0.820*	-0.112
	(0.18)	(0.24)	(0.63)	(0.32)	(0.40)	(1.38)	(1.20)	(0.17)	(0.51)	(0.55)	(0.46)	(0.27)
NZS/VP	0.269*	-0.145	-1.244**	-0.022	0.229	-1.769	2.994***	-0.015	1.083***	0.434	-0.452	-0.629***
	(0.15)	(0.20)	(0.51)	(0.26)	(0.32)	(1.12)	(0.98)	(0.14)	(0.41)	(0.45)	(0.37)	(0.22)
R-squared	0.017	0.025	0.040	0.021	0.244	0.055	0.106	0.055	0.083	0.094	0.076	0.041
Observations	7,431	7,431	7,431	7,431	7,431	7,431	7,431	7,431	7,431	7,431	7,431	7,431

Notes: All coefficients have been multiplied by 100, so represent percentage point effects. All regressions are weighted using the HES sample weights (expenditure sample weights in 2018/19), based on the specification in column 3 of Table 3; standard errors are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table A2: Health outcomes and diagnosis codes for housing-related hospitalisations

Health Outcome	Diagnosis Codes (ICD10-AM)
Asthma	J45, J46
Bronchitis	J20, J40, J41, J42
COPD	J43, J44
Cough	R05
Gastroenteritis	A0-A9; R11; K528, K529
H. pylori	C161-C169 (Non-cardia gastric cancer) K25-K28 (Peptic Ulcer) K293-K295 (Gastritis)
Haemophilus influenza (Hib)	J14, A413, A492
Hepatitis A	B15
Meningococcal Disease	A39
Pneumonia/LRTI	B59, J09-J13, J15-J18, J20, J22, A481, A482*
Respiratory Infections	J (ICD10 Chapter 10)
Rheumatic Fever	I00, I01, I02
RSV/Bronchiolitis	B974, J121, J205, J21
Tuberculosis	A15-A19, J65, N740, N741
URTI	J00-J06, J32, J36, J37
Winter colds or flu	J00 (common cold), J09-J11 (influenza)

Notes: These health outcomes have been found in the literature to have sufficient evidence to be associated with exposure to damp, cold, mould, or household crowding. The health outcome, respiratory infections, excluded any diagnoses codes in this chapter not otherwise specified.