

The impact of the Canterbury earthquakes on household income and expenditure in the Canterbury region in New Zealand

Quanfu Zhang^a, Ilan Noy^{a,b}, Yigit Saglam^a

^aSchool of Economics and Finance, Victoria University of Wellington, Wellington 6011, New Zealand

^bGran Sasso Science Institute, L'Aquila, Italy

Correspondence to: Quanfu Zhang (quanfu.zhang@vuw.ac.nz)

Abstract. Using New Zealand's Integrated Data Infrastructure (IDI), we evaluate the effects of the 2010-2011 Canterbury earthquakes on household income and expenditure. We link data from the Household Economic Survey (HES) to administrative address histories and meshblock-level earthquake intensity, and implement a propensity-score-matched difference-in-differences (PSM-DiD) design comparing exposed Canterbury households with matched North Island households. We find that total and regular household income increase by approximately 7,000-7,900 New Zealand Dollars (NZD) in both low- and high-intensity areas. Total expenditure shows no clear average DiD effect, but its composition changes substantially. In high-intensity areas, receipts and refunds, an HES category that includes insurance reimbursements and related inflows, more than double, while spending on general insurance, transportation, telecommunications, diary-recorded day-to-day purchases, fees and subscriptions also increases. Expenditure responses are broader among above-median-income households than among below-median-income households. Households later observed outside Canterbury have housing costs around NZD 25,000 higher and mortgage and loan payments around NZD 9,600 lower than households that remain, while conditional income differences are statistically insignificant. These findings show that post-disaster income growth can coexist with heterogeneous recovery costs and substantial changes in household expenditure.

1. Introduction

Disasters such as earthquakes, floods, and cyclones impose large and persistent economic costs. Beyond damage to housing and infrastructure, major shocks disrupt employment, reallocate

income sources, and force difficult choices about consumption, saving, and relocation. Understanding these household-level adjustments is essential for evaluating disaster relief, social protection, insurance arrangements, and the incidence of disaster-related losses.

The 2010 – 2011 Canterbury earthquakes in New Zealand provide a particularly informative setting. A sequence of major earthquakes caused extensive residential damage, prompted large-scale land-zoning decisions and demolition, and triggered an unprecedented reconstruction programme in and around Christchurch. The institutional environment was also distinctive: residential insurance coverage was widespread, public and private compensation flows were substantial, and the rebuild generated strong local labour demand over several years. Together, these features make the Canterbury earthquakes an informative case for studying how households adjust income and expenditure after a large but geographically concentrated shock.

A substantial macroeconomic and regional literature has documented how disasters affect aggregate output and local growth. Cross-country and regional studies find persistent level losses alongside reconstruction-driven expansions, with effects varying considerably across events and locations (e.g. Cavallo et al., 2013; Noy and duPont, 2018). More recent work using quasi-experimental designs and high-resolution data has refined estimates of local impacts and recovery dynamics (Roth Tran & Wilson, 2025). Using municipality-level panels and a synthetic-control design, duPont et al. (2015) document long-lived regional economic costs after the Kobe earthquake, consistent with persistent economic scarring. These perspectives, however, provide limited evidence on how aggregate recovery patterns translate into household behaviour and welfare.

Household- and individual-level evidence remains comparatively limited, but existing studies demonstrate that recovery paths can diverge sharply across households and institutional settings. Carter et al. (2007) show that severe environmental shocks can generate persistent asset losses and poverty-trap dynamics among vulnerable households in Ethiopia and Honduras. By contrast, Gignoux and Menéndez (2016) find that earthquake-exposed households in rural Indonesia experienced short-run economic losses followed by medium-run recovery and, in some cases, longer-run income and welfare gains. U.S. studies following Hurricane Katrina

and other disasters document substantial changes in migration, labour supply, and income trajectories, with outcomes varying across demographic groups and locations (Groen and Polivka, 2010; Deryugina et al., 2018). Work on household balance sheets and credit markets further shows that disasters can trigger substantial portfolio re-optimisation, involving interactions among insurance, debt, and government transfers that are largely invisible in macroeconomic aggregates (Gallagher and Hartley, 2017). Nevertheless, relatively little evidence exists on how disasters reshape the composition of household spending across detailed categories, including routine consumption, housing costs, insurance-related flows, debt repayment, and retirement saving. An additional limitation of much of the existing literature is that insurance settlements and other compensatory inflows are often combined with household income or are not observed separately. The HES allows us to distinguish regular household income from receipts and refunds recorded in the expenditure accounts, a category that includes insurance reimbursements and related inflows.

For New Zealand, microeconomic evidence on the Canterbury earthquakes has begun to emerge. Existing research documents macroeconomic and sectoral consequences of the rebuild, including construction-led productivity gains and the reallocation of economic activity within Christchurch (Parker and Steenkamp, 2012; Wood et al., 2016). More recent work uses administrative microdata to track individual incomes and labour-market trajectories in the affected region, highlighting heterogeneous impacts across sectors and along the income distribution (Abdeljawad and Noy, 2025a, 2025b). However, this literature does not directly analyse household-level income and expenditure responses using nationally representative survey data, nor does it examine in detail how the composition of spending changes across expenditure categories and household types.

This paper addresses three questions. First, did the Canterbury earthquakes change household income and the composition of expenditure for households located in exposed areas relative to matched households in comparable unexposed areas? Second, how did these responses vary across the income distribution and with earthquake intensity within Canterbury? Third, how did income and expenditure outcomes differ between households that remained in Canterbury and those that relocated elsewhere after the earthquakes?

To answer these questions, we link the Household Economic Survey (HES) to administrative data within the Integrated Data Infrastructure (IDI). The HES component is analysed as a repeated cross-section, and we compare changes in average household outcomes across survey waves. Using linked administrative address histories and meshblock-level Modified Mercalli Intensity measures, we identify households whose reference person lived in Canterbury at the time of the earthquakes and construct a matched comparison group from households that consistently resided in the North Island. Propensity-score matching is conducted separately within each survey wave. We then implement a difference-in-differences design, supplemented by event-study specifications and joint pre-trend tests, to compare pre- and post-earthquake outcomes across exposure groups.

The main results are as follows. Both total household income and regular household income increased for earthquake-exposed Canterbury households relative to the matched comparison group. The estimated effects are similar in magnitude in the low- and high-intensity groups, suggesting that the income response reflects broader regional recovery and reconstruction dynamics rather than a simple linear relationship between local ground-shaking intensity and household income.

Total household expenditure shows no clear average difference-in-differences effect. However, expenditure composition changes substantially. In high-intensity areas, significant increases appear in receipts and refunds, general insurance, transportation, telecommunications, diary-recorded day-to-day purchases, fees and subscriptions, education, recreation, sport and culture, and household maintenance. Housing costs increase significantly in low-intensity areas, while fees and subscriptions increase in both intensity groups. These results indicate that households adjusted through multiple channels, including insurance and reimbursement activity, routine consumption, mobility, communication, housing and maintenance expenditure, and subscription- and service-related spending.

The composition of adjustment also differs markedly by household income. Above-median-income households exhibit relatively broad expenditure responses across insurance-related flows, housing and maintenance, telecommunications, miscellaneous payments, and fees and subscriptions. Below-median-income households show a narrower

pattern, with significant changes concentrated in receipts and refunds, medical and health expenditure, diary-recorded purchases, and travel. Insurance- and refund-related entries increase for both income groups, but the estimated response is substantially larger among above-median-income households, consistent with differences in asset ownership, insurance depth, and claim size.

Finally, among exposed households, those later observed outside Canterbury face substantially higher housing costs and lower mortgage and loan expenditure than households that remain in the region. Conditional on age, sex, and household size, the two groups do not differ significantly in total income, regular income, or total expenditure. Because relocation is self-selected, these estimates are interpreted as descriptive conditional associations rather than causal effects of migration.

This paper contributes to the disaster and household-finance literatures in three ways. First, it provides new quasi-experimental evidence on household income and expenditure following the Canterbury earthquakes using nationally representative survey data linked to administrative residential histories. This complements existing macroeconomic and individual-level administrative studies by examining changes at the household level.

Second, by disaggregating expenditure into detailed categories and examining heterogeneity by income, earthquake intensity, and migration status, the paper identifies adjustment mechanisms that are obscured by aggregate measures of income and total expenditure. In particular, it distinguishes regular household income from receipts and refunds recorded separately in the HES, including insurance reimbursements and related compensatory inflows. This distinction helps separate labour-market and recurring-income responses from insurance- and compensation-related financial flows, while also identifying changes in routine consumption, housing expenditure, mobility costs, debt commitments, and longer-term financial contributions.

Third, the paper illustrates how repeated cross-sectional survey data can be combined with administrative records, geographically detailed hazard measures, propensity-score matching, and difference-in-differences methods to study geographically localised shocks. The approach

to reconstructing pre-disaster residence from multiple linked address sources is also applicable to other household-level research conducted within the IDI.

The remainder of the paper is structured as follows. Section 2 presents the empirical strategy, including the difference-in-differences framework, propensity-score matching procedure, event-study specification, and alternative weighting estimator. Section 3 describes the data sources and linkage, the construction of the treatment and comparison groups, and the income and expenditure outcomes. Section 4 reports descriptive evidence on household characteristics, income, expenditure, and post-earthquake relocation. Section 5 presents the main estimates, heterogeneity analyses, and robustness checks. Section 6 discusses the findings and limitations before concluding.

2. Methodology

2.1 Difference-in-differences

Difference-in-differences (DiD) is a widely used econometric design for estimating the causal effects of policy changes or exogenous shocks on outcomes of interest when randomised controlled trials are infeasible. The basic idea is to compare changes in outcomes over time for an exposed (treated) group relative to a suitable comparison (control) group, thereby netting out time-invariant differences between groups and shocks common to all units in a given period (Wing et al., 2018).

The HES is analysed as a repeated cross-section rather than a household panel. The index i therefore denotes households observed in survey wave t , not the same household followed over time. The DiD estimator compares changes in average outcomes for exposed and matched comparison households across waves. In this study, we use DiD to estimate the causal impact of the 2010 – 2011 Canterbury earthquake sequence on household income and expenditure. We treat the earthquakes as an exogenous shock and exploit cross-regional variation in earthquake intensity to classify households into exposure groups. By interacting this intensity-based exposure measure with an indicator for the post-earthquake period, we estimate how household income and expenditure evolved with different levels of seismic exposure.

The conventional DiD approach relies on the parallel trends assumption: in the absence of the shock, exposed and comparison households would have followed similar trajectories over time. Under this assumption, the difference in changes between the two groups can be interpreted as the causal effect of the earthquakes (Angrist and Pischke, 2009). In our empirical specifications, we include region fixed effects and year fixed effects to account for time-invariant regional characteristics and year-specific shocks shared by all households.

Finally, because disaster impacts plausibly vary with exposure intensity, we follow recent contributions in the disaster economics literature and incorporate earthquake intensity into heterogeneous DiD specifications (e.g. Abdeljawad and Noy, 2025b). Specifically, we interact earthquake intensity with the post-earthquake indicator to estimate how the effects on household income and expenditure differ across intensity levels.

In summary, the regression model adopted in this study takes the following form:

$$Y_{irt} = \alpha + \beta_1(LowMMI_i \times Post_t) + \beta_2(HighMMI_i \times Post_t) + X'_{it}\gamma + \lambda_t + \mu_r + \epsilon_{irt} \quad (1)$$

Here, Y_{irt} denotes the outcome (dependent) variables, including *Total Household Expenditure*, *Total Household Income*, *Total Household Regular Income* and different expenditure categories, where i indexes HES households and t indexes survey years. $LowMMI_i$ and $HighMMI_i$ identify treated Canterbury households in the low- and high-intensity categories, respectively. The omitted category is the matched North Island comparison group, so β_1 and β_2 are interpreted relative to the unexposed baseline. $Post_t$ is a post-earthquake dummy variable, taking the value of 1 for the years 2011/12 and onward, and 0 otherwise. (For total and detailed expenditure outcomes, the first available post-earthquake full-expenditure wave is 2012/13, because the full expenditure module is only available in 2006/07, 2009/10, 2012/13 and 2015/16.) X_{it} includes the age of the household reference person, age squared, the reference person's sex, and household size.

The regression model controls for year fixed effects λ_t and region fixed effects μ_r , where the regions are defined at the Territorial Authority (TA) level, in order to account for spatial correlation and regional heterogeneity. To ensure robustness in statistical inference, standard

errors are clustered at the TA level to adjust for potential intra-regional correlation in household income and expenditure outcomes (Bertrand et al., 2004).

2.2 Propensity score matching

To improve the control group's suitability as a counterfactual to the earthquake-affected households group, we construct a control group that closely resembles the earthquake-treated group in terms of observable household characteristics that are predetermined or slow-moving relative to the earthquake shock. This facilitates a more credible assessment of the causal effects of the earthquakes on household income and expenditure. Following standard practice in the disaster economics literature (Bondonio & Greenbaum, 2018; Yan, Zhou & Qi, 2025), we employ PSM to select an appropriate control sample.

To respect the Stable Unit Treatment Value Assumption (SUTVA), in particular its no-interference component, which requires that treatment effects on treated units do not spill over to control units, we restrict attention to households that were never exposed to the Canterbury earthquake sequence. We draw on existing research indicating that the impacts of the earthquakes were largely confined to the Canterbury region and its immediate surroundings (Parker & Steenkamp, 2012; Wood et al., 2016). Accordingly, when constructing the control group we exclude all households located in the South Island, as well as those that had migrated out of the Canterbury region. The control pool therefore consists only of households that consistently resided in the North Island over the sample period.

During the matching stage, we estimate each household's propensity to belong to the Canterbury treated group in 2010 using a logit model based on key pre-earthquake demographic characteristics. Specifically, the propensity score p_i for household i is obtained from the following logit specification:

$$Pr(D_i = 1|X_i) = p_i = \frac{e^{X_i\beta}}{1+e^{X_i\beta}} \quad (2)$$

where $D_i=1$ if household i was located in Canterbury in 2010, and $D_i=0$ if it was located in the North Island. The vector X_i contains pre-earthquake household-level covariates. On the basis of the estimated propensity scores, we implement 1:2 nearest-neighbour matching

without replacement, matching each treated household with two North Island households that serve as control observations.

To enhance matching quality, we impose a strict caliper restriction (caliper = 0.2) on the propensity score distance to ensure a high degree of similarity in the covariate space for variables unlikely to be influenced by the earthquakes, including the age of the household reference person, household size, the reference person's gender, and their highest educational qualification. Moreover, all matches are required to fall within the same survey year in order to eliminate potential confounding arising from inter-year variation (Austin, 2011a).

To evaluate the quality of the matching, we assess covariate balance for each survey year from 2006/07 to 2017/18 using two standard diagnostics: the standardized mean difference (SMD) and the variance ratio (VR). Across all years, the matched samples satisfy commonly used balance criteria: all SMD values are below 0.1 and the variance ratios for continuous variables are below 2 (see Appendix B), indicating a high level of covariate balance and satisfactory matching performance (Stuart, 2010).

On this basis, we obtain a high-quality control group pool that underpins the subsequent DiD estimates and strengthens the identification of the causal effects of the Canterbury earthquakes on household income and expenditure. Table 1 reports, for each survey year, the number of treated households, the size of the eligible control pool used for matching, and the final number of matched control households selected through PSM. These figures confirm that efficient matching was consistently achieved across all years (Austin, 2011a; Austin, 2011b; Stuart, 2010).

Table 1. Sample sizes of treated households, eligible control pool, and matched controls by survey year

SurveyYear	Treated Households	Eligible control pool	Matched control households
2006/07	417	1512	831
2007/08	462	1917	924
2008/09	459	1887	921
2009/10	432	1692	867

SurveyYear	Treated Households	Eligible control pool	Matched control households
2010/11	432	1971	867
2011/12	486	1938	972
2012/13	411	1608	822
2013/14	438	1896	873
2014/15	705	3135	1407
2015/16	420	1995	840
2016/17	456	2118	909
2017/18	633	3165	1263

Besides, because matching is conducted separately within each survey wave, treated households are compared with contemporaneously matched controls rather than with a single pooled comparison group. To further assess whether compositional changes across waves threaten identification, we estimate pseudo-outcome balance tests using key household characteristics and broad household-composition categories as outcomes. The $\text{Treat} \times \text{Post}$ coefficients are small and statistically insignificant for the main covariates and for broad household-composition groups, suggesting that the matched treated and control samples do not exhibit systematic differential compositional shifts after the earthquakes. The four-cell means and pseudo-outcome tests are reported in Appendix B.

2.3 Examining the parallel trends assumption

A key assumption of the DiD design is that, in the absence of the earthquake shock, treated and control groups would have followed parallel trends. To assess this assumption, we estimate an event-study specification that traces the dynamic effects before and after the earthquakes.

In this specification, we interact the treatment-group indicator with a full set of year dummies, using 2006/07 as the omitted (base) year. This allows us to recover the trajectory of treatment effects over time. The regressions control for key household covariates, including the age, age squared, sex of the reference person, and household size, and include two-way fixed effects for year and region. Robust standard errors are clustered at the territorial authority level to account for within-area correlation in the error terms and to ensure valid inference.

It is important to note that each wave of the HES runs from July of one year to June of the next, and is therefore labelled as a "cross-year" survey. For example, "2006/07" refers to interviews conducted between July 2006 and June 2007. Although the magnitude 7.1 earthquake on 4 September 2010 was substantial, the most severe destruction resulted from the major aftershock on 22 February 2011, which caused extensive loss of life and property and had a shallower epicentre much closer to the Christchurch city centre.

The 22 February 2011 event severely disrupted data collection. For instance, the 2011 Census, originally scheduled for March 2011 (shortly after the most damaging earthquake), was postponed to 2013. Many of the most severely affected households were therefore not captured in the 2010/11 HES wave. In this study, we accordingly classify the 2006/07 – 2010/11 survey waves as the pre-earthquake period and the 2011/12 – 2017/18 waves as the post-earthquake period.

Furthermore, documentation from Stats NZ indicates substantial differences in how expenditure data were collected across HES waves. In the 2006/07, 2009/10, 2012/13 and 2015/16 waves, the total household expenditure variable aggregates spending across all categories. In the remaining waves, however, this variable covers only housing-related costs, such as rent, mortgage payments, local authority rates and building insurance. As a result, comparability in total expenditure across waves is limited and the series is affected by changes in data-collection methods and sampling design. Given these inconsistencies, we restrict our analysis of total household expenditure to the 2006/07, 2009/10, 2012/13 and 2015/16 waves.

Appendix C presents event-study estimates for total household income, regular household income, and total household expenditure. For the income outcomes, the omitted reference period is 2006/07, and the pre-earthquake interaction coefficients correspond to the 2007/08, 2008/09, 2009/10, and 2010/11 survey waves. These coefficients fluctuate around zero and are individually imprecisely estimated. Joint Wald tests also fail to reject the null hypothesis that the four pre-earthquake coefficients are jointly equal to zero for total household income ($p=0.814$) and regular household income ($p=0.624$).

For total household expenditure, comparable full-expenditure data are available only in 2006/07, 2009/10, 2012/13, and 2015/16. The pre-earthquake comparison is therefore limited to the 2009/10 interaction coefficient relative to the 2006/07 reference wave. This coefficient is not statistically significant, and the corresponding Wald test fails to reject the null of no pre-earthquake difference ($p=0.544$). Because only one comparable pre-earthquake expenditure coefficient is available, the evidence for expenditure is necessarily weaker than for the two income outcomes.

Taken together, the event-study estimates and joint pre-trend tests provide supportive evidence for the parallel-trends assumption. The post-earthquake income coefficients become positive in several later survey waves, while the total-expenditure coefficients remain positive but imprecisely estimated.

2.4 Alternative weighting estimator: ATT-IPW robustness check

As an additional robustness check, we estimate an inverse-probability-weighted DiD specification targeting the average treatment effect on the treated (ATT). This exercise assesses whether the main PSM-DiD results are sensitive to the nearest-neighbour matching procedure and to the exclusion of unmatched comparison households.

The ATT-IPW estimator uses the same treated group and eligible North Island control pool as the baseline design. However, instead of selecting a matched subset of control households, it retains the full eligible comparison pool and reweights control households according to their estimated propensity scores. This provides a complementary estimator: the baseline PSM specification compares treated households with close contemporaneous controls, while the ATT-IPW specification uses a broader comparison sample but gives greater weight to control households that are more similar to the treated group.

For the ATT-IPW specification, we estimate propensity scores using an augmented version of the core balancing covariates, including the age of the household reference person, age squared, household size, the reference person's sex, and highest educational qualification. In richer robustness specifications, we also include housing tenure and broad household-composition categories.

For the ATT-IPW estimator, treated households receive weight one, while control households receive weights proportional to:

$$w_i = \frac{e(X_i)}{1-e(X_i)} \quad (3)$$

where $e(X_i)$ is the estimated propensity score. The weighted control group is therefore reweighted to resemble the distribution of observed covariates among treated households. We then estimate the same DiD outcome specification as in Section 2.1, using these ATT-IPW weights. Standard errors are clustered at the Territorial Authority level, as in the main analysis.

For total household expenditure and detailed expenditure categories, the ATT-IPW specification follows the same sample restriction as the baseline expenditure analysis: only the full-expenditure HES waves, namely 2006/07, 2009/10, 2012/13 and 2015/16, are used. Income outcomes are estimated using all annual HES waves from 2006/07 to 2017/18.

We report the ATT-IPW estimates as an appendix robustness check rather than as the preferred baseline estimator. The baseline PSM-DiD design offers a more transparent comparison between treated households and close contemporaneous controls within each survey wave, while the ATT-IPW estimator relies more heavily on model-based reweighting of the full eligible control pool. Nevertheless, ATT-IPW is useful for assessing whether the estimated effects are driven by the specific matching algorithm or by the exclusion of unmatched control households.

To evaluate the ATT-IPW specification, we conduct three diagnostic checks. First, we examine covariate balance after weighting using standardized mean differences and variance ratios. Second, we inspect the distribution of estimated weights to assess whether the results are driven by extreme weights or weak common support. Third, we compare the main PSM-DiD estimates with a sequence of bridge specifications that progressively move from the baseline PSM model to the full ATT-IPW model. This bridge exercise helps identify whether changes in statistical precision arise from additional covariate controls, the use of a larger control pool, or the weighting estimator itself.

The ATT-IPW estimates should therefore be interpreted as a complementary robustness exercise. When the ATT-IPW coefficients remain similar in sign and magnitude to the PSM-DiD estimates but are less precisely estimated, this suggests that the substantive interpretation of the results is not driven by the baseline matching procedure, even though statistical precision varies across estimators.

3. Data

3.1 Data sources and treated group identification

The Household Economic Survey is a nationwide survey conducted annually by Stats NZ to measure households' economic conditions, including income, housing costs and living expenditures. Information on total household income and regular income is collected every year. By contrast, the full expenditure module is fielded only once every three years: in the 2006/07, 2009/10, 2012/13 and 2015/16 waves. In the remaining years, the annual questionnaire covers income and housing costs only. Accordingly, our analysis of detailed expenditure relies exclusively on these four waves. Because HES households are not followed as a balanced panel, we treat the survey as a repeated cross-section. The household observations in different waves should therefore be interpreted as wave-specific samples rather than repeated observations of the same households.

A key challenge in identifying the impact of the earthquakes lies in defining the treated group, households that were residing in the Canterbury region at the time of the earthquakes and were therefore directly exposed to the seismic shock. Because households may have relocated for various reasons before or after the earthquakes, the residential address recorded in the HES when the survey was conducted does not necessarily reflect their location at the time of the disaster. To address this, we combine multiple administrative data sources to reconstruct the residential history of each household reference person over time.

Specifically, each household reference person in the HES sample is linked, via their unique identifier (snz_uid), to address records from five administrative sources: Inland Revenue (IR), Accident Compensation Corporation (ACC), the Ministry of Social Development (MSD), the

Household Labour Force Survey (HLFS) and the cross-agency address notification dataset (address_notify). The address reference period is defined as 1 January 2010 to 22 February 2011—the date of the main earthquake—hereafter referred to as the pre-earthquake address "reference window"¹.

By combining administrative address data with residential information from the HES survey year, we classify all sample households into four groups.

Group 1 consists of households whose HES survey address is in Canterbury and whose last registered administrative address prior to the earthquakes is also in Canterbury. These households were living in the affected area both before the earthquakes and at the time of the survey, and are therefore included in the treated group. For around 20% of household reference persons, the region code of their most recent pre-earthquake address is missing. In these cases, if the most recent non-missing pre-earthquake address and the first post-earthquake address are located in the same meshblock in Canterbury, we also classify the household as having resided in Canterbury at the time of the earthquakes and allocate it to Group 1.

Group 2 comprises households whose HES survey address is outside Canterbury but whose last administrative address prior to the earthquakes is in Canterbury. These households are treated as part of the treated group unless there is clear evidence that they had relocated before the earthquakes. Specifically, a small number of Group 2 households whose HES interview date falls after the date of their last administrative address record and whose HES survey address is outside Canterbury are excluded from the treated group, as they are likely to have moved out of Canterbury before the earthquakes. The remaining Group 2 households are retained because the linked address information indicates Canterbury residence immediately before the earthquakes, even though they are later observed outside Canterbury in the HES.

¹The IR dataset records only address changes and does not include observations when an address remains unchanged. As a result, households that stayed at the same address throughout the address reference window would not have any corresponding record in the IR system for the address reference window, which may lead to the erroneous exclusion of households that were in fact stably residing in the Canterbury region during this period. To mitigate this potential bias, we relax the address timestamp requirement for cases in which address information is available only from the IR source and is absent from all other sources. For these IR-only cases, we extend the acceptable timeframe for the most recent address record back to 1 January 2000, effectively retaining more than eleven years of historical address data in order to recover pre-earthquake residential information as accurately as possible.

Group 3 comprises households whose HES survey address is in Canterbury but whose last administrative address prior to the earthquakes is outside Canterbury. These households are likely to be post-quake in-migrants to the earthquake-affected region, and are therefore excluded from the treated and control groups.

Group 4 includes households whose HES survey address and pre-earthquake administrative address are both outside Canterbury. These households were not directly exposed to the disaster and are included in the control-group pool if they resided in the North Island (and excluded altogether if they resided in the South Island).

Under this classification, the treated group consists only of households for which there is reliable evidence that the reference person resided in Canterbury at the time of the earthquakes, while the control group comprises households that have never been recorded as living in Canterbury in any of the linked address sources.

Group sizes vary across waves. Group 1 includes 387-648 households per wave; Groups 2 and 3 are small (typically 12-66 and 12-63 households, respectively; suppressed in some waves); and Group 4 is the largest group (1,890-3,774 households per wave). The full year-by-year distribution is reported in Table A.1 Appendix A.

3.2 Analysis of missing address rates

Despite linking HES records to multiple administrative datasets, pre-earthquake addresses cannot be recovered for a non-trivial share of households. Across survey waves, the share of reference persons with no usable pre-earthquake address record ranges from about 15 to 19 percent, with no evidence of a systematic time trend. This suggests that the missing-address problem is persistent but relatively stable over time, rather than being driven by earthquake-related disruptions in a particular survey year. Table A.2 in Appendix A reports the year-by-year unmatched rates.

To assess whether these missing-address rates primarily reflect limitations of our linkage procedure or more general structural features of the underlying data, we perform a parallel exercise using the 2013 Census. Linking Census records to Inland Revenue address histories yields unmatched rates of roughly 11 to 23 percent across HES survey years, a pattern that is

very similar in magnitude and timing to the rates observed for the HES sample itself (Table A.3 in Appendix A). This close correspondence suggests that the missing-address problem is largely driven by the nature of the administrative address data, rather than by the specific way in which HES records were linked.

Further analysis and discussions with Stats NZ indicate that the unmatched cases can be broadly divided into two groups. The first consists of young adults whose address histories are genuinely short or incomplete: in the HES these reference persons are often under age 22, have recently left the parental home, and are still transitioning between study and work, so that few reliable historical addresses are available. The second group comprises hard-to-track individuals, including those with very frequent moves, intermittent labour-market attachment, or periods of living overseas. These individuals are systematically more likely to have missing or inconsistent address records in both administrative data and Census linkages.

The fact that similar unmatched rates are observed in both the HES-linked sample and the standalone Census linkage, and that the unmatched cases cluster in these two specific demographic groups, increases confidence that the missing-address issue does not introduce arbitrary selection into the treated and control groups. While non-random missingness at the extreme margins cannot be ruled out, the patterns are logically coherent and consistent across multiple data sources and survey waves. These patterns reduce, although they do not eliminate, concerns that missing address histories are driving the main results.

3.3 Variable definition and construction

The level of earthquake intensity experienced by each household is measured using the MMI scale. To capture earthquake exposure, we utilise high-resolution seismic observation data for the February 2011 earthquake provided by New Zealand's national earthquake monitoring agency, GeoNet. These data are spatially overlaid with meshblock boundaries, small geographic units defined by Stats NZ, to construct a meshblock-level map of maximum ground-shaking intensity. Each household in the sample is then matched to its corresponding meshblock, yielding a household-level MMI value that serves as the key treatment-intensity indicator in our earthquake-exposure-heterogeneity analysis.

Earthquake exposure is categorised among treated Canterbury households using meshblock-level MMI. To distinguish substantively meaningful shaking from very weak exposure, we exclude treated households with MMI values below 4. This lower bound is consistent with earthquake early-warning research that evaluates operational target intensities beginning at approximately MMI 4 – 4.5 (Saunders et al., 2022). Among the remaining treated households, those with MMI values in the range [4, 7) form the low-intensity group, while those with $\text{MMI} \geq 7$ form the high-intensity group. The threshold at MMI 7 is consistent with the New Zealand MMI scale: MMI 4 is generally noticed indoors, whereas MMI 7 is associated with general alarm, difficulty standing, movement of furniture, and more substantial physical damage (GeoNet, n.d.). The terms “low” and “high” therefore describe relative exposure within the treated Canterbury sample. The comparison group consists of matched North Island households that were not exposed to the Canterbury earthquake sequence and serves as the omitted category in the DiD specifications.

Although all treated-group households are identified from administrative records as residing in the Canterbury region prior to the earthquakes, the number of households that can ultimately be matched to MMI intensity data is slightly smaller than the total treated group. This reflects two main factors, discussed below.

First, a small number of households that are classified as treated based on their Canterbury residence are located in meshblocks with MMI values below 4, reflecting lower modelled ground-shaking intensity. Second, some households have confirmed pre-earthquake administrative addresses in Canterbury but lack corresponding meshblock-level geographic identifiers. For these units, no precise spatial match can be performed and no MMI value can be assigned. Together, these two subsets account for only a small fraction of the treated group and are excluded solely from the heterogeneity analysis conducted later in the study.

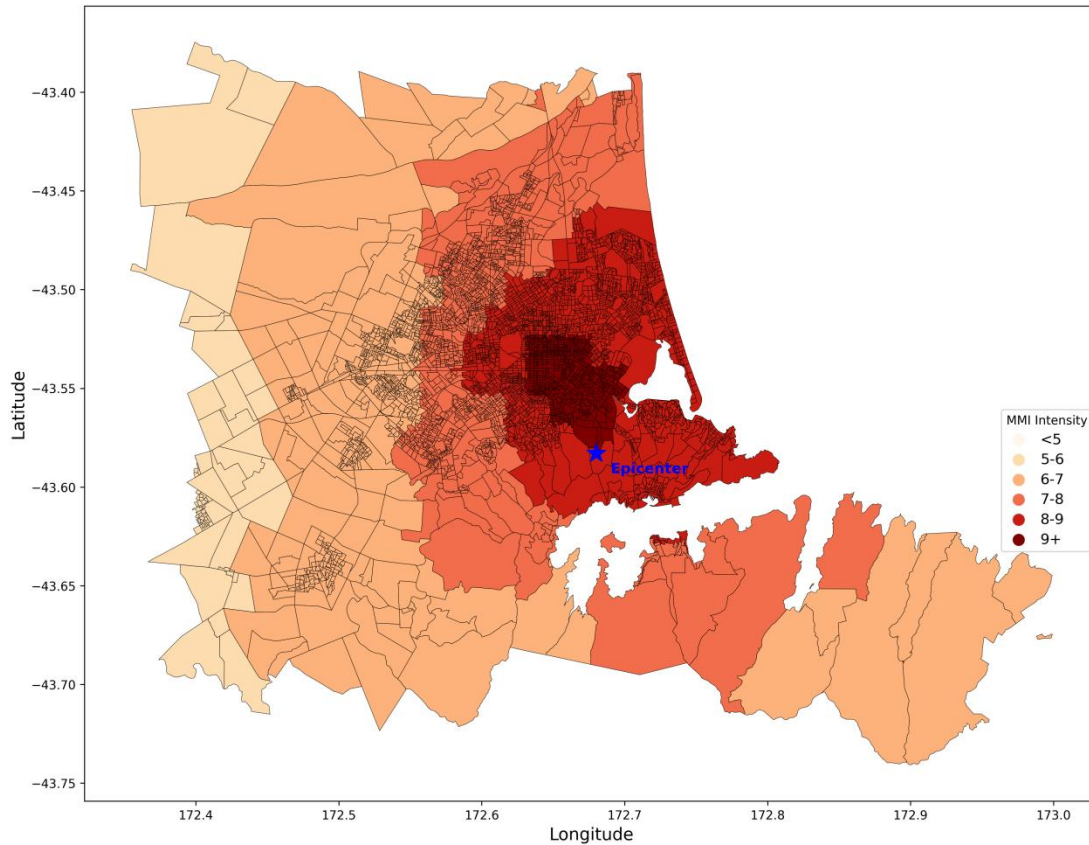


Figure 1. Meshblock-level earthquake intensity (MMI) in Christchurch City

Note: The figure displays a meshblock-level map of maximum Modified Mercalli Intensity (MMI) for the 22 February 2011 earthquake, constructed using shaking data from GeoNet and meshblock boundaries from Stats NZ. Darker shading indicates stronger ground shaking. The epicentre is marked with a blue asterisk.

In addition to earthquake intensity, we also consider households' homeownership status as a supplementary indicator of economic stability and post-disaster recovery capacity. We construct a binary variable with two categories: "owned/trust", which includes dwellings directly owned by household members or held through a family trust (a relatively common arrangement in Aotearoa New Zealand); and "rented", which includes market rentals, public or social housing, and other non-ownership occupancy arrangements.

This distinction is particularly important in the context of disaster resilience. Existing studies show that homeowners typically possess more assets and have better access to credit, which supports stronger recovery resilience and a greater capacity to mobilise resources following a disaster (Sawada & Shimizutani, 2008). By contrast, renting households are more likely to face

liquidity constraints and housing insecurity, potentially limiting their ability to adjust economic behaviour in response to shocks. We use this variable to describe pre-existing differences in housing stability and as an additional control in the robustness specifications. In specifications where tenure is included as an additional control, results should be interpreted as conditional on this housing-status dimension.

For the expenditure analysis, we follow the HES classification and divide total household expenditure into 18 categories that together cover both routine and longer-term expenses. These categories are: domestic fuel and power; housing costs; receipts and refunds; general insurance; miscellaneous payments; mortgages and loans; other property; transportation; retirement-savings and contribution schemes; medical and health; diary-recorded day-to-day purchases; travel; telecommunications; fees and subscriptions; education, recreation, sport and culture; household operations; household maintenance; and credit and debit accounts. As discussed in Section 2.3, the definition of total household expenditure differs across HES waves; we therefore restrict total-expenditure analyses to 2006/07, 2009/10, 2012/13 and 2015/16.

It is important to note that the HES employs three different data-collection methods for different types of expenditure:

1. Expenditure diary: Respondents record small and easily forgotten purchases over seven consecutive days, with the diary to be completed within 14 days. We use the annualised diary-derived expenditure values supplied in the HES. According to the HES documentation, these amounts are annualised using a factor of 26.07.

2. Expenditure questionnaire - most recent payment: This module is used for infrequent, periodic or fixed large payments. Interviewers may verify bills on-site or ask respondents for immediate estimates. Reported amounts are annualised using frequency-specific factors—for example, weekly ($\times 52$), fortnightly ($\times 26$), monthly ($\times 12$), annually ($\times 1$) or every four weeks ($\times 13$).

3. Expenditure questionnaire - past 12 months: This module retrospectively captures expenditures that may have occurred sporadically over the previous year.

4. Descriptive Evidence

To clarify the relationship between earthquake exposure and household economic characteristics, the descriptive analysis is organised into two subsections: a cross-sectional comparison by earthquake intensity and control-group status (Section 4.1), and an analysis of heterogeneity within the treated group that compares households who remained in place with those who relocated (Section 4.2). These descriptive comparisons are not interpreted causally; rather, they provide context for the DiD estimates reported in Section 5.

In constructing the summary statistics, we treat variables as follows. The age of the reference person and household size are reported as arithmetic means, as is standard in the descriptive-statistics literature and because these variables are bounded and not driven by extreme outliers. By contrast, total household income, regular income, total expenditure and housing expenditure are summarised using medians to reduce the influence of outliers, including very high-income households and households with unusually low or negative reported income.

For mortgages and loans, a large proportion of households report zero or missing values, so the median is equal to zero in most years and provides little discriminatory power. We therefore report the mean for this variable, which better reflects the average level of mortgage and loan payments among households with positive expenditure.

4.1 By earthquake intensity and control-group status

Table D.1 in Appendix D reports key characteristics of the sample across groups, including the average age of the reference person, household size, the share of female reference persons, and the homeownership rate. The high-intensity group ($\text{MMI} \geq 7$) has a slightly lower average age of the reference person than both the low-intensity and control groups (mean age: High = 51.5; Low = 54.6; Control = 52.7). This pattern may reflect the greater presence of younger households in central urban areas, which were also exposed to higher seismic intensity, but the differences are modest and should be interpreted with caution.

There are more pronounced differences in homeownership status. Across all survey years, the share of households that own their dwelling or hold it in a family trust is approximately 10 percentage points lower in the high-intensity group than in the low-intensity group, whereas differences between the treated and matched control groups are less pronounced. This pattern is consistent with the concentration of rental housing in central urban areas, which were also among the locations most severely affected by the 2011 earthquake (Kaiser et al., 2012).

4.2 Heterogeneity within the treated group: stayers versus relocators

To further explore differences in household responses to the earthquakes, we divide the treated group into two categories. The "stay" group consists of households whose HES survey-year address and last pre-earthquake administrative address are both located in the Canterbury region. The "relocation" group comprises households whose post-earthquake HES survey address is outside Canterbury, but whose last pre-earthquake administrative address was within the region².

When defining relocation, we classify households based on the combination of their location at the time of the earthquakes (using administrative address records for January 2010 – February 2011) and their location at the time of each HES interview. Households in the relocation group are those whose reference person lived in Canterbury immediately before the earthquakes but are later observed in HES interviews residing outside Canterbury. In contrast, the stay group comprises households whose reference person remained in Canterbury both at the time of the earthquakes and at the time of interview. In the post-earthquake waves (2011/12 – 2017/18), the relocation group therefore primarily captures households that moved out after the earthquakes.

As shown in Table D.2 in Appendix D, the average age of reference persons in the stay group is generally higher than in the relocation group across most survey years. This pattern suggests

² The relocation group does not exclusively represent post-earthquake out-migrants. By construction, it includes households whose HES survey address lies outside Canterbury while their last administrative address prior to the earthquakes is in Canterbury, and who therefore are treated as having been resident in the affected area immediately before the disaster. There is a small subset of cases in which the HES interview date falls after the date of the last recorded administrative address and the HES survey address is outside Canterbury. For these households, there is evidence that they may have moved out of Canterbury before the earthquakes; we therefore exclude them from the treated group to avoid misclassification.

that migration behaviour is more prevalent among relatively younger individuals, consistent with international evidence on post-disaster migration (Fussell, 2015).

There are also sizeable differences in homeownership. In the post-earthquake years, the homeownership rate in the stay group is generally around or above 70%, whereas in the relocation group it fluctuates around 50%. This suggests that households remaining in Canterbury tend to enjoy greater housing stability, while households that relocate are more likely to be in rental or temporary accommodation.

In terms of income, relocation households have lower median income than stay households in several years and exhibit larger dispersion in some waves. However, these descriptive differences should be interpreted cautiously because the relocation group is small in many waves and migration is self-selected.

For expenditure, the two groups display broadly similar medians in total and housing-related spending across most years. However, there are pronounced differences in the mortgages-and-loans variable, with the stay group consistently reporting higher values. This pattern is consistent with these households being more commonly owner-occupiers in the mortgage repayment phase.

Taken together, the descriptive evidence suggests that relocated households differ from stay households along several dimensions, especially age, homeownership and housing-related expenditure. These differences are consistent with selective mobility after the earthquakes, but they should not be interpreted as evidence that relocation itself caused weaker economic outcomes.

5. Results

5.1 DiD Results for income

As shown in Table 2, both total household income and regular household income increase for households in earthquake-exposed areas relative to the matched control group. For total household income, households in the low-intensity group experience

an increase of around NZD 7,043 in the post-earthquake period, while households in the high-intensity group experience an increase of around NZD 7,252. Both total-income estimates are statistically significant at the 5% level. For regular household income, the estimated increase is around NZD 7,924 for the low-intensity group and around NZD 7,412 for the high-intensity group, with the former statistically significant at the 1% level and the latter at the 5% level.

The specification reported in Table 2 includes age, age squared, the sex of the reference person and household size as household-level controls. The positive income effects observed in both low- and high-intensity areas suggest that post-earthquake income gains were not confined only to the most severely shaken locations. The similarity in magnitude between the low- and high-intensity estimates further suggests that the income response may reflect broader regional recovery and reconstruction dynamics rather than a strictly monotonic relationship between local shaking intensity and household income gains.

Table 2. DiD results for total and regular household income

Variable	Total Household Income	Total Regular Household Income
MMI group:Low × Post	7043.47* (2889.43)	7924.42** (2673.83)
MMI group:High × Post	7251.79* (3559.44)	7411.86* (3286.86)
Age of reference person	3810.44*** (301.28)	3462.94*** (295.27)
Age ²	-37.83*** (3.03)	-34.60*** (2.97)
Sex of reference person	-11894.36*** (991.53)	-11689.34*** (944.59)
Household size	14074.52*** (740.65)	13960.68*** (718.84)

Observations: 17,193

* p < 0.05, ** p < 0.01, *** p < 0.001

Each column reports coefficients from a separate DiD regression. Robust standard errors clustered at the Territorial Authority (TA) level are in parentheses. Year and TA fixed effects are included. The positive coefficient on age and the negative coefficient on age squared are consistent with a concave life-cycle income profile, while the positive coefficient on household

size reflects the fact that larger households typically have more potential earners. The negative coefficient on the sex of the reference person should be interpreted as a household-level association, rather than as an individual-level gender effect, since it may capture broader differences in household structure and income arrangements.

This pattern can be understood in light of the distinction between aggregate output measures and household-level outcomes. Although disasters are often associated with output losses, regional GDP in Canterbury did not fall markedly after the earthquakes (Stats NZ, 2023). In this setting, a large reconstruction programme and related labour-demand effects provide a plausible channel through which household incomes could rise in the post-earthquake period, particularly in the years following the shock (e.g. Wood et al., 2016). The fact that regular household income also increases supports the interpretation that labour-market and earnings-related channels are likely to be important, rather than the results being driven solely by one-off compensation or insurance-related inflows.

Second, post-disaster migration may affect the observed income distribution through compositional change. In our linked-address classification, the treated group includes only households for which there is reliable evidence that the reference person resided in Canterbury at the time of the earthquakes (Groups 1 and 2), while likely post-earthquake in-migrants to Canterbury are excluded (Group 3). As a result, migration-related compositional effects in our estimates operate primarily through selective out-migration among pre-earthquake residents and differences between stayers and movers, rather than through in-migration into the affected region. For example, if lower-income or more housing-insecure households were more likely to leave Canterbury after the earthquakes, the average observed income profile of exposed households could change even without broad-based income gains for all households. Consistent with this possibility, Section 4.2 and Table D.2 document that households observed outside Canterbury after the earthquakes tend to be younger and less likely to own their home than those who remain. Section 5.4 therefore examines stayers and relocators separately as a descriptive heterogeneity analysis.

Existing literature provides complementary evidence on these mechanisms. Roth Tran and Wilson (2025) show that in the United States, per capita income in disaster-affected counties

increased by an average of 0.5% over the eight years following a disaster, with some areas, such as those impacted by Hurricane Katrina, experiencing increases of more than 3%. The mechanisms they highlight include job growth in construction and related sectors, higher average wages, and inflows of insurance payouts and government reconstruction funds. These mechanisms align with the interpretation of our findings, suggesting that in settings with substantial reconstruction activity and compensation flows, household incomes may increase even as disasters impose large welfare losses through housing damage, displacement and disruption.

It is important to emphasise that the spatial distribution and group-specific heterogeneity of post-earthquake income changes remain important avenues for future research. Given the repeated cross-sectional structure of the HES and sample-size constraints, especially the small number of households in Groups 2 and 3 in each wave, this study cannot fully characterise differential recovery trajectories by ethnicity, income source, housing tenure, household structure or social capital. Future work leveraging longitudinal micro-level tracking in the IDI could provide more granular evidence on changes in the income distribution and the inequality implications of large natural hazards (Abdeljawad and Noy, 2025a).

5.2 DiD results for expenditure

In contrast to the income results, the DiD estimates for total household expenditure are positive but not statistically significant in either the low- or high-intensity group (Table 3). The estimated coefficient is around NZD 3,414 for the low-intensity group and around NZD 2,011 for the high-intensity group, but both estimates are imprecisely estimated. The control variables display plausible expenditure patterns: age and age squared indicate a concave life-cycle profile for household expenditure, household size is positively associated with total expenditure, and the sex of the reference person is also significantly associated with expenditure levels. However, the absence of statistically significant average treatment effects for total expenditure suggests that the earthquakes did not generate a clear overall increase in aggregate household spending. When total expenditure is disaggregated into categories, however, several components exhibit sizeable and statistically significant treatment effects.

673 **Table 3.** DiD results for total household expenditure

Variable	Estimate
MMI group:Low $\times$ Post	3413.71 (2507.03)
MMI group:High $\times$ Post	2010.89 (1469.80)
Age of reference person	1170.95*** (100.93)
Age ²	-13.32*** (0.97)
Sex of reference person	-3388.78*** (768.82)
Household size	6805.15*** (594.64)
Observations: 5,022	

674 * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

675 Certain expenditure categories, such as receipts and refunds, exhibit highly skewed
676 distributions. For receipts and refunds, for example, a large share of observations are zero, and
677 the remaining positive or negative values are concentrated in a heavy tail. Estimating ordinary
678 least squares (OLS) regressions directly on these raw levels forces the model to fit a small
679 number of very large observations, allowing them to exert disproportionate influence on the
680 estimated coefficients.

681 To address these distributional features and improve interpretability, we estimate the
682 category-level regressions using a log transformation of the absolute value plus one of each
683 expenditure variable, that is, $\log(1+|y|)$. This transformation substantially stabilises the
684 regression estimates and yields coefficients that capture relative differences in the magnitude
685 of expenditure rather than being driven by a few extreme observations. For example, after this
686 transformation, the coefficient for receipts and refunds in the high-intensity treatment group is
687 0.729, implying that the magnitude of receipts and refunds (plus one) in the treatment group is
688 approximately 2.07 times ($e^{0.729}$) that in the control group, equivalent to an increase of about
689 107.2%.

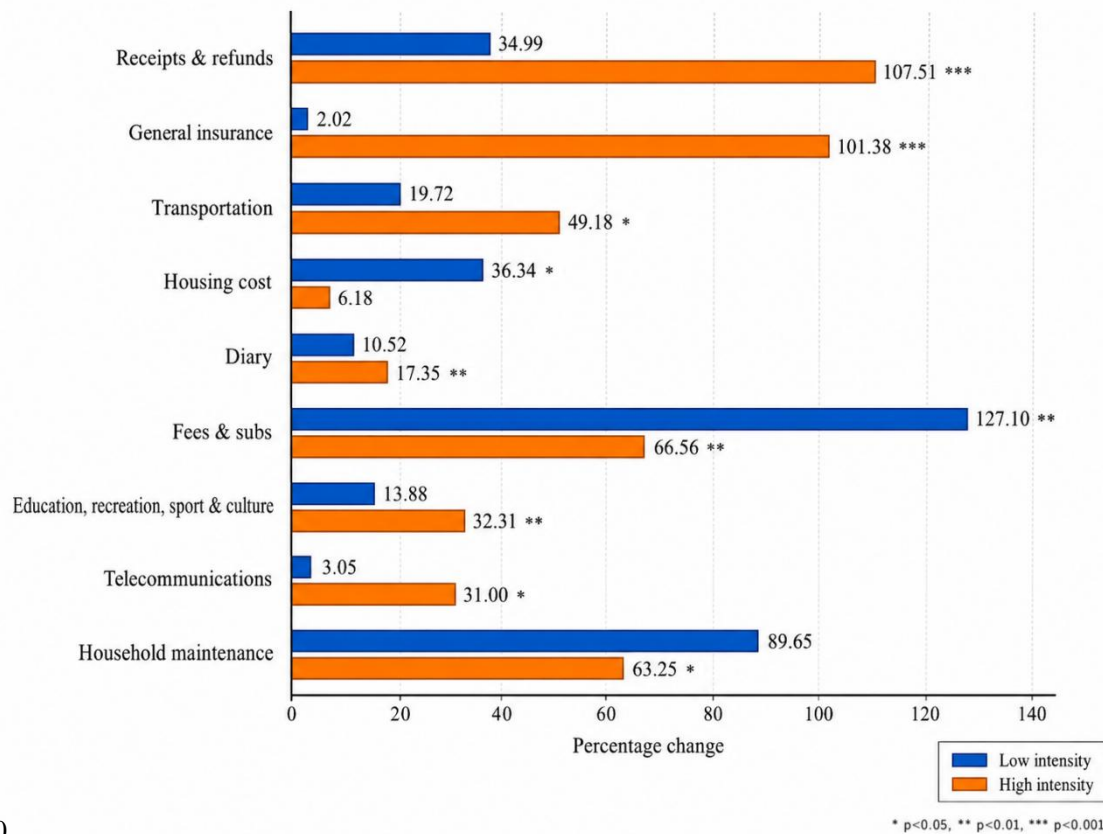


Figure 2. Post-earthquake changes in expenditure categories by earthquake intensity

As shown in Figure 2, with full regression estimates reported in Appendix E, several expenditure categories exhibit statistically significant responses to the earthquakes, particularly among households in high-intensity areas.

Households in high-intensity areas show a large and statistically significant increase in receipts- and refunds-related entries following the earthquakes. As noted above, this category also captures insurance reimbursements, which are recorded as negative expenditure items rather than income to avoid double-counting with other income sources such as wages or transfer payments (Stats NZ, 2020). The estimated log coefficient for the high-intensity group is 0.729, corresponding to an increase of about 107.2% in the magnitude of receipts and refunds plus one. This pattern is consistent with increased insurance claims and related reimbursement flows after the disaster. The corresponding estimate for the low-intensity group is also positive, but it is not statistically significant.

General insurance expenditure also increases substantially in high-intensity areas. The estimated coefficient implies an increase of approximately 101.4%, and the effect is statistically significant at the 0.1% level. This pattern is consistent with increased insurance-related activity after the earthquakes, including changes in premiums, policy adjustments, or greater engagement with insurance products following heightened risk salience. Because the category captures expenditure on insurance rather than claim receipts, it should be interpreted separately from the receipts-and-refunds category.

Compared with the control group, the diary-recorded day-to-day expenditure category increases by around 17.0% in high-intensity areas, and the estimate is statistically significant at the 1% level. This category captures small and frequently purchased items recorded through the HES expenditure diary. The increase may reflect higher routine living costs, immediate replacement needs, or changes in day-to-day consumption in the aftermath of the earthquakes. Because the diary module records transactions close to the time of purchase, it is less vulnerable to long recall periods than retrospective expenditure questions, although it may still be sensitive to short reporting windows.

Transportation and telecommunications expenditure also increase significantly in high-intensity areas. Transportation expenditure rises by approximately 49.2%, while telecommunications expenditure increases by approximately 31.0%; both estimates are statistically significant at the 5% level. These patterns are consistent with additional mobility and communication needs after the disaster, including commuting disruptions, vehicle-related expenses, and coordination with family members, insurers, repair providers, and public agencies.

Fees-and-subscriptions expenditure increases significantly in both low- and high-intensity areas. The estimated increase is approximately 127.1% in the low-intensity group and 66.5% in the high-intensity group, with both estimates statistically significant at the 1% level. According to the HES classification, this category includes payments for club memberships, professional association fees, subscriptions to recreational or educational programmes, and other recurring service fees. The observed increase suggests that households did not uniformly reduce discretionary or subscription-based spending after the earthquakes. Instead, some households

may have maintained or expanded spending on services that provided continuity, professional connection, education, recreation or social engagement during the recovery period.

Unexpectedly, it is the low-intensity group, rather than the high-intensity group, that experiences a statistically significant increase in housing costs. The estimated increase is approximately 36.3% for the low-intensity group and is statistically significant at the 5% level, whereas the high-intensity estimate, approximately 6.2%, is not statistically significant. This pattern may reflect moderate disruption in less severely affected areas, including repairs, temporary accommodation, or upward pressure on rents in neighbouring areas due to population displacement from harder-hit locations. The absence of a statistically significant housing-cost response in the high-intensity group may reflect more severe damage that delayed reconstruction, insurance-managed repairs, or expenditure being recorded through institutional channels rather than directly as household outlays.

Expenditure on education, recreation, sport and culture also increases in high-intensity areas. The estimated increase is approximately 32.3% and is statistically significant at the 1% level. Household-maintenance expenditure rises by approximately 63.2% in the high-intensity group and is statistically significant at the 5% level. These results are consistent with households facing additional repair and maintenance needs while also maintaining or adjusting expenditure on educational, recreational, and cultural activities during the recovery period. The corresponding low-intensity estimates are positive but statistically insignificant.

Taken together, the expenditure results suggest that the Canterbury earthquakes did not generate a clear average increase in total household expenditure, but they did change the composition of spending. Statistically significant category-level responses are concentrated in receipts and refunds, general insurance, diary-recorded purchases, transportation, telecommunications, fees and subscriptions, education, recreation, sport and culture, household maintenance, and housing costs in low-intensity areas. This pattern is consistent with households adjusting along several margins after the disaster, including insurance and reimbursement flows, routine living expenses, mobility and communication needs, housing and maintenance costs, and subscription- and service-related expenditure.

As shown in Appendix F, a comparison of census data across New Zealand's 16 regions between 2006 and 2013 indicates that Canterbury recorded the second-highest annual growth rate and the second-largest absolute increase in median household income, in both cases behind only the West Coast (Stats NZ, 2014). This growth rate is substantially higher than in most other regions, suggesting that Canterbury's post-2006 income growth was relatively strong in the post-earthquake period, potentially reflecting reconstruction and recovery alongside other regional factors. This broader regional context is consistent with the income results reported in Section 5.1, while the expenditure results in this section indicate that increased income did not translate into a uniform increase in total household expenditure, but rather into category-specific adjustments.

5.3 Heterogeneous effects by income group

As shown in Figure 3 and Tables G.1 and G.2 in Appendix G, expenditure responses differ substantially between households above and below the median income. Among households with income above the median, post-earthquake expenditure adjustments are broad and appear across several categories. The regression results indicate statistically significant increases in housing costs, receipts and refunds, general insurance, miscellaneous payments, telecommunications, fees and subscriptions, and household maintenance in at least one of the two intensity groups. Although travel expenditure is not statistically significant in the full-sample category regression, a significant increase appears among below-median-income households in the low-intensity group. These patterns suggest that above-median-income households adjusted along multiple margins, including insurance-related activity, housing and maintenance expenditure, communication needs, and discretionary or subscription-based spending.

For above-median-income households, receipts and refunds increase markedly, especially in high-intensity areas. The estimated coefficient for the high-intensity group is 1.04, corresponding to an increase of approximately 182.9% in the magnitude of receipts and refunds plus one. The low-intensity coefficient is also positive, corresponding to an increase of about 76.8%, although it is not statistically significant.

This pattern is consistent with the interpretation that higher-income households were more likely to receive larger insurance reimbursements, refunds, proceeds from second-hand sales, or other compensatory inflows after the earthquakes. It also aligns with international evidence that post-disaster reconstruction activity and insurance payouts often benefit middle- and high-income households more, because these households are better positioned to finance reconstruction and make effective use of incoming funds (Kousky, 2014).

General insurance expenditure also rises sharply among above-median-income households in high-intensity areas. The estimated coefficient of 0.89 implies an increase of approximately 143.5%, and the effect is statistically significant. This increase should be interpreted separately from receipts and refunds: while receipts and refunds include insurance reimbursements and related inflows, the general-insurance category captures expenditure on insurance products. The combination of increased receipts and refunds and increased general-insurance expenditure suggests a broader intensification of insurance-related financial activity among higher-income households following the earthquakes.

Fees and subscriptions also show large statistically significant increases among above-median-income households. The estimated increase is particularly large in the low-intensity group, where the coefficient of 1.31 corresponds to an increase of approximately 270.6%. The high-intensity coefficient is also positive and statistically significant, corresponding to an increase of about 82.2%. The observed increase suggests that higher-income households did not uniformly reduce discretionary or subscription-based spending after the earthquakes. Instead, some households may have maintained or expanded spending on services that provided professional connection, continuity, recreation, education, or social engagement during the recovery period.

Above-median-income households also show significant increases in telecommunications expenditure in both low- and high-intensity areas. The estimated increases are approximately 47.7% for the low-intensity group and 35.0% for the high-intensity group. This pattern is consistent with greater communication and

coordination needs after the disaster, including contact with insurers, contractors, employers, family members, and public agencies.

Housing costs also increase significantly in both intensity groups. The estimates imply increases of approximately 32.3% in the low-intensity group and 15.0% in the high-intensity group. Household-maintenance expenditure exhibits a different pattern. The estimated increase is approximately 127.1% in the low-intensity group, but this estimate is not statistically significant. In the high-intensity group, household-maintenance expenditure increases by approximately 166.4% and is statistically significant at the 1% level. These findings are consistent with housing-related adjustment, repair, maintenance, and accommodation costs playing an important role in higher-income households' post-disaster expenditure responses.

Miscellaneous payments also increase significantly for above-median-income households in high-intensity areas, with an estimated increase of approximately 95.4%. Given the broad nature of this category, the result should be interpreted cautiously. However, it is consistent with the broader pattern that higher-income households adjusted across a relatively wide set of expenditure categories after the earthquakes.

For households with income below the median, post-earthquake expenditure adjustments are more concentrated. Significant increases appear in receipts and refunds, medical and health expenditure, diary-recorded day-to-day purchases, and travel. In high-intensity areas, receipts and refunds increase by approximately 40.5%, medical and health expenditure increases by approximately 41.9%, and diary-recorded purchases increase by approximately 18.5%. These categories are more closely related to immediate liquidity, health needs, and routine household consumption than to broader discretionary or asset-related adjustment. This pattern suggests that lower-income households' post-disaster expenditure responses were more focused on essential or near-essential needs.

The increase in diary-recorded day-to-day purchases among below-median-income households is particularly relevant because the diary category captures routine,

frequently purchased items such as food, beverages and personal care. The estimated high-intensity effect of 0.17 corresponds to an increase of approximately 18.5%. This finding is consistent with lower-income households facing higher routine living costs or immediate replacement needs after the earthquakes, while having more limited capacity to adjust across wider expenditure categories.

Receipts and refunds also increase significantly for below-median-income households in high-intensity areas. The estimated increase of approximately 40.5% suggests that lower-income households also received post-disaster reimbursements or related inflows. However, the magnitude is much smaller than the corresponding increase for above-median-income households, for whom the high-intensity estimate is approximately 182.9%. This gap is consistent with differences in asset ownership, insurance depth, and the value of insurable possessions. Higher-income households typically own more valuable assets and have greater capacity to engage with insurance and repair processes, generating larger claim amounts and subsequent reimbursements. Thus, while receipts and refunds provide liquidity relief across income groups, the difference in effect size points to underlying wealth and asset inequalities in recovery capacity (Owen & Noy, 2019).

Medical and health expenditure also increases significantly for below-median-income households in high-intensity areas. The estimated increase of approximately 41.9% suggests that lower-income households may have faced greater health-related needs or out-of-pocket health costs after the earthquakes. This result is consistent with the broader literature showing that disasters can impose substantial non-property costs on vulnerable households, including health, stress, and disruption-related expenses.

One notable result for below-median-income households is the large increase in travel expenditure in the low-intensity group. The estimated coefficient of 1.19 corresponds to an increase of approximately 228.7%. Because travel expenditure can be highly skewed and episodic, this result should be interpreted cautiously. Nevertheless, it may reflect temporary relocation, visits to relatives, travel to avoid disrupted areas, or other mobility needs following the earthquakes. The fact that this effect appears in the

low-intensity group rather than the high-intensity group suggests that travel responses may not follow a simple monotonic relationship with local shaking intensity.

The relatively narrower range of significant spending increases among below-median-income households is consistent with constrained post-disaster financial capacity. Compared with higher-income households, lower-income households tend to have less access to savings, credit, insurance, social protection, and informal financial support, which limits their ability to absorb disaster losses and adjust expenditures beyond immediate needs (Deryugina, 2017; Hallegatte et al., 2020). The results therefore suggest that lower-income households respond to the earthquakes mainly through liquidity-related inflows, routine purchases, health-related spending, and selected mobility costs, rather than through broad-based discretionary, insurance, or housing-maintenance adjustments.

Taken together, these findings highlight the heterogeneous nature of household financial responses to disasters. Above-median-income households exhibit broad-based post-disaster adjustments across insurance, housing, maintenance, communications, and subscription-related categories. Below-median-income households respond more narrowly, with significant changes concentrated in receipts and refunds, medical and health expenditure, diary-recorded purchases, and travel. This divergence reinforces the central role of income and financial capacity in shaping differential post-disaster expenditure dynamics and is consistent with the broader literature on disaster-related inequality and socioeconomic resilience (Roth Tran & Wilson, 2025; Hallegatte et al., 2020).

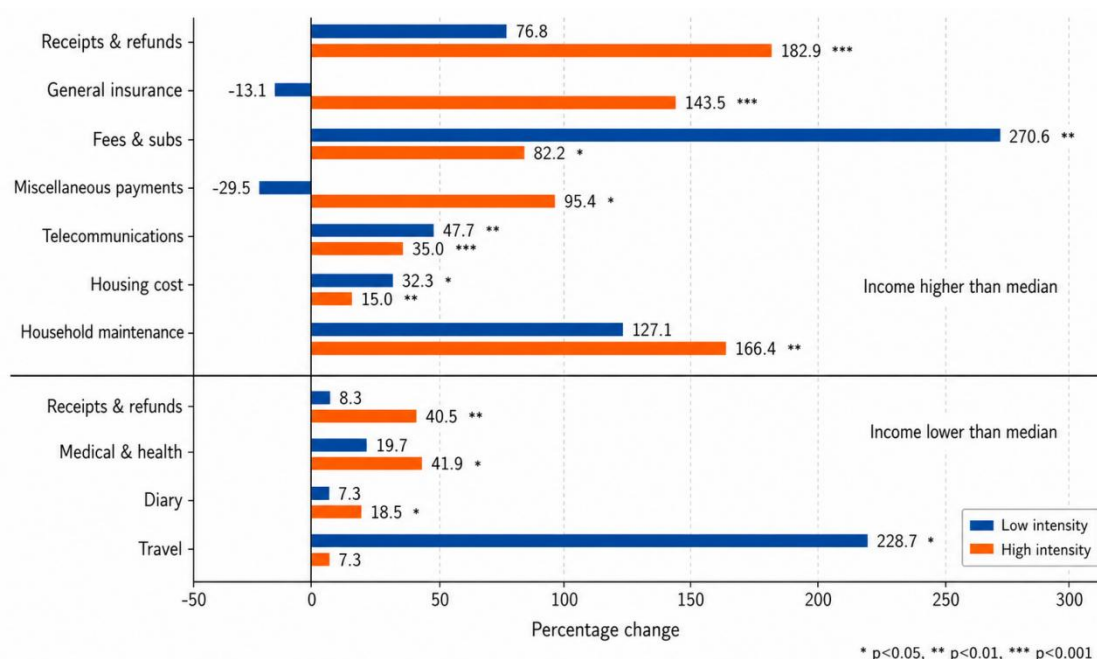


Figure 3. Post-earthquake changes in expenditure by income group

5.4 Heterogeneous effects of migration

To assess the heterogeneous effects associated with post-earthquake migration, we compare households that remained in Canterbury ("stay" households) with those that relocated out of the region ("relocation" households) within the treated sample. We examine six key economic indicators: total household income, total regular household income, total household expenditure, housing costs, other property expenditure, and mortgages and loans. These variables capture income, overall spending, and housing-related financial commitments. We estimate separate linear regressions for each outcome, including a dummy variable for relocation households, and control for the age of the reference person, the sex of the reference person, and household size. The regression results are reported in Table 4.

Table 4. Covariate-adjusted differences between stay and relocation households (descriptive)

	total income	regular income	total expenditure	housing cost	other property	mortgages and loans
(Intercept)	61,677.26*** (6,818.50)	59,522.58*** (6,251.13)	24,994.55*** (1,623.00)	39,211.64*** (5,953.41)	-1,008.71 (651.17)	21,205.25*** (5,657.55)
relocator indicator	427.33	1,005.00	664.48	24,901.31***	-606.05	-9,585.52*

(1 = relocation group, 0 = stay group)	(5,112.98)	(4,687.53)	(1,217.04)	(4,464.28)	(488.30)	(4,242.43)
age	-234.00** (87.06)	-226.48** (79.82)	-258.44*** (20.72)	-668.64*** (76.02)	23.39** (8.31)	-386.82*** (72.24)
sex	-11,136.70*** (2,731.56)	-11,127.09*** (2,504.27)	-254.33 (650.19)	-2,137.92 (2,385.00)	-445.32 (260.87)	-4,243.34 (2,266.48)
household size	18,204.61*** (1,154.96)	17,665.86*** (1,058.85)	2,584.06*** (274.91)	14,427.12*** (1,008.42)	397.67*** (110.30)	13,461.55*** (958.31)
observations	3,546	3,546	3,546	3,546	3,546	3,546

911 * p < 0.05, ** p < 0.01, *** p < 0.001

912 **Notes:** Estimates report covariate-adjusted mean differences between the relocation and stay groups within the
913 treated sample. The relocation indicator reflects a post-earthquake migration outcome that may be endogenous;
914 coefficients should therefore be interpreted as associations conditional on included covariates, not causal effects of
915 relocation.

916 First, for total household income, the difference between the two groups is not statistically
917 significant. The coefficient on the relocation indicator is positive but small relative to its
918 standard error. This suggests that, conditional on age, sex, and household size, households that
919 relocated out of Canterbury do not exhibit systematically higher or lower total income than
920 households that remained in the region. Migration status is therefore not strongly associated
921 with average household income once basic household composition is taken into account.

922 Second, the difference in regular household income between the two groups is also statistically
923 insignificant. Even for more stable and predictable income sources, such as wages and salaries,
924 pensions, and fixed transfer payments, there is no statistically detectable gap between
925 relocation and stay households. This further suggests that relocation is not primarily associated
926 with a conditional difference in regular income. Instead, migration decisions are more likely to
927 reflect residential, housing, family, or disruption-related coping strategies in response to the
928 disaster, rather than income differences alone.

929 The coefficient on total household expenditure is also positive but not statistically significant.
930 This indicates that, after controlling for age, sex, and household size, relocation households do
931 not exhibit a clear difference in overall expenditure relative to stay households. This result is

consistent with the broader expenditure findings in Section 5.2, where total expenditure does not show a statistically significant average DiD effect, even though several expenditure categories respond strongly.

By contrast, more pronounced heterogeneity emerges when focusing on housing-related expenditure. For housing costs, relocation households exhibit substantially higher spending than stay households, with an estimated coefficient of NZD 24,901 and statistical significance at the 0.1% level. This indicates that households observed outside Canterbury after the earthquakes faced much higher housing-related costs, conditional on basic household characteristics. This pattern is consistent with relocation involving higher rents in destination areas, temporary accommodation, rental bonds, or other costs associated with securing new housing after displacement.

Other property expenditure is lower among relocation households, but the coefficient is not statistically significant. Given the lack of statistical precision, this result should be interpreted cautiously and should not be treated as evidence of a systematic difference between relocation and stay households. It nevertheless points in the same broad direction as the mortgages-and-loans result, suggesting that households that left Canterbury may have been less tied to property-related financial commitments than those that remained.

For mortgages and loans, the relocation group shows significantly lower expenditure than the stay group. The estimated coefficient is approximately NZD -9,586 and is statistically significant at the 5% level. This suggests that relocated households were less likely to carry large mortgage or loan payments than households that remained in Canterbury, conditional on age, sex, and household size. One plausible interpretation is that stay households were more likely to retain owner-occupied properties, continue mortgage repayments, or finance repair and reconstruction, whereas relocation households were more likely to move into rental or temporary accommodation and avoid new long-term housing debt.

The control variables also behave largely as expected. Household size is positive and highly significant across all six outcomes, indicating that larger households have higher income, greater total expenditure, and larger housing-related financial commitments. This pattern is

consistent with theoretical expectations regarding household scale: as households grow in size, they typically include more potential earners and face greater aggregate consumption needs (Browning et al., 2013). Age is negatively associated with income, total expenditure, housing costs, and mortgages and loans, but positively associated with other property expenditure. This pattern is broadly consistent with life-cycle variation in income, expenditure, and asset-related commitments (Modigliani, 1986), although the linear age specification should be interpreted as an average association over the observed age distribution rather than as a full life-cycle profile.

In summary, although migration is self-selected and the estimated differences cannot be interpreted as causal effects of relocation, the data reveal clear and systematic contrasts in housing-related expenditure between relocated and non-relocated households. Relocation households do not differ significantly from stay households in total income, regular income, or total expenditure after adjustment. The main differences are instead concentrated in housing costs and mortgages and loans: relocation households face substantially higher housing costs, while stay households appear to carry higher mortgage and loan payments.

This shift in expenditure structure underscores the considerable heterogeneity in housing consumption and asset-investment decisions following post-disaster migration. It is also consistent with international evidence that post-disaster migration is selective, with younger, more mobile, and less housing-secure households more likely to move after major disasters (Fussell, 2015). These results reinforce the importance of distinguishing between income effects and housing-cost effects when evaluating household recovery after disasters. They also provide useful evidence that post-earthquake migration was closely related to the structure of household housing costs and housing-related financial commitments, even though average income differences between movers and stayers are limited once observable characteristics are taken into account.

5.5 Robustness check

Table 5. Robustness checks for total household income

	Baseline	Caliper = 0.1	Ratio = 1:1	Remove "sex"
MMI group: Low× Post	7043.47* (2889.43)	7686.24* (2943.46)	8088.52* (3348.37)	7736.24* (2939.97)
MMI group: High× Post	7251.79* (3559.44)	7841.80* (3695.82)	8265.12* (3508.18)	7888.43* (3759.07)
Observations	17193	17166	11457	17190

* p < 0.05, ** p < 0.01, *** p < 0.001

Table 6. Robustness check for total household regular income

	Baseline	Caliper = 0.1	Ratio = 1:1	Remove "sex"
MMI group: Low× Post	7924.42** (2673.83)	8408.67** (2719.98)	9345.04** (3159.44)	8491.53** (2734.90)
MMI group: High× Post	7411.86* (3286.86)	7862.88* (3453.67)	8821.37* (3397.87)	7941.28* (3538.33)
Observations	17193	17166	11457	17190

* p < 0.05, ** p < 0.01, *** p < 0.001

Table 7. Robustness check for total household expenditure

	Baseline	Caliper = 0.1	Ratio = 1:1	Remove "sex"
MMI group: Low× Post	3413.71 (2507.03)	2525.22 (2432.83)	2340.68 (2836.63)	2984.13 (2433.23)
MMI group: High× Post	2010.89 (1469.80)	1310.34 (1326.58)	1019.43 (1897.99)	1715.67 (1329.33)
Observations	5022	5016	3354	5025

* p < 0.05, ** p < 0.01, *** p < 0.001

To ensure the reliability of the estimated post-earthquake effects on household income and expenditure, we conduct a series of robustness checks using alternative PSM specifications. The original model employs nearest-neighbour matching with a caliper of 0.2, a 1:2 matching ratio, a logit-based propensity score and four matching covariates: age of the reference person, household size, sex and education. We then alter one dimension at a time and re-estimate the DiD models. The results are summarised in Tables 5 – 7.

Using a stricter caliper, reducing the maximum propensity-score distance from 0.2 to 0.1, yields estimates that are very similar to the baseline results. For total household income, the estimated post-earthquake effects remain positive and statistically significant for both the low- and high-intensity groups. The coefficients are slightly larger than in the baseline specification, increasing from around NZD 7,043 to NZD 7,686 for the low-intensity group and from around NZD 7,252 to NZD 7,842 for the high-intensity group. The corresponding estimates for regular household income are also slightly larger and remain statistically significant. These results indicate that the income findings are not driven by relatively distant matches in the propensity-score space.

When we reduce the matching ratio from 1:2 to 1:1, the income estimates again remain positive and statistically significant. The point estimates are somewhat larger than in the baseline specification: for total household income, the low- and high-intensity coefficients increase to around NZD 8,089 and NZD 8,265, respectively; for regular household income, they increase to around NZD 9,345 and NZD 8,821. The number of observations is smaller under 1:1 matching, as expected, because only one matched control household is retained for each treated household. The persistence of positive and statistically significant income effects under this more restrictive matching design suggests that the income results are not sensitive to the choice of matching ratio.

We next exclude the reference person's sex from the set of matching covariates. Removing sex has only a modest impact on the estimated income effects. The low- and high-intensity coefficients remain positive and statistically significant for both total household income and regular household income, and their magnitudes are close to those in the baseline model. This suggests that the main income results are not driven by the inclusion of sex in the propensity-score matching procedure.

The robustness checks for total household expenditure show a different but consistent pattern. Across all specifications, the estimated expenditure effects remain positive but statistically insignificant for both the low- and high-intensity groups. Under the stricter caliper, the coefficients decline to around NZD 2,525 for the low-intensity group and around NZD 1,310 for the high-intensity group. Under 1:1 matching, they are smaller still, at around NZD 2,341

and NZD 1,019, respectively. When sex is removed from the matching covariates, the estimates remain positive but imprecise. These results reinforce the conclusion from Section 5.2 that the earthquakes did not generate a statistically clear average increase in total household expenditure, even though several expenditure categories changed substantially.

Lastly, we replace the logit model with a probit specification when estimating the propensity scores. The resulting DiD estimates are nearly identical to the baseline, indicating that the matched-sample results are robust to the choice of link function in the propensity-score model. We also re-estimate the models using propensity scores derived from a linear probability model (LPM) instead of a logit specification. Diagnostic statistics, such as the maximum standardised mean difference and variance ratios, indicate that the LPM-based matching procedure achieves covariate balance that is comparable to that of the baseline logit-based matching. However, the estimated treatment effects obtained from the LPM-based propensity scores are smaller in magnitude and generally less statistically significant than those based on logit or probit models. This pattern is consistent with existing methodological evidence. Austin (2011b) shows that matching on the logit of the propensity score with a caliper of 0.2 standard deviations tends to yield substantial bias reduction and efficiency gains, whereas alternative specifications perform less well. Thus, while balance metrics look similar on average, the attenuated coefficients and reduced significance under the LPM specification likely reflect its weaker ability to distinguish units at the extremes of the treatment-probability distribution, leading to marginally poorer matches and larger standard errors near the boundaries.

We further conduct a series of additional checks in which each matching covariate is removed one at a time. The results are consistent with the patterns described above. Excluding any single covariate generally worsens covariate balance to some extent, as reflected in slightly higher standardised mean differences or variance ratios, but the estimated treatment coefficients remain directionally stable and their magnitudes are not materially altered.

We also conduct two additional sets of robustness checks, reported in Appendices H and I. First, we estimate a continuous-MMI specification to assess whether the results are sensitive to the discrete low/high intensity classification. The continuous-MMI interaction terms are not statistically significant, suggesting that the post-earthquake response does not follow a simple

linear gradient in shaking intensity. This supports the use of the grouped intensity specification in the main analysis. Second, we compare the baseline specification with alternative age controls and ATT-IPW bridge specifications. The income effects remain positive across these checks, while the total-expenditure effects remain statistically imprecise, reinforcing the main interpretation.

Taken together, these robustness checks show that the main conclusions are stable across a range of alternative matching specifications. The income effects remain positive and statistically significant across the stricter caliper, 1:1 matching, and the specification excluding sex from the matching covariates. The total-expenditure estimates remain positive but statistically insignificant across the same checks. These patterns reinforce the credibility of the main interpretation: the Canterbury earthquakes are associated with increases in household income, while changes in expenditure are more visible in specific expenditure categories than in total household expenditure.

6. Conclusion and Discussion

This study examines the medium- to longer-term effects of the 2010 – 2011 Canterbury earthquakes on household income and expenditure. By linking nationally representative Household Economic Survey data to administrative address histories and meshblock-level earthquake-intensity measures within New Zealand’s Integrated Data Infrastructure, we identify households that resided in Canterbury at the time of the earthquakes and compare their outcomes with those of contemporaneously matched North Island households. The analysis treats the HES as a repeated cross-section and combines propensity-score matching with a difference-in-differences design, supplemented by event-study estimates, compositional-balance tests, alternative matching specifications, and an ATT-IPW robustness exercise.

6.1 Key findings

The first main finding is that both total household income and regular household income increased in earthquake-exposed areas relative to the matched comparison group. For total

household income, the estimated post-earthquake increases are approximately NZD 7,043 in low-intensity areas and NZD 7,252 in high-intensity areas. The corresponding estimates for regular household income are approximately NZD 7,924 and NZD 7,412. The similarity of the estimates across the two intensity groups suggests that the income response was not confined to the locations experiencing the most severe ground shaking.

The event-study evidence supports this interpretation. The pre-earthquake coefficients for the two income outcomes are individually imprecise, and joint Wald tests fail to reject the null hypothesis that they are jointly equal to zero. Post-earthquake coefficients become positive in several subsequent waves, with some of the largest estimates appearing during the later reconstruction period. These patterns are consistent with reconstruction-related labour demand, wage and employment effects, and broader regional recovery dynamics (Wood et al., 2016; Roth Tran & Wilson, 2025). The positive effects on regular income are particularly important because they indicate that the results are unlikely to be explained solely by one-off insurance settlements or compensation payments.

The income results should not, however, be interpreted as evidence that the earthquakes improved household welfare. Increased labour demand, insurance-related inflows, and public reconstruction expenditure may raise measured income while households simultaneously experience housing damage, disruption, displacement, health effects, and additional living costs. This distinction is central to the economic interpretation of disaster recovery: positive income growth after a disaster may reflect costly reconstruction and compensatory activity rather than a net welfare gain.

Second, total household expenditure does not exhibit a statistically significant average DiD effect. The estimated coefficients are positive for both the low- and high-intensity groups, but they are imprecisely estimated. This result is stable across alternative matching calipers, matching ratios, and covariate specifications. The event-study evidence for total expenditure is necessarily more limited because comparable full-expenditure information is available in only four HES waves, leaving only one comparable pre-earthquake coefficient. That coefficient is not statistically significant, but the limited pre-period means that the parallel-trends evidence for expenditure is weaker than for income.

The absence of a clear effect on total expenditure masks substantial changes in expenditure composition. In high-intensity areas, the magnitude of receipts and refunds increases by approximately 108%, and general-insurance expenditure increases by approximately 101%. Significant increases also appear in transportation, telecommunications, diary-recorded day-to-day purchases, fees and subscriptions, education, recreation, sport and culture, and household maintenance. In low-intensity areas, housing costs and fees and subscriptions increase significantly. These findings indicate that households adjusted across several distinct margins, including insurance and reimbursement flows, routine consumption, mobility, communication, housing and maintenance, and subscription- and service-related spending.

Receipts and refunds deserve particular attention. In the HES, this category includes insurance reimbursements and related inflows recorded as negative expenditure entries. The large high-intensity effect is therefore consistent with substantial post-earthquake insurance and compensation activity. At the same time, the category is broader than insurance settlements alone, and the use of $\log(1+|y|)$ means that the estimates capture changes in the magnitude of recorded entries rather than their direction as net cash inflows or outflows. The results should consequently be interpreted as evidence of increased insurance- and refund-related financial activity, rather than as a direct estimate of household compensation income.

Third, expenditure responses differ markedly by household income. Above-median-income households exhibit significant adjustments across a relatively broad range of categories, including receipts and refunds, general insurance, housing costs, household maintenance, telecommunications, miscellaneous payments, and fees and subscriptions. In high-intensity areas, the estimated increase in the magnitude of receipts and refunds is approximately 183% for above-median-income households, compared with approximately 41% for below-median-income households. This gap is consistent with differences in asset ownership, insurance depth, claim size, and households' capacity to engage with repair and compensation processes (Kousky, 2014; Owen & Noy, 2019).

Below-median-income households exhibit a narrower pattern of adjustment. Significant changes are concentrated in receipts and refunds, medical and health expenditure, diary-recorded day-to-day purchases, and travel. This pattern is consistent with more

constrained households focusing their adjustments on liquidity, health, essential consumption, and selected mobility needs rather than on a broad set of discretionary, housing-maintenance, or asset-related categories. The results therefore suggest that income and asset ownership shape not only the scale of post-disaster adjustment but also the margins through which households are able to respond.

Fourth, the migration analysis identifies substantial differences in housing-related expenditure between households that remained in Canterbury and those later observed outside the region. Conditional on age, sex, and household size, relocation households do not differ significantly from stay households in total income, regular income, or total expenditure. They do, however, report housing costs approximately NZD 24,901 higher and mortgage-and-loan expenditure approximately NZD 9,586 lower.

These estimates are descriptive associations rather than causal effects of relocation. Migration was self-selected and was likely influenced by housing damage, tenure, employment, family circumstances, financial resources, and other unobserved characteristics. Nevertheless, the results indicate that post-earthquake mobility was closely associated with the structure of housing expenditure. Relocation households appear to have faced greater accommodation or resettlement costs, while households that remained were more likely to retain mortgage and loan commitments connected to existing properties, repairs, or reconstruction. This pattern is consistent with evidence that post-disaster migration is selective and that younger and less housing-secure households are generally more mobile after major shocks (Fussell, 2015).

The main conclusions are supported by a range of robustness checks. The income estimates remain positive under stricter propensity-score calipers, 1:1 matching, alternative matching covariates, and alternative age specifications. Pseudo-outcome tests provide no evidence of systematic differential changes in the principal demographic covariates or broad household-composition categories. The continuous-MMI interactions are not statistically significant, suggesting that the response does not follow a simple linear gradient in ground-shaking intensity and that the grouped estimates may capture non-linear differences in household adjustment. ATT-IPW estimates generally preserve the direction of the income effects but are less precisely estimated when the full eligible control pool is used. The

weighting results therefore support the substantive direction of the PSM findings while also indicating that statistical precision depends on the choice of comparison sample and estimator.

6.2 Limitations and directions for future research

Several limitations qualify these conclusions. First, the HES is a repeated cross-section rather than a longitudinal household panel. The analysis therefore identifies changes in average outcomes across survey waves but cannot directly trace the same households before and after the earthquakes. Although matching is conducted separately within each wave and the pseudo-outcome tests show no systematic differential compositional changes, unobserved changes in sample composition cannot be completely excluded. Selective migration is a particularly relevant concern because households that left Canterbury differ from those that remained in age and housing tenure.

Second, the DiD interpretation relies on the assumption that exposed and matched comparison households would have followed parallel trends in the absence of the earthquakes. The event-study estimates and joint Wald tests provide supportive evidence for this assumption for total and regular household income, but statistical non-rejection does not prove that counterfactual trends were identical. For total expenditure, the parallel-trends assessment is substantially weaker because only one comparable pre-earthquake full-expenditure wave is available.

Third, income and expenditure in the HES are self-reported and are subject to recall, classification, and annualisation error. Diary-recorded expenditures are collected over a short observation window and may be affected by atypical purchases, seasonal variation, or behavioural responses to diary keeping. Retrospective expenditure modules may be subject to longer recall periods. These measurement features can reduce precision and may partly explain why significant movements appear in individual categories even when total expenditure shows no clear average effect.

The category-level transformation also imposes an interpretive limitation. The use of $\log(1+|y|)$ reduces the influence of extreme observations and facilitates proportional interpretation, but it removes the original sign of expenditure entries. This is especially relevant for receipts and

refunds, where negative expenditure values can represent reimbursements or other inflows. The estimates therefore describe changes in the magnitude of these entries, not changes in their signed net contribution to household resources.

Fourth, pre-earthquake address histories cannot be recovered for a non-trivial share of HES reference persons. The similarity between missing-address patterns in the HES linkage and the Census-based comparison suggests that this problem largely reflects structural limitations in the administrative address data. Nevertheless, missingness is not necessarily random, particularly among young adults, highly mobile individuals, and people with periods overseas, and some selection risk remains.

Fifth, meshblock-level MMI captures local ground-shaking intensity but does not directly measure dwelling-level damage, repair costs, insurance coverage, or access to compensation. The low- and high-intensity categories necessarily simplify a continuous and spatially heterogeneous hazard. The absence of a significant linear continuous-MMI interaction further indicates that earthquake effects may depend on thresholds, local damage, housing characteristics, or institutional responses rather than on a uniform marginal increase in shaking intensity.

Finally, several heterogeneity analyses are constrained by sample size. This applies particularly to relocation households, income-specific expenditure models, and small address-classification groups. The migration estimates are also endogenous by construction and should not be interpreted as causal effects of moving. The ATT-IPW results, while directionally informative, are less precise than the matched-sample estimates and therefore do not provide identical statistical confirmation of the baseline effects.

Future research could address these limitations by combining longitudinal administrative income and employment records with household-linkage methods that follow individuals and family units over time. This would make it possible to distinguish changes in wages, labour supply, transfers, business income, and household composition, and to separate individual recovery from compositional changes in repeated cross-sections (Abdeljawad & Noy, 2025a). Linking more detailed property-damage, insurance-claim, tenure, mortgage, and

1229 residential-mobility information would help identify the mechanisms connecting hazard
1230 exposure to compensation, debt, repair, and relocation.

1231 Further work could also examine heterogeneity by ethnicity, household structure, presence and
1232 age of children, housing tenure, employment sector, and social capital. These dimensions are
1233 likely to influence access to insurance, credit, informal support, and reconstruction
1234 opportunities. More frequent expenditure data or linked financial-transaction records would be
1235 especially valuable for distinguishing short-run emergency spending from longer-term changes
1236 in consumption and saving.

1237 Overall, the Canterbury earthquakes produced neither a uniform economic loss nor a uniform
1238 recovery at the household level. Household incomes increased relative to the matched
1239 comparison group, but total expenditure did not change clearly on average, and the
1240 composition of spending shifted substantially. The scale and form of these adjustments differed
1241 by income, exposure intensity, and migration status. These findings demonstrate that disaster
1242 recovery cannot be evaluated adequately using aggregate income or output alone. A complete
1243 assessment must also account for insurance and reimbursement flows, housing and relocation
1244 costs, health and routine consumption needs, debt commitments, and the unequal resources
1245 households can mobilise during reconstruction.

References

- Abdeljawad, Y., & Noy, I. (2025a). The impact of 2010–11 Christchurch earthquakes on the individuals' income by sector and income level. *The Annals of Regional Science*, 74, Article 60.
- Abdeljawad, Y., & Noy, I. (2025b). The impact of the Christchurch earthquakes (2010–2011) on labour productivity in the Canterbury region in New Zealand. *Environmental Hazards*, 24(2), 178–193.
- Angrist, J. D., & Pischke, J. S. (2009). *Mostly harmless econometrics: An empiricist's companion*. Princeton University Press.
- Austin, P. C. (2011a). An introduction to propensity score methods for reducing the effects of confounding in observational studies. *Multivariate Behavioral Research*, 46(3), 399–424.
- Austin, P. C. (2011b). Optimal caliper widths for propensity-score matching when estimating differences in means and differences in proportions in observational studies. *Pharmaceutical Statistics*, 10(2), 150–161.
- Bertrand, M., Duflo, E., & Mullainathan, S. (2004). How much should we trust differences-in-differences estimates? *The Quarterly Journal of Economics*, 119(1), 249–275.
- Bondonio, D., & Greenbaum, R. T. (2018). Natural disasters and relief assistance: Empirical evidence on the resilience of U.S. counties using dynamic propensity score matching. *Journal of Regional Science*, 58(3), 659–680.
- Browning, M., Chiappori, P. A., & Lewbel, A. (2013). Estimating consumption economies of scale, adult equivalence scales, and household bargaining power. *The Review of Economic Studies*, 80(4), 1267–1303.
- Carter, M. R., Little, P. D., Mogues, T., & Negatu, W. (2007). Poverty traps and natural disasters in Ethiopia and Honduras. *World Development*, 35(5), 835–856.

- Cavallo, E., Galiani, S., Noy, I., & Pantano, J. (2013). Catastrophic natural disasters and economic growth. *The Review of Economics and Statistics*, 95(5), 1549–1561.
- Deryugina, T. (2017). The fiscal cost of hurricanes: Disaster aid versus social insurance. *American Economic Journal: Economic Policy*, 9(3), 168–198.
- Deryugina, T., Kawano, L., & Levitt, S. (2018). The economic impact of Hurricane Katrina on its victims: Evidence from individual tax returns. *American Economic Journal: Applied Economics*, 10(2), 202–233.
- duPont IV, W., Noy, I., Okuyama, Y., & Sawada, Y. (2015). The long-run socio-economic consequences of a large disaster: The 1995 earthquake in Kobe. *PLOS ONE*, 10(10), e0138714.
- Fussell, E. (2015). The long-term recovery of New Orleans' population after Hurricane Katrina. *American Behavioral Scientist*, 59(10), 1231–1245.
- Gallagher, J., & Hartley, D. (2017). Household finance after a natural disaster: The case of Hurricane Katrina. *American Economic Journal: Economic Policy*, 9(3), 199–228.
- GeoNet. (n.d.). Earthquake intensity. GNS Science.
- Gignoux, J., & Menéndez, M. (2016). Benefit in the wake of disaster: Long-run effects of earthquakes on welfare in rural Indonesia. *Journal of Development Economics*, 118, 26–44.
- Groen, J. A., & Polivka, A. E. (2010). Going home after Hurricane Katrina: Determinants of return migration and changes in affected areas. *Demography*, 47(4), 821–844.
- Hallegatte, S., Vogt-Schilb, A., Rozenberg, J., Bangalore, M., & Beaudet, C. (2020). From poverty to disaster and back: A review of the literature. *Economics of Disasters and Climate Change*, 4(1), 223–247.
- Kaiser, A., Holden, C., Beavan, J., Beetham, D., Benites, R., Celentano, A., Collett, D., Cousins, J., Cubrinovski, M., Dellow, G., Denys, P., Fielding, E., Fry, B., Gerstenberger, M., Langridge, R., Massey, C., Motagh, M., Pondard, N., McVerry, G., ... Zhao, J. (2012).

The Mw 6.2 Christchurch earthquake of February 2011: Preliminary report. *New Zealand Journal of Geology and Geophysics*, 55(1), 67–90.

Kousky, C. (2014). Informing climate adaptation: A review of the economic costs of natural disasters. *Energy Economics*, 46, 576–592.

Modigliani, F. (1986). Life cycle, individual thrift, and the wealth of nations. *Science*, 234(4777), 704–712.

Noy, I., & duPont IV, W. (2018). The long-term consequences of disasters: What do we know, and what we still don't. *International Review of Environmental and Resource Economics*, 12(4), 325–354.

Owen, S., & Noy, I. (2019). Regressivity in public natural hazard insurance: A quantitative analysis of the New Zealand case. *Economics of Disasters and Climate Change*, 3(3), 235–255.

Parker, M., & Steenkamp, D. (2012). The economic impact of the Canterbury earthquakes. *Reserve Bank of New Zealand Bulletin*, 75(3), 13–25.

Roth Tran, B., & Wilson, D. J. (2025). The local economic impact of natural disasters. *Journal of the Association of Environmental and Resource Economists*, 12(6), 1667–1704.

Saunders, J. K., Minson, S. E., & Baltay, A. S. (2022). How low should we alert? Quantifying intensity threshold alerting strategies for earthquake early warning in the United States. *Earth's Future*, 10(3), e2021EF002515.

Sawada, Y., & Shimizutani, S. (2008). How do people cope with natural disasters? Evidence from the Great Hanshin-Awaji (Kobe) earthquake in 1995. *Journal of Money, Credit and Banking*, 40(2–3), 463–488.

Stats NZ. (2014). *2013 Census QuickStats about income*. Stats NZ.

Stats NZ. (2020). *Household Economic Survey (HES) expenditure classification and methodology*. Stats NZ.

Stats NZ. (2023). *Regional gross domestic product: Year ended March 2022*. Stats NZ.

- Stuart, E. A. (2010). Matching methods for causal inference: A review and a look forward. *Statistical Science*, 25(1), 1–21.
- Wing, C., Simon, K., & Bello-Gomez, R. A. (2018). Designing difference in difference studies: Best practices for public health policy research. *Annual Review of Public Health*, 39, 453–469.
- Wood, A., Noy, I., & Parker, M. (2016). The Canterbury rebuild five years on from the Christchurch earthquake. *Reserve Bank of New Zealand Bulletin*, 79(3).
- Yan, J., Zhou, C., & Qi, S. (2025). Extreme climate risk and rural household consumption. *Journal of the Asia Pacific Economy*. Advance online publication.

Appendices

Appendix A: Sample classification and missing address rates

Table A.1. Distribution of households across the four sample groups, by survey year³

Survey Year	Group 1	Group 2	Group 3	Group 4
2006/07	399	18	18	1890
2007/08	447	15	18	2349
2008/09	450	12	12	2304
2009/10	426	S	15	2088
2010/11	435	S	18	2454
2011/12	465	21	24	2385
2012/13	387	27	21	2040
2013/14	408	30	24	2253
2014/15	648	54	45	3744
2015/16	390	30	48	2367
2016/17	414	42	42	2526
2017/18	567	66	63	3774

³In all tables in this paper, the letter "S" indicates that the sample size for that cell in the given year is fewer than 6 and has been suppressed in accordance with Stats NZ confidentiality rules. All other numeric values have been subject to RR3 random rounding as required.

Table A.2. Unmatched pre-earthquake address records by HES survey year

Survey Year	Valid HES Households	Unmatched Addresses	Unmatched rate (%)
2006/07	2799	477	17.00%
2007/08	3393	561	16.50%
2008/09	3285	501	15.30%
2009/10	3033	492	16.20%
2010/11	3435	516	15.00%
2011/12	3450	555	16.10%
2012/13	2934	453	15.40%
2013/14	3363	645	19.20%
2014/15	5505	1014	18.40%
2015/16	3465	627	18.10%
2016/17	3675	645	17.60%
2017/18	5451	969	17.80%

Table A.3. Unmatched address rates from linkage to 2013 Census records

Survey Year	Matched	Unmatched	Unmatched Rate (%)
2006/07	2226	669	23.10%
2007/08	2691	804	23.00%
2008/09	2706	672	19.90%
2009/10	2580	543	17.40%
2010/11	2943	591	16.70%
2011/12	3006	555	15.60%
2012/13	2664	339	11.30%
2013/14	2922	462	13.70%
2014/15	4827	732	13.20%
2015/16	3069	426	12.20%
2016/17	3138	564	15.20%
2017/18	4629	846	15.50%

Appendix B. Matching quality and compositional balance checks

Table B.1 PSM matching quality: Max SMD and Variance Ratio by Survey Year

Survey Year	Max SMD	Max VR
2006/07	0.05	1.10
2007/08	0.07	1.08
2008/09	0.02	1.09
2009/10	0.02	1.09
2010/11	0.03	1.06
2011/12	0.05	0.97
2012/13	0.03	1.13
2013/14	0.04	1.01
2014/15	0.03	0.94
2015/16	0.02	1.04
2016/17	0.04	1.07
2017/18	0.03	1.16

Table B.2 Four-cell means of key covariates by treatment and post status

variable	treat	post	n obs	mean value	sd value
age	0	0	1695	50.54	17.42
age	0	1	1659	54.89	17.07
age	1	0	846	50.77	17.75
age	1	1	831	54.29	17.56
household size	0	0	1695	2.46	1.27
household size	0	1	1659	2.36	1.31
household size	1	0	846	2.45	1.26
household size	1	1	831	2.34	1.31
female	0	0	1695	0.56	0.50
female	0	1	1659	0.60	0.49
female	1	0	846	0.55	0.50
female	1	1	831	0.60	0.49
own/trust	0	0	1695	0.69	0.46
own/trust	0	1	1659	0.70	0.46
own/trust	1	0	846	0.72	0.45
own/trust	1	1	831	0.72	0.45
higher education	0	0	1695	0.28	0.45
higher education	0	1	1659	0.29	0.45
higher education	1	0	846	0.29	0.46
higher education	1	1	831	0.30	0.46

Note: Treat = 1 denotes Canterbury households; Post = 1 denotes post-earthquake survey waves.

Table B.3 Pseudo-outcome DiD tests for key covariates

variable	n_obs	coef_treat_post	se_treat_post	p_treat_post
age	5028	-0.83	0.85	0.33
household size	5028	-0.01	0.05	0.89
female	5028	0.00	0.02	0.86
own/trust	5028	-0.01	0.02	0.73
higher education	5028	0.00	0.02	0.84

Table B.4 Pseudo-outcome DiD tests for household-composition categories

variable	n_obs	coef_treat_post	se_treat_post	p_treat_post	weighting
one-family household	17214	-0.01	0.01	0.43	unweighted
two-family household	17214	0.01	0.01	0.28	unweighted
three-plus-family household	17214	-0.01	0.01	0.32	unweighted
other multiperson household	17214	-0.01	0.01	0.34	unweighted
one-person household	17214	0.01	0.01	0.51	unweighted
unidentifiable household	17214	0.01	0.01	0.10	unweighted
nonfamily or one-person hhld	17214	0.01	0.01	0.28	unweighted
one-family household	17214	0.01	0.01	0.54	weighted
two-family household	17214	0.00	0.02	0.94	weighted
three-plus-family household	17214	0.00	0.01	0.78	weighted
other multiperson household	17214	-0.01	0.01	0.53	weighted
one-person household	17214	-0.02	0.01	0.22	weighted
unidentifiable household	17214	0.02	0.01	0.12	weighted
nonfamily or one-person hhld	17214	-0.01	0.02	0.75	weighted

Appendix C. Event-study estimates and pre-trend tests

Table C.1. Event-study estimates

	total household income	regular household income	total household expenditure
year = 0708 × treat (pre)	7712.23 (7900.57)	5381.47 (7447.81)	
year = 0809 × treat (pre)	10009.71 (8882.54)	9576.58 (8808.09)	
year = 0910 × treat (pre)	10277.22 (9696.48)	9682.52 (9336.30)	1057.73 (1741.46)
year = 1011 × treat (pre)	9168.49 (7961.18)	7480.27 (7740.02)	
year = 1112 × treat (post)	13230.184 (7190.26)	13528.07* (6712.10)	
year = 1213 × treat (post)	11666.19 (9582.08)	10043.08 (8942.25)	3128.01 (1930.54)
year = 1314 × treat (post)	11939.18 (6723.37)	11550.66 (6106.45)	
year = 1415 × treat (post)	20425.94* (9421.72)	20450.81* (9290.82)	
year = 1516 × treat (post)	17783.92** (6212.52)	15547.38* (6080.73)	2830.61 (2738.57)
year = 1617 × treat (post)	4558.82 (6041.94)	4592.35 (6105.49)	
year = 1718 × treat (post)	15085.98 (10687.29)	14853.11 (10230.82)	
age	3813.21*** (304.91)	3464.62*** (299.85)	1172.62*** (99.32)
age ²	-37.83*** (3.06)	-34.60*** (3.00)	-13.33*** (0.96)
sex	-11895.73*** (987.72)	-11688.95*** (934.91)	-3386.70*** (769.00)
household size	14079.33*** (743.24)	13963.05*** (720.67)	6807.11*** (593.60)
Num.Obs.	17193	17193	5022
R2	0.14	0.15	0.36
R2 Adj.	0.14	0.14	0.35
R2 Within	0.1	0.11	0.15
R2 Within Adj.	0.1	0.11	0.15
Std.Errors	by: TA code	by: TA code	by: TA code

* p < 0.05, ** p < 0.01, *** p < 0.001

Table C.2. Joint Wald tests of pre-earthquake coefficients

outcome	test	statistic	p_value
total household income	pre-trend joint Wald	0.39	0.81
regular household income	pre-trend joint Wald	0.65	0.62
total household expenditure	pre-trend joint Wald	0.37	0.54

Appendix D. Descriptive Statistics

Table D.1. Descriptive Statistics by Earthquake Intensity and Control Group Classification

Survey Year	Group	Counts	Household size		Ref_sex	Housing		Total household income		Total household regular income		Total household expenditure		Housing cost		Mortgages and loans cost	
			Mean	SD		%Owned	%Trust	Median	SD	Median	SD	Median	SD	Median	SD	Mean	SD
2006/07	Treated	417	49.6	2.5	54.0%	73.4%		64357	56067.5	63046.4	55089.4	45611.9	34314.9	9308	43656.5	25118	54783.2
	-Low	147	51.7	2.5	60.5%	85.7%		53790.5	37811.2	53289.7	37379.4	31652.1	27992.3	7529	21990.8	23040.7	47578.3
	-High	270	48.5	2.4	51.1%	65.6%		54938.3	53458.5	54259.6	52027.7	38767.6	37179.4	10529.2	51191.7	26346.5	58473.4
	Control	831	49.2	2.5	54.2%	67.5%		68761.8	54185.6	67350.9	53128.5	49739.4	41711.8	12480.3	36026.2	26093.9	54666.2
2007/08	Treated	462	51.6	2.4	54.5%	71.4%		70301.9	84767.9	65753.1	56468.7	10538.8	10208.4	9102	34326.1	25366.2	48469.3
	-Low	159	53.3	2.3	49.1%	73.6%		51768.6	46150.1	49807.9	41013.1	6830.7	8063.8	8571.8	19636.4	18654.5	37386.4
	-High	297	50.5	2.5	57.6%	70.7%		55191	99493	53738.8	63034.4	10528.6	11035.6	9563.2	39756.4	29212.2	53319.7
	Control	924	50.6	2.4	56.2%	66.9%		75385.2	75361.4	73243.6	69668.5	11512.2	12451.5	12457	34971	25877.7	63821.8
2008/09	Treated	459	50.8	2.3	59.5%	69.9%		70666.2	69565.6	68583.4	67340.8	10049.2	9801.4	9600.7	41916.9	23598.4	52551.3
	-Low	150	52.2	2.4	52.0%	78.0%		57320	84465.8	57180	81707.4	3910.7	8517.2	7236	31454	15289.3	35406.7
	-High	306	50.2	2.3	63.7%	64.7%		56935	61228	53627.6	59283.1	8645.9	10249.9	11262.9	45948.1	27469.2	58914.4
	Control	921	50.1	2.4	60.3%	65.8%		73543.9	72210.6	72179.9	70173.7	11657.7	12346.6	12884.3	52254.2	25221.3	64566.4
2009/10	Treated	432	51.7	2.4	57.6%	70.8%		72656.9	72316	70499	70158.2	48909.3	33203.6	11855.8	49444.8	21351.5	44541.5
	-Low	135	53.5	2.3	60.0%	75.6%		49969.2	101713.1	49969.2	100370	36842.3	27598.5	8817.5	24629.6	12933	30059.6
	-High	294	50.9	2.5	58.2%	68.4%		59306.4	53813.5	58229.3	50705.9	44945.7	35100.1	13713.6	55829.2	25320.2	49470.6
	Control	867	52.1	2.5	57.4%	67.8%		73793.7	68841.3	71960.4	64739	50816.5	36407.9	14165.9	53662.4	24036.5	59536.4
2010/11	Treated	432	51.7	2.4	54.9%	70.8%		75699.9	68565.1	72469.3	63612.4	10409.3	9248.4	11531.3	41544.3	23744.8	48414.3
	-Low	165	54.7	2.3	54.5%	80.0%		55872.4	49228.2	55872.4	40949.1	3076	7296.2	7538.3	23680.7	13885.6	31630
	-High	267	49.8	2.5	55.1%	65.2%		63023.4	77767.8	60691.2	73826.6	11471.4	9810.5	15214.4	48373	29963.4	55617.7
	Control	867	51.4	2.5	55.7%	66.4%		82139.7	78790.6	80312.9	74871.1	13325.5	14847.6	14235	48192.9	29323.3	66567.5
2011/12	Treated	486	51.7	2.4	59.9%	67.9%		74452.6	61820.2	73046.9	61000.5	10664.5	10269.8	12822	82047.8	24249.6	64118
	-Low	168	52.1	2.4	64.3%	73.2%		58914.3	76456.4	58914.3	76281.2	6470.3	7567.2	12304.7	20909.3	16072	37698
	-High	312	51.6	2.4	56.7%	64.4%		62134.1	52676.9	60360.9	51316.7	8503.7	11312.4	13992.1	99026.9	28377.3	74466.6
	Control	972	52.2	2.3	59.9%	64.2%		74060.4	64046.6	72457.7	62714.3	12736.6	11830.3	14020	42652.4	23521.3	53682.1
2012/13	Treated	411	52.8	2.4	62.8%	71.5%		81298.6	83189.7	77624.8	77034.9	51844.2	33178.1	13893.2	45717.5	22435.7	49592.2
	-Low	162	55.6	2.2	64.8%	81.5%		58639.3	48121	57527.3	44310.7	42478	26283.4	11112	20319.1	18423.9	34691.4
	-High	246	50.8	2.4	61.0%	64.6%		65801.7	99454.8	63171.9	92149.9	47026.3	36671.7	16382.5	55550.9	25276.4	57949.6
	Control	822	53.3	2.4	63.1%	67.5%		81347.4	83490	79490.7	81195.2	53823.7	41361.8	17002.7	53688.3	23630.5	56608.2
2013/14	Treated	438	52.5	2.4	59.6%	71.2%		84491.6	92265.4	80831.4	66605.9	12044	11290.2	13452.3	71752.7	21013.9	44766.4
	-Low	174	54.8	2.3	62.1%	77.6%		58407.3	58500.8	58059.7	56391.3	5560.7	10364	11658.7	28368.9	15100.8	31554.3
	-High	258	51.3	2.4	59.3%	68.6%		65306.3	108628.5	65306.3	71203.3	10428.6	11860.4	17822.1	89559.1	25367.7	51795.6
	Control	873	53	2.4	58.4%	68.4%		85110.4	83869.4	80480.6	72556	1349.7	16130.5	16263.2	48815.6	30211.5	77836
2014/15	Treated	705	53.5	2.4	57.4%	68.5%		90790.8	79499	87540.6	73970.3	12599.3	13184.5	17623.6	60999.3	29568.5	72044.8
	-Low	276	56	2.4	54.3%	79.3%		68774.3	68803.1	66349.4	64562.2	6302	14116.1	15705.1	25218.5	27010.9	71597.8
	-High	381	52.7	2.5	59.8%	65.4%		76181.3	85255.7	74581.4	78678.1	10349.9	12898.6	20087.6	73035.6	33831.7	75223.5
	Control	1407	53.6	2.4	57.8%	65.0%		81138.9	79995.4	78896.1	76931.4	13216.5	13355.3	16470.9	41896.1	25433.2	65626.7
2015/16	Treated	420	55.7	2.3	57.1%	73.6%		86660.2	73841.7	82928.3	66300.7	12563.5	11205	21828.6	52092.7	33720.7	83451.3

Survey Year	Group	Counts	Age Mean	Household size Mean	Ref_sex %Female	Housing %Owned/Trust	Total household income Median	Total household income SD	Total household regular income Median	Total household regular income SD	Total household expenditure Median	Total household expenditure SD	Housing cost Median	Housing cost SD	Mortgages and loans cost Mean	Mortgages and loans cost SD
2016/17	-Low	186	57.9	2.3	58.1%	75.8%	67450.1	63738.4	67033.8	62304.5	7038.2	9893.1	21104.8	29328.1	30785.1	63720.1
	-High	225	53.9	2.3	57.3%	72.0%	72157	81134	70191.3	69153.8	9508	12160	23508.9	64997.8	37427.5	97968.2
	Control	840	55.9	2.3	56.8%	71.1%	80748.5	76421.3	79128.7	71456.3	13931	16181.6	21897.8	53715.5	34915	149370.3
	Treated	456	55.1	2.4	54.6%	71.7%	87109.8	84423.4	84200.8	81756	13054.3	12826.2	21904.5	92275.3	34046.1	81449.2
	-Low	177	56	2.4	54.2%	83.1%	72783	91954.2	69050.9	88930.5	5974.3	13523.2	20341.9	48042.3	38837.3	94530.5
	-High	273	54.8	2.3	53.8%	64.8%	67284	79973.1	65545.2	77544.3	10417.9	12408.2	24158.3	112139.7	31458.5	72472.3
	Control	909	55.5	2.3	56.8%	68.3%	90476	95165.5	86835.6	81678.3	14253.5	13765.5	21505.7	61104.5	35654.3	102858.4
	Treated	633	53.7	2.3	55.0%	68.2%	92544.4	104170.4	90020.4	101953.1	14460	11991.1	27108.6	96966.1	37102.3	78344.9
	-Low	255	54.8	2.3	57.6%	74.1%	66596.6	62110.4	64480	61366.4	11469.4	11940	25070	92862.6	36894.6	72329
	-High	369	53.1	2.4	52.8%	63.4%	76510.6	125409.8	74052.7	122655.3	13035	12090.4	29198.4	99901.4	37432	82871.4
2017/18	Control	1263	54.2	2.3	57.0%	63.7%	86898.4	99394.7	84527.9	95787.6	14336	13506.8	23580.1	60452	33655.1	82833

Notes: (i) This table reports descriptive statistics by survey wave and exposure/control classification. Monetary variables are in NZD. For most

monetary outcomes we report medians together with a dispersion measure (SD). (ii) Mortgages and loans cost is reported as a mean (with SD) because the distribution is strongly right-skewed; therefore, statistics across columns, especially those mixing means and medians, are not mechanically additive. (iii) The Stats NZ total household expenditure variable is not defined consistently across HES waves: in 2006/07, 2009/10, 2012/13 and 2015/16 it aggregates spending across all expenditure categories, whereas in the remaining waves it covers housing-related expenditure only (e.g., rent, mortgage payments, local authority rates and building insurance). Consequently, in housing-only waves, total household expenditure may appear close to housing cost plus mortgage repayments by construction. Where recorded separately in the HES classification, housing cost excludes mortgage repayments. Accordingly, the DiD analysis uses total household expenditure only in 2006/07, 2009/10, 2012/13 and 2015/16; other waves are shown here for descriptive purposes only. (iv) A small number of Canterbury meshblocks have very low seismic intensity ($MMI < 4$). These observations are included in the treated category in the descriptive tables because they are resident in Canterbury at the time of the earthquakes, but they are excluded from the DiD intensity-group specification and therefore do not enter the Low- or High-intensity groups, nor the control group.

Table D.2. Descriptive Statistics by Migration

Survey Year	Group	Counts	Age		Household size		Ref_sex	Housing		Total household income		Total household regular income		Total household expenditure		Housing cost		Mortgages and loans cost	
			Mean	S	Mean	S		%Owned/Trust	Median	SD	Median	SD	Median	SD	Median	SD	Median	SD	Mean
2006/07	Relocation	18	S	S															
	Stay	399	50	2.5	54.9%	74.4%	62448.4	48236.2	61079.9	46985.8	44800	33704.6	9074.6	43268.5	23502.4	47973.3			
2007/08	Relocation	15	S	S															
	Stay	447	51.9	2.4	55.0%	72.5%	70238.9	85952.1	65526.9	56971.9	10470.3	10303.8	8985.9	34118.5	25846	49144.7			
2008/09	Relocation	12	S	S															
	Stay	450	51.3	2.3	59.3%	70.0%	71128.9	70289.1	69058.3	68036.7	10097.4	9900.7	9570.3	41752.1	23994.9	53086.1			
2009/10	Relocation	S	S	S															
	Stay	426	51.9	2.4	57.7%	71.1%	72608.4	72365.9	70502.2	70316.2	48732	33152.6	11816.1	46818.3	21580.3	44750.2			
2010/11	Relocation	S	S	S															
	Stay	435	51.7	2.4	54.5%	70.3%	75699.9	68565.1	72469.3	63612.4	10409.3	9248.4	11531.3	41544.3	23744.8	48414.3			
2011/12	Relocation	21	42.2	3.2	S	S	57969.9	38186.8	57849.6	38183.4	16664	11513.2	61302.8	211406.7	14186.9	34283.4			
	Stay	465	52.2	2.3	59.4%	69.0%	75197	62601.9	73733.2	61771.4	10393.6	10140.3	12539.5	68271.3	24704.1	65128.5			
2012/13	Relocation	27	53.3	2.1	55.5%	44.4%	58116.9	43829.7	58116.9	43829.7	51231.3	26814.6	13988.1	63087.4	7268.6	21237			
	Stay	387	52.8	2.4	62.8%	72.9%	82864.1	85001.1	78942.2	78635.9	51885.6	33592.9	13893.2	44258.7	23460	51175.4			
2013/14	Relocation	30	41.4	2.4	60.0%	40.0%	77288.6	56194.2	77102.9	56320.7	18155.4	12085.6	28678.6	37718.9	20134.8	38166			
	Stay	408	53.4	2.4	59.6%	72.8%	85038.8	94468.5	81114.7	67374.3	11579.6	11106.3	13064.2	73680.9	21080.7	45268.5			
2014/15	Relocation	54	45.9	2.3	55.6%	44.4%	82883.6	101393.9	80950.5	99735.8	12894.6	9346.4	22942.8	82296.6	17962.8	51141.9			
	Stay	648	54.1	2.5	57.9%	70.8%	91461.9	77417.9	88099.9	71431.8	12574.3	13465.1	17287.5	58639.1	30553.5	73491.2			
2015/16	Relocation	30	48.9	2.6	50.0%	60.0%	82299.1	71753.2	77287.4	68710.8	14659	11511.1	22624	46080.7	26168.3	49464.9			
	Stay	390	56.3	2.3	57.7%	74.6%	87019	74089.5	83392.3	66168.5	12391.1	11177.1	21792	52608.1	34341.9	85661.6			
2016/17	Relocation	42	47.9	3	57.1%	64.3%	99701.6	100424.1	93235.5	94387.3	19767.7	16687.8	24045	241829.9	47791.5	88175.1			
	Stay	414	55.8	2.3	53.6%	73.2%	85862.8	82709.2	83306	80472.6	12389.4	12204.6	21531.5	57814.8	32684.8	80739.8			
2017/18	Relocation	66	44.7	2.4	50.0%	50.0%	125513	229835.2	123099.4	228960.8	18447	14496.8	34412.4	163711.7	29196.4	59267.8			
	Stay	567	54.7	2.3	55.6%	60.3%	88758.2	77310	86221.6	74240.9	14002.1	11596.7	26292.6	85904.1	38010.3	80238			

Notes: (i) This table reports descriptive statistics by survey wave and post-earthquake migration classification. Monetary variables are in NZD. For most monetary outcomes we report medians

together with a dispersion measure (SD). (ii) Mortgages and loans cost is reported as a mean (with SD) because the distribution is strongly right-skewed; therefore, statistics across columns, especially those mixing means and medians, are not mechanically additive. (iii) The Stats NZ total household expenditure variable is not defined consistently across HES waves: in 2006/07, 2009/10, 2012/13 and 2015/16 it aggregates spending across all expenditure categories, whereas in the remaining waves it covers housing-related expenditure only (e.g., rent, mortgage payments, local authority rates and building insurance). Consequently, in housing-only waves, total household expenditure may appear close to housing cost plus mortgage repayments by construction. Where recorded separately in the HES classification, housing cost excludes mortgage repayments. Accordingly, the DiD analysis uses total household expenditure only in 2006/07, 2009/10, 2012/13 and 2015/16; other waves are shown here for descriptive purposes only. (iv) Some cells are suppressed (S) where required by Stats NZ confidentiality rules.

Appendix E. DiD Results for Expenditure Categories

term	estimate	std.error	expenditure category	nobs
MMI group: High $\times$ post	0.35	0.19	retirement-savings and contribution schemes	5016
MMI group: Low $\times$ post	-0.04	0.53	retirement-savings and contribution schemes	5016
MMI group: High $\times$ post	0.09	0.10	credit debit accounts	5016
MMI group: Low $\times$ post	-0.24	0.36	credit debit accounts	5016
MMI group: High $\times$ post	0.16**	0.05	diary	5016
MMI group: Low $\times$ post	0.10	0.11	diary	5016
MMI group: High $\times$ post	-0.04	0.11	domestic fuel and power	4197
MMI group: Low $\times$ post	0.02	0.10	domestic fuel and power	4197
MMI group: High $\times$ post	0.28**	0.10	education recreation sport and culture	5016
MMI group: Low $\times$ post	0.13	0.52	education recreation sport and culture	5016
MMI group: High $\times$ post	0.51**	0.18	fees and subs	5016
MMI group: Low $\times$ post	0.82**	0.30	fees and subs	5016
MMI group: High $\times$ post	0.70***	0.15	general insurance	5016
MMI group: Low $\times$ post	0.02	0.42	general insurance	5016
MMI group: High $\times$ post	0.49*	0.19	household maintenance	5016
MMI group: Low $\times$ post	0.64	0.55	household maintenance	5016
MMI group: High $\times$ post	-0.08	0.21	household operations	5016
MMI group: Low $\times$ post	0.15	0.68	household operations	5016
MMI group: High $\times$ post	0.06	0.07	housing costs	5016
MMI group: Low $\times$ post	0.31*	0.14	housing costs	5016
MMI group: High $\times$ post	0.24	0.15	medical and health	5016
MMI group: Low $\times$ post	0.30	0.35	medical and health	5016
MMI group: High $\times$ post	0.37	0.19	miscellaneous payments	5016
MMI group: Low $\times$ post	0.01	0.56	miscellaneous payments	5016
MMI group: High $\times$ post	-0.10	0.15	mortgages and loans	5016
MMI group: Low $\times$ post	0.02	0.26	mortgages and loans	5016
MMI group: High $\times$ post	0.00	0.06	other property	5016
MMI group: Low $\times$ post	0.10	0.13	other property	5016
MMI group: High $\times$ post	0.73***	0.12	receipts and refunds	5016
MMI group: Low $\times$ post	0.30	0.25	receipts and refunds	5016
MMI group: High $\times$ post	0.27*	0.10	telecommunications	5016
MMI group: Low $\times$ post	0.03	0.21	telecommunications	5016
MMI group: High $\times$ post	0.40*	0.15	transportation	5016
MMI group: Low $\times$ post	0.18	0.27	transportation	5016
MMI group: High $\times$ post	0.45	0.23	travel	5016
MMI group: Low $\times$ post	0.83	0.46	travel	5016

* p < 0.05, ** p < 0.01, *** p < 0.001

Appendix F. Median Household Income and Annual Growth of New Zealand regions
in 2006 and 2013 (in NZD)

region	median Household Income (2006)	median Household Income (2013)	annual Growth Rate (%)
Auckland	63,400	76,500	2.72%
Canterbury	47,900	65,000	4.46%
Wellington	59,700	74,300	3.17%
Waikato	49,400	59,600	2.72%
Bay of Plenty	45,400	54,600	2.67%
Hawke's Bay	44,200	53,200	2.68%
Manawatu-Wanganui	41,200	50,000	2.80%
Northland	40,200	46,900	2.23%
Southland	44,200	57,400	3.80%
Otago	44,400	56,400	3.48%
West Coast	37,800	55,000	5.50%
Marlborough	45,500	55,200	2.80%
Nelson	43,900	54,300	3.08%
Tasman	43,000	53,500	3.17%
Gisborne	41,000	50,500	3.02%
Taranaki	44,700	58,400	3.89%

Appendix G. Heterogeneous expenditure responses by income group

Table G.1. Expenditure responses among households above the median income

	housing costs	receipts and refunds	general insurance	miscellaneous payments	telecommunica tions	fees and subs	household maintenance
MMI group: Low × post	0.28* (0.12)	0.57 (0.42)	-0.14 (0.47)	-0.35 (0.86)	0.39** (0.14)	1.31** (0.45)	0.82 (0.72)
MMI group: High × post	0.14** (0.05)	1.04*** (0.19)	0.89*** (0.16)	0.67* (0.33)	0.30*** (0.08)	0.60* (0.28)	0.98** (0.36)
Rented (reference category: Owned/Trust)	1.27*** (0.09)	-0.43 (0.29)	-0.41 (0.27)	0.15 (0.22)	-0.50** (0.17)	-0.44 (0.36)	-3.12*** (0.20)
sex	-0.01 (0.06)	0.07 (0.10)	-0.10 (0.16)	-0.07 (0.13)	0.00 (0.06)	0.00 (0.09)	0.18 (0.28)
log_income	0.34*** (0.09)	0.58*** (0.12)	1.73*** (0.12)	1.80*** (0.18)	0.45*** (0.09)	1.69*** (0.20)	1.67*** (0.26)
age	-0.07*** (0.01)	-0.01 (0.02)	0.11*** (0.03)	0.08** (0.03)	0.04* (0.02)	0.02 (0.04)	0.12** (0.04)
age ²	0.00*** 0.00	0.00 0.00	-0.00** 0.00	-0.00** 0.00	-0.000 0.00	-0.000 0.00	-0.00* 0.00
household size	0.19*** (0.02)	-0.04 (0.06)	0.15* (0.07)	-0.18* (0.07)	0.14** (0.04)	0.87*** (0.06)	0.06 (0.07)
Num. Obs.	2508	2508	2508	2508	2508	2508	2508
R2	0.174	0.046	0.077	0.192	0.061	0.163	0.12
R2 Adj.	0.154	0.023	0.054	0.172	0.038	0.142	0.10
R2 Within	0.121	0.015	0.058	0.045	0.041	0.118	0.10
R2 Within Adj.	0.118	0.011	0.054	0.042	0.038	0.115	0.10
Std. Errors	by: TA code	by: TA code	by: TA code	by: TA code	by: TA code	by: TA code	by: TA code

*p < 0.05, ** p < 0.01, *** p < 0.001

Table G.2. Expenditure responses among households below the median income

	receipts and refunds	medical and health	diary	travel
MMI group: Low × post	0.08 (0.28)	0.18 (0.41)	0.07 (0.19)	1.19* (0.59)
MMI group: High × post	0.34** (0.11)	0.35* (0.16)	0.17* (0.07)	0.07 (0.32)
Rented (reference category: Owned/Trust)	-0.22 (0.12)	-0.71*** (0.19)	-0.56*** (0.09)	-1.29*** (0.16)
sex	-0.03 (0.08)	0.03 (0.12)	-0.058 (0.06)	-0.047 (0.13)
log_income	0.06** (0.02)	0.21** (0.07)	0.23*** (0.05)	0.36*** (0.07)
age	0.02 (0.01)	0.03 (0.02)	0.05*** (0.02)	0.08** (0.03)
age ²	-0.00* (0.00)	-0.00 (0.00)	-0.00*** (0.00)	-0.00** (0.00)
household size	0.03 (0.04)	0.12 (0.10)	0.48*** (0.03)	0.13 (0.12)
Num. Obs.	2508	2508	2508	2508
R2	0.03	0.08	0.2	0.11
R2 Adj.	0.00	0.05	0.18	0.08
R2 Within	0.01	0.02	0.17	0.03
R2 Within Adj.	0.00	0.02	0.17	0.03
Std. Errors	by: TA code	by: TA code	by: TA code	by: TA code

* p < 0.05, ** p < 0.01, *** p < 0.001

Appendix H. Alternative intensity and functional-form checks

Table H.1. Continuous-MMI specification

	total income	household income	regular household income	total expenditure	household expenditure
age	3957.43*** (270.43)	3491.72*** (241.24)	1278.85*** (136.42)		
age2	-39.31*** (2.58)	-35.00*** (2.19)	-14.05*** (1.48)		
sex	-11585.84*** (1426.55)	-11206.81*** (1273.90)	-3406.79*** (465.44)		
Household size	15016.62*** (492.31)	14795.02*** (471.05)	8219.67*** (601.35)		
mmi_intensity × post	-1114.46 (1917.09)	-1036.31 (1790.38)	-1001.20 (630.66)		
Num. Obs.	5697	5697	1662		
R ²	0.15	0.17	0.43		
Adjusted R ²	0.14	0.16	0.42		
Within R ²	0.12	0.13	0.23		
Adjusted Within R ²	0.11	0.13	0.23		
Std. Errors	by: TA code	by: TA code	by: TA code		

* p < 0.05, ** p < 0.01, *** p < 0.001

Table H.2. Bridge specifications with alternative age controls

	total household income, age	regular household income, age	total household expenditure, age	total household income, age square	regular household income, age square	total household expenditure, age square	total household income, age-bin	regular household income, age-bin	total household expenditure, age-bin
mmi_group = Low × post	5957.074* (2975.07)	6910.070* (2827.29)	3409.32 (2325.26)	7043.473* (2889.43)	7924.418** (2673.83)	3413.71 (2507.03)	6804.016* (2800.89)	7713.664** (2569.90)	3517.84 (2456.78)
mmi_group = High × post	7152.296* (3278.08)	7330.460* (3026.27)	1818.91 (1399.49)	7251.790* (3559.44)	7411.856* (3286.86)	2010.89 (1469.80)	7367.213* (3669.48)	7525.615* (3383.48)	2019.98 (1450.39)
age	-181.587*** (23.84)	-188.441*** (24.10)	-238.167*** (21.35)	3810.439*** (301.28)	3462.944*** (295.27)	1170.946*** (100.93)			
household size	14965.277*** (734.27)	14766.590*** (705.13)	7139.099*** (589.71)	14074.521*** (740.65)	13960.682*** (718.84)	6805.154*** (594.64)	14782.474*** (692.90)	14656.381*** (670.47)	7199.692*** (595.74)
age ²				-37.827*** (3.04)	-34.604*** (2.97)	-13.324*** (0.97)			
sex				-11894.360*** (991.53)	-11689.336*** (944.59)	-3388.781*** (768.82)			
age_bin = <25							-12357.616*** (1060.61)	-12116.284*** (1016.00)	-3550.438*** (809.24)
age_bin = 45-64							-10892.041*** (3023.78)	-10806.575*** (3193.55)	-4478.207*** (1357.84)
age_bin = 65+							18122.426*** (902.79)	15565.734*** (931.60)	2468.111* (1086.21)
Num.Obs.	17193	17193	5022	17193	17193	5022	-13045.425*** (1749.74)	-12643.043*** (1710.35)	-10875.653*** (1046.37)
R2	0.11	0.12	0.34	0.14	0.15	0.36	17193	17193	5022
R2 Adj.	0.11	0.12	0.33	0.14	0.14	0.35	0.14	0.15	0.36
R2 Within	0.07	0.08	0.13	0.10	0.11	0.15	0.10	0.11	0.15
R2 Within Adj.	0.07	0.08	0.13	0.10	0.11	0.15	0.10	0.11	0.15
Std.Errors	by: TA code	by: TA code	by: TA code	by: TA code	by: TA code	by: TA code	by: TA code	by: TA code	by: TA code

* p < 0.05, ** p < 0.01, *** p < 0.001

Appendix I. Alternative weighting estimator and bridge specifications

Table I.1. ATT-IPW bridge specifications for household regular income

	A. PSM with basic controls	B. PSM with main controls	C. PSM + richer controls	D. IPW full pool unweighted	E. IPW full pool, ATT-IPW	F. IPW full pool, ATT-IPW + richer controls	G. IPW full pool, ATT-IPW + composition control	H. IPW full pool, trimmed ATT-IPW + richer controls
mmi_group = Low × post	6847.60* (2767.78)	7924.42** (2673.83)	4543.965 (2519.99)	5103.907 (2763.36)	6846.41 (4464.09)	6821.11 (4701.83)	5372.5 (4427.00)	5516.69 (4335.10)
mmi_group = High × post	7398.89* (3065.04)	7411.86* (3286.86)	7895.02* (3432.49)	5755.759 (2898.55)	9957.33 (8324.29)	11747.76 (8831.25)	12670.65 (8243.96)	10918.58 (7457.45)
age	-188.107*** (24.11)	3462.94*** (295.27)	2825.33*** (271.86)	-165.067*** (47.32)	-186.866*** (31.62)	3194.79*** (281.34)	3139.52*** (302.21)	3132.37*** (290.16)
household size	14766.51*** (704.69)	13960.68*** (718.84)	13960.68*** (718.84)	12803.28*** (833.67)	15470.20*** (564.79)	13692.62*** (619.66)	4062.07*** (924.68)	13675.93*** (594.61)
age ²		-34.60*** (2.97)	-30.756*** (2.90)			-34.54*** (3.10)	-33.64*** (3.11)	-33.97*** (3.16)
sex		-11691.51*** (944.59)	-11689.34*** (959.81)			-11041.91*** (677.67)	-5772.921*** (854.51)	-11047.16*** (623.82)
Rented (reference category: Owned/Trust)			-37117.03*** (3431.99)			-34547.85*** (3184.62)	-27974.08*** (3001.50)	-34825.30*** (3040.81)
hh_comp:One-family household							-8885.78** (3077.83)	
hh_comp:One-person household							-47288.630*** (3066.75)	
hh_comp:Other multi-person household							-7764.16 (4921.72)	
hh_comp:Other or sparse composition							-12867.02 (10515.28)	
hh_comp:Three-or-more-family household							-48911.52*** (3376.42)	
hh_comp:Two-family household							-1194.05 (4178.82)	
Num.Obs.	17193	17193	17193	30384	30384	30384	30384	30384
R2	0.12	0.15	0.18	0.11	0.12	0.17	0.21	0.17
R2 Adj.	0.12	0.14	0.17	0.11	0.12	0.17	0.2	0.17
R2 Within	0.08	0.11	0.14	0.06	0.08	0.13	0.17	0.13
R2 Within Adj.	0.08	0.11	0.14	0.06	0.08	0.13	0.17	0.13
Std.Errors	by: TA code	by: TA code	by: TA code	by: TA code	by: TA code	by: TA code	by: TA code	by: TA code

* p < 0.05, ** p < 0.01, *** p < 0.001

Table I.2. ATT-IPW bridge specifications for total household income

	A. PSM with basic controls	B. PSM with main controls	C. PSM + richer controls	D. IPW full pool unweighted	E. IPW full pool, ATT-IPW	F. IPW full pool, ATT-IPW + richer controls	G. IPW full pool, ATT-IPW + composition control	H. IPW full pool, trimmed ATT-IPW + richer controls
mmi_group = Low × post	5899.80* (2938.53)	7043.47* (2889.43)	3498.53 (2693.56)	4418.52 (2921.07)	6734.3 (4465.03)	6732.74 (4741.51)	5234.9 (4428.02)	5183.1 (4343.21)
mmi_group = High × post	7210.27* (3312.97)	7251.79* (3559.44)	7747.01* (3710.14)	5806.342 (3128.71)	10353.18 (8900.27)	12248.17 (9444.53)	13211.16 (8834.39)	11300.72 (7986.80)
age	-181.299*** (23.85)	3810.44*** (301.28)	3139.60*** (286.54)	-152.085** (47.35)	-176.759*** (32.18)	3521.65*** (300.32)	3458.83*** (331.21)	3470.66*** (307.49)
household size	14965.20*** (733.90)	14074.52*** (740.65)	13352.17*** (720.08)	12995.95*** (870.46)	15813.16*** (616.96)	13894.59*** (658.34)	3981.21*** (1029.99)	13864.59*** (641.63)
age ²		-37.83*** (3.03)	-33.78*** (3.02)			-37.68*** (3.27)	-36.68*** (3.35)	-37.22*** (3.31)
sex		-11894.36*** (991.53)	-11894.36*** (991.53)			-11361.77*** (821.14)	-5941.29*** (950.53)	-11359.47*** (778.92)
Rented (reference)						-36428.69*** (3250.95)	-29610.07*** (3102.25)	-36787.37*** (3136.11)
Owned/Trust								
hh_comp:One-family household							-8135.41* (3773.55)	
hh_comp:One-person household							-48169.41*** (3868.33)	
hh_comp:Other multi-person household							-6972.53 (5209.46)	
hh_comp:Other or sparse composition							-13522.24 (9749.48)	
hh_comp:Three-or-more-family household							-48924.73*** (4050.59)	
hh_comp:Two-family household							-434.27 (4827.00)	
Num.Obs.	17193	17193	17193	30384	30384	30384	30384	30384
R2	0.11	0.14	0.17	0.1	0.11	0.16	0.19	0.16
R2 Adj.	0.11	0.14	0.16	0.1	0.11	0.16	0.19	0.16
R2 Within	0.07	0.1	0.13	0.06	0.07	0.12	0.16	0.12
R2 Within Adj.	0.07	0.1	0.13	0.06	0.07	0.12	0.16	0.12
Std.Errors	by: TA code	by: TA code	by: TA code	by: TA code	by: TA code	by: TA code	by: TA code	by: TA code

* p < 0.05, ** p < 0.01, *** p < 0.001

Table I.3. ATT-IPW bridge specifications for total household expenditure

	A. PSM with basic controls	B. PSM with main controls	C. PSM richer controls	D. IPW pool unweighted	E. IPW pool, ATT-IPW	F. IPW full pool, ATT-IPW richer controls	G. IPW full pool, ATT-IPW composition control	H. IPW full pool, trimmed ATT-IPW + richer controls
mmi_group = Low × post	3287.32 (2326.64)	3413.71 (2507.03)	2613.99 (2471.88)	2703.8 (2234.89)	1159.57 (1821.58)	693.99 (1900.64)	526 (1845.68)	1366.04 (2040.79)
mmi_group = High × post	1806.18 (1414.29)	2010.89 (1469.80)	2111.11 (1462.83)	1194.03 (1330.82)	-644.640 (1203.53)	265.85 (1226.89)	602.56 (1218.57)	682.18 (1242.00)
age	-238.211*** (21.37)	1170.95*** (100.93)	1055.66*** (107.00)	-225.629*** (28.85)	-229.062*** (25.47)	1184.77*** (144.16)	1172.51*** (175.01)	1164.73*** (133.09)
household size	7137.82*** (590.43)	6805.15*** (594.64)	6686.32*** (593.34)	6247.51*** (526.25)	7851.63*** (715.12)	7289.10*** (712.51)	4318.29*** (551.14)	7175.53*** (696.81)
age ²		-13.32*** (0.97)	-12.66*** (0.99)			-13.89*** (1.21)	-13.79*** (1.49)	-13.70*** (1.12)
sex		-3388.78*** (768.82)	-3366.05*** (779.15)			-3364.48*** (714.60)	-1255.97* (538.00)	-3438.09*** (682.75)
Rented (reference category: Owned/Trust)			-9521.24*** (1185.35)			-8881.61*** (1130.73)	-6387.90*** (1099.68)	-8857.47*** (1182.48)
hh_comp:One-family household							969.14 (2571.16)	
hh_comp:One-person household							-13436.20*** (2013.80)	
hh_comp:Other multi-person household							-2672.37 (1350.09)	
hh_comp:Three-or-more-family household							-17652.56*** (2755.51)	
hh_comp:Two-family household							1217.42 (1792.02)	
Num.Obs.	5022	5022	5022	8406	8406	8406	8406	8406
R2	0.34	0.36	0.36	0.33	0.36	0.38	0.41	0.39
R2 Adj.	0.33	0.35	0.36	0.33	0.35	0.38	0.41	0.38
R2 Within	0.13	0.15	0.16	0.11	0.15	0.19	0.22	0.18
R2 Within Adj.	0.13	0.15	0.16	0.11	0.15	0.19	0.22	0.18
Std.Errors	by: TA code	by: TA code	by: TA code	by: TA code	by: TA code	by: TA code	by: TA code	by: TA code

* p < 0.05, ** p < 0.01, *** p < 0.001

Code and data availability

The data used in this study are not publicly available due to access restrictions and confidentiality requirements. The primary microdata are from Stats NZ's Household Economic Survey (HES) and were accessed within the Stats NZ Integrated Data Infrastructure (IDI) under an approved project MAA2024-54 in accordance with the *Data and Statistics Act 2022* and Stats NZ microdata access protocols.

Researchers who meet the eligibility criteria may apply to access these data via Stats NZ's Data Lab and the IDI. Access procedures and conditions are available from Stats NZ.

To support reproducibility, the empirical code (data cleaning scripts, variable construction, and estimation routines) and documentation sufficient to replicate the reported tables and figures will be made available in a GitHub upon acceptance. If an embargo is required, the repository will provide time-stamped releases and version tags, and the materials can be shared with reviewers through the Copernicus "access limited to reviewers" mechanism during peer review.

Supplement link

Not applicable at this stage.

Team list

Not applicable.

Author contributions

Quanfu Zhang: Conceptualization; Data curation; Formal analysis; Methodology; Software; Visualization; Writing – original draft.

Ilan Noy: Conceptualization; Methodology; Supervision; Writing – review & editing.

Yigit Saglam: Methodology; Validation; Writing – review & editing.

Competing interests

The authors declare that they have no competing interests.

Disclaimer

Disclaimer for output produced from Stats NZ surveys

Access to the data used in this study was provided by Stats NZ under conditions designed to give effect to the security and confidentiality provisions of the *Data and Statistics Act 2022*.

The results presented in this study are the work of the author, not Stats NZ or individual data suppliers.

Disclaimer for output produced from the IDI

These results are not official statistics. They have been created for research purposes from the Integrated Data Infrastructure (IDI) which is carefully managed by Stats NZ. For more information about the IDI please visit <https://www.stats.govt.nz/integrated-data/>

Acknowledgements

We thank Stats NZ for providing access to the data used in this study and for the support of the Data Lab research environment. We are grateful to colleagues and seminar participants for helpful comments and suggestions. Any remaining errors are our own.

Financial support

Quanfu Zhang acknowledges financial support from the Ministry of Business, Innovation and Employment PhD scholarship.