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Disaster Exposure and Household Financial Disruption

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Keywords: disaster, debt, hardship, household finance

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Disaster Exposure and Household Financial Disruption*

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Abstract

How do households finance a costly, sudden shock? We study this question using natural disasters as a source of exogenous variation. Exploiting 15 waves of Australian longitudinal data and individual fixed-effects models, we estimate the effects of direct disaster exposure on household expenditure, income, assets, liabilities, and financial hardship, including coping margins that are typically unobserved in administrative data. Disaster exposure increases expenditure, driven by repair-related spending, while income and assets remain largely unchanged. The main adjustment occurs through significant increases in personal debt and informal financing. For many households, these responses are insufficient to avoid material deprivation, with increases in the likelihood of going without meals, missing utility payments, and being unable to heat the home. These effects are larger among households without insurance coverage and are concentrated in the year of the shock.

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1 Introduction

Natural disasters are an increasingly important source of household financial risk (Gallagher and Hartley 2017; Billings et al. 2022). Such events can trigger large, unexpected expenses that cannot easily be deferred, while simultaneously disrupting employment and damaging income-generating assets (Deryugina et al. 2018). These disruptions are especially severe for individuals who are uninsured, underinsured, or face limited access to affordable credit (Kousky, 2019). Yet the mechanisms by which households bridge this financing gap, and whether they can do so without falling into hardship, remain poorly understood, in part because administrative and credit bureau data captures behaviour in formal credit markets, but miss the informal borrowing, asset sales, and material deprivation that matter for households at the financial margin. Understanding how households adjust, which financial resources provide protection, and how long any strain persists are central questions for the design of disaster insurance, public safety nets, and access to affordable credit (Ferreira, 2024; You and Kousky, 2024; Collier et al., 2024).

What makes disasters particularly difficult to finance is that they impose a large, non-deferrable expenditure shock when common channels of adjustment (e.g. insurance payouts, income growth, government transfers) have not yet arrived (Agarwal et al., 2024). Even a household that is solvent in a lifetime sense may face a binding liquidity constraint, as it cannot borrow against future reimbursements, nor quickly draw on illiquid assets such as housing equity or retirement savings (Zeldes 1989; Hurst and Stafford 2004). When formal credit is limited by collateral or creditworthiness, households are pushed toward forced asset sales or high-cost borrowing, such as credit cards, payday loans, and informal loans from family (Morse, 2011). For households with few liquid assets and no insurance, these adjustment margins may be exhausted before the shock is fully absorbed, translating into material deprivation (Leete and Bania 2010; Dobkin et al. 2018).

In this paper, we examine how households adjust financially following direct exposure to a natural disaster. Using 15 waves of longitudinal survey data from Australia, we estimate the effects of disaster damage to the home on expenditures, income, assets, liabilities, informal financing and material deprivation. Our focus on direct household damage is important. Much of the existing literature assigns disaster exposure at the area level, while our measure isolates households that report home damage or destruction, and are therefore most likely to face the immediate financial pressures that are central to the household finance problem studied here.

Our results show that disaster exposure generates a sharp increase in expenditure that are not offset by income or liquid asset buffers. We find that disaster exposure leads to a significant increase in total household expenditure, driven primarily by direct disaster-related costs, including a 21% rise in home repairs, a 6% increase in motor vehicle repairs, and a 4% increase in insurance premiums. These elevated expenditures are not offset by changes in income or asset holdings, including equities.[1] More notably, disaster exposure leads to a 84% increase in the amount of personal debt. This category includes personal, car and hire-purchase loans, and is often associated with high interest rates. Households also respond by pawning assets (30% increase) and by seeking financial help from friends or family (19% increase).

For some households, these financial responses - reliance on high-cost credit, informal transfers, and forced asset sales - are insufficient to prevent material deprivation. We find that the likelihood of going without meals increases by 39%, unpaid utility bills by 13%, and inability to heat the home by 42%. These patterns highlight the severe downstream consequences of disaster-related financial strain, even in a high-income setting with access to social safety nets.

Effects are particularly pronounced among individuals without home and contents insurance. This is consistent with insurance reducing the size of the immediate out-of-pocket shock, and lowering the need to rely on high-cost borrowing or to cut essential spending in the aftermath of damage. We also show that financial impacts are driven primarily by direct household damage rather than broader local economic disruption, highlighting the importance of modelling disasters as household-level shocks rather than regional economic shocks.

This set of findings contribute to a literature that has used administrative or credit bureau data to examine outcomes such as debt accumulation and mortgage delinquency (Gallagher and Hartley, 2017; Billings et al., 2022; Fu and Tao, 2022; Ho et al., 2023; Del Valle et al., 2024).[2] For example, Gallagher and Hartley (2017) use credit bureau data to show that flood exposure from Hurricane Katrina increased credit card borrowing in the short term and led to long-run reductions in total debt. These studies provide important insights into behaviour in formal credit markets, but the administrative and credit bureau records do not include information on informal borrowing, asset liquidation, reliance on family networks, or the downstream deprivation outcomes that arise when formal credit is unavailable or exhausted. Survey data of the kind used here can better capture the full chain of financial adjustment, from initial expenditure responses through to material deprivation.[3]

The two studies most similar to ours in approach are Johar et al. (2022) and Sachdeva et al. (2024). Johar et al. (2022) use earlier waves of the same Australian survey to

[1] Bharath and Cho (2023) find using U.S. survey data that natural disaster exposure reduces participation in the risky asset market and the fraction of wealth in risky assets. We do not find similar evidence in this study.

[2] A similar literature explores the financial resilience of businesses following disasters, rather than households. Recent studies include: Collier et al. (2025), Cardella et al. (2025), Marshall (2025).

[3] Financial institutions have also been shown to adjust following disasters. Schüwer et al. (2019) examine how banks adjusted capital ratios and lending after Hurricane Katrina, while Cortés and Strahan (2017) use natural disasters as shocks to local credit demand and show how multi-market banks transmit those shocks across connected credit markets.

show that disasters significantly increase financial hardship and risk aversion, but do not examine the margins through which this occurs. In particular, the paper does not use the survey's wealth module and so provides no evidence on debt accumulation or asset drawdown, nor does it examine expenditure responses in detail or the role of insurance in moderating financial outcomes.[4] Sachdeva et al. (2024) also use household panel survey data to examine disaster impacts, but focus on formal credit and asset balances rather than the broader set of outcomes examined here.

Our findings also contribute to the literature on disaster insurance. Empirical studies have shown that insured households experience better post-disaster outcomes, including increased likelihood of rebuilding, lower financial distress, faster recovery, and reduced reliance on public assistance (Kousky, 2019). Yet in practice, coverage is often incomplete or unaffordable. Recent research highlights several contributing factors. Wagner (2022) shows that even in high-risk flood zones, willingness to pay for coverage is low, and that insurance markets are adversely selected. Keys and Mulder (2024) document a sharp increase in homeowners insurance premiums in recent years, with higher costs disproportionately affecting climate-exposed areas due to pass-through of reinsurance prices. Hu (2022) finds that flood insurance take-up is influenced by social networks and peer exposure, underscoring the role of informational frictions in insurance decisions. Together, these findings highlight the growing barriers to insurance access in disaster-prone regions.[5]

The rest of the paper proceeds as follows. Section 2 presents a brief conceptual framework that organises the empirical analysis and generates testable predictions. Section 3 describes the data. Section 4 outlines the empirical strategy. Section 5 presents the main results. Section 6 discusses robustness and heterogeneity analyses. Section 7 provides concluding remarks.

2 Conceptual framework for household financial adjustment

In this section, we outline a simple framework that organises the empirical analysis and clarifies the predictions being tested. Consider a household observed in period t, whose flow budget constraint is given by:

[4] Rather than examining the margins of financial adjustment, Johar et al. (2022) focus on heterogeneity in vulnerability to disaster exposure, using a Group Fixed Effects estimator to identify latent vulnerability types. They find that effects are more pronounced among older households, parents, those in poor health, and those with limited social support.

[5] Our study also relates to a broader literature examining how major household shocks, such as job loss, serious illness, or death, affect financial wellbeing. Prior work has shown that such events can lead to increased debt, depletion of liquid assets, and material hardship, especially when households lack insurance or financial buffers (Crossley and Low, 2014; Dobkin et al., 2018; Andersen et al., 2023). For example, Dobkin et al. (2018) find that hospital admissions lead to higher medical spending and bankruptcy, particularly among the uninsured.

$$C_t = Y_t + \Delta B_t - \Delta A_t + T_t + I_t \tag{1}$$

where C_t denotes total expenditure, Y_t after-tax income, ΔB_t net new borrowing, ΔA_t net asset accumulation, T_t net transfers from government or informal sources, and I_t insurance receipts.

A disaster that damages or destroys the home generates a large, sudden increase in required expenditure, for repairs, temporary accommodation, replacement of damaged durables, and relocation costs (shown in Section 5.1). Let $D_t > 0$ denote this additional expenditure, so realised spending becomes $C_t + D_t$. Because insurance coverage is predetermined, payouts reduce the effective size of the shock from D_t to $D_t - I_t$, thereby lowering the financing gap.

This gap must be closed through one or more adjustment margins: (i) increased income Y_t (for example, through additional work), (ii) new borrowing $\Delta B_t > 0$, either formally (e.g. personal loans) or informally (e.g. loans from family), (iii) asset liquidation $\Delta A_t < 0$, such as drawing down savings, and (iv) transfers $T_t > 0$, including assistance from welfare organisations, community networks, or government programs. Because disasters are large, sudden, and not easily anticipated, the additional spending $C_t + D_t - I_t$ is unlikely to be financed primarily through contemporaneous changes in income. Adjustment is therefore expected to occur mainly through borrowing, asset drawdown, and transfers. Empirically, this implies increases in ΔB_t and T_t and, potentially, reductions in A_t, depending on households' initial financial positions. Sections 5.2 to 5.4 investigate each of these margins.

An important asymmetry arises when households face borrowing constraints. Suppose borrowing is capped at $\bar{B}_i$, reflecting collateral, credit history, and lender assessments of repayment capacity. In the short run, households can draw on liquid assets $L_t \subseteq A_t$, such as bank account balances, but cannot readily liquidate illiquid holdings such as housing equity or superannuation. When the net shock exceeds these immediately available resources, that is, when $D_t - I_t > L_t + \bar{B}_i$, the constraint binds and the household cannot smooth consumption. Non-disaster-related spending must then fall, generating hardship outcomes such as unpaid bills, missed meals, or inability to heat the home (explored in Section 5.4).

Insurance, liquid assets, and tenure status shape how households adjust. Insurance reduces the effective shock $D_t - I_t$, lowering the likelihood of borrowing, asset liquidation, and hardship. Differences by home-ownership status are more complicated. Homeowners are directly responsible for repair and rebuilding costs, implying a larger immediate expenditure shock, whereas renters may face lower repair costs but higher relocation risk and associated expenses. These factors generate systematic heterogeneity in financial responses, which we examine in Section 6.2.

3 Data

3.1 Data source and sample

This study uses data from the Household, Income and Labour Dynamics in Australia (HILDA) Survey, a nationally representative longitudinal survey that has followed Australian households annually since 2001.[6] All individuals aged 15 years and over are interviewed each year on a wide range of topics. Most relevant for our purposes is the detailed information HILDA collects on income, expenditures, and financial hardship on an annual basis, together with comprehensive wealth modules covering both assets and liabilities at four-year intervals. In addition, and unusually for a household panel survey, HILDA asks respondents whether their home was damaged or destroyed by a natural disaster, enabling direct measurement of personal disaster exposure rather than reliance on regional proxies.

Our analysis uses Waves 9–23 (2009–2023), the period over which the disaster exposure measure is available, and focuses on respondents aged 15–80 living in private dwellings. We restrict attention to individuals observed in at least two waves, consistent with our fixed-effects framework, and we require non-missing information on the relevant outcome and the disaster exposure measure in each analysis.

Estimation samples are constructed at the individual–year level and vary by outcome because financial variables are measured at different frequencies in HILDA. Expenditure, income, and financial hardship outcomes are observed annually and are therefore available in each wave, while asset and debt outcomes are drawn from the four-yearly wealth modules and consequently yield fewer observations. As a result, sample sizes range from 33,352 individuals and 306,300 observations for annually measured outcomes to 26,069 individuals and 68,300 observations for wealth-related outcomes.

Note, all the financial variables described below are self-reported rather than drawn from administrative or credit bureau records. While self-reported data may be subject to recall error or misreporting, HILDA incorporates several design features to improve accuracy. Respondents are encouraged to consult financial documents when completing financial questions, and for the wealth modules, interviewers are instructed to identify and interview the household member best placed to answer questions. These features are likely to reduce, though not eliminate, measurement error relative to a standard survey instrument. Despite this limitation, self-reported panel data captures important outcomes (e.g. informal borrowing, asset sales, and material hardship) that administrative records cannot, making it the appropriate dataset for the research questions examined here.

[6] The initial sample comprised 7,682 households and 13,969 individuals. The sample expands over time as children of original sample members enter the survey upon turning 15, and as new adults join households containing original sample members. To maintain representativeness, a major top-up sample of 2,153 households and 5,462 individuals was added in 2011.

3.2 Disaster exposure

Our key explanatory variable is an indicator equal to one if an individual reports that their home was damaged or destroyed by a weather-related disaster (such as floods, bushfires, or cyclones) in the twelve months preceding the interview. In the estimation sample, 1.6% of individual-year observations report direct disaster exposure (see Appendix Table A1). While this incidence is relatively low, it reflects the fact that the measure captures personal exposure to a major disaster shock.

This approach differs from much of the existing literature, which typically assigns disaster exposure based on whether a respondent resides in a disaster-affected area. Geographic measures may capture a wide range of exposure intensities, including many households that experience limited or no direct losses. By contrast, our measure isolates households that face immediate repair and replacement costs, insurance claims, and potential displacement, making it well suited for analysing household financial adjustment following large, sudden financial shocks.

3.3 Financial outcomes

We examine a broad set of financial outcomes that capture household financial adjustment and downstream financial stress following disaster exposure. Financial variables are measured at the household level and assigned to individuals within the household on a per-capita basis.[7] Summary statistics for the main variables and variable definitions are provided in Appendix A.

Expenditure is measured using annual household spending across a set of regularly reported consumption and service categories, including groceries, alcohol and tobacco, public transport and fuel, meals eaten out, clothing and footwear, telephone and internet, electricity and gas, education fees, healthcare and health insurance, other insurance, and home and motor vehicle repairs and maintenance. All expenditure items are adjusted for inflation and converted to per-capita annual values using household size, with total annual expenditure having a mean of $18,200. While these expenditure items capture routine consumption and repair spending, they do not represent a comprehensive measure of household expenditure, as several items that may arise following disasters are omitted, such as relocation costs and replacement of major durables.

Income is measured as annual per-capita after-tax regular income, constructed by aggregating labour earnings, business income, investment income, private pensions, regular private transfers, and Australian government transfers reported at the household level.

[7] Individual-level outcomes reduce sensitivity to changes in household composition that mechanically affect household-level financial aggregates. In a panel setting with individual fixed effects, such composition changes could otherwise be misattributed to disaster exposure. We therefore assign household-level financial variables to individuals on a per-capita basis. Results using household-level expenditure are similar and discussed in the Results section.

All income components are adjusted for inflation and converted to per-capita annual values using household size. Mean annual per-capita income in the sample is $50,267. In addition to regular income, we separately examine irregular income (e.g. inheritances and lump-sum transfers from family or friends), which can provide short-term financial support.

Asset holdings and liabilities are drawn from the HILDA wealth modules, which are administered every four years and collect detailed information on household balance-sheet components. Total assets are constructed as the sum of financial assets, including bank accounts, equities, retirement savings (superannuation), and trusts, and non-financial assets, including owner-occupied housing, other property, vehicles, businesses, and collectibles. Total debt is measured as the sum of property debt, business debt, unpaid credit card balances, and personal debt (e.g. personal, car and hire purchase loans). All asset and debt measures are adjusted for inflation and expressed in per-capita terms. Mean per-capita asset holdings in the sample are $606,553, while mean per-capita debt is $99,660.

To capture downstream financial stress, we examine seven binary indicators drawn from a HILDA question asking whether, due to a shortage of money, any of the following occurred in the past year: inability to pay electricity, gas, or telephone bills on time; inability to pay the mortgage or rent on time; pawning or selling something; going without meals; being unable to heat the home; asking for financial help from friends or family; and asking for help from welfare or community organisations. We group these into two categories. The first captures informal financing that provides short-term liquidity: pawning possessions, seeking help from family, and accessing welfare organisations. The second captures direct material deprivation: unpaid utility bills, going without meals, inability to heat the home, and difficulty meeting housing payments.

4 Methodology

4.1 Empirical specification and identification

Our empirical strategy is designed to capture how disaster exposure affects household financial behaviour along two distinct margins: whether individuals engage in a given financial activity at all (the extensive margin), and the intensity of that activity (the intensive margin). This distinction is important because natural disasters may alter not only the level of financial outcomes, such as expenditure or debt, but also the likelihood that households rely on particular financial channels in the first place.

Accordingly, for each financial domain, we estimate models for both (i) binary indicators capturing whether the individual has any expenditure, income, asset holding, debt, or hardship outcome in a given category; and (ii) the level of the corresponding outcome, measured in dollars where applicable. Both sets of outcomes are analysed within the same fixed-effects panel framework and rely on the same identifying variation.

Formally, we model the conditional mean of the outcome as a function of disaster exposure and a set of fixed effects:

$$E\left[Y_{iast} \mid \cdot\right] = g\left(\beta_0 + \beta_1 D_{iast} + X'_{iast}\delta + \lambda_i + \lambda_a + \lambda_{st}\right) \tag{2}$$

where Y_{iast} denotes a financial outcome for individual i, residing in local government area (LGA) a within state s, observed in year t. Outcomes include expenditure, income, assets, liabilities, and indicators of financial hardship. D_{iast} equals one if the individual reports that their home was damaged or destroyed by a weather-related disaster in the twelve months preceding the interview, and zero otherwise. This definition targets the specific event that (in the framework of Section 2) generates non-deferrable expenditure and consequently a financing gap. By focusing on households that report actual home damage, rather than residence in a disaster-affected area, D_{iast} identifies the subset of households facing the acute liquidity pressure the framework predicts.

The function $g(.)$ depends on the nature of the outcome variable. For outcomes measured in dollars, such as expenditure amounts, asset values, and debt balances, we specify $g(.)$ as the exponential function and estimate the model using Poisson pseudo–maximum likelihood (PPML). This estimator accommodates the skewed distribution and yields consistent estimates under correct specification of the conditional mean. Coefficient estimates from these models are interpreted as semi-elasticities, capturing the percentage change in the outcome associated with disaster exposure. For binary outcomes capturing the extensive margin, such as whether an individual incurs any expenditure in a given category, holds any personal debt, or experiences a specific form of financial hardship, we estimate linear probability models.

The vector X'_{iast} includes a parsimonious set of time-varying controls, namely age and age squared. We deliberately avoid conditioning on variables such as income, employment status, or household composition, which may themselves respond to disaster exposure. Individual fixed effects (λ_i) absorb all time-invariant individual characteristics, including baseline wealth, housing quality, risk preferences, and long-run location choices. Local government area fixed effects (λ_a) control for persistent spatial differences in economic conditions and disaster risk. State-by-year fixed effects (λ_{st}) flexibly absorb aggregate shocks, policy changes, and macroeconomic conditions common to all individuals within a state in a given year. Standard errors are clustered at the individual level to allow for serial correlation in unobserved shocks over time.[8]

Identification relies on within-individual changes in disaster exposure over time. Specif-

[8] We also present results using standard errors clustered at the area level to allow for spatially correlated shocks. These are very similar to those clustered at the individual level.

ically, we compare financial outcomes for the same individual in the year following damage or destruction to their home, relative to outcomes for the same individual in other years and to contemporaneous changes for individuals who are not exposed in that period. Because the disaster indicator is defined over the twelve months preceding the interview, the treatment effects represent short-run financial response to disaster exposure, rather than longer-run adjustment. Extensions examining dynamic effects beyond the initial exposure period are discussed in later sections.

Although the empirical design shares features with difference-in-differences, disaster exposure in our setting is a rare, individual-level, time-specific shock rather than an absorbing treatment state. Individuals are not permanently "treated," and exposure does not define a lasting post-treatment regime. As a result, the estimates do not rely on comparisons between earlier- and later-treated units that characterize staggered-adoption settings in two-way fixed-effects models. They are therefore less exposed to the weighting and treatment-timing concerns highlighted in recent critiques of that framework.[9]

4.2 Identification and Robustness

The key identifying assumption is that, within individual and area, the timing of disaster exposure is uncorrelated with unobserved determinants of financial outcomes. The main threat to this assumption is selection: households more likely to experience future damage may differ systematically in their pre-existing financial trajectories. Although disasters are plausibly exogenous events, we assess this possibility directly by estimating models in which future disaster exposure D_{iast} is regressed on a set of individual demographic, economic, health, and social characteristics observed in period t (see Appendix Table A2). Across both samples, individual characteristics are jointly and individually insignificant at the 5% level, providing little evidence that time-varying personal circumstances predict subsequent exposure. Importantly, the same characteristics strongly predict subsequent residential relocation, confirming that the null result for disaster exposure reflects genuine exogeneity rather than insufficient statistical power or measurement error in the covariates (see Appendix Table A3)

We assess the robustness of the main results to four additional concerns, with results reported in Section 5.5. First, residential mobility may confound estimates if individuals relocate across areas with different disaster risk profiles in response to unobserved shocks. To address this, we estimate specifications that include individual–area fixed effects, so that identification comes from variation in disaster exposure within an individual's current location. Second, local economic conditions or disaster preparedness may evolve differently

[9]We assessed the proportion of treatment–control comparisons receiving negative weights following the decomposition approaches in de Chaisemartin and D'Haultfoeuille (2020) and Goodman-Bacon (2018). As expected, these were negligible (less than 1%), indicating that the estimates are not materially affected by negative-weighting concerns.

across areas over time. We supplement the baseline specification with area-specific linear time trends to account for this possibility. Third, non-random attrition is a concern in long-running household panels if disaster exposure affects survey participation. We construct inverse-probability weights based on a model of continued panel participation and re-estimate the main specifications using these weights. Finally, we assess sensitivity to functional form by re-estimating the main models using OLS in place of Poisson pseudo-maximum likelihood. Across all four robustness checks, the central results remain stable.

4.3 Extensions

We conduct three extension analyses, reported in Section 5, that explore the mechanisms, heterogeneity, and timing of the estimated effects. First, we examine whether the financial effects of disaster exposure operate through direct household damage or broader regional economic disruption. Even households that do not experience direct damage may be indirectly affected by disasters occurring in their area, such as through local labor market disruption, price effects, or reduced access to services. To separate these channels, we estimate specifications that include both the individual-level disaster indicator and measures of area-level disaster incidence constructed from administrative records. This allows us to assess whether the estimated financial effects are driven by the household-level financing gap or by wider local economic conditions.

Second, we examine heterogeneity in financial responses by insurance status. Property insurance is designed to limit the out-of-pocket shock facing affected households, reducing the net financing gap from D_t to $D_t - I_t$ as outlined in Section 2, and thereby lowering the need to rely on borrowing or to cut consumption. We estimate separate effects for households with and without home and contents insurance, allowing us to assess whether insurance moderates the financial consequences of disaster exposure in the way the framework predicts.

Third, we examine the dynamic profile of financial responses by augmenting the baseline specification with leads and lags of the disaster indicator, spanning two years prior to exposure and three years afterwards. This event-time framework serves two purposes: it provides a formal test for pre-trends, assessing whether households on different financial trajectories are differentially likely to experience future exposure, and it allows us to evaluate whether the financial effects of disasters are transitory, concentrated in the year of the shock, or persist into subsequent periods, as would be the case if accumulated debt generates lasting repayment burdens.

5 Main Results

5.1 Expenditure

We begin by examining how disaster exposure affects measured annual expenditure, both in aggregate and across specific spending categories. Table 1 reports estimates for both whether individuals report any spending in a given category and total dollars spent. The HILDA expenditure module captures routine spending but omits several large, infrequent items that may arise following disasters. The expenditure results should therefore be interpreted alongside the debt and hardship findings that follow.

We first consider total annual expenditure. Because all individuals report some positive level of overall spending, only the intensive margin is relevant for this outcome. Disaster exposure is associated with a statistically significant 3.46 per cent increase in total annual expenditure. Evaluated at the sample mean of $18,200, this corresponds to an increase of approximately $630 in annual spending in the year following home damage or destruction.

Turning to disaggregated expenditure categories, the most notable response is a substantial increase in home repair expenditure along both margins. The probability of reporting any home repair spending rises by 3.2 percentage points relative to a baseline of 64 per cent, while conditional spending increases by approximately 21 per cent. Motor vehicle repair expenditure also increases, rising by around 6 per cent.

Although repair expenditure rises following disaster exposure, the magnitudes are modest relative to the scale of physical damage that some households experience. Two institutional features explain this pattern. First, renters are typically not responsible for financing repairs to their dwellings, with costs generally borne by landlords. Consistent with this mechanism, we find that disaster exposure is associated with a statistically significant rise in home repair expenditure for homeowners, but not for renters. Second, insurance coverage may substantially offset out-of-pocket repair costs. Households with comprehensive home or vehicle insurance are likely to finance a large share of repair and replacement expenditure through insurance claims rather than direct spending. We examine heterogeneity by insurance coverage in a later section.

Beyond repair costs, most routine consumption categories show little response. We find no statistically meaningful changes in spending on groceries, meals eaten outside the home, alcohol, clothing, or medical expenses at either margin. A notable exception is tobacco expenditure, which increases along both margins. Disaster exposure raises the probability of reporting any tobacco spending by around 1.6 percentage points (approximately 7 per cent relative to the mean) and increases spending levels by about 8 per cent. This pattern is consistent with evidence that exposure to stressful events can affect consumption of tobacco products. Another exception is public transport expenditure, which also modestly increases at both margins. One possible interpretation is that temporary displacement or vehicle damage increases reliance on alternative transport in the short run.

We also examine whether households adjust insurance-related expenditures following disaster exposure. Spending on home and motor vehicle insurance increases at the intensive margin, while the likelihood of holding these forms of insurance changes little. This suggests that disasters primarily affect insurance expenditures through higher premiums or adjustments to coverage among already insured households, rather than through large changes in insurance take-up. This finding differs from results in past studies (e.g. Kousky (2017)) that show increases in insurance demand post-disaster.

The expenditure measures available in HILDA capture routine consumption and repair spending but omit several large, infrequent items that commonly arise following disasters, such as relocation costs, replacement of major durables, and new building expenses. Disaster exposure increases the probability of residential relocation by approximately 10 percentage points (see Appendix Table A4), implying that relocation-related costs alone are likely to be economically meaningful. These unmeasured expenditures suggest that the observed 3.5 per cent increase in recorded spending understates the full financial burden.[10]

5.2 Income

We next examine whether disaster exposure affects individual income. Table 2 reports results for both the likelihood of receiving income from each source and the level of income received. The first row presents estimates for total income, followed by results for specific income components.

The level regression for total income in Table 2 shows no statistically meaningful effect of disaster exposure, providing little evidence of short-run income disruption following direct damage to the home.[11] We then disaggregate total income into its components: salary and wages, business income, investment income, pension income, private transfers, and government transfers. Across these categories, the estimates are similarly small and statistically insignificant, in both probability and level specifications.[12] In particular, we find no detectable changes in labour income, which is the dominant source in the sample (sample mean of $45,762).

The only systematic response appears for irregular income. Disaster exposure increases

[10]Given disaster exposure is relatively rare in the sample, we conduct a permutation-based randomization check using an approach similar to Conley and Taber (2011), assigning placebo treatment to untreated individuals within the same state and re-estimating the models across 500 iterations. The distribution of placebo estimates is centered near zero, and the observed effect for total expenditure lies outside this distribution, indicating that it is unlikely to arise by chance (Figure A1).

[11]All individuals receive income from at least one source and we therefore focus on how disaster exposure affects the level of total income and not the likelihood of receiving income.

[12]The statistically insignificant effect on government transfers, which includes income support payments, non-income support benefits, and other public transfers, is somewhat surprising given the role of government in disaster response. It is possible that targeted disaster assistance is not reported. In addition, some households may experience damage from smaller events that do not trigger eligibility for formal government support.

the probability of receiving irregular income by 2.6 percentage points, relative to a baseline incidence of 8%. Irregular income includes inheritances, severance payments, and transfers from non-household members, among other items (see Appendix A1). However, the amount received does not change significantly. This pattern suggests that disasters may trigger access to one-off financial resources for some households.

The absence of a meaningful income response is itself informative. It indicates that the financing gap created by disaster exposure is not resolved through earnings adjustments: households do not work more, draw on government transfers, or receive larger private transfers in response to damage. The adjustment must therefore occur through other coping margins.

5.3 Assets and Debt

This subsection examines how disaster exposure affects asset holdings and liabilities, with results reported in Table 3. For total assets and debt, we see that mean per-capita holdings are approximately $606,550 and $99,660, respectively, heavily weighted towards housing and other property. Disaster exposure is not significantly related to changes in total assets, but total debt increases at the dollar margin: exposure is associated with a 13.3% rise in debt, corresponding to an increase of roughly $13,255 relative to the mean.

Analyses of specific asset categories generates close to zero, statistically insignificant estimates at both the participation and dollar margins. The absence of systematic asset drawdown reflects the structure of household wealth in the sample, with most holdings concentrated in housing equity and retirement savings, both of which are illiquid and cannot be rapidly converted to cash to meet immediate financing needs. Consistent with this, we find in the next sub-section a significant increase in the likelihood of pawning or selling smaller personal items, a form of asset liquidation that the wealth survey modules do not capture.

The liability side of the balance sheet shows that disaster exposure is strongly related with increases in personal debt, which encompasses car loans, hire purchase agreements, and personal loans from financial institutions. The estimate equals a 84% increase in the dollar amount of debt, which equates to an increase of $5,375 at the sample mean. These forms of borrowing are typically unsecured, relatively quick to access, and well suited to financing short-term unplanned expenses, making them a natural margin of adjustment following disaster-related shocks. However, they often carry high interest rates or penalty fees, making them an expensive form of financing that can exacerbate financial strain in the aftermath of a disaster.

Notably, we find no statistically meaningful response in credit card balances. This result partly reflects a feature of the HILDA measure, which captures unpaid balances remaining after the statement due date rather than credit card spending per se. Households may draw on credit cards to finance disaster-related expenses while still managing to clear their balances on time, in which case no effect would appear in this measure. The aggregate null also masks meaningful heterogeneity that is examined in Section 6.2: among uninsured households, credit card balances rise significantly, suggesting that insured households are better placed to manage balances within the billing cycle. Section 6.2 also discusses a related pattern by tenure status. Homeowners can more readily access secured personal loans and therefore may substitute away from revolving credit, while renters facing the same shock are more likely to rely on credit cards as the most accessible unsecured borrowing channel. Consistent with this, the personal debt effect in Table 3 is driven by homeowners rather than renters.

5.4 Financial Hardship

We group the seven financial hardship indicators into two categories that reflect a sequence of household adjustment: (i) informal financing that provides short-term liquidity - pawning assets, help from family, and help from welfare organisations; and (ii) material deprivation that arise when those coping margins prove insufficient - unable to pay utility bills, unable to pay the mortgage or rent, going without meals, and unable to heat the home. Results are reported in Table 4. Because all outcomes are binary, we estimate linear probability models within the same fixed-effects framework.

We begin with informal financing. In the estimation sample, 5.7% of individuals report pawning or selling belongings, 12.4% report asking friends or family for financial help, and 4.1% report seeking assistance from welfare or community organisations. Disaster exposure increases reliance on all three margins. The probability of pawning or selling belongings rises by 1.7 percentage points (30% relative to the mean), asking friends or family for help rises by 2.4 percentage points (19%), and reliance on welfare organisations rises by 3.1 percentage points (76%). The latter effect is particularly striking and indicates that for a meaningful subset of households, disaster exposure generates financial pressure severe enough to require formal charitable support (due to a shortage of money). These patterns are consistent with the framework prediction that households operating near their borrowing constraint draw on residual adjustment margins when formal credit is insufficient.

When these coping margins prove insufficient, households face direct reductions in consumption of basic necessities. In the baseline sample, 4.1% report going without meals, 6.1% report difficulty paying mortgage or rent on time, 12.0% report difficulty paying utility bills, and 3.3% report being unable to adequately heat their home. Disaster exposure significantly increases several of these outcomes. The probability of going without meals rises by 1.6 percentage points (39% relative to the mean), difficulty paying utility bills increases by 1.6 percentage points (13%), and inability to heat the home rises by 1.4

percentage points (42%). We do not detect a statistically meaningful effect on difficulties paying mortgage or rent on time, consistent with housing payments being prioritized when households face financial strain, or with lender forbearance protecting this margin specifically in the aftermath of a disaster.

5.5 Sensitivity Analyses

In this subsection we examine the sensitivity of the main expenditure, income, assets and personal debt estimates, as well as two composite indicators of informal financing and material deprivation, to alternative specifications and samples (see Appendix Table A5). We first consider three specification-level checks. Panel A reports results from specifications that include individual-area fixed effects, so that identification comes from variation in disaster exposure within an individual's current area of residence. This approach generates estimates that are quantitatively similar to the baseline specification. Panel B augments the baseline model with area-specific linear time trends to control for potential time-varying differences in local conditions. This produces nearly identical estimates. Panel C clusters standard errors at the area level rather than the individual level, leaving statistical inference unchanged.

We next consider sensitivity to potential attrition bias. Panel D reports estimates constructed using inverse-probability weights based on a model of continued panel participation as a function of disaster exposure and base-line characteristics. The weighted estimates are quantitatively similar to the baseline results, suggesting that selective survey dropout is unlikely to drive our findings.

Finally, we assess sensitivity to functional form by re-estimating the main models using OLS rather than Poisson pseudo-maximum likelihood. The main patterns are consistent across all outcomes: total expenditure and repair spending increase following disaster exposure (Appendix Table A6), income remains largely unaffected (Appendix Table A7), and personal debt rises (Appendix Table A8). Taken together, the robustness checks provide no evidence that the main findings are sensitive to specification, sample, or functional form assumptions.

6 Mechanisms, Heterogeneity, and Dynamics

6.1 Direct Damage versus Area-Level Exposure

A central question is whether financial effects operate through direct household damage or through broader regional disruption, such as local labour market dislocation, price effects, or reduced access to services. We address this by estimating specifications that include both individual-level disaster exposure and area-level disaster incidence simultaneously (Table 5). The estimates for direct exposure remain large across the main outcomes: total expenditure increases by 3.8%, total debt rises by 11.7%, personal debt by 74%, and the probability of reporting informal financing and material deprivation rises by 3.0 and 2.0 percentage points, respectively.

In contrast, the coefficients on area-level disaster exposure (measured at the local government level) are small and statistically indistinguishable from zero across all outcomes. The absence of area effects indicates that the financial adjustments documented in the main results are not driven by local economic conditions following disasters, but instead reflect the direct balance-sheet consequences of household-level damage, including the need to repair property, replace durable goods, and manage short-run liquidity pressures.

This distinction matters for the interpretation of estimated effects. Many studies assign disaster exposure based on residential location rather than direct damage, generating estimates that average across directly affected households and unaffected neighbours. The results here suggest that the financial impacts operate primarily through the subset of households experiencing direct damage, rather than through broader local labour market or price effects.

6.2 Heterogeneity by Insurance Coverage

Property insurance is designed to limit the immediate out-of-pocket shock facing affected households and thereby lowering the need to rely on borrowing or to cut essential spending in the aftermath of damage. However, the extent to which insurance delivers on this function is an empirical question. Even among insured households, coverage may be incomplete relative to actual repair costs, payouts may be delayed or disputed, or policies may exclude specific disaster types, meaning that nominally insured households may face financing gaps that are not much smaller than those of uninsured households. We examine this directly by estimating heterogeneous financial responses by baseline insurance status. Information on home and contents insurance is not available in HILDA prior to Wave 14, so we use insurance status measured in that wave as a baseline proxy. This approach identifies differential responses across households with and without insurance rather than the causal effect of insurance itself.

Table 6 reports estimates for the key outcomes examined in the sensitivity and area-level analyses above: total expenditure, home repair spending, total debt, formal personal borrowing, and composite indicators of informal financing and material deprivation. Columns 1–3 present results for households without home and contents insurance, while columns 4–6 report results for insured households.

Disaster exposure leads to larger expenditure responses among uninsured households, consistent with insurance absorbing a portion of repair costs and reducing the size of the immediate out-of-pocket shock. Total expenditure rises by 5.7% for uninsured households compared with 2.3% among insured households. Home repair expenditure is driving this difference: increasing by 82% for uninsured households and by 13% for insured households. Key differences also emerge on the liability side. Total debt increases more strongly among uninsured households (28.5%) than among insured households (13.4%). This difference is driven by credit card debt, which is estimated to rise by 77% (p-value < 0.05) for uninsured households, compared to -8% (p-value > 0.10) for insured households. Financial strain is similarly more pronounced among households without insurance. The probability of informal financing increases by 7.3 percentage points among the uninsured, compared with 1.5 percentage points among insured households, and the probability of material deprivation rises by 4.9 percentage points among the uninsured versus 0.7 percentage points among the insured.

One potential concern is that uninsured households are disproportionately renters, raising the possibility that the insurance heterogeneity reflects a renter–owner divide rather than the protective role of coverage per se: approximately 63% of households without home and contents insurance are renters, compared with 18% of insured households. To assess this, we estimate separate effects by homeownership status (Appendix Table A10). The results suggest that tenure and insurance status capture distinct dimensions of financial vulnerability. On the liability side, formal personal debt rises significantly among homeowners (144.7%, $p < 0.01$) but not among renters, consistent with homeowners bearing direct repair and replacement costs that require financing. On the hardship side, material deprivation is concentrated among renters ([4.1] percentage points, $p < 0.05$), consistent with renters having lower income and savings on average and therefore less capacity to absorb the shock without cutting essential consumption. Together, the results suggest that the insurance heterogeneity documented in Table 6 is not simply a proxy for homeownership status.

Another potential concern is that the results are based on an assumption that insurance status measured in survey wave 14 (2014) is a useful proxy for insurance status in previous waves. As a robustness check, we restrict the sample to Waves 15 onward and re-estimate the models so that insurance status is measured strictly prior to subsequent disaster exposure and financial outcomes. Estimates reported in Appendix Table A9 are less precise due to the reduced within-individual variation, but the same directional patterns emerge, with greater expenditure, debt, and financial hardship among individuals without insurance.

6.3 Dynamics of Financial Responses

In the final sub-section, we assess whether effects emerge prior to exposure, which would indicate selection or anticipation, and to evaluate how long the observed impacts persist after the disaster. We estimate event-time models that include indicators spanning two years prior to exposure and three years afterwards. Figure 1 plots coefficients for six outcomes: total expenditure, total income, total assets, personal debt, informal financing and material deprivation.[13]

Across all outcomes, the estimates show no systematic evidence of pre-trends. Coefficients in the years preceding disaster exposure are close to zero and statistically insignificant, suggesting that households who later experience damage do not exhibit differential financial trajectories prior to the event. This supports the identifying assumption that disaster exposure is not preceded by anticipatory changes in financial behaviour, and is consistent with the plausibly exogenous nature of weather-related damage.

Consistent with the main results, the graphs for income and assets demonstrate flat profiles across the event window, while the immediate post-disaster effects (time zero) for the remaining outcomes are economically meaningful. In the year of exposure (time zero), expenditure rises, personal debt increases sharply, and both hardship indicators deteriorate. In contrast, effects in subsequent years are smaller and statistically indistinguishable from zero. This pattern suggests that households face an acute, short-lived financial shock. Households cut back on essential and non-essential expenditure, borrow funds, and rely on support networks to cover repair, replacement, and relocation costs in the immediate aftermath of the disaster, but these financial pressures do not persist.

The absence of elevated personal debt or hardship in the years following exposure is informative. Expensive short-term borrowing could plausibly generate persistent repayment burdens and a debt spiral, yet we do not observe sustained increases in liabilities or deprivation. One interpretation is that households restore their financial position relatively quickly through insurance payouts, family support, and public assistance. An alternative is that debt repayment is financed through cuts to discretionary spending (e.g. holidays, major durables, or private schooling) that fall outside the HILDA expenditure module and are therefore not captured in the measured aggregate. Distinguishing between these interpretations would require more comprehensive expenditure data than is available here.

7 Conclusion

For many households, natural disasters are primarily a financial shock. When a home is damaged, households face large, unavoidable expenditures alongside their ongoing living costs. Whether they can meet these costs without falling into financial distress depends on their savings, access to credit, insurance coverage, and the strength of public and informal support systems. Understanding how households actually adjust in the immediate aftermath of disasters is therefore central to assessing financial resilience.

[13] Personal debt is measured using the HILDA wealth modules, which are collected at four-year intervals, whereas disaster exposure is recorded annually. This allows event-time effects to be estimated at yearly horizons, but each coefficient is identified using the subset of individuals whose wealth is observed at that particular relative time since exposure.

Using Australian longitudinal data that track individuals before and after direct damage to their home, we document a clear pattern of financial adjustment. Annual expenditure rises by 3.5 per cent in the year following disaster exposure, driven primarily by higher spending on home and motor-vehicle repairs, with smaller increases in other categories. Income does not change in a statistically meaningful way, and there is little evidence of short-run asset drawdown. Instead, the adjustment occurs on the liability side of the household budget: total debt increases by roughly 13 per cent (around $13,000 relative to the mean), with especially large responses in personal borrowing. At the same time, indicators of financial strain significantly worsen. The probability of pawning or selling belongings, going without meals, and struggling to pay utility bills all rise following disaster exposure. Particularly striking is the increase in reliance on welfare organisations, which rises by 76% relative to the baseline, indicating that for a meaningful subset of households, disaster-related financial pressure is severe enough to require charitable support.

Taken together, the results point to a clear mechanism. Disaster exposure generates an immediate expenditure shock (repairs, replacement of damaged durables, and relocation-related costs) that is not matched by contemporaneous changes in income. Households therefore bridge the gap using short-term financing and coping strategies rather than through earnings adjustments or large asset sales. The prominence of personal debt and informal support is consistent with responses occurring under time pressure and with limited liquidity, often using forms of credit that are quick to access but potentially expensive. These effects are concentrated in the year of the shock and largely dissipate thereafter, consistent with households managing their debt obligations without sustained financial strain, though cuts to discretionary spending outside the measured expenditure categories cannot be ruled out.

Insurance plays an important buffering role, though not a complete one. Households without home and contents insurance experience larger increases in expenditure and debt and face more pronounced financial hardship after disasters. The differences are especially visible in personal and credit-related borrowing and in measures of difficulty meeting basic needs. However, insured households are not fully protected as they also experience higher expenditure and increased reliance on borrowing, indicating that insurance reduces but does not eliminate the need for households to absorb disaster-related costs using their own financial resources.

We also find that the financial impacts are driven by direct household losses rather than broader local economic disruption. When we control for area-level disaster exposure, the estimated effects of individual damage on expenditure, debt, and hardship remain largely unchanged, while regional exposure itself is not systematically associated with financial outcomes. This indicates that the main mechanism operates through the immediate financial consequences faced by directly affected households. An implication is that disaster policy focused on regional economic recovery may miss the households most in need of support, since financial impacts are concentrated among those experiencing direct damage rather than spread across the broader affected population.

More broadly, the results underscore that even in a high-income country with developed financial markets and established safety nets, direct exposure to natural disasters can produce intense but short-lived financial stress. As climate risks intensify and insurance becomes more costly or unavailable (Keys and Mulder, 2024), the capacity of households to absorb these shocks without relying on high-cost borrowing or sacrificing essential consumption will become increasingly difficult.

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Main Results

Table 1: Disaster Exposure and Household Expenditure

	Indicators of Any Expenditure			Expenditures in Dollars		
	Mean (1)	Estimate (2)	Std Error (3)	Mean (4)	Estimate (5)	Std Error (6)
Total Expenditure	-	-	-	18200.16	3.458***	(1.019)
Home Repairs	0.640	0.032***	(0.007)	1688.63	21.230***	(8.359)
Motor Vehicle Repairs	0.870	0.003	(0.005)	535.94	6.175***	(2.449)
Grocery Bills	0.990	0.000	(0.002)	5319.80	1.261	(0.780)
Meals Eaten Outside	0.890	0.003	(0.005)	1692.19	2.044	(1.525)
Alcohol Expenses	0.690	0.001	(0.006)	740.64	0.544	(2.039)
Tobacco Expenses	0.230	0.016***	(0.005)	415.18	8.168***	(3.077)
Utility Bills	0.990	0.002*	(0.001)	2046.74	1.698	(1.754)
Clothing Expenditures	0.900	0.005	(0.005)	910.86	0.912	(2.068)
Private Health Insurance	0.600	0.002	(0.005)	758.40	-1.447	(1.573)
Home and Vehicle Insurance	0.830	-0.002	(0.005)	857.53	3.912**	(1.735)
Medical Expenses	0.930	0.005	(0.004)	781.25	-1.283	(2.768)
Public Transport	0.310	0.031***	(0.007)	274.25	10.984*	(6.578)
Fuel Expenses	0.930	0.005	(0.003)	1259.35	-0.987	(1.697)

Note: The dependent variables in columns 1–3 are binary indicators equal to one if the individual reports any expenditure in the relevant category. The dependent variables in columns 4–6 are annual per-capita expenditure amounts in dollars. Dollar outcomes are estimated using Poisson pseudo-maximum likelihood; binary outcomes using linear probability models. Coefficients for dollar outcomes are interpreted as percentage changes. All models include individual, LGA, and state-year fixed effects, and control for age and age-squared. The estimation sample covers waves 9–23 (2009–2023) and includes approximately 215,000 observations. Standard errors clustered at the individual level are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1% level, respectively.

Table 2: Disaster Exposure and Household Income

	Indicators of Any Income			Income in Dollars		
	Mean (1)	Estimate (2)	Std Error (3)	Mean (4)	Estimate (5)	Std Error (6)
Total Income	-	-	-	50267.33	-0.873	(0.895)
Salary and Wages	0.81	-0.006	(0.005)	45761.94	-0.809	(1.029)
Business Income	0.14	0.007	(0.005)	3332.19	-9.666	(6.918)
Investment Income	0.51	0.000	(0.007)	3888.08	-3.354	(7.351)
Pension	0.16	0.008	(0.005)	3099.51	1.586	(5.589)
Private Transfers	0.12	0.002	(0.005)	404.26	-2.505	(15.769)
Government Transfers	0.53	0.003	(0.007)	5127.34	1.933	(1.461)
Irregular Income	0.08	0.026***	(0.006)	3655.75	-14.632	(13.793)

Note: The dependent variables in columns 1–3 are binary indicators equal to one if the individual receives income from the relevant source. The dependent variables in columns 4–6 are annual per-capita income amounts in dollars. Dollar outcomes are estimated using Poisson pseudo-maximum likelihood; binary outcomes using linear probability models. Coefficients for dollar outcomes are interpreted as percentage changes. Because all individuals report positive total income, the extensive margin for total income is not estimated (indicated by —). Irregular income includes inheritances, bequests, redundancy and severance payments, irregular transfers from non-resident parents, lump-sum workers compensation, and other irregular payments. All models include individual, LGA, and state-year fixed effects, and control for age and age-squared. The estimation sample covers waves 9–23 (2009–2023) and includes approximately 215,000 observations. Standard errors clustered at the individual level are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1% level, respectively.

Table 3: Disaster Exposure and Household Balance Sheet

	Indicators of Asset or Debt			Asset or Debt in Dollars		
	Mean (1)	Estimate (2)	Std Error (3)	Mean (4)	Estimate (5)	Std Error (6)
Assets	1.00	0.000	(0.001)	606553.07	2.159	(3.065)
Debt	0.75	0.008	(0.013)	99660.18	13.300**	(6.290)
Assets						
Housing and Other Property	0.69	0.020	(0.012)	259436.27	1.887	(3.766)
Retirement Savings	0.90	-0.005	(0.008)	133282.21	0.572	(5.678)
Businesses and Farms	0.12	-0.015	(0.010)	22910.68	6.375	(32.087)
Equity Investments	0.34	0.017	(0.013)	26204.35	-14.200	(15.051)
Bank Accounts	0.99	0.002	(0.005)	33091.76	9.399	(8.583)
Cars and Other Vehicles	0.95	-0.002	(0.007)	17480.60	-1.823	(3.322)
Other Assets	0.17	0.026*	(0.014)	2502.99	13.601	(24.051)
Debts						
Housing and Other Property	0.48	0.028*	(0.015)	83579.05	9.725	(6.684)
Businesses and Farms	0.03	0.002	(0.006)	4149.96	-31.964	(25.908)
Credit Cards	0.20	-0.005	(0.013)	657.29	5.932	(13.749)
Personal Debt	0.16	0.005	(0.014)	6392.94	84.075***	(42.625)

Note: The dependent variables in columns 1–3 are binary indicators equal to one if the individual holds any asset or debt in the relevant category. The dependent variables in columns 4–6 are per-capita asset and debt values in dollars. Dollar outcomes are estimated using Poisson pseudo-maximum likelihood; binary outcomes using linear probability models. Coefficients for dollar outcomes are interpreted as percentage changes. All models include individual, LGA, and state-year fixed effects, and control for age and age-squared. The estimation sample covers wealth module waves only and includes approximately 55,000 observations. Standard errors clustered at the individual level are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1% level, respectively.

Table 4: Disaster Exposure, Informal Financing and Material Deprivation

	Mean	Estimate	Std Error
	(1)	(2)	(3)
Informal Financing			
Pawned or Sold Something	0.057	0.017***	(0.005)
Help from Friends or Family	0.124	0.024***	(0.006)
Help from Welfare Organisations	0.041	0.031***	(0.005)
Material Deprivation			
Went Without Meals	0.041	0.016***	(0.004)
Could Not Pay Mortgage/Rent	0.061	0.006	(0.005)
Could Not Pay Utility Bills	0.120	0.016***	(0.006)
Unable to Heat Home	0.033	0.014***	(0.004)

Note: All outcomes are binary indicators equal to one if the event occurred in the past year due to a shortage of money. Informal financing captures active responses to raise short-term liquidity; material deprivation captures outcomes where basic consumption needs are unmet. Models are estimated using linear probability models with individual, LGA, and state-year fixed effects, and control for age and age-squared. The estimation sample covers waves 9-23 (2009-2023) and includes approximately 209,803 observations. Standard errors clustered at the individual level are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1% level, respectively.

Table 5: Direct versus Area-Level Disaster Exposure

	Total Expenditure	Total Income	Total Debt	Personal Debt	Informal Financing	Material Deprivation
	(1)	(2)	(3)	(4)	(5)	(6)
Disaster Exposure (Individual)	3.807***	-0.906	11.69**	74.37**	0.030***	0.020***
	(1.062)	(0.932)	(6.07)	(41.89)	(0.007)	(0.006)
Disaster Exposure (Area)	-0.404	0.247	-2.62	-8.12	0.003	0.001
	(0.373)	(0.255)	(1.89)	(8.99)	(0.002)	(0.002)
Mean Dependent Variable	18278.90	50179.19	99612.92	6380.85	0.154	0.153
Observations	200678	200678	55206	55206	196240	196240

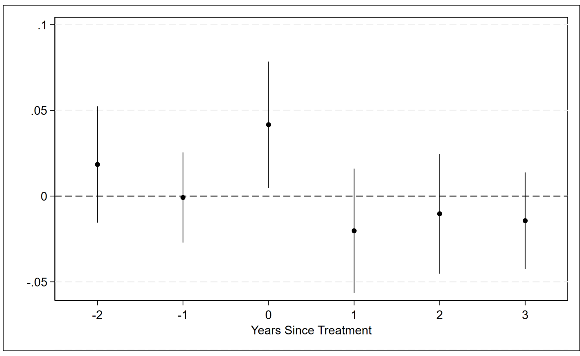
Note: Each column reports results from a model that includes both an individual-level disaster indicator and an area-level disaster incidence measure, allowing the direct household effect to be estimated net of regional economic conditions. Coefficients for columns 1-4 are interpreted as percentage changes; coefficients for columns 5-6 are percentage point changes. The smaller sample for debt outcomes reflects the four-yearly wealth module. All models include individual, LGA, and state-year fixed effects, and control for age and age-squared. Standard errors clustered at the individual level are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1% level, respectively.

Table 6: Disaster Exposure and Household Finance: Heterogeneity by Insurance Status

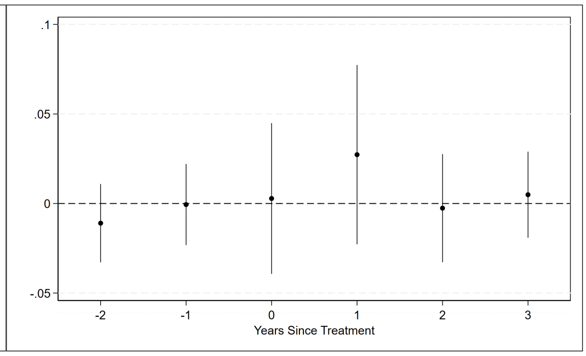
	Without Insurance			With Insurance		
	Mean (1)	Estimate (2)	Std Error (3)	Mean (4)	Estimate (5)	Std Error (6)
Dollar Outcomes						
Expenditure	14886.45	5.742***	(2.086)	19393.24	2.257*	(1.251)
Income	39611.52	0.653	(1.849)	52963.48	-1.346	(1.121)
Total Debt	54592.86	28.526*	(18.433)	110845.51	13.362**	(6.950)
Personal Debt	4057.06	87.367	(86.052)	7528.25	76.550***	(47.890)
Binary Outcomes						
Informal Financing	0.295	0.073***	(0.016)	0.108	0.015**	(0.007)
Material Deprivation	0.295	0.049***	(0.016)	0.114	0.007	(0.007)

Note: The sample is split by home and contents insurance status measured at Wave 14, used as a baseline proxy for pre-disaster coverage. Dollar outcomes are estimated using Poisson pseudo-maximum likelihood; coefficients are interpreted as percentage changes. Informal financing and material deprivation are binary indicators estimated using linear probability models; coefficients represent percentage point changes. All models include individual, LGA, and state-year fixed effects, and control for age and age-squared. The estimation sample covers waves 9–23 (2009–2023) and includes approximately 38,000 observations without insurance in the first three columns and 147,000 observations with insurance in the last three columns. The outcomes related to the wealth module comprise approximately 10,000 observations without insurance and 38,400 observations with insurance. Standard errors clustered at the individual level are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1% level, respectively.

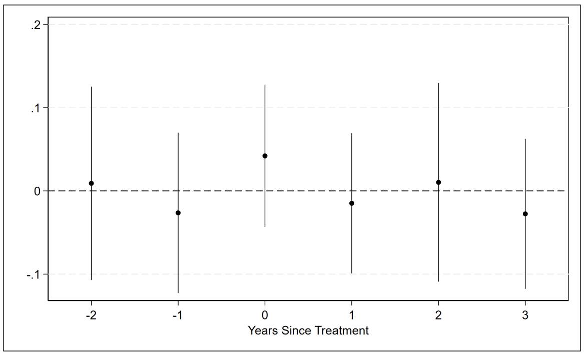
Figure 1: Disasters and Household Finance: Pretrends



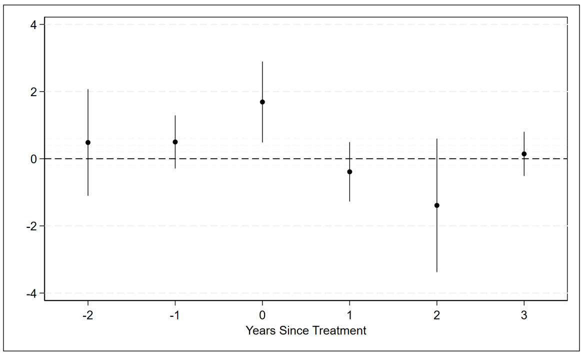
(a) Expenditure



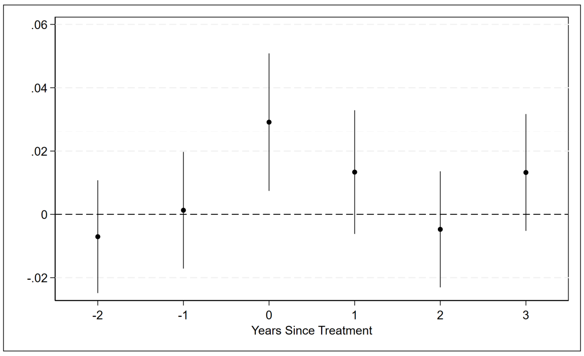
(b) Income



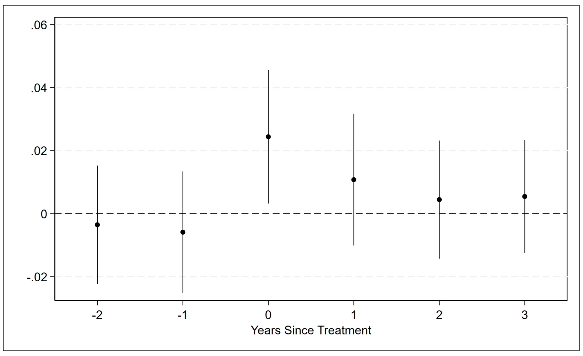
(c) Assets



(d) Personal Debt



(e) Informal Financing



(f) Material Deprivation

Note: The plots provide coefficients and the associated 95% confidence intervals for current, past and future disasters. All the regressions include individual fixed effects, LGA fixed effects, state-year fixed effects and the following controls: age and age-squared of the respondents. Standard errors clustered at an individual level.

Online Appendix (Not for Publication)

Disaster Exposure and Household Financial Disruption

A1 Variable Definitions

This section provides a list of the variables used, their definitions and the way they are constructed. All the variables are mentioned in per capita terms based on the number of members in an household, unless mentioned otherwise, and also adjusted for inflation.

Expenditure Module: Table 2

Expenditure: The variable represents the annual per capita real expenditure of an individual. First, the sum of the following individual expenditures is calculated: home repairs, motor vehicle repairs, grocery bills, meals eaten outside, telephone rent, calls and internet charge, electricity and gas bills, clothing expenditures, alcohol and tobacco expenses, education fees, private health insurance, other insurance (home, contents and motor vehicle), fees paid to medical practitioners, expenses on medicines, prescriptions and pharmaceuticals, public transport and taxi expenses and fuel expenses. The total expenditure calculated is adjusted for inflation and then based on the number of members in an household, per capita expenditure at an individual level is calculated.

Home Repairs: Total annual expenditure on home repairs, renovations and maintenance.

Motor Vehicle Repairs: Total annual expenditure on motor vehicle repairs and maintenance.

Utility Bills: Sum of telephone rent, calls and internet charges, electricity bills, gas bills and other heating fuel.

Clothing Expenditure: Sum of men's, women's and children's clothing and footwear expenditure in a year.

Home, Contents and Motor Vehicle Insurance: Annual expenditure on home, contents or motor vehicle insurance.

Medical Expenses: Sum of fees paid to health practitioners and expenditures on medicines, prescriptions, pharmaceuticals, alternative medicines.

Public Transport: Annual expenditure on public transport and taxis.

Fuel Expenses: Annual expenditure on motor vehicle fuel.

Income Module: Table 3

Income: The variable represents the annual per capita real income (after-tax regular income) of an individual calculated using the following items: Salary and wages, business income, investment income, regular private pensions, regular private transfers and Australian government public transfers.

Salary and Wages: Household financial year gross wages and salary.

Business Income: Calculated as the difference between positive and negative values of financial year business income. The resulting negative values (bottom one percentile of the sample) are replaced with zeroes.

Investment Income: Calculated as the difference between positive and negative values of financial year investment income which refers to the sum of interest, rent, royalties, dividends from shares and dividends from own incorporated businesses. The resulting negative values (bottom one percentile of the sample) are replaced with zeroes.

Pension Payments: Household financial year regular private pensions.

Private Transfers: Household financial year regular private transfers

Government Transfers: The variable refers to the sum of Australian Government income support payments and non-income support payments, plus other Australian Government benefits not elsewhere classified.

Irregular Income: The variable refers to the sum across all household members of financial year inheritances, bequests, redundancy and severance payments, irregular transfers from non-resident parents, irregular payments from other non-household members, lump sum workers compensation and other irregular payments. The total irregular income of an household is adjusted for inflation and represented at an individual level using the number of household members.

Wealth Module: Table 4

Assets: The variable represents the total wealth of an individual (in real terms), calculated as the sum of the financial and non-financial assets. Financial assets are equity investments, cash investments, trusts, own bank accounts, joint bank accounts, children's bank accounts, redeemable insurance policies, retirees superannuation, and non-retirees superannuation. Non-financial assets are home value, other property value, collectibles, businesses and vehicles.

Debt: The sum of property debt, business debt, total credit card debt, HECS debt, other debt, and overdue household bills.

Other Assets: Total of substantial assets such as antiques, works of art, and collectibles for the household. Excludes home general contents. The variable is adjusted for inflation and measured in per capita terms.

Other Debts: The variable aims to capture debt in the form of car loans, hire purchase agreements, investment loans, personal loans from a bank/financial institution, loans from other lenders, loans from friends/relatives and overdue personal bills.

Table A1: Disasters and Household Finance: Summary Statistics

Variables	Mean	Standard Deviation
Expenditure	18200.16	12816.59
Income	50267.33	34958.19
Assets	606553.10	842324.70
Debt	99660.18	199509.70
Disaster	0.016	0.127
Age	44.27	17.70

Note: The table reports summary statistics for the key aggregated outcome variables used in the analysis, along with individual-level disaster exposure and age. The sample includes all individuals aged 15–80 residing in private dwellings.

Table A2: Individual Predictors of Future Disaster Exposure

	Entire Sample		Wealth Module	
	Beta	Standard Error	Beta	Standard Error
Number of Children	0.0010	0.0007	-0.0009	0.0016
Married or in a Defacto Relationship	0.0006	0.0017	0.0012	0.0050
Separated or Divorced	0.0005	0.0031	-0.0104	0.0096
Employed Full Time	0.0025*	0.0013	0.0012	0.0038
Employed Part Time	0.0023*	0.0013	0.0030	0.0040
Own Home or Paying Mortgage	0.0017	0.0013	0.0010	0.0045
Health Condition is Fair or Poor	0.0015	0.0014	0.0073*	0.0042
Mental Health Factor	-0.0006	0.0006	-0.0001	0.0015
Physical Health Factor	-0.0002	0.0006	-0.0002	0.0019
Life Satisfaction	0.0002	0.0021	-0.0019	0.0063
Worsening of household finances	-0.0012	0.0027	-0.0031	0.0075
Expenditure	0.0004	0.0009	0.0020	0.0026
Income	0.0003	0.0008	-0.0021	0.0029
Assets			-0.0010	0.0019
Debt			0.0010	0.0008
F-Statistic [P-Value]	0.95	[0.500]	0.65	[0.839]
Observations	181746		35982	

Note: The dependent variable is an indicator equal to one if an individual reports that their home was damaged or destroyed by a weather-related disaster in period $t+1$. Models are estimated using OLS with individual, LGA, and state-year fixed effects. The Entire Sample columns use the annual estimation sample; the Wealth Module columns are restricted to waves 10, 14, and 18. Assets and Debt are only available in the Wealth Module sample. Standard errors clustered at the individual level are reported in parentheses. *, **, and *** denote significance at 10%, 5%, and 1% level, respectively.

Table A3: Disasters and Household Finance: Predicting Moving Residence

Variables	Entire Sample		Wealth Module	
	Beta	Standard Error	Beta	Standard Error
Number of Children	-0.0238***	0.0018	-0.0298***	0.0038
Married or in a Defacto Relationship	-0.0496***	0.0059	-0.0571***	0.0141
Separated or Divorced	-0.0575***	0.0098	-0.0616***	0.0239
Employed Full Time	-0.0234***	0.0037	-0.0200**	0.0102
Employed Part Time	-0.0247***	0.0035	-0.0334***	0.0099
Own Home or Paying Mortgage	-0.2336***	0.0048	-0.2809***	0.0135
Is the Health Condition Fair or Poor	-0.0094***	0.0034	-0.0294***	0.0096
Mental Health Factor	-0.0008	0.0015	-0.0012	0.0042
Physical Health Factor	0.0056***	0.0016	-0.0020	0.0044
Life Satisfaction	-0.1465***	0.0058	-0.1697***	0.0160
Major Worsening in Financial Situation	-0.0099	0.0062	-0.0182	0.0180
Expenditure	-0.0168***	0.0024	-0.0178***	0.0064
Income	0.0147***	0.0022	0.0055	0.0072
Assets			0.0147***	0.0050
Debt			0.0013	0.0021
F-Statistic [P-Value]	292.38	[0.000]	57.16	[0.000]
Observations	181758		35976	

Note: *, **, and *** denote significance at 10%, 5%, and 1% level, respectively. The outcome variable is whether an individual has moved to a new residence in $t+1$. Columns 1 and 2 include the entire sample, whereas the last two columns only include the wealth module, i.e., waves 10, 14, and 18. All the regressions include individual-fixed effects, LGA fixed effects, and state-year fixed effects. Standard errors clustered at an individual level are reported in the parenthesis.

Table A4: Disasters and Household Finance: Relocation

	Overall Sample	Renter	Owner
	(1)	(2)	(3)
$\text{Disaster}_{\text{t}}$	0.10***	0.21***	0.06***
	(0.01)	(0.02)	(0.01)
Mean Dependent Variable	0.166	0.331	0.098
Observations	214878	62238	152432

Note: *, **, and *** denote significance at 10%, 5%, and 1% level, respectively. All the regressions include individual fixed effects, LGA fixed effects, state-year fixed effects and the following controls: age and age-squared of the respondents. Standard errors clustered at an individual level are reported in the parenthesis.

Table A5: Disasters and Household Finance: Robustness Exercises

	Mean Outcomes	Observations	Coefficient	Standard Error
	(1)	(2)	(3)	(4)
	Panel A: Individual-LGA Fixed Effects			
Expenditure	18200.16	215072	3.096***	(1.036)
Household Income	50267.33	215072	-0.457	(0.925)
Total Assets	606553.07	55522	2.338	(3.315)
Personal Debt	8096.61	55522	63.273***	(28.80)
Informal Financing	0.147	200820	2.855***	(0.654)
Material Deprivation	0.149	200820	1.728***	(0.624)
	Panel B: LGA-Specific Linear Time Trends			
Expenditure	18200.16	215072	3.458***	(1.019)
Household Income	50267.33	215072	-0.873	(0.896)
Total Assets	606553.07	55522	2.143	(3.068)
Personal Debt	8096.61	55522	66.207***	(25.78)
Informal Financing	0.151	206614	3.188***	(0.640)
Material Deprivation	0.152	206618	2.142***	(0.623)
	Panel C: Clustering SEs at an LGA Level			
Expenditure	18200.16	215072	3.458***	(1.04)
Household Income	50267.33	215072	-0.873	(0.933)
Total Assets	606553.07	55522	2.143	(2.686)
Personal Debt	8096.61	55522	66.207***	(31.35)
Informal Financing	0.151	206614	3.187***	(0.710)
Material Deprivation	0.152	206618	2.142***	(0.716)
	Panel D: Addressing Potential Attrition Bias			
Expenditure	18216.18	211237	3.482***	(1.025)
Household Income	50301.51	211237	-0.822	(0.883)
Total Assets	625128.24	49225	2.063	(3.06)
Personal Debt	13189.75	30507	66.095***	(26.18)
Informal Financing	0.153	206614	3.270***	(0.643)
Material Deprivation	0.152	206618	2.165***	(0.625)

Note: *, **, and *** denote significance at 10%, 5%, and 1% level, respectively. All the regressions include individual fixed effects, LGA fixed effects, state-year fixed effects and the following controls: age and age-squared of the respondents. For outcomes measured in levels (columns 3 and 4), coefficients are converted into percentage changes. For binary outcomes, coefficients are expressed in percentage points. Standard errors clustered at an individual level are reported in the parenthesis in Panels A, B and D.

Table A6: Disasters and Household Finance: By Expenditure Type (OLS)

	Mean Outcomes	Coefficient	Standard Error
	(1)	(2)	(3)
Total Expenditure	18200.16	616.21***	(185.45)
Home Repairs	1688.63	318.52**	(138.84)
Motor Vehicle Repairs	535.94	35.835**	(13.929)
Grocery Bills	5319.80	69.121*	(41.675)
Meals Eaten Outside	1692.19	33.250	(23.802)
Alcohol Expenses	740.64	-1.447	(15.416)
Tobacco Expenses	415.18	44.803***	(17.047)
Utility Bills	2046.74	32.886	(35.736)
Clothing Expenditures	910.86	7.330	(17.615)
Private Health Insurance	758.40	-9.619	(11.123)
Home and MV Insurance	917.60	33.972**	(16.498)
Medical Expenses	781.25	-11.415	(22.196)
Public Transport	274.25	27.272*	(14.388)
Fuel Expenses	1259.35	-14.866	(23.522)

Note: *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The explanatory variable indicates whether a weather-related disaster (flood, bushfire, or cyclone) damaged or destroyed the individual's home within the twelve months preceding the interview. All regressions include individual fixed effects, LGA fixed effects, and state-year fixed effects, along with controls for the respondent's age and age-squared. The estimation sample covers waves 9–23 (2009–2023) and includes approximately 215,000 observations. Standard errors clustered at the individual level are reported in parentheses.

Table A7: Disasters and Household Finance: By Income Type (OLS)

	Mean Outcomes	Coefficient	Standard Error
	(1)	(2)	(3)
Total Income	50267.33	-436.92	(436.79)
Salary and Wages	45761.94	-323.80	(431.94)
Business Income	3191.17	-436.25	(348.94)
Investment Income	3619.59	-22.67	(322.56)
Pension	3099.51	-0.190	(168.99)
Private Transfers	404.26	-3.290	(58.81)
Government Transfers	5127.34	143.47*	(83.42)
Irregular Income	3655.75	-396.01	(571.03)

Note: *, **, and *** denote significance at 10%, 5%, and 1% level, respectively. All the regressions include individual fixed effects, LGA fixed effects, state-year fixed effects and the following controls: age and age-squared of the respondents. The estimation sample covers waves 9–23 (2009–2023) and includes approximately 215,000 observations. Standard errors clustered at an individual level are reported in the parenthesis.

Table A8: Disasters and Household Finance: By Wealth Type (OLS)

	Mean Outcomes	Coefficient	Standard Error
	(1)	(2)	(3)
Assets	606553.07	-2107.26	(21045.97)
Debt	99660.18	12584.27**	(6036.39)
Individual Assets			
Housing and Other Property	259436.27	588.06	(11686.60)
Pensions and Superannuation	133282.21	-4189.51	(6750.91)
Businesses and Farms	22910.68	1939.48	(7987.21)
Equity Investments	26204.35	-3526.16	(3116.45)
Bank Accounts	33091.76	1613.45	(2693.42)
Cars and Other Vehicles	17480.6	-551.52	(719.72)
Other Assets	2502.99	3.950	(768.11)
Non-Financial Assets	396043.07	10726.38	(17554.89)
Financial Assets	210510	-12833.64	(9149.08)
Debts			
Housing and Other Property	83579.05	8086.88	(5388.85)
Businesses and Farms	4149.96	-72.39	(1132.13)
Credit (and similar) Cards	657.29	107.18	(92.357)
Personal Debt	6392.24	4875.43**	(2477.91)

Note: *, **, and *** denote significance at 10%, 5%, and 1% level, respectively. All the regressions include individual fixed effects, LGA fixed effects, state-year fixed effects and the following controls: age and age-squared of the respondents. The estimation sample covers wealth module waves only and includes approximately 55,000 observations. Standard errors clustered at an individual level are reported in the parenthesis.

Table A9: Disasters and Household Finance - Heterogeneity by Insurance - Wave 15 and above

	Without Insurance			With Insurance		
	Mean (1)	Estimate (2)	Std Error (3)	Mean (4)	Estimate (5)	Std Error (6)
Dollar Outcomes						
Expenditure	14732.89	8.425***	(2.901)	19100.32	1.986	(1.526)
Income	41234.95	2.374	(2.404)	53530.38	-0.298	(1.331)
Total Debt	68218.85	16.76	(20.86)	110844.51	7.467	(9.870)
Formal Debt	4107.54	18.87	(71.25)	6452.36	63.22	(73.93)
Binary Outcomes						
Informal Financing	0.270	0.071***	(0.021)	0.102	0.016*	(0.009)
Material Deprivation	0.278	0.040**	(0.020)	0.108	-0.001	(0.009)

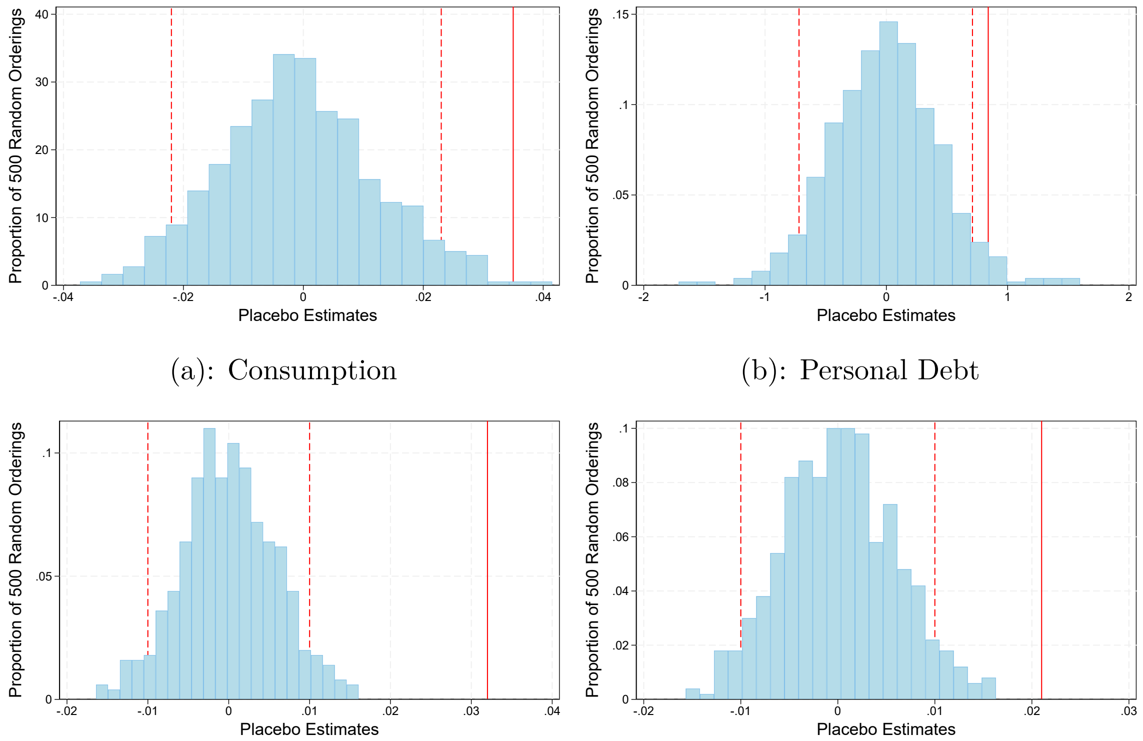
Note: *, **, and *** denote significance at 10%, 5%, and 1% level, respectively. All the regressions include individual fixed effects, LGA fixed effects, state-year fixed effects and the following controls: age and age-squared of the respondents. The reported numbers (except the last three rows) show the percentage change in each outcome after a disaster, obtained by converting the estimates into percentage effects. The estimation sample covers waves 15–23 (2015–2023) and includes approximately 23,000 observations without insurance in the first three columns and 88,000 observations with insurance in the last three columns. The outcomes related to the wealth module comprise approximately 4,900 observations without insurance and 18,600 observations with insurance. Standard errors clustered at an individual level are reported in the parenthesis.

Table A10: Disasters and Household Finance - Home Ownership

	Mean Outcomes	Coefficient	Standard Error	Mean Outcomes	Coefficient	Standard Error
	(1)	(2)	(3)	(4)	(5)	(6)
	Rented			Own		
Expenditure	16283.78	2.858	(2.061)	19786.89	2.854*	(1.491)
Household Income	44134.07	-0.437	(1.710)	52759.34	-2.124	(1.643)
Total Assets	263604.68	-2.381	(13.097)	806819.71	3.444	(4.029)
Personal Debt	4484.45	26.74	(34.290)	8018.00	144.718***	(78.144)
Informal Financing	0.245	2.903	(0.018)	0.092	1.787**	(0.009)
Material Deprivation	0.257	4.058**	(0.018)	0.105	0.818	(0.009)

Note: *, **, and *** denote significance at 10%, 5%, and 1% level, respectively. All the regressions include individual fixed effects, LGA fixed effects, state-year fixed effects and the following controls: age and age-squared of the respondents. The reported numbers (except the last two rows) show the percentage change in each outcome after a disaster, obtained by converting the estimates into percentage effects. The estimation sample covers waves 9–23 (2009–2023) and includes approximately 32,000 renters and 95,000 homeowners. The outcomes related to the wealth module comprise approximately 8,500 observations for renters and 25,500 observations for homeowners. Standard errors clustered at an individual level are reported in the parenthesis.

Figure A1: Disasters and Household Finance: Permutation Exercise



(a): Consumption

(b): Personal Debt

(c): Informal Financing

(d): Material Deprivation

Note: The figure presents the results of a randomization inference procedure based on 500 replications, in which the treatment indicator is randomly reassigned across individuals within each state. The dashed lines indicate the 95th percentile of the distribution of placebo estimates, while the solid line represents the baseline estimate.