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ABSTRACT

The Economic Impacts of Direct Natural Disaster Exposure*

This paper studies how having your home damaged or destroyed by a natural disaster impacts on economic and financial outcomes. Our context is Australia, where disasters are frequent. Estimates of regression models with individual, area and time fixed-effects, applied to 10 waves of data (2009-2018), indicate that residential destruction has no average impact on employment and income, but increases financial hardship and financial risk aversion. These impacts are generally short-lived, larger for renters than home owners, and greater for smaller isolated disasters. Using a Group Fixed Effects estimator, we find that around 20% of the population have low resilience to financial shocks, and for these individuals we find a substantive increase in financial hardships. The most vulnerable are the young, single parents, those in poor health, those of lower socioeconomic status, and those with little social support. These results can help target government aid after future natural disasters to those with the greatest need.

JEL Classification: Q54, J21, I31, G50, C23, H84

Keywords: natural disasters, financial hardship, risk aversion, mental health, resilience

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1. Introduction

Natural disasters are one of the major problems facing humankind (Strömberg, 2007). In 2019 there were 396 natural disasters, killing 11,755 people, affecting 95 million others, and costing nearly US$130 billion.[1] Crucially, disaster events are predicted to increase in magnitude and severity due to population movements and climate change (e.g. Field et al., 2012; Dell et al., 2014; IPCC, 2015). Consequently, there is considerable interest in understanding the economic, social and health impacts of natural disasters on households and communities. Establishing who is the most affected and in what circumstances, and distinguishing why some people are more resilient than others, is important for identifying potential policy interventions for future disasters. Some groups may even thrive following a disaster, such as those displaced to areas with better labour market opportunities (Deryugina et al., 2018), those employed in the construction industry (Groen et al., 2019), and those who gain more broadly from increased infrastructure investment (Gignoux and Menéndez, 2016; Kirchberger, 2017). Answering these types of questions is important for predicting the 'damage function', and costs of climate change (Tol, 2009; Dell et al., 2014; Auffhammer, 2018).

Over the last two decades economists have contributed strongly to understanding the impacts of natural disasters on households. One of the most extensively studied disasters is Hurricane Katrina in 2005 (Gallagher and Hartley, 2017; Deyugina et al., 2018; Deryugina and Molitor, 2019; Groen et al., 2019). A key finding from this Katrina literature is that the extent and nature of home insurance coverage, displacement opportunities, and the focus and generosity of post-disaster governmental aid, are important factors in dampening the economic impacts on individuals and households (Deryugina, 2017; Acconcia et al., 2019; Franklin and Labonne, 2019). Other recent studies have focused on the economic impacts of previous U.S. hurricanes (Deryugina, 2017; Mahajan and Yang, 2020), the Great East Japan Earthquake of 2011 (Hanaoka et al., 2018), the Indian Ocean Earthquake of 2004 (Callen, 2015), earthquakes and flooding in Indonesia (Cameron and Shah, 2015; Gignoux and Menéndez, 2016; Kirchberger, 2017), earthquakes in the Italy and Peru (Caruso and Miller, 2015; Acconcia et al., 2019; Porcelli and Trezzi, 2019), earthquake zones across 96 countries (Sinding Bentzen, 2019), flooding in Australia (Page et al., 2014), forest fires in Indonesia (Sheldon and Sankaran, 2017; Rosales-Rueda and Triyana, 2019), typhoons in the Philippines (Deachert and

[1] According to the EM-DAT global database on natural disasters: https://www.emdat.be/

Felfe, 2015; Franklin and Labonne, 2019), and major floods globally (Kocornik-Mina et al., 2020).[2]

A nearly ubiquitous feature of these economics studies is that they estimate the effects of residing in a disaster zone.[3] In many natural disasters, the majority of residents in affected areas don't have their home, business or other vital property destroyed. This means there is little explicit evidence on the economic impacts of directly experiencing destruction. In this paper our main aim is to help fill this gap, by providing causal estimates of the impact of having your home damaged or destroyed by a natural disaster. We contrast these effects with the indirect effects of residing in a disaster zone without experiencing residential damage.

An advantage of our data is that we are able to directly identify individuals whose homes were either damaged or destroyed. We use annual data from the Household, Income and Labour Dynamics in Australia (HILDA) Survey, which is an ongoing nationally-representative longitudinal study of households. While experiencing damage or destruction of your home by a natural disaster is a low-probability and potentially high-impact event (Weitzman, 2009), Australia provides a salient context to study the economic impacts because it experiences a wide range of natural disasters (Ladds et al., 2017).[4] Importantly, we observe a large number of reports of home damage or destruction over our ten-year panel window (2009-18). Our main economic outcome measures include employment, household income, financial hardship, financial risk aversion and financial time preference. We also provide results for mental and physical health to help explain the economic effects.

[2] Earlier economic studies on the economic and health impacts of natural disasters include Anbarci et al. (2005), Kahn (2005), Baade et al. (2007), Landry et al. (2007), Strömberg (2007), Belasen and Polachek (2008, 2009), Groen and Polivka (2008, 2010), McIntosh (2008), Paxson and Rouse (2008), Sawada and Shimizutano, 2008, Vigdor, 2008; Eckel et al. (2009), Jayachandran (2009), Cavallo et al. (2010), De Silva et al. (2010), Zissimopoulos and Karoly (2010), Frankenberg et al. (2011), Strobl (2011), Torche (2011), Boustan et al. (2012), Hornbeck (2012), Imberman et al. (2012), Paxson et al. (2012), Sacerdote (2012), Cavallo et al. (2013), Currie and Rossin-Slater (2013), Fomby et al. (2013), Sastry and Gregory (2013), Gallagher (2014) and Hornbeck and Suresh (2014). More broadly, recent studies include the effect of natural disasters on religiosity (Bentzen, 2019), financial markets (Bourdeau-Brien and Kryanowski, 2020), sectoral output (Ulbasoglu et al., 2019, consumer prices (Heinen et al., 2019) and economic growth (Berlemann and Wenzel, 2018).

[3] An exception is Deuchert and Felfe (2015) who analyse the effects of survey reported housing damage, caused by the 1990 Philippines typhoon Mike, on children's short- and long-term education and health outcomes. In some studies, proxies for likely damage are used. For example, Deryugina et al. (2018) classify homes in New Orleans as more likely to have been damaged by Hurricane Katrina if they were in FEMA declared "look and leave" zip codes; areas in which residents could only return to their homes during the day.

[4] The most prominent recent natural disaster example is the 2019-20 wildfires, in which thousands of fires simultaneously burnt across the country. In addition to wildfires, there is a high risk of storms, major flooding, and cyclones. For example, major flooding in Queensland in 2010/2011 impacted 75% of the state, affecting over 2.5 million people. In 2017, Cyclone Debbie caused major flooding throughout Queensland and New South Wales, with severe damage to property. In fact, since 2000 there have been over 130 tropical cyclones hitting Australia (BoM, 2020).

Aside from data limitations, a major empirical difficulty in estimating the impacts of natural disasters on victims is that it is problematic to compare residents whose homes were directly affected by the disaster and those who were spared, because certain variables such as housing quality and choice of residential locations are non-randomly determined. Our approach, given the availability of individual-level longitudinal data, is to estimate regression models with individual, time, and area fixed-effects. This means that our identification comes from comparing changes over time in the outcomes of direct disaster victims with changes over time in the outcomes of residents who were not affected.[5] We demonstrate with this regression approach that being a natural disaster victim is not predictable by changes in an individual's economic, social or health circumstances, supporting our identification assumptions.

We additionally contribute to the disaster literature by identifying the characteristics of individuals who are least resilient (or more vulnerable) by exploring the extent of heterogeneity in the responses to disaster using the Group Fixed Effects (GFE) estimator developed by Bonhomme and Manresa (2015). The GFE model we estimate includes group-level heterogeneity in the speed of adaptation to economic shocks, allowing us to classify individuals into groups that we interpret as reflecting different levels of economic resilience.

The remainder of the paper is structured as follows. Section 2 describes the HILDA data which we use, highlighting its unique features. Section 3 presents results estimated using conventional panel data methods. These include the estimated average effects of direct and indirect disaster exposure, as well as effect persistence. Section 4 is focussed on effect heterogeneity, including results which use the GFE estimator. Section 5 concludes.

2. Longitudinal Data on Disaster Exposure and Economic Status

Our data are drawn from the Household, Income and Labour Dynamics in Australia (HILDA) Study. Wave 1 (2001) contained a sample of 19,914 panel members from 7,682 households, and in each subsequent year members of these households have been followed-up, along with new household members resulting from changes in the composition of the original households and new households from the wave 11 top-up sample. Annual data are currently available from 2001 to 2018, and each year includes detailed information on a variety of economic and social

[5] Our identification strategy, based on individual variation, then complements the approach taken by most other studies, where identification comes from selecting a valid control group e.g. areas (e.g. states, districts, cities, urban zones) observed pre-disaster which are demographical and economically 'similar' to the disaster zone, but where not affected by the disaster. This selection is not straight forward as it is sometimes difficult to fully rule out spill-over or general equilibrium effects across different areas.

outcomes, including employment, income, health, wellbeing and major life events. The survey comprises a face-to-face interview and a confidential self-completion questionnaire.

2.1. Disaster exposure measure

Of particular importance to this study is the natural disaster question included in waves 9 (2009) to 18 (2018) of the self-completion questionnaire. Respondents are asked whether in the past 12 months: "A weather-related disaster (e.g., flood, bushfire, cyclone) damaged or destroyed your home"? We are unaware of any comparable longitudinal data that regularly asks respondents about natural disaster victimisation, and so HILDA data therefore provide a unique opportunity to investigate the consequences of direct disaster exposure.

We restrict our analysis to respondents aged 25-80 living in private dwellings, who are observed in multiple waves between wave 9 and 18. This provides a working sample of 15,008 individuals and 111,003 observations. On average, 1.5% of individuals per year report damage or destruction due to a natural disaster, adding up to 1,663 reports in total, reported by 9.2% (1,376) of the sample. Of the respondents who have ever reported a disaster, 84% (1,153 individuals) report only one occurrence across the 10 available years, 13% (175 individuals) report two occurrences, and 3% (48 individuals) report three or more occurrences.

We have matched these disaster exposure reports with details of known disasters or severe weather events. Specifically, we matched information in HILDA on the timing[6] and location of reported disaster events with information from the Australian Government's "Disaster Assist" database of declared natural disasters[7], plus media reports of disasters. This approach has allowed us to match approximately two-thirds of the HILDA reports to known events. Importantly, we do see a clear bunching of disaster reports with known disasters. For example, of the HILDA respondents residing in the Statistical Divisions of 'Northern' and 'Far North' Queensland, we observe 106 out of 171 observations (62%) reporting a damaged or destroyed home at the time of Cyclone Yassi in 2011. Similarly, for Cyclone Oswald in 2013 we observe 19% of potentially affected respondents reporting a disaster; for Cyclone Marcia in 2015 we observe 39%; and for Cyclone Debbie in 2017 we observe 54%.

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[6] HILDA respondents are asked whether the disaster damage occurred 0-3 months ago, 4-6 months ago, 7-9 months ago or 10-12 months ago. We use this information to help match disasters to publically reported events, and also to explore the time dynamics of the disaster effects.

[7] Disaster Assist is an Australia Government website containing information on the Australian Local Government areas that have been declared natural disasters by the States and Territories, and where individuals are eligible to apply for disaster assistance payments. See: https://www.disasterassist.gov.au/Pages/home.aspx.

Appendix Table A1 describes the estimation sample by presenting mean values of characteristics for all respondents, and also separately for subsamples defined by any direct disaster exposure in the years 2009 to 2018. The sample means indicate that respondents who had their home damaged or destroyed are similar to other respondents with respect to age, sex, number of children, marital status, labour market outcomes and income. In contrast they have slightly lower educational attainment, and are much more likely to reside in a regional or remote area. The geographical distribution of HILDA disaster reports is shown in Appendix Figure A1. It is clear that the occurrence of natural disasters is not uniform across areas. Rather we see that areas of Queensland, the Northern Territory and Northern Western Australia, where cyclones and tropical storms are common, are the most commonly affected.

2.2. Economic outcome measures

To explore the economic consequences of disaster exposure, we examine effects on household income and employment status. Financial outcomes are explored using three additional variables. First is a variable indicating that the person reported a "major worsening" in their financial situation during the past 12 months. This is reported by 3.1% of individuals on average each year, and by 14.8% of individuals at least once across the period 2009-2018. Second, is a measure of serious financial hardship during the current calendar year.[8] Respondents are asked whether due to a shortage of money they: (1) could not pay electricity, gas or telephone bills on time; (2) could not pay the mortgage or rent on time; (3) pawned or sold something; (4) went without meals; (5) was unable to heat home; (6) asked for financial help from friends or family; and (7) asked for help from welfare or community organisations. We use the sum of the seven hardships. The sample mean equals 0.44 and the standard deviation equals 1.08. Each year, approximately 80% experience no hardships, and around 7% experience 3 or more hardships.

The final outcomes are measures of financial risk and time preferences. The HILDA study does not contain a general risk preferences module, such as the hypothetical lottery questions used by Hanaoka et al. (2018), but it does contain an annual question asking people to describe the "amount of financial risk that you are willing to take with your spare cash?". From this question we create a binary variable indicating that the person is "not willing to take

[8] We note that most HILDA surveys take place between August and October each year, and so this question refers to hardships experienced during the past 8-10 months.

any financial risks".[9] 49% of individuals respond with this answer. Time preferences are measured using the question "In planning your saving and spending, which of the following time periods is most important to you?", which was asked every 2 years. The six response options range from "the next week" to "more than 10 years ahead". Our binary outcome equals one if the individual's most important time period is "the next week" or "the next few months". The sample mean is 46%.

3. Estimated Average Effects of Disaster Exposure

3.1. Fixed-effects regression approach

Our baseline empirical specification is a fixed-effects panel data model, as shown in equation (1). This model draws on repeated observations of individuals over time:

$$y_{iat} = \beta D_{iat} + \alpha_i + \tau_t + \gamma_a + e_{iat} \quad (1)$$

Here, a given outcome variable (y_{iat}) for person i, living in area a, in year t is specified as a linear function of the direct effect (β) of natural disaster exposure (D_{iat}), an individual component α_i fixed over time, a time-varying component τ_t fixed across individuals, a component common across people living in the same local area (γ_a), and an idiosyncratic error term (e_{iat}) that includes all other remaining determinants of y. Importantly, equation (1) includes no time-varying (contemporaneous) characteristics as regressors, such as marital or economic status, since these may be influenced by disaster exposure. In other words, they are potential mediators.

Our primary aim is to estimate β for a range of different outcome variables. The exposure variable D_{iat} is binary. It is equal to 1 in year *t* for people who reported that a disaster damaged or destroyed their home during the previous 12 months. This regression (1) will yield consistent estimates of β under the strict exogeneity assumption; that the exposure variable is independent of e_{it} in all time periods. There are a number of potential violations of this assumption that should be discussed.

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[9] There are five possible responses, including "I take substantial financial risks expecting to earn substantial returns", "I take above-average financial risks expecting to earn above-average returns", and "I take average financial risks expecting to earn average returns". A person may also respond that they "never have any spare cash", in which case, they are subsequently asked to assume they had some spare cash that could be used for savings or investments. Our binary risk variable also equals one if the answer to this hypothetical question was that they "would not be willing to take any financial risks".

The strict exogeneity assumption is violated if the probability of exposure is influenced by other major life events. This would imply a correlation between D_{iat} and e_{iat} or e_{iat-1}. For example, someone who loses their job may subsequently move to an area or to a dwelling that is more prone to disaster-related damage. If so, the estimated effect of D_{iat} may be biased upwards, as it may also pick-up the effect of the labour market shock. We explore this potential issue by analysing the association between the probability of disaster exposure and life events.

First, we present in Column (1) of Table 1 estimated coefficients from a regression model of future disaster exposure (D_{iat+1}), including as regressors the same fixed effects as in equation (1), α_i, τ_t and γ_a, a set of observed characteristics X_{iat}, and health and economic outcomes y_{iat}. The estimates suggest that observed demographics, employment status, income, self-assessed wellbeing, and health have very little explanatory power for future disaster exposure. Individually, each coefficient is small and is not significantly different from zero at the 5% level. An F-test of the joint explanatory power of these variables gives an F-statistic of 0.99 with p-value equal to 0.463. This finding similarly holds true if we model the binary disaster outcome using the conditional logit model (results available upon request). These results suggest that disaster events are plausibly exogenous.

In Column (2) we use the same regression specification to model future moves (i.e. changed residence in the next 12 months). This set of results demonstrates that observed characteristics are strongly predictive of other life events, and that the low predictive power for future disaster exposure is not driven by our choice of regressors. The results also demonstrate the clear endogeneity of moves, and by extension location of residence. This motivates the inclusion of the area level fixed effects (γ_a) in equation (1). However, in practice, our results are not sensitive to their inclusion.[10]

Under the specification in equation (1), the strict exogeneity assumption also requires that any effect of exposure is both immediate and temporary. This assumption is violated if for example the effect is either sustained beyond the first year or if the effect is delayed. If there are such lagged effects, these are incorporated in e_{it-1}, which would be correlated with D_{it} in equation (1), leading to coefficient estimates which are biased towards zero. In some versions of the analysis, we incorporate such lagged effects directly, as shown in equation (2):

$$y_{it} = \beta D_{it} + \delta D_{it-1} + \alpha_i + \tau_t + \gamma_a + e_{it} \tag{2}$$

[10] Area of residence may in some cases be a consequence of disaster exposure rather than a cause, and so the inclusion of area fixed-effects in equation (1) closes one of the potential pathways in which exposure affects y_{iat}, leading to estimates of β that are biased towards zero. Our approach is therefore the conservative one.

Another threat to validity is that disasters have indirect effects, for example for people whose homes were not directly affected by the disaster, but who live in areas with high levels of exposure. We address this by also estimating models in which the indirect area level effects are included explicitly in the model. Equation (3) shows such a model:

$$y_{iat} = \beta D_{iat} + \pi H_{at} + \alpha_i + \tau_t + \gamma_a + e_{iat} \quad (3)$$

Where H_{at} measures area-year level exposure. It is specified as a vector of binary variables – each indicating exposure at a given level of intensity. A local area is coded as having high intensity exposure if more than 10% of respondents reported exposure in a given year, and medium intensity of between 5% and 10% reported exposure. π then captures the effect of living in an area with medium or high level disaster exposure, net of any direct effect (damage or destruction of your home).

Standard errors are clustered at the individual level for the estimates generated from equations (1) and (2) and at the area level for equation (3).

3.2. Estimated average effects

3.2.1 Effects on home repairs, insurance, and relocation

Before estimating the effects of natural disasters on our main economic outcomes, we examine a range of more directly proximal effects. The aim of this analysis is to demonstrate that our disaster survey measure predicts outcomes we expect to be affected by housing damage and destruction. The outcomes are: (a) household expenditure on repairs, renovations and maintenance of home (any expenditure and log expenditure); (b) changing residence (any move and a distant move); and (c) total household expenditure on non-medical insurance (any expenditure and log expenditure).[11]

Rows (1) and (2) of Table 2 present estimated effects on repairs, renovations and maintenance expenditures. These expenditures are unlikely to include repairs covered by insurance, and therefore will not reflect the full value of all repairs. Nevertheless, we find that disaster exposure is estimated to increase the probability of having any expenditure by 2.0

[11] HILDA respondents are first asked to report total household annual expenditure on private health insurance and then on “other insurance, such as home and contents and motor vehicle insurance”. Therefore, the insurance expenditure outcome measure that we use may reflect insurance expenditure on property other than the household’s place of residence.

percentage points, and increase the dollar amount of expenditures by 13% (100[exp(0.123) - 1]). Rows (3) and (4) show that disaster damage also increases the likelihood of changing residence during the past 12 months (by 9.2 percentage points), with a proportion of these moves being greater than 50kms away. We interpret these distant moves as relocations away from the disaster affected region.

Most HILDA respondents had some positive non-medical insurance expenditure (91%), and the likelihood of any expenditure did not increase following the disaster (row 5). However, the amount spent on insurance by the 91% with positive expenditure increased by 4.2% (row 6). The increased insurance expenditure among disaster victims may have been caused by increased insurance coverage – many people have inadequate coverage to disasters that is only recognised post-disaster – or by increased insurance premiums. In recent years, insurers have substantially increased premium prices in disaster-prone areas (Booth and Tranter, 2018).

Overall, the estimates shown in Table 2 are as broadly expected, and provide confidence that the disaster exposure measure reflects true significant housing damage and destruction.

3.2.2 Effects on economic status

Table 3 presents the estimated average effects of disaster exposure on key economic outcomes. Panel A shows results from the baseline specification using the fixed-effects regression outlined in equation (1). Columns (1) and (2) suggest that direct exposure (home damage or destruction) has no effect on the probability of full-time employment or on household income. We find similarly small effects for alternative labour market outcomes, such as employed part- or full-time (-0.004), weekly work hours if employed (0.203), and log weekly wages (0.010).

In contrast, there is evidence of substantive negative effects on financial outcomes. Column (3) shows that direct disaster exposure is estimated to increase the likelihood of reporting a "major worsening in financial situation" during the past 12 months: a 4.9 percentage point increase, which is 160% relative to the sample mean of 3%. Column (4) shows a positive effect on reported number of financial hardships: 16% relative to a sample mean of 0.44 hardships. This indicates an increased likelihood of going without meals, needing help from welfare organisations, and not paying rent and utility bills, among other financial hardships. The hardship effect appears to be driven by an increased likelihood of reporting several hardships simultaneously: the effect of direct disaster exposure on an indicator of reporting three or more hardships equals 2.3 percentage points ($p = 0.001$), relative to a sample mean of 6.3%.[12]

Columns (5) and (6) present the estimated effects of home destruction on our financial risk and time preference measures. It is estimated that disaster victims are 2.3 percentage points more likely to report that they are "not willing to take any financial risks" (5% relative to sample mean). In other words, direct disaster exposure is estimated to increase risk aversion.[13] The estimated effect on having a myopic time-horizon when making saving and spending decisions is smaller and statistically insignificant ($p = 0.379$). However, a limitation is that this time preference measure is only observed every second year (wave), and so the statistical power to detect effects is lower.

Panel B of Table 3 shows corresponding results from the specification that includes a lag exposure variable (indicating disaster between 12 and 24 months ago). Lagged disaster exposure has no significant effect on the three financial outcomes that had clear effects in Panel A; suggesting that the negative financial effects are short-lived. However, the sum of the first and second year effects remain statistically significant for "major worsening in financial situation" and for "number of financial hardships": estimates equal 0.053 ($p < 0.001$) and 0.139 ($p = 0.002$), respectively. In both cases, the total effect of direct exposure is larger than the effect shown in Panel A; twice as large for financial hardship.

Column (2) shows a statistically significant lagged effect on household income. However, subsequent analysis will demonstrate that this may not be caused by direct disaster exposure (having your home damaged or destroyed), but instead caused by indirect disaster exposure (living in a disaster zone). However, it is also possible that reduced health – especially mental health – could be the cause of the delayed negative income effect. In the next subsection we present the health effects of direct disaster exposure.

We also explore how disaster effects evolve over time by using additional information provided in HILDA on when in the last four quarters the disaster event occurred. Appendix Table A3 shows that the effects for major worsening of finances and financial hardship are largest in magnitude for housing damage or destruction that occurred in the past 3 months. We

[12] Table A2 presents estimates using an alternative treatment variable indicating that every surveyed person in the household reports their home was damaged or destroyed. This alternative indicator might identify more serious incidents, but also overweights effects experienced by single person and small households Nevertheless, the estimates are quantitatively similar, with effects generally larger in magnitude.

[13] Our finding of increased risk aversion following direct disaster residential damage is inconsistent with the findings in Page et al. (2014), which is one of the only other studies that explores the effect of direct disaster exposure on risk taking behaviour. Investigating the effects of the 2011 Brisbane Floods, they find that flood victims were more likely to opt for a risky gamble (a scratch card) than a sure comparable value of $10. The differences in results could be driven by the different measures of risk-taking behaviour.

are however reticent to conclude that direct disaster exposure causes only very short-lived negative effects. This is because most HILDA surveys are completed in August-October, and large severe disasters, such as cyclones, wildfires and widespread flooding, typically occur during Australia's summer (December-February). Therefore, more recent reported disasters are typically less severe winter disasters, such as localised storms. Consequently, the effects presented in Appendix Table A3 may to some extent be confounded by disaster severity and type.

Panel C of Table 3 tests whether there exist indirect disaster effects in addition to direct effects. Included in the fixed-effects regressions are variables signalling whether, in the survey immediately before the disaster, the survey respondent was residing in an area where 5-10% or >10% of other survey respondents reported housing damage or destruction. Residing in an area that experienced a disaster (but not having your home destroyed) is associated with a significant 1.4% reduction in household income.[14] This short-run negative income effect is consistent with results from the analysis of Hurricane Katrina (Deryugina et al., 2018).

Interestingly, there is some evidence that the indirect disaster effect on risk aversion is of opposite sign to the direct effect. Having your home damaged or destroyed causes a substantial 3.0 percentage point increase in risk aversion, while residing in a severe disaster zone has a near zero effect, and residing in a moderate disaster zone causes a 1.3 percentage point reduction in risk aversion. This indirect effect for moderate disaster zones is in-line with the findings from Hanaoka et al.'s (2018) analysis of the Great East Japan Earthquake, but not with Cameron and Shah's (2015) analysis of natural disasters in Indonesia. They find that individuals from villages that experienced a flood or earthquake exhibit more risk-aversion. Our finding of increased risk aversion for those directly affected and reduced risk aversion for those indirectly affected may help explain the varied results within the literature: the sign and magnitude of the effect may depend upon the proportions of people who have been directly and indirectly affected.

In Table 4 we explore if there is a differential impact on home owners (A) compared to renters (B), and show estimates from separate models. We see some interesting differences, with the results suggesting a greater vulnerability for renters. For those renting their home we see some weak evidence ($p = 0.095$) of reduced household income following the direct impact

[14] If we replace the moderate and severe disaster zone indicators with one indicator indicating a moderate or serve disaster zone, the indirect exposure effect for log income equals -0.014 ($p = 0.013$). Expanding the specification to include a one-year lag of this indirect exposure indicator gives a lag effect estimate of -0.008 ($p = 0.216$).

of a disaster, whereas we find no such impact for home owners.[15] Similarly, we find a larger point estimate for reporting a major worsening in financial situation for renters (0.058) than home owners (0.045), although this difference is not statistically significant. Importantly, the worse post-disaster outcomes for renters causes a significant increase in financial hardships (0.268). Two potential reasons for these differences is that renters more often need to move following residentially damage (with the associated costs), and that renters are less likely to have their belongings insured.[16]

In terms of attitudes towards taking financial risks, we see similar modest increases in risk aversion for home owners (0.029) and renters (0.023), although this is only statistically significant for home owners. In contrast, renters are more impacted than home owners in terms of a change in time preference, increasing their time-frame for saving and spending decisions following a disaster that damages or destroys their home.

3.2.3. Effects on physical and mental health

We next explore the effects of direct disaster exposure on health. There is a large multidisciplinary literature on the health effects of natural disasters, including important economics contributions (e.g. Kahn, 2005; Deuchert and Felfe, 2015; Deryugina and Molitor, 2019). Our aim in this subsection is to demonstrate the health effects with our data and alternative methodological approach in order to inform on the economic and financial effects presented above. For instance, it has been demonstrated that health shocks increase financial hardship (García-Gómez et al., 2013) and increase risk aversion (Decker and Schmitz, 2016).

The estimated effects for three health outcomes are shown in Appendix Table A4. They suggest that direct disaster exposure reduces the mental health and physical health indices, which are both increasing in good health, by 5.1% ($p = 0.003$) and 3.4% of a standard deviation ($p = 0.059$), respectively. The effect on the likelihood of reporting fair or poor general health equals 1.3 percentage points ($p = 0.106$), relative to a sample mean of 18%.

[15] Interestingly, Gallagher and Hartley (2017) found that following Hurricane Katrina any impact on household finances were modest and short-lived, with home owners using flood insurance to repay their mortgages rather than to rebuild.

[16] For context, 27% of the sample are home renters, the prevalence of home damage or destruction for natural disasters is similar for owners (1.4% per year) and renters (1.5% per year), and the proportion living in a severe disaster zones is also similar (2.2% and 2.4%, respectively). For home owners, and those owning an investment property they rent out (which is common in Australia given favourable personal tax incentives), it is typically a requirement by mortgage lenders that the owner holds insurances against fire, floods and other building damage. In contrast, for renters' contents insurance is optional. Therefore, many renters face direct financial costs for replacing damaged or destroyed contents.

There is little evidence of delayed or persistent effects of disaster exposure on health. Nevertheless, if the model is specified to allow for the possibility of lagged impacts, then both the immediate and total effects are larger. For example, the total mental health effect (-0.082) is larger than the one-year effect (-0.051). There is also little evidence that indirect disaster exposure has negative effects on health. In fact, there is suggestive evidence that living in a disaster zone without directly experiencing residential damage or destruction (indirect exposure) improves mental wellbeing. The effect of residing in a moderate or severe disaster zone is estimated to improve mental health by 2.1% of a standard deviation ($p = 0.011$).

To summarise, in our data residing in a disaster zone is deleterious for health, particularly mental health, but only if the individual's home was directly damaged or destroyed. These negative health effects may be a pathway through which disasters worsen economic and financial outcomes, and change economic preferences.

4. Heterogeneous Effects

So far, we have shown estimated average treatment effects under various assumptions. But effects may be larger for different types of disaster events, or for some people more than for others. We now explicitly consider such heterogeneity, using a pair of complementary techniques. The first is a simple extension of the baseline model, with the treatment variable interacted with severity and type of disaster. We focus only on those outcomes for which there are clear direct average disaster effects (Table 3, Panel A); namely, major worsening in finances, number of financial hardships, and risk aversion.[17] Our second approach uses a Group Fixed Effects model (Bonhomme and Manresa, 2015), and we focus on the number of financial hardships for the empirical reason that it is the most continuous outcome measure.

4.1 Heterogeneity by severity and type of disaster

Table 5 shows results from six regressions that consider three dimensions of heterogeneity and three outcome variables. Panel A shows results from models in which the treatment variable is interacted with an indicator for residing in a severe disaster area. Surprisingly, the interaction effects have the opposite sign to the main effects. This means that the negative effects of direct disaster exposure are actually smaller if caused by major disasters. Indeed, the estimated effects

[17] Regression results for the other three economic outcomes show that the coefficients on all disaster exposure indicators (main effects and interaction effects) are small and statistically insignificant.

are essentially zero in such areas: the estimated effects in severe disaster areas equal 0.007 (p = 0.582) and -0.066 (p = 0.176) for major worsening in finances and number of hardships, respectively. A plausible explanation for this finding is that special financial support and services from governments and community organisations are concentrated in more severe disaster areas. For example, around 1,000 people reported significant damage to their home following Cyclone Yasi in Northern Queensland in February 2011, and the Australian Federal Government processed more than $A250 million worth of recovery grants in the first three weeks after the storm.[18] Areas in which the damage is more isolated, for example, where storm damage has only affected a few households in an area, do not typically receive such support. Another related explanation is that insurance companies are under increased scrutiny and pressure to process claims quickly and sympathetically in the aftermath of more severe disasters. We have investigated these explanations by estimating the same regression specification but with household irregular income as the outcome.[19] In support of the above explanations, we find that irregular income received following direct disaster exposure is significantly greater in severe disaster areas (p = 0.046).

In Panel B, we explicitly consider heterogeneity by disaster type. However, we note that the results for this detailed breakdown should be taken as only suggestive because of the relatively large standard errors, with most of the interaction effects being not statistically significant. Again, there is some evidence however that the detrimental effects are larger for household damage or destruction caused by smaller isolated disasters; which are events that we could not match with any widely reported disaster. This is consistent with the result in Panel A which suggests that the detrimental effects for minor or isolated disasters are larger than those for severe disasters.[20]

4.2. Heterogeneity by GFE groups

4.2.1. Econometric approach

In this section we examine how the effects of disaster exposure vary with individual resilience to financial hardship shocks. We use the Grouped Fixed Effects (GFE) estimator (Bonhomme

[18] See: https://knowledge.aidr.org.au/resources/cyclone-cyclone-yasi-queensland/

[19] Irregular income includes transfers and payments from non-household members, and other irregular payments.

[20] An alternative approach is to classify disasters in to more and less severe groupings using estimated damage costs (these estimates are only available for larger disasters). Direct exposure experienced during one of the top 20 largest disasters caused small and statistically insignificant effects on all outcomes. In contrast, direct exposure experienced during smaller (out of the top 20) disasters caused large significant effects: major worsening in finances, number of financial hardships and financial risk aversion increased by 0.058, 0.074 and 0.027, respectively.

and Manresa, 2015) to classify individuals into distinct groups that differ by their resilience to these shocks, based on 18 years of HILDA data on individuals' history of financial hardship. We then interact the disaster treatment indicator with individuals estimated "types" to test if more resilient individuals are affected less by exposure to natural disasters. In particular, we assume that each individual belongs to one of G distinct groups, and individuals within the group share parameters determining the lifecycle evolution of financial hardship, as well as the speed of adaptation to shocks to hardship. Let g_i denote group membership of individual i with $g = \{1, 2, \dots, G\}$. Given group membership, the lifecycle profile of financial hardship FH_{it} is determined as follows:

$$FH_{it} = \alpha_{g_i}(a_{it}) + FH_{it-1}\gamma_{g_i} + X_{it}'\beta + \varepsilon_{it} \tag{4}$$

where $\alpha_{g_i}(a_{it})$ is a polynomial function of age a_{it}, and the polynomial coefficients α_{g_i} are group-specific, including the intercept. The parameters γ_{g_i} measure the speed of adaptation of individuals in group g to last year's innovation to financial hardship.[21] In other words, after experiencing a negative financial shock, high γ_{g_i} individuals experience more intense hardship for a longer time period than low γ_{g_i} individuals. We therefore interpret this parameter as a measure of financial "resilience". The vector X_{it} contains survey wave and detailed SA3 local area indicators, with coefficients β being the same across the groups.

The GFE estimator allows estimation of unobserved heterogeneity in the panel data context in a flexible and parsimonious way. It is more flexible than the standard fixed effects method because it allows for unobserved heterogeneity in the slope and intercept coefficients. It resembles the standard finite mixture model in that it identifies a finite number of groups with the same parameters of the conditional mean function. However, in comparison to standard finite mixture models the GFE is less restrictive in the specification of the mixture component probabilities[22], and does not require parametric assumptions about the distribution of the dependent variable because it focuses on fitting the conditional mean only.[23]

[21] This interpretation relies on the assumption that the unobserved individual heterogeneity in the permanent component of FH_{it} is fully accounted for by the group-specific intercepts.

[22] Bonhomme and Manresa (2015) show that the GFE can be interpreted as the solution to maximisation of the pseudo-likelihood of a mixture of normals model where each cross-sectional unit has its own unrestricted mixture probabilities.

[23] We also explored using GFE to estimate unobserved heterogeneity in the disaster treatment effect, by re-specifying the main equation (1) as $y_{iat} = \beta_{g_i} D_{iat} + \alpha_i + \tau_t + \gamma_a + e_{iat}$. Unfortunately, this specification was unsuitable. The majority of individuals in our data are treated only once during the study period, and therefore provide insufficient information for the GFE algorithm to separate heterogeneous treatment effects from

We let the heterogeneity in the lifecycle profiles and adaptation to innovations be captured by three groups (i.e. G = 3). We use Bonhomme and Manresa's (2015) proposed BIC-like model selection criterion in combination with less formal considerations of computational costs and sample size to motivate this choice. Full estimation results of the GFE models with *G*=3 are presented in Appendix Table A5, and further details of the GFE model selection process and estimation procedure are in Appendix B.

The estimates of the type-specific resilience parameters and type proportions are presented in Table 6. We label the types according to the magnitude of the estimated resilience coefficient – type 1 has the lowest persistence, while type 3 has the highest persistence. The three groups from the GFE model for financial hardship exhibit different degrees of resilience to shocks. For the largest group 1 (77% of individuals) the persistence parameter is equal to 0.108, for the second-largest group 2 (16% of individuals) the persistence parameter is equal to 0.31, and for the smallest group 3 (7% of individuals) the persistence parameter is much larger and is equal to 0.45.

4.2.2. Estimated effects separately by GFE groups

Panel A in Table 6 presents results from a specification in which the disaster exposure variable is interacted with the GFE group indicators within our main specification (1). The disaster treatment effects are noticeably different across the groups, and increasing with the group-specific persistence parameter. In the most resilient group 1 the effect of direct disaster exposure is close to zero (0.010). In the less resilient groups, the effects are significantly larger: direct disaster exposure increases the number of hardships by 0.16 and 0.23 in groups 2 and 3, respectively (relative to a sample mean of 0.44). If we re-estimate these models using an indicator of three or more hardships as the outcome, the corresponding effects for groups 2 and 3 equal 5.5 (p = 0.012) and 7.2 percentage points (p = 0.053). Relative to a sample mean of 6.3%, these estimates demonstrate that direct disaster exposure causes serious financial distress for approximately one-quarter of the population.

Next we present results from models that include lagged exposure variables. The rationale is that lower financial resilience may result in longer-lasting disaster effects. We find no clear evidence for this, but the regressions are arguably underpowered (especially for the

idiosyncratic errors experienced during the treatment period. This intuition is supported by Monte-Carlo simulations. For example, in simulations with G=2 the units that experienced positive and negative random errors during the treatment year were classified into different groups. As a result, the estimated heterogeneous treatment coefficients $\hat{\beta}_1$ and $\hat{\beta}_2$ were severely biased in the direction of these errors.

smaller group 3). The sum of the 0-12 month and 12-24 month effects equal 0.324 ($p = 0.015$) and 0.355 ($p = 0.160$) for Group 2 and 3, respectively.

4.2.3. Correlates of GFE group membership

To understand the determinants of resilience (or the least vulnerable) to natural disasters we follow Bonhomme and Manresa (2015) and investigate the predictors of group membership. Table 7 presents marginal effect estimates from a multinomial logit model with group type (1, 2 or 3) as the dependent variable. The estimates show that the probability of belonging to the most vulnerable financial hardship groups is higher for people who are: female, younger, single, parents, low socioeconomic status (low education, income, wealth), and in poor health.

Especially interesting are the results for locus of control (LOC) and individual social capital, which are potentially malleable. The LOC measure is higher for people with a more 'external' LOC; these are people who are more likely to believe they "have little control over the things that happen to me", and less likely to believe that "what happens to me in the future mostly depends on me".[24] We find that a one standard deviation increase in external LOC increases the probability of belonging to group 2 and 3 (low resilience groups) by 1.5% points and 1.0 percentage points, respectively. This finding is consistent with economics studies highlighting that an internal LOC is a valuable non-cognitive skill (e.g. Cebi, 2007; Caliendo et al., 2015; Buddelmeyer and Powdthavee, 2016; Schurer, 2017).

The individual social capital index is constructed from five questions measuring a person's strength of agreement with statements like: "I don't have anyone that I can confide in", "I have no one to lean on in times of trouble", and "people don't come to visit me as often as I would like". Importantly, this index has a strong positive association with membership in the low resilience groups. A four standard deviation increase in the index – a move from being socially included and supported to being socially isolated and neglected – increases the probability of belonging to group 2 and 3 by almost 10 percentage points each (relative to sample means of 16% and 7%).

[24] The LOC measure is generated from survey modules in Waves 3, 4, 7, 11 and 15 that ask respondents to evaluate seven statements using a one (strongly disagree) to seven (strongly agree) scale. We add the responses (some items reversed) to form a locus of control index, which is again re-scaled to have mean zero and standard deviation one, and then averaged across waves.

5. Conclusion

Despite growing concerns about the increased frequency and severity of natural disasters, there is still much to learn about the economic and financial consequences for affected residents (Gallagher and Hartley, 2017). In this paper we contribute to this knowledge base by analysing unique Australian longitudinal data that asks respondents every wave (year) about whether their home has been damaged or destroyed by a natural disaster in the last 12 months. Such direct exposure is potentially a major financial shock, requiring expensive repairs or a re-build, and a change of residence at least temporarily. Using these data, we estimate how direct residential disaster exposure impacts on economic and financial outcomes. However, we also compare these direct effects with the indirect effects for those residing in a disaster zone, but who report no home damage.

Our empirical strategy has been to estimate regressions with individual, time, and area fixed-effects, which means that identification comes from comparing changes over time in the outcomes of direct disaster victims with changes over time in the outcomes of residents who were not directly affected. We have provided evidence that exposure to natural disasters is plausibly exogenous in our context.

Overall, we find little evidence of any significant impacts on employment or household income, but some evidence of increased financial hardships. Examples of increased financial hardships include the inability to afford meals and heating, and to pay utility bills and rent. We also find some evidence of increased risk aversion following direct home damage, and of reduced mental health. There are no corresponding effects for those living in a disaster zone but with no home damage or destruction.

Interestingly we find a greater financial hardship effect for renters than home owners, which might reflect a greater need to move home (and absorb the costs) for renters, and a lower take-up of home contents insurance. We also find that the impact on financial hardships is larger for more isolated disaster events, where only a few homes are damaged, than for more severe events such as cyclones and major flooding. One reason for this could be the significant aid and investment that is provided by government for such disasters. This is consistent with studies of Hurricane Katrina that have found little evidence of long-term adverse effects and some evidence of improved economic outcomes (e.g. Gallagher and Hartley, 2017; Deryugina et al., 2018). In this context, Deryugina (2017) notes that natural disaster victims in developed countries are better insured than previously thought.

A particular focus of this study has been to provide new evidence on the extent of heterogeneity in these impacts, and to identify individuals who are likely to be the least resilient (most vulnerable) to such direct shocks. To do this we have applied the Grouped Fixed Effects (GFE) estimator (Bonhomme and Manresa, 2015) to classify individuals into distinct groups that differ by their resilience to financial shocks. Importantly, we find that the least resilient, representing around 20% of the population, are more like to be young, single parents, those in poor health, those with low socioeconomic status (low education, low income, low wealth), and those with little social support. For these individuals we find a more substantive increase in financial hardships following residential damage or destruction. These results can help target future governmental aid and assistance to the most vulnerable following a natural disaster.

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Table 1: The Predictability of Future Disaster Exposure

	Disaster damaged or destroyed home in the next 12 months		Moved residence in the next 12 months	
	(1)		(2)	
Number of children	0.000	(0.001)	-0.007***	(0.003)
Married or cohabitating	-0.004	(0.003)	-0.084***	(0.010)
Divorced or separated from partner	-0.005	(0.005)	-0.079***	(0.014)
Employed full-time	-0.001	(0.002)	-0.016***	(0.005)
Employed part-time	-0.000	(0.002)	-0.007	(0.005)
Unemployed	-0.003	(0.003)	0.013	(0.010)
Log household income	0.000	(0.001)	0.020***	(0.003)
Own home or paying mortgage	0.003	(0.002)	-0.226***	(0.008)
Number of bedrooms	0.001	(0.001)	-0.011***	(0.003)
Fair or poor health	0.002	(0.002)	-0.011***	(0.004)
Mental health index	-0.000	(0.001)	0.000	(0.002)
Physical health index	-0.000	(0.001)	-0.001	(0.002)
Life satisfaction index	0.001	(0.001)	-0.050***	(0.002)
Major worsening in financial situation	-0.003	(0.003)	0.008	(0.008)
Sample size	97,170		97,170	
F-statistic [p-value]	0.99	[0.463]	118.47	[0.000]

Notes: Figures are estimates from two linear regressions with year, area, and individual fixed effects. Standard errors clustered at the individual-level are presented in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% level. The presented F-statistic refers to a test of the joint statistical significance of all listed variables.

Table 2: Relevance of the Disaster Exposure Measure

Dependent variable	Sample size	Sample mean	Estimated disaster exposure coefficient	
(1) Positive expenditure on home repairs	111003	0.674	0.020**	(0.009)
(2) Log expenditure on home repairs	74866	7.044	0.123***	(0.043)
(3) Moved in past 12 months	111003	0.138	0.092***	(0.011)
(4) Moved > 50kms away	111003	0.027	0.016***	(0.005)
(5) Positive insurance expenditure	111003	0.911	-0.007	(0.005)
(6) Log insurance expenditure	100702	7.229	0.042**	(0.020)

Notes: Each row presents results from a linear regression of the listed dependent variable on the natural disaster exposure indicator (disaster within the last 12 months that damaged / destroyed home), and year, area, and individual fixed effects. Regressions (2) and (6) are estimated using individuals who had positive expenditure on repairs and positive insurance expenditure, respectively. Standard errors clustered at the individual-level are presented in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% level.

Table 3: Estimated Effects of Disaster Exposure on Economic and Financial Outcomes

	Employed full-time (1)	Log annual household income (2)	Major worsening in financial situation (3)	Number of financial hardships (4)	Unwilling to take financial risks (5)	Planning horizon weeks or months (6)
A. Main effect						
Direct exposure	0.001	-0.010	0.049***	0.071**	0.023**	-0.015
	(0.008)	(0.011)	(0.008)	(0.028)	(0.009)	(0.017)
Sample size	110395	110395	110395	99399	101507	55408
B. Lagged effect						
Direct exposure	-0.003	-0.018	0.055***	0.093***	0.022**	-0.009
	(0.009)	(0.013)	(0.009)	(0.030)	(0.010)	(0.019)
Direct exposure lagged	0.003	-0.024**	-0.002	0.046	-0.011	-0.019
	(0.008)	(0.012)	(0.006)	(0.030)	(0.011)	(0.015)
Sample size	91128	91128	91128	81522	90641	50221
C. Indirect effect						
Direct exposure	0.001	-0.006	0.054***	0.060*	0.030***	-0.008
	(0.008)	(0.013)	(0.008)	(0.031)	(0.010)	(0.019)
Moderate disaster zone	-0.001	-0.014**	0.001	-0.004	-0.013**	-0.009
	(0.005)	(0.006)	(0.003)	(0.013)	(0.006)	(0.011)
Severe disaster zone	-0.000	-0.013	-0.011***	-0.023	0.001	0.007
	(0.006)	(0.010)	(0.004)	(0.018)	(0.008)	(0.019)
Sample size	97994	97994	97994	88746	90846	49,320

Notes: Each panel (A, B, C) presents results from six linear regressions with year, area, and individual fixed effects. Employed full-time (col 1), major worsening in financial situation (col 3), unwilling to take any financial risks (col 5), and planning horizon weeks or months (col 6) are binary variables. The financial hardships ranges from 0 hardships to 7 hardships. The financial hardship is missing from survey year 2010, the financial risk variable is missing from survey year 2009, and the financial time preference variable is missing every second year. The smaller sample sizes in panel B are due to the inclusion of lagged disaster exposure. The smaller sample sizes in panel C are due to the omission of areas with small numbers of HILDA respondents. Standard errors clustered at the individual-level are presented in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% level.

Table 4: Estimated Effects of Disaster Exposure Separately for Home Owners and Renters

	Employed full-time (1)	Log annual household income (2)	Major worsening in financial situation (3)	Number of financial hardships (4)	Unwilling to take financial risks (5)	Planning horizon weeks or months (6)
A. Home owners	0.002	-0.001	0.045**	-0.018	0.029***	-0.010
	(0.009)	(0.013)	(0.009)	(0.027)	(0.011)	(0.019)
Sample size	80221	80221	80221	72178	73671	40298
B. Renters	-0.007	-0.037*	0.058**	0.268***	0.023	-0.078**
	(0.016)	(0.022)	(0.018)	(0.073)	(0.021)	(0.039)
Sample size	30174	30174	30174	27221	27836	15119

Notes: Estimates from 12 linear regressions with year, area, and individual fixed effects. Employed full-time (col 1), major worsening in financial situation (col 3), unwilling to take any financial risks (col 5), and planning horizon weeks or months (col 6) are binary variables. The financial hardships ranges from 0 hardships to 7 hardships. The financial hardship is missing from survey year 2010, the financial risk variable is missing from survey year 2009, and the financial time preference variable is missing every second year. Standard errors clustered at the individual-level are presented in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% level.

Table 5: Testing for Heterogeneity in Disaster Exposure Effect by Introducing Interactions by Severity and Type of Natural Disaster

	Major worsening in financial situation	Number of financial hardships	Unwilling to take financial risks
	(1)	(2)	(3)
A. Disaster Severity Interaction			
Direct exposure	0.071***	0.101***	0.024**
	(0.010)	(0.037)	(0.012)
Severe disaster effect	-0.063***	-0.167***	0.017
	(0.017)	(0.061)	(0.024)
B. Disaster Type Interaction			
Direct exposure	0.022	0.093	0.023
	(0.015)	(0.062)	(0.021)
Bushfire effect	-0.041	-0.142	0.010
	(0.041)	(0.131)	(0.071)
Storm effect	0.018	-0.030	-0.008
	(0.021)	(0.080)	(0.028)
Cyclone effect	-0.007	-0.114	0.006
	(0.022)	(0.081)	(0.031)
Isolated disaster effect	0.068***	0.011	0.001
	(0.020)	(0.073)	(0.026)

Notes: Estimates from 6 linear regressions with year, area, and individual fixed effects. The 'direct exposure' effect should be interpreted as the effect for: people who people residing in areas with minor/moderate disasters in panel A; people who experienced a flood in panel B. Standard errors clustered at the individual-level are presented in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% level.

Table 6: Estimated Effects of Disaster Exposure on Financial Hardship by Estimated GFE Groups

	Average Effect	Separate Effects for GFE Groups		
		Group 1	Group 2	Group 3
A. Main effect				
Disaster damaged home	0.071**	0.010	0.161**	0.233*
	(0.028)	(0.020)	(0.081)	(0.137)
B. Lagged effects				
Disaster damaged home	0.093***	0.011	0.257***	0.276*
	(0.030)	(0.020)	(0.093)	(0.154)
Disaster damaged home lagged	0.046	0.033	0.067	0.079
	(0.030)	(0.021)	(0.078)	(0.190)
% of observations in group		77%	16%	7%
Estimated persistence coefficient		0.108	0.311	0.449

Notes: The 'average effect' column repeats relevant estimates from Table 3. For each panel, the 'separate effects for GFE groups' columns provide estimates from one linear regression with year, area, and individual fixed effects. In this regression, the indicator for 'disaster damaged home' is interacted with group membership. The 'estimated persistence coefficient' is from the Group Fixed Effects model estimates that are provided in full in Appendix A. Standard errors clustered at the individual-level are presented in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% level.

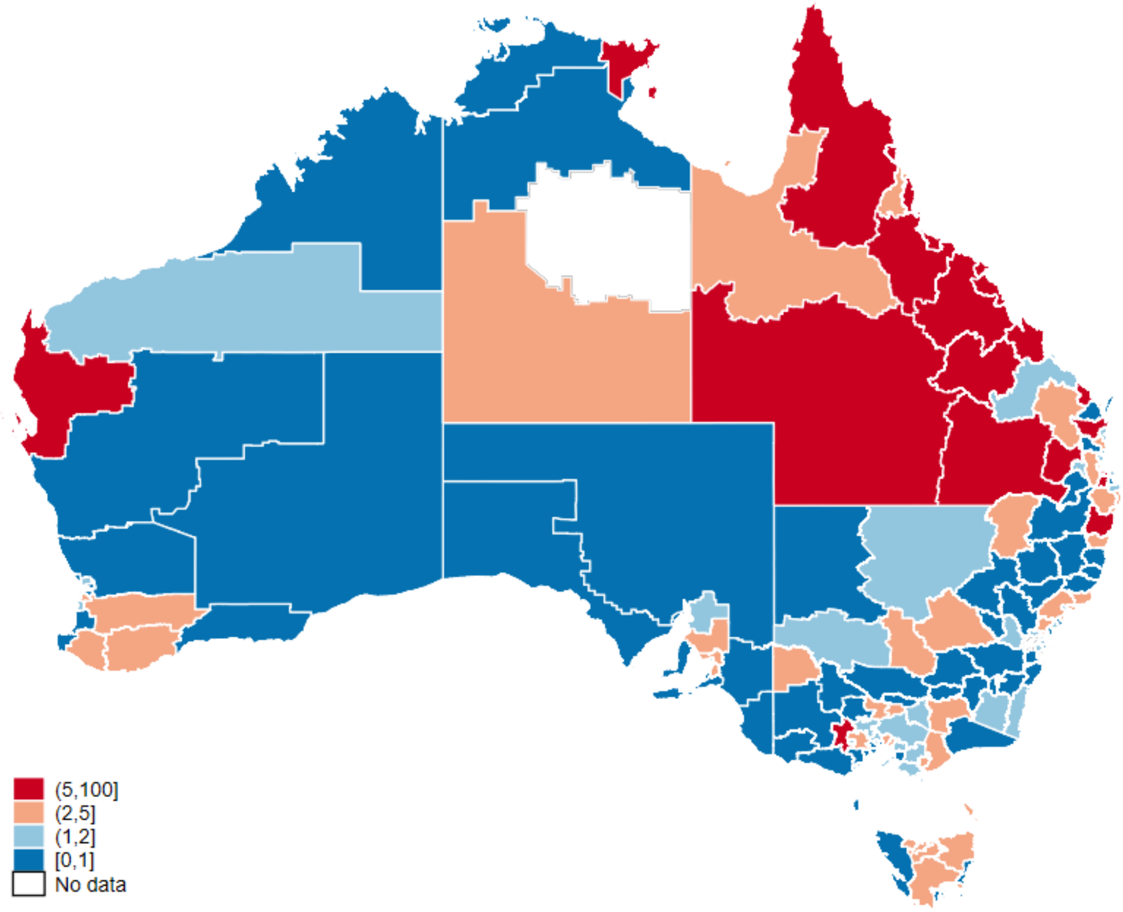
Table 7: Multinomial Logit Marginal Effect Estimates of Probability of Belonging to Estimated Groups from GFE Models

	Group 2		Group 3	
Female	0.018***	(0.006)	0.008**	(0.004)
Aged <35	0.120***	(0.015)	0.095***	(0.010)
Aged 35-49	0.137***	(0.014)	0.096***	(0.010)
Aged 50-64	0.107***	(0.013)	0.069***	(0.010)
Married or cohabitating	-0.019**	(0.009)	-0.030***	(0.005)
Number of children	0.028***	(0.004)	0.016***	(0.002)
University education	-0.027***	(0.009)	-0.035***	(0.007)
Log household net worth	-0.040***	(0.002)	-0.017***	(0.001)
Log household annual income	-0.036***	(0.009)	-0.019***	(0.005)
Long-term health condition	0.069***	(0.011)	0.048***	(0.006)
Inner regional area	0.001	(0.008)	0.001	(0.005)
Outer regional or remote area	-0.004	(0.011)	-0.011	(0.007)
External locus of control index	0.015***	(0.004)	0.010***	(0.002)
Individual social capital index	0.024***	(0.004)	0.023***	(0.002)
Cognitive ability index	0.005	(0.004)	0.002	(0.002)
% of observations in group	16%		7%	

Notes: Figures are marginal effect estimates from a multinomial logit regression of the probability that an individual was assigned to a GFE group. All time-varying characteristics are average values generated using data from 2001-2018. Figures in parentheses are robust standard errors. *, ** and *** denote statistical significance at the 10%, 5% and 1% level.

Appendix A – Supplementary Figures and Tables

Appendix Figure A1: Percentage of Individual-Year Observations in each SA3 that Reported Damage or Destruction of Home due to a Natural Disaster



Appendix Table A1: Descriptive Statistics

	All	Any reported disaster damage or destruction in period 2009-2018	
		No	Yes
Age	49.41	49.36	49.84
Male	0.47	0.47	0.48
Number of children	0.59	0.59	0.61
Married or cohabitating	0.74	0.74	0.76
Divorced or separated from partner	0.11	0.10	0.12**
Educational attainment: University degree	0.30	0.30	0.25***
Educational attainment: Vocational certificate	0.35	0.34	0.40
Educational attainment: High school graduate	0.11	0.11	0.11
Own home or paying mortgage	0.73	0.72	0.74
Number of bedrooms in home	3.28	3.28	3.34***
Reside in major city	0.62	0.64	0.47***
Inner regional area	0.25	0.25	0.30***
Outer regional or remote area	0.13	0.11	0.24***
Employed full-time	0.46	0.46	0.47
Log annual household income	11.23	11.24	11.22
Major worsening in financial situation	0.03	0.03	0.05***
Number of financial hardships	0.44	0.42	0.62***
Unwilling to take financial risks	0.49	0.49	0.50
Planning horizon weeks or months	0.46	0.46	0.48

Notes: Figures are sample means. *, ** and *** denote that there is a statistically significant difference in the estimated sample means in at the 0.10, 0.05 and 0.01 levels, respectively, between people who reported damage and destruction and those that did not.

Appendix Table A2: Estimated Effects using Alternative Disaster Exposure Measure

Dependent variable	Sample size	Estimated disaster exposure coefficient	
(1) Employed full-time	110395	0.003	(0.010)
(2) Log annual household income	110395	-0.043***	(0.016)
(3) Major worsening in financial situation	110395	0.037***	(0.010)
(4) Number of financial hardships	99399	0.117***	(0.038)
(5) Unwilling to take any financial risks	101507	0.033***	(0.013)
(6) Planning horizon weeks or months	55,408	-0.021	(0.024)

Notes: For this table, the dependent variable is equal to one for households from which every surveyed person reports their home was damaged or destroyed. Figures from 6 linear regressions with year, area, and individual fixed effects. Standard errors clustered at the individual-level are presented in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% level.

Appendix Table A3: Estimated Effects using Indicators for Reported Quarter of Disaster Occurrence

	Employed full-time (1)	Log annual household income (2)	Major worsening in financial situation (3)	Number of financial hardships (4)	Unwilling to take financial risks (5)	Planning horizon weeks or months (6)
Disaster 0-3 months ago	-0.008	-0.049	0.044**	0.266***	0.035	0.013
	(0.019)	(0.033)	(0.019)	(0.095)	(0.026)	(0.038)
Disaster 4-6 months ago	0.002	-0.004	0.035***	0.055	0.027	0.004
	(0.015)	(0.019)	(0.013)	(0.047)	(0.019)	(0.035)
Disaster 7-9 months ago	-0.001	-0.031	0.015	0.026	0.027	0.010
	(0.014)	(0.020)	(0.012)	(0.046)	(0.017)	(0.036)
Disaster 10-12 months ago	-0.000	0.018	0.024	0.139*	-0.003	-0.030
	(0.018)	(0.024)	(0.016)	(0.071)	(0.022)	(0.043)

Notes: Results from six linear regressions with year, area, and individual fixed effects. Employed full-time (col 1), major worsening in financial situation (col 3), unwilling to take any financial risks (col 5), and planning horizon weeks or months (col 6) are binary variables. The financial hardships ranges from 0 hardships to 7 hardships. Standard errors clustered at the individual-level are presented in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% level.

Appendix Table A4: Estimated Effects of Disaster Exposure on Health and Wellbeing

	Fair or Poor Health (1)	Physical Health Index (2)	Mental Health Index (3)
A. Main effect			
Direct exposure	0.013	-0.034*	-0.051***
	(0.008)	(0.018)	(0.017)
Sample size	109686	109686	109686
B. Lagged effect			
Direct exposure	0.018*	-0.042**	-0.070***
	(0.010)	(0.020)	(0.020)
Direct exposure lagged	-0.012	0.031*	-0.012
	(0.009)	(0.019)	(0.019)
Sample size	90639	90639	90639
C. Indirect effect			
Direct exposure	0.017*	-0.042**	-0.062***
	(0.009)	(0.021)	(0.019)
Moderate disaster zone	-0.003	0.000	0.023**
	(0.004)	(0.010)	(0.010)
Severe disaster zone	-0.008	0.009	0.016
	(0.006)	(0.015)	(0.014)
Sample size	97368	97368	97368

Notes: Each panel (A, B, C) presents results from three linear regressions with year, area, and individual fixed effects. Fair and poor health (col 1) is a binary variable. The mental health and physical health indices are continuous variables with a standard deviation of one. The smaller sample sizes in panel B are due to the inclusion of lagged disaster exposure. The smaller sample sizes in panel C are due to the omission of areas with small numbers of HILDA respondents. Standard errors clustered at the individual-level are presented in parentheses. *, ** and *** denote statistical significance at the 10%, 5% and 1% level.

Appendix Table A5: Estimated Group Fixed Effects (GFE) Models

	Group 1	Group 2	Group 3
Lagged dependent variable	0.1079	0.3110	0.4487
	(0.0042)	(0.0069)	(0.0120)
Intercept – Group 1	-0.0448	0.8811	1.8660
	(0.1103)	(0.1119)	(0.1232)
Age – Group 1	-0.0047	-0.0200	-0.0223
	(0.0003)	(0.0016)	(0.0043)
Age squared – Group 1	0.00004	0.00014	0.00015
	(0.00001)	(0.00003)	(0.00009)
Sample size	156,515		
BIC	1.0212		

Notes: Estimates from the GFE specification described in equation (4) with 3 groups estimated using data on 18 waves of HILDA. The model specifies heterogeneity across groups coefficients on lagged dependent variable, age, age squared and intercept, as well as group-invariant coefficients on dummy variables for 335 SA3 areas and HILDA survey waves. Asymptotic clustered standard errors are in parentheses.

Appendix B – Further Details of the Group Fixed Effects (GFE) Approach

The GFE estimator optimally groups n cross-sectional units into G number of group (G<n) using the least squares criterion. The parameters and group membership are simultaneously estimated to minimize the least squares criterion over the parameters and over all possible groupings of cross sectional units. The algorithm follows an iterative strategy starting from an initial guess of parameters, followed by assignment of cross-sectional units to groups to yield the smallest mean squared error. Then, the regression parameters are updated after the model is re-estimated based on the resulting assignment; this process is repeated until convergence of parameter values (Bonhomme and Manresa 2015).[25]

In the context of equation (4) the GFE estimator is the solution to the following problem:

$$\hat{a}, \hat{\beta}, \hat{\gamma}, \hat{\delta} = \frac{argmin}{(\beta,\alpha,\gamma,\delta)\in \mathrm{B}\times\Omega^G\times\Delta_G} \sum_{i=1}^{N}\sum_{t=1}^{T}(W_{it}-\alpha_{g_i}(a_{it})-W_{it-1}\gamma_{g_i}-X_{it}'\beta)^2, \quad (5)$$

where the minimum is taken over all possible groupings $\delta = [g_1, g_2, \dots, g_N]$ of the N cross-sectional units into G groups and over group-specific and common parameters α, γ and β.

To obtain the solution to problem (8) we use Algorithm 1 of Bonhomme and Manresa (2015). In particular, the algorithm iterates between the following steps:

1. Randomly choose starting values of parameters α, γ and β for g=[1,…,G]. Let j=0.
2. For all cross sectional units i=[1,…,N] compute the optimal group assignment, given the parameter values obtained in the previous step:
$$g_i^{j+1} = \frac{argmin}{g\in[1,\dots,G]}\sum_{t=1}^{T}(W_{it}-\alpha^j(a_{it})-W_{it-1}\gamma^j-X_{it}'\beta^j)^2$$
3. Compute the next iteration of parameters, given the group assignment in the previous step:
$$\Omega^{j+1} = \frac{argmin}{(\alpha,\gamma,\beta)\in \mathrm{B}\times\Omega^G}\sum_{i=1}^{N}\sum_{t=1}^{T}(W_{it}-\alpha_{g_i^{j+1}}(a_{it})-W\gamma_{g_i^{j+1}}-X_{it}'\beta)^2.$$
4. Let j=j+1, go to step 2 and iterate between steps 2-4 until convergence.

Bonhomme and Manresa (2015) provide conditions for consistency and asymptotic normality of the GFE estimator under the assumptions of N and T going to infinity, where T can grow considerably slower than N. Bonhomme and Manresa (2015) show that the GFE estimator performs well (i.e. small bias in the estimated parameters and high accuracy in group classification) even in an application with a relatively short panel (T=7) and the dependent variables taking a relatively small number of unique values. They also show that the asymptotic variance underestimates the finite-sample dispersion of the

[25] It is possible that the algorithm will not reach a global minimum of the least squares criterion if the starting values are too far off. To ensure the algorithm reaches a global minimum, Bonhomme and Manresa (2015) recommend using multiple starting values and to choose the solutions with the lowest least squares criterion. In our empirical application we execute the algorithm 1000 times.

parameters in this case. The length of the panel in our application is sufficiently long (18 years) to able to use asymptotic standard errors clustered at the level of cross-sectional units for inference.

We let the heterogeneity in the life cycle profiles and adaptation to innovations be captured by three groups (i.e. G=3). We use a formal model selection criteria in combination with less formal considerations of computational costs and sample size to motivate this choice. Bonhomme and Manresa (2015) proposed a BIC-like model selection criterion for the GFE models. To measure the goodness of fit of competing models this criterion uses the mean squared error, penalized by the number of estimated parameters including group assignment g_i for i=1,…,N. For the model in equation (4) with G groups this criterion can be written as follows:

$$BIC(G) = \frac{1}{Nobs}\sum_{i=1}^{N}\sum_{t=1}^{T}(W_{it} - \hat{\alpha}^{G}_{\hat{g}_i}(a_{it}) - W_{it-1}\hat{\gamma}^{G}_{\hat{g}_i} - X'_{it}\hat{\beta}^{G})^2 + \hat{\sigma}^2\frac{GK_G + N + K_X}{Nobs}\ln(Nobs), \qquad (6)$$

where Nobs is the count of individual/year observations, K_G is the number of parameters that vary across the groups, K_X is the number of group-invariant coefficients of vector X_{it}, and $\hat{\sigma}^2$ is the estimated regression error variance from the model with the largest G among those considered in the model comparison exercise[26]. For both dependent variables we consider GFE models with G=2 and 3, and find that models with G=3 outperforms the models with G=2 using the above criterion. We do not consider models with G>3 due to high computation costs of estimating such models on our very large dataset[27]. Furthermore, a GFE model with a large number of groups will partition the sample into subsamples that may be too small to estimate group-specific treatment effects precisely. Our approach to the choice of the number of the GFE groups in an application with a relatively short panel and a large number of cross-sectional units is consistent with the previous literature, e.g. Guner et al. (2018) who assumed G=2 when modelling lifecycle profiles of self-assessed health of PSID respondents.

[26] We compute $\hat{\sigma}^2$ using the estimates from the GFE model with G=3 as follows:

$$\hat{\sigma}^2 = \frac{1}{Nobs - 3K_G - N - K_X}\sum_{i=1}^{N}\sum_{t=1}^{T}(W_{it} - \hat{\alpha}^{G=3}_{\hat{g}_i}(a_{it}) + W_{it-1}\hat{\gamma}^{G=3}_{\hat{g}_i} + X'_{it}\hat{\beta}^{G=3})^2$$

[27] For example, 1000 iterations of the GFE estimation algorithm for the model in equation (4) with G=3 using data on 17 waves of HILDA (about 146,000 person/year observation) took about 40 hours on 2.9GHz Intel Xeon E5-2690 (8 Cores) computer node.