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GUIDANCE NOTE FOR REPLICATION OF CASE STUDIES

Black Saturday, Tropical Cyclone Oswald, Queensland floods and Toodyay bushfire

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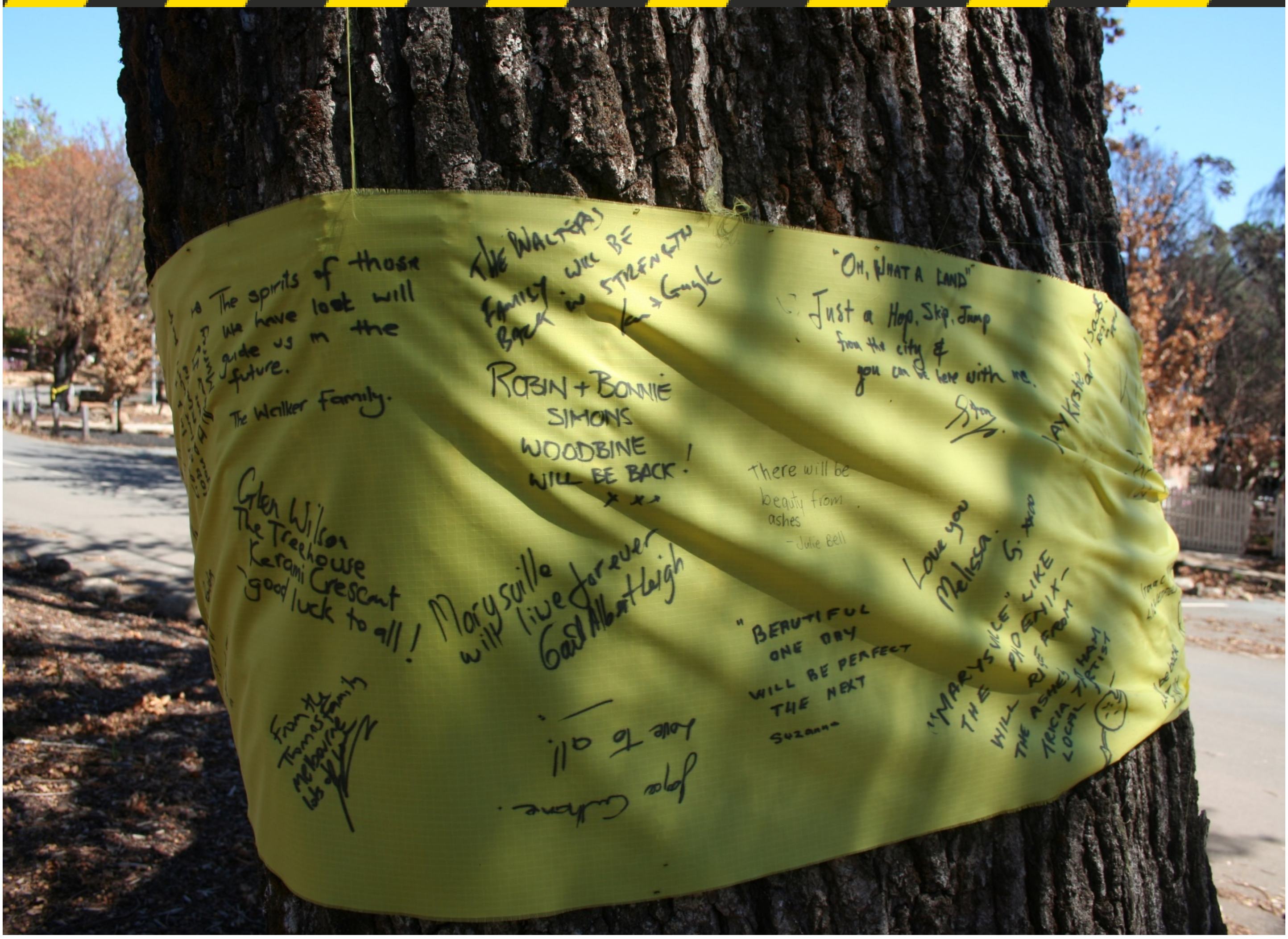

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Cover: Messages from the Marysville community following the 2009 bushfires. Photo: David Bruce

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1 IDENTIFICATION OF TREATMENT AND COMPARISON GROUPS

There are two main considerations in the first step of our methodology:

- identification of disaster-affected areas, the treatment group, which may be measured in different statistical units, such as Statistical Area-Level 2 (SA2) or Local Government Area (LGA); and
- determination of the comparison SA2s/LGAs (i.e., areas that can provide the counterfactual incomes had there been no disaster, the control group).

It is preferable to identify the disaster-affected areas at finer units because this approach will yield more precise estimates for the disaster effects.

Generally speaking, SA2s[1] provide finer units for analysing the disasters that struck regional areas because LGAs are relatively large in regional areas. By contrast, when analysing disasters that hit metropolitan/urban areas, LGAs provide the finer geographic variation while also capturing the jurisdictional differences that might be relevant to disaster impacts.

Before embarking on each case study, we carried out detailed ArcGIS work using Geographic Information System (GIS) tools. In identifying the disaster-hit SA2s or LGAs, we first overlaid the disaster zones on statistical and administrative maps. In particular, we used disaster maps to apply location-based analysis, that is, to vectorise and transform our raster data to map coordinates by using the ESRI shapefile formats provided by the Australian Bureau of Statistics (ABS). Here, it is important to achieve an almost perfect overlap between the raster and our target data in terms of the location, shape, and attributes of geographic features for different statistical units. In the following step, we determined the disaster-hit SA2s or LGAs.

In determining the comparison groups, again it is important to consider the nature of the disaster-hit areas. For regional disasters, it is more appropriate to choose neighbouring areas that are not hit by the disaster but share similar economic, geographic and topographic characteristics with the disaster-hit areas. For disasters that strike metropolitan areas, comparability is likely to be obtained from other metropolitan areas. In Australia, typically, capital cities mimic each other in terms of their economic, demographic and geographic characteristics. For example, Brisbane, Melbourne, Sydney, Perth and Adelaide are located on river banks and have agriculture-based hinterlands.

Below we describe how we specifically determined disaster-hit areas for each case study.

1.1 BLACK SATURDAY BUSHFIRES 2009

The Black Saturday Royal Commission (2009) provides detailed mapping of 12 different pockets of fires, all of which constitute the Black Saturday Bushfires (BSB)

[1] SA2s in Australia host 3,000–25,000 people, with an average population of about 10,000 individuals.

(see chapters 3 to 14 of the Commission report). To illustrate how we constructed our BSB-hit areas, consider Figure 1, which depicts one of these 12 maps. We used these maps to apply location-based analysis.

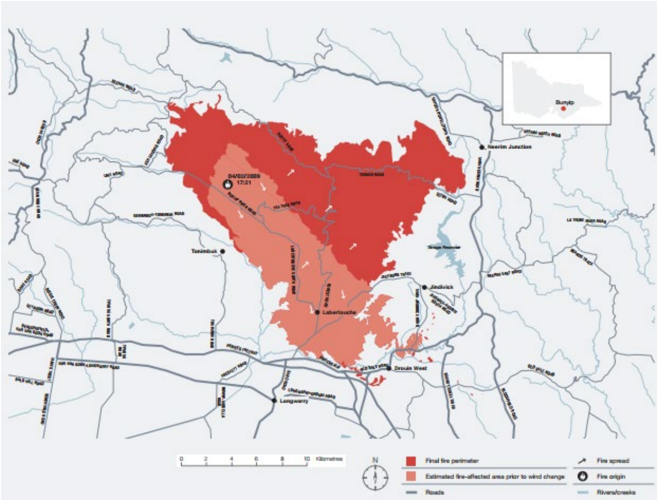

FIGURE 1. THE EXTENT OF THE BUSHFIRE AREA FOR DELBURN. SOURCE: BLACK SATURDAY ROYAL COMMISSION (2009).

Figure 2 presents our georeferencing outcomes for all 12 pockets. The red area shows the burnt SA2s during the BSB. Blue coloured areas represent the unburnt SA2s that share a border with a burnt SA2, and green areas depict the rest of Victoria. Our GIS work produced 37 SA2s that were hit by the BSB (with different shares of burnt areas) and 77 SA2s neighbouring the burnt SA2s that were not burnt by the bushfires. Our demographic profiling analysis using the ABS Census 2006 data found that these two sets of SA2s were comparable in terms of mean income, the shares of most sectors in employment, and average age, education, and gender profile. Our knowledge of the Victorian regional economy also confirmed this comparability.

The detailed availability of the shares of burnt areas in total surface area also provided us with a unique opportunity to compute an *intensity* measure. This percentage ranged from 0.1% to 72.1%.

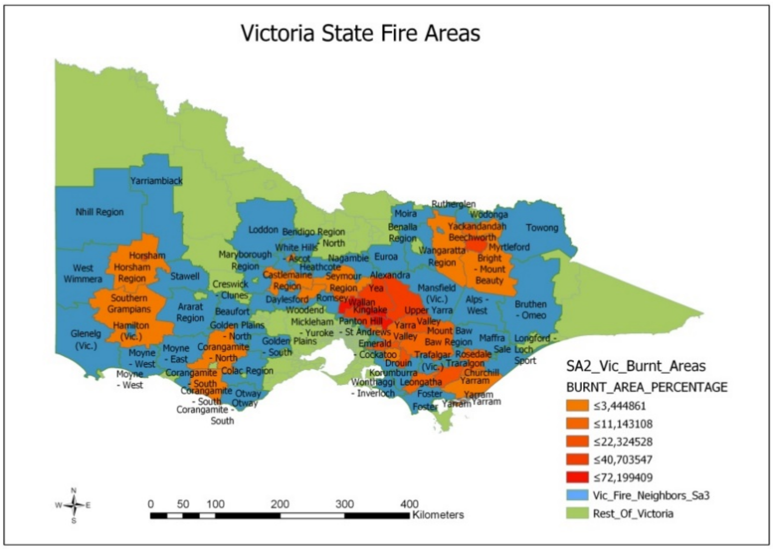

FIGURE 2. THE FIGURE DISPLAYS THE PERCENTAGE BURNT OF A PARTICULAR SA2 FROM THE BSB. THE BLUE PART OF THE MAP REPRESENTS THE SA2 AREAS THAT SHARE A BORDER WITH A BURNT AREA BUT WERE NOT DIRECTLY IMPACTED BY THE FIRES. THE GREEN AREAS SHARE NO BORDERS WITH THE BUSHFIRE-HIT AREAS. SOURCE: OWN CALCULATIONS.

1.2 QUEENSLAND FLOODS 2010-11: A CASE STUDY OF THE BRISBANE RIVER CATCHMENT AREA

The project team worked with the Queensland Reconstruction Authority to identify the treatment group. As a result of these discussions, four LGAs in the Brisbane River Catchment Area (Brisbane, Ipswich, Somerset, Lockyer Valley) were chosen as the treatment group (Figure 3).

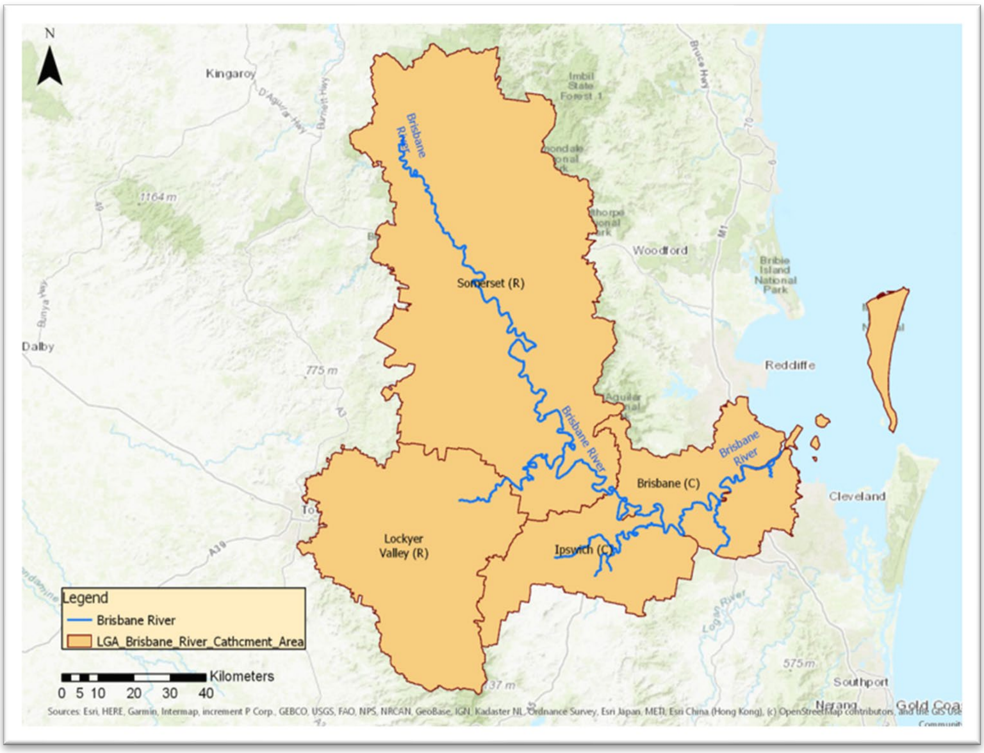

FIGURE 3. THE FOUR LGAS IN THE BRISBANE RIVER CATCHMENT AREA INCLUDED IN THE TREATMENT GROUP.

Eighty-four kilometres of the Brisbane River was flooded. We used this information to identify the LGAs in other Australian capital cities that are within 84km of the coastline, to form our control group. We identified these groups to be the Swan River catchment area, incorporating Perth (capturing 24 LGAs), the Yarra River catchment area, incorporating Melbourne (13 LGAs), the Parramatta River catchment area, incorporating Sydney (13 LGAs), and the Torrens River catchment area, incorporating Adelaide (9 LGAs), with the number is parentheses referring to the number of LGAs in the respective comparison catchment areas. Our demographic profiling analysis using the ABS Census 2006 data showed that these five metropolitan areas have comparable economic and demographic features to the treatment group.

We undertook an additional step in our modelling, called entropy balancing (in short, EBALANCE). This technique helps us 'pick' the individuals from the control group who most closely resemble individuals in our treatment group. We thus include individuals in our control group not just based on whether they have similar incomes as of the pre-disaster period, but also, for instance, that they have similar education, marital status, age, and residential mover/non-mover status, to individuals in our treatment group (Figure 4). [2]

EBALANCE: Individuals with similar characteristics (income levels, education, etc) and residence areas (living along a riverbed)

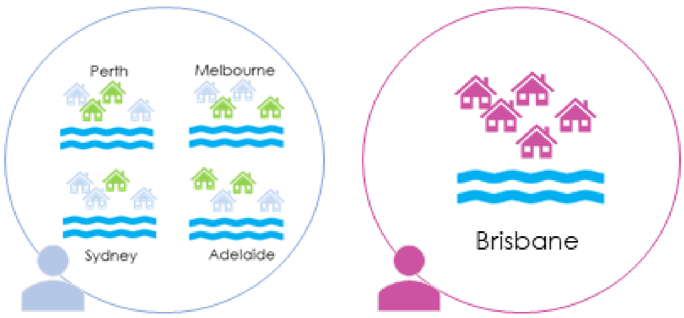

FIGURE 4. ILLUSTRATION OF MODELLING WITH ENTROPY BALANCING.

We also investigated the regional effects on Somerset, Lockyer Valley, and Ipswich separately in order to better understand the effects on the agricultural sector.[3]

1.3 CYCLONE OSWALD 2013: A CASE STUDY OF SMALL BUSINESS OWNERS IN BURNETT RIVER CATCHMENT AREA

Once again the project team worked with the Queensland Reconstruction Authority to identify the treatment and comparison groups. It was important that the treatment group intersected with the trajectory of Tropical Cyclone Oswald and that the control group had similar characteristics to the treatment group except for the exposure to the cyclone (Figure 5). Following discussions, four LGAs within the Burnett River catchment area (QLD) and three LGAs within the Richmond River catchment area (NSW) were agreed upon to be our treatment group and control group, respectively (Figures 6 and 7, respectively). Our demographic profiling analysis using the ABS Census 2011 data showed that these two regional catchment areas have comparable economic and demographic features.

[2] Entropy balancing is a pre-processing procedure that allows researchers to create the most comparable treatment and control groups for the subsequent estimation of treatment effects. That is, it permits selecting individuals from the control group who most closely resemble the individuals in the treatment group, based on a range of characteristics, such as similar education, marital status, age, and residential mover/non-mover status.

[3] The comparison group for this regional analysis included regional LGAs in the outer Perth metropolitan area: Bassendean, East Fremantle, Kalamunda, Mosman Park, Peppermint Grove, Victoria Park, Vincent, Wandering, and York.

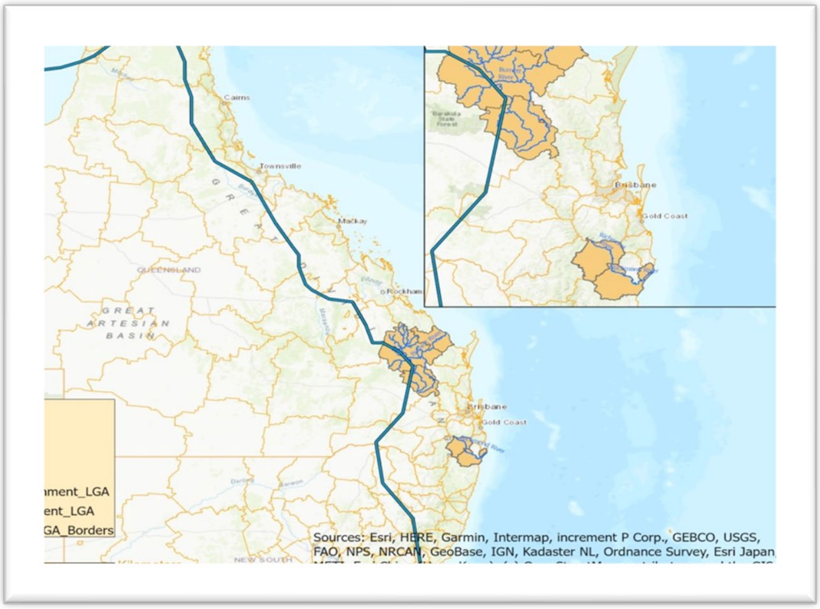

FIGURE 5. TROPICAL CYCLONE OSWALD 2013 TRAJECTORY AND TREATMENT AND CONTROL GROUPS.

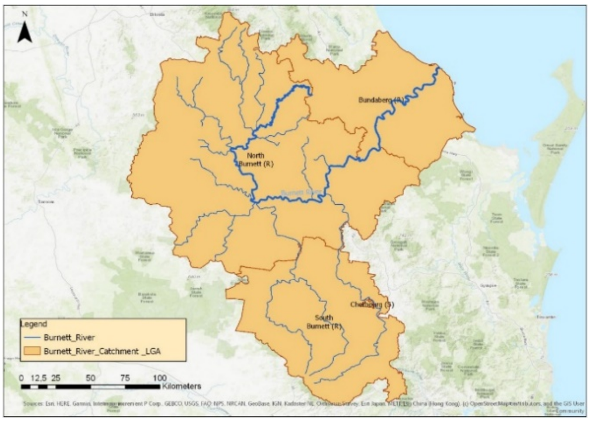

FIGURE 6. BURNETT RIVER CATCHMENT AREA LGAS.

FIGURE 7. RICHMOND RIVER CATCHMENT AREA.

1.4 TOODYAY BUSHFIRE 2009

To investigate the effect of the Toodyay bushfire, we held discussions with the Department of Fire and Emergency Risk Services Western Australia and determined the treatment and control groups. We identified Toodyay SA2 as our treatment group and its neighbouring SA2s, Northam and Chittering, as our control group. Our demographic profiling analysis using the ABS Census 2006 shows that these three SA2s had very similar socio-economic and demographic characteristics in the pre-disaster period.

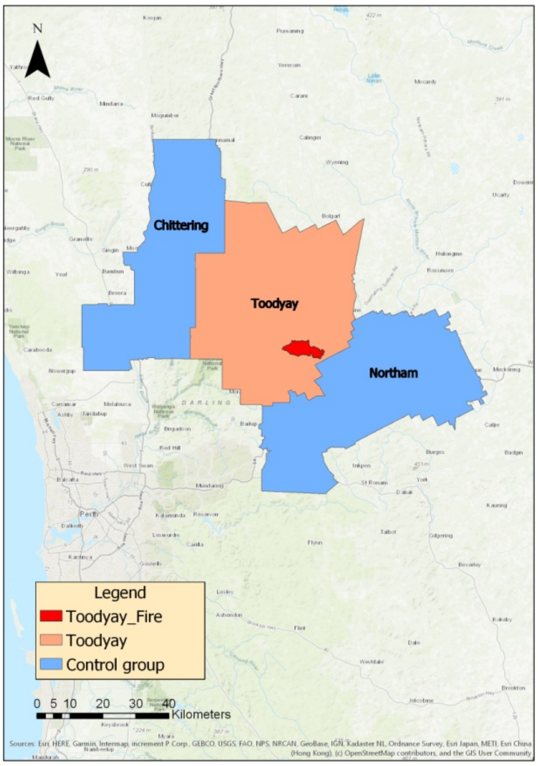

FIGURE 8. TOODYAY BUSHFIRE 2009 TREATMENT AND CONTROL GROUPS.

2 THE DIFFERENCE-IN-DIFFERENCES MODELLING

The second part of our analysis involved a difference-in-difference (DID) estimation based on individual-level longitudinal data. In particular, we adopt the Australian Longitudinal Census Dataset 2006-11-16, which is a 5% representative sample of the Australian population and provides a range of economic and demographic information. Because our dataset provides data for three different time periods, we were able to observe the treatment and control groups before and after the disaster event. This strategy enabled us to identify income effects of the aforementioned disasters for individuals (notwithstanding some limitations, to be discussed below). Figure 9 depicts the estimation method. In a nutshell, our modeling compared the income trajectories of two groups, the disaster-hit group and the disaster-unaffected group, over time. In the hypothetical example provided in Figure 9, the disaster-hit group is faced with a hazard before 2011 and then experiences a decline in income. What would have been their income, had the disaster not struck? This is given by dashed yellow line, which is inferred from the comparison group. In other words, the reduction in income, due to the disaster, is the difference between the actual income that we observe, and the income that the individuals would have earned had the disaster not struck.

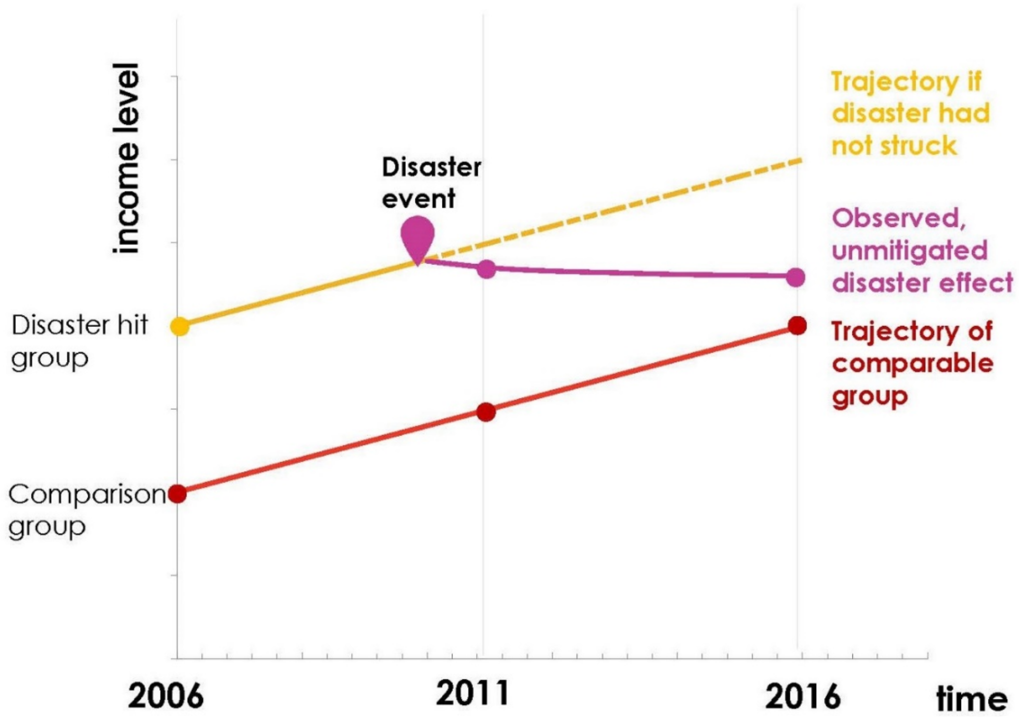

FIGURE 9. DIFFERENCE-IN-DIFFERENCES MODELLING

Lastly, we undertook entropy balancing, a powerful tool to produce comparable samples. We used entropy balancing for the QLD floods 2010–11 and Cyclone Oswald 2013 case studies, because these case studies used comparison groups from other states. This method assists with choosing individuals or small business owners from the control group who most closely resemble those in a treatment group on a range of characteristics, not just income. Here, these characteristics included: education, marital status, age, and mover/non-mover status (i.e. movements to/from residential address, and therefore treatment and comparator regions across 2006, 2011 and 2016). Notably, in order to be practicable, entropy balancing requires a binary measure of disaster impact (e.g., burnt vs unburnt, flooded vs unflooded), which we have in these two case studies.

For the BSB case study, we were not able to use entropy balancing because this case study adopts an intensity measure to gauge the disaster impact (i.e., the share of burnt areas in total surface area). This measure has advantages in itself because some SA2 areas were burnt more heavily than others and the measure captures the variations in disaster severities for a more precise impact estimation. In addition, the limitation of not being able to use entropy balancing in this case study was not very concerning because the comparison groups come from the neighbouring areas within the same state (i.e., Victoria). In the Toodyay bushfire case, we had a very small sample and minimal number of treatment and comparison SA2s, so the entropy balancing was unlikely to be practicable, nor was it deemed necessary.

3 DATA

In this section we explain how we built our panel data. To facilitate the research at the household level, we used the Australian Census Longitudinal Dataset (ACLD), 2006, 2011 and 2016 from the Australian Bureau of Statistics (2018). This dataset brings together a nationally representative 5% sample from the 2006 Census with records from the 2011 and 2016 censuses.

We merged 2006-2011-2016 combined census data with our corresponding treatment and control groups for each case study. This merging enabled us to identify the individuals who resided in one of the burnt areas or neighbouring the burnt areas during the sample period. For the BSB, Toodyay bushfire, and Queensland Floods case studies, our treatment group was formed by individuals who resided in the respective disaster-hit areas prior to the disaster year, that is, an individual's usual address in the 2006 Census. For Cyclone Oswald, this baseline year became an individual's usual address in the 2011 Census.

For our panel data construction we excluded the following individuals from our sample in the following order: (i) individuals that were not within the working age (i.e., those below 15 and those above 65); (ii) individuals who were not in the labour force in 2006 (this is to investigate how the disaster affected the labour force in 2006); and (iii) individuals who reported negative income (coded '1' by ABS, so practically unusable) or chose not to report any sort of income. The second criterion was also motivated by the sectoral disaggregation of employment to follow, because we would not know the sector of an individual if s/he was not in the labour force.

For our sectoral and demographic analysis, we defined our subgroups in the following way. For all the sectors in our study, we investigated the impact of the disaster on individuals who were in a particular sector as of 2006. For instance, if an individual was recorded to work in the agriculture sector in 2006 in the treatment group, we explored their income change in 2011 compared to the groups of individuals who were in the agriculture sector in the control group, regardless of their sectoral movement or change in employment status in 2011.

Our strategy was also the same for each of the demographic groups we studied. For the low-(high-) income group, we compared individuals whose income was in the bottom 33rd (upper 66th) percentile both in the treatment and control groups in 2006. We could thus track these individuals' income changes within these groups and report the differential impact of the disaster on this group. In essence, this definition ensures that we compared the low-income group within the treatment group with the low-income group in the control group. Or, for instance, when investigating the impact of disaster on renters, we ensured that we compared the income of individuals who were renters in 2006 in both our treatment and control groups, regardless of their house ownership status in 2011.

It should be noted that for the Cyclone Oswald case study we classified individuals according to their statuses in 2011, instead of 2006. This is because the closest census before this event is 2011 Census, unlike other investigated disasters.

4 ADDITIONAL CHECKS

In our samples, there was a significant migration between the census dates. It is well known that some of the individuals who were severely hit by the disasters migrated out of the treatment areas. It may also be the case that individuals moved into disaster-hit areas to reap some of the economic opportunities. To account for that, we initially ran our estimates both with the full sample and non-movers sample without controlling for their migration decisions. We computed the following migration indicators: an individual (i) moved out of the treatment area to rest of Australia excluding the control group; (ii) moved into the treatment area from the rest of Australia; (iii) moved into the treatment area from the control area; (iv) moved out of the treatment area into the control area; (v) returned to the treatment area by the 2016 Census, after moving out to rest of Australia in 2011; (vi) returned to control group area by the 2016 Census, after moving out to rest of Australia in 2011; (vii) returned to another treatment area by 2016 Census, after moving out in 2011 to a location in a treatment group; and (viii) returned to another control area by the 2016 Census, after moving out in 2011 to another location in a control area. With the inclusion of migration indicators, however, we introduced a possible endogeneity, as people may have moved in/out following their income decisions. To this end, we report three regression outputs: (i) full sample without migration indicators; (ii) full sample with migration indicators; and (iii) non-movers sample. For the Queensland Floods 201011 case study, we have a neat definition of a non-movers sample. In the census, individuals respond to a question regarding whether they were living in the same address one year ago. Since the Queensland Floods fall into this time-frame, the time-frame is not as broad. For the BSB, individuals who did not move between the 2006 and 2011 Censuses form the non-movers sample, and for the Cyclone Oswald 2013 analysis, individuals who did not move between 2011 and 2016 Censuses form the non-movers sample.

In an alternative setting, we also ran our regressions with a comprehensive set of time-varying control variables, which included marital status indicators (i.e., married, never married, separated, divorced, widowed), education indicators (i.e., 8 or less years of schooling, 9–12 years of schooling, bachelor degree, higher education degree), number of children, employment indicators (i.e., unemployed, employed, not in the labour force), house ownership indicators (outright or mortgage owned). However, as in the migration decision, some of these control variables may have been affected by individuals' income, which may cause a reverse causality problem. Thus, we set our model without these control variables as our benchmark. The estimates with these control variables are not reported but can be provided upon request.

5 IMPORTANT LIMITATIONS

One limitation of the census data is that availability of the data every five years poses a challenge to isolate the effect of a particular shock. This is because there could be other shocks during the five-year period. Ideally we would use annual data, but the good thing about the census data is that it is large and provides a huge number of observations for various disaggregated analyses. We tried to overcome the limitations related to the five-year data by taking several steps, such as checking if there were additional declared disasters. It would be fair to say that our short-term effects are relatively accurate, whereas our long-term effects may be compounded by the effects of other disasters.

Also, unavailability of the 2001 census linked to the 2006 census made it difficult to track how individuals fared before the disasters, which was important to ensure that the parallel trends assumption was satisfied. This limitation adversely affected the causal inference in the BSB, Toodyay fires and Queensland floods case studies. By contrast, for the Cyclone Oswald 2013 case study, we had the 2006 Census linked to 2011, which enabled us to check if the parallel trends assumption was satisfied. In the former three case studies, we carried out several demographic profiling checks, which showed that the economic structures of treatment and control groups were largely similar before the disasters. Entropy balancing also helped in the case of Queensland floods 2010–11. However, a caution is in order to interpret the results as strong correlations rather than causations.

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