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Are You Okay? Effects of a National Peer-Support Campaign on Mental Health

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Are You Okay? Effects of a National Peer-Support Campaign on Mental Health*

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Abstract

National public health awareness campaigns that emphasize peer-to-peer support are increasingly adopted, but evidence on the effects of peer-based programs at scale remains limited. Using quasi-experimental methods, we examine whether the prominent nationwide "R U OK? Day" campaign affects short-term mental health outcomes in Australia. Leveraging survey and administrative data, we find R U OK? Day leads to a 4% of a standard deviation increase in self-reported mental wellbeing, with the effect particularly pronounced among middle-aged males who experience a 9% of a standard deviation increase. We find no detectable effects on mental health care utilization, and we detect no statistically significant changes in suicide-related deaths in the short run, though the mortality outcomes are rare and power is limited. Our results underscore that peer-based campaigns can improve mental wellbeing, especially for high-risk groups, and point to a distinction between short-run psychological responses and outcomes that require behavioural follow-through.

Keywords:
Public Health Awareness Campaigns; Peer-to-Peer; Mental Health; Program Evaluation; Suicide Prevention.

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Data Availability Statement

The data used in this article cannot be redistributed by the authors. The empirical analyses combine restricted-access administrative, survey, and social media data: the Australian Bureau of Statistics Person Level Integrated Data Asset (PLIDA), the Household, Income and Labour Dynamics in Australia (HILDA) Survey, and Twitter/X data and are obtained under license and/or data-use agreement. These data are governed by third-party access conditions, confidentiality obligations, secure-access procedures, platform terms, and/or contractual restrictions. Accordingly, the authors cannot deposit the raw data, derived datasets, intermediate datasets, or any de-identified substitute datasets in a public repository.

To support reproducibility within these constraints, the authors will deposit the full analysis code, data construction scripts, table and figure scripts, documentation, and a README in the Journal of Marketing Dataverse. The README will identify each data source, describe the access procedures for qualified researchers, document the required inputs, and explain the workflow needed to reproduce the results for researchers who obtain authorized access to the same data. The code is designed to run end-to-end conditional on authorized access to the required source data. No synthetic data are provided because a they would risk misrepresenting the structure, dependence, and moments of the restricted underlying data.

Large-scale public health campaigns increasingly rely on advertising to encourage people to take specific actions in their everyday lives. Rather than marketing a conventional product or service, these campaigns use marketing tools to promote responses that range from immediate interpersonal engagement to more sustained behavioural change intended to improve population health. Such interventions are grounded in the idea that campaign exposure can shift beliefs, attention, or motivation and, in turn, translate into downstream psychological and behavioural responses (Abroms and Maibach 2008; Wakefield, Loken, and Hornik 2010). Understanding which parts of this sequence are affected by campaign exposure, and which are not, is central to the design and evaluation of public health campaigns.

This issue is particularly relevant in mental health, where governments and non-profit organizations have increasingly invested in nationwide awareness campaigns intended to improve population mental wellbeing and reduce suicide risk (Knox, Conwell, and Caine 2004; WHO 2012). While varying in implementation, a common component of many initiatives is a set of peer-based actions: strengthening informal support networks by encouraging people to initiate conversations, provide social support, and, when appropriate, encourage professional help-seeking. This growth reflects the scale of the underlying burden. Mental disorders account for approximately 16% of global disability-adjusted life years, and suicide claims more than 700,000 lives each year (Whiteford et al. 2013; World Health Organization 2021; GBD 2019 Mental Disorders Collaborators and others 2022). Given their population-scale ambitions, the success of such campaigns depends on whether exposure translates into downstream outcomes consistent with these aims. Yet, despite worldwide roll-out, there is limited evidence directly connecting these campaigns to improved health outcomes (Mann et al. 2005; Reininghaus et al. 2024; McGinty et al. 2024).

This paper evaluates the mental health impacts of R U OK? Day, Australia's leading nationwide mental health promotion and suicide-prevention campaign. The organisation's published mission is to create a world in which people are "connected and protected from suicide," and its strategy centres on strengthening informal community support as a form of early intervention (R U OK? 2016). The campaign promotes peer-to-peer engagement through four recommended steps: (i) ask friends, colleagues, and neighbours, “Are you OK?”; (ii) listen to responses with an open mind; (iii) encourage action if needed, including encouraging people to visit a mental health professional; and (iv) check back in with them over subsequent weeks. Campaign materials emphasise that these steps are intended to support people who may be experiencing difficulties before they reach crisis. This setting allows us to provide causal evidence on whether such campaigns translate into downstream psychological and behavioural outcomes. Although broader shifts in awareness or social norms may occur as secondary effects, R U OK Day’s primary theory of change is grounded in immediate interpersonal connection and timely support, rather than gradual attitudinal change over longer horizons.

We study the impact of the campaign across outcomes that align with its stated objectives and documented theory of change. R U OK? Day aims to strengthen informal support networks, prompt early conversations about difficulties, and encourage appropriate help-seeking when needed, with the overarching goal of reducing suicide risk. Our empirical outcomes therefore capture changes in psychological wellbeing, mental health care use, deaths by suicide and accidental poisoning, and suicide-related internet search activity. Together, these measures allow us to test whether the campaign generates short-run outcomes consistent with its intended pathways. We focus on short-term outcomes because the campaign explicitly encourages immediate actions, such as initiating social connection or seeking professional help, which can influence mental wellbeing, healthcare utilization and suicide risk in the near term. We do not evaluate potential long-run shifts in social norms, awareness, or suicide risk. These outcomes are important to the campaign’s broader objectives, but they are difficult to attribute reliably to a recurring national campaign. Our design is instead suited to estimating short-run responses around the focal campaign day, when campaign attention is highest and the encouraged actions are most immediate

We draw on three complementary sources of data spanning nine years between 2011 and

2019, during which the campaign's core messaging remained stable: (i) a nationally representative survey capturing self-reported mental wellbeing; (ii) daily administrative records on mental health service use and suicide-related deaths; and (iii) Google Trends data on suicide-related search activity. These sources provide daily measurements, covering periods before and after each annual campaign. Our empirical approach is a temporal difference-in-differences strategy, which is a quasi-experimental method commonly used to estimate causal effects in marketing research (Goldfarb, Tucker, and Wang 2022; Sim et al. 2022; Liaukonytė, Tuchman, and Zhu 2023). This approach identifies short-term changes in mental health outcomes by comparing the four weeks immediately before and after the focal day of the campaign during high-activity years to baseline years with lower campaign activity. We estimate the intention-to-treat (ITT) effects of scaling the campaign from a small, low-visibility initiative to a large, high-visibility program.

We find that scaling up the campaign increases self-reported mental wellbeing by 4% of a standard deviation, equivalent to moving from the 50th to 53rd percentile of mental wellbeing. This effect is concentrated among men aged 25–49, for whom wellbeing increases by 9% of a standard deviation. This is noteworthy because this sub-population is known for having insufficient mental healthcare uptake (Meadows et al. 2015; Yousaf, Grunfeld, and Hunter 2015). In contrast, we find no evidence of short-run effects on mental healthcare utilisation or suicide-related search behaviour in the weeks following the campaign, and we detect no statistically significant changes in deaths by suicide or accidental poisoning—though these mortality outcomes are rare and our power to detect even moderate short-run effects is limited. The absence of detectable effects on mental healthcare use is important because encouraging action, including connection to professional help where appropriate, is part of the campaign's stated theory of change. In this respect, the campaign did not achieve one of its intended short-run objectives. A battery of robustness and falsification tests supports our causal interpretation of these estimates.

While R U OK? Day is grounded in public health objectives, its design and reach align with a broader class of advertising-centric social marketing interventions that seek to influence individual behaviour and downstream social outcomes (Andreasen 2002; Chandy et al. 2021; Kees and Vallen 2024; Stremersch 2008; Davis, Grewal, and Hamilton 2021). Our findings show that scaling a campaign that promotes a prescribed set of peer-support actions can improve population mental wellbeing. However, effects on behavioural outcomes—such as mental health service use and suicide-related outcomes—are not detectable in the short run. This divergence echoes a persistent challenge in marketing: psychological responses to persuasive communication do not necessarily translate into behavioural change (Rothschild 1999; Schamp et al. 2023). More broadly, the results suggest that campaigns built around prescribed peer-support actions can shift population mental wellbeing even when effects on behavioural follow-through are constrained or difficult to detect. This distinction is likely to extend to other social marketing campaigns that use advertising to promote prescribed actions, where behavioural follow-through can be constrained by time, effort, or access to complementary services. Consistent with evidence that persuasion-based public health campaigns often produce modest behavioural changes (Snyder et al. 2004), our results highlight the value of evaluating both psychological outcomes and behavioural follow-through when assessing campaign effectiveness.

RELATED LITERATURE

Our work connects to several strands of literature. Web Appendix A.1 summarizes the most closely related studies organizing them by health focus, empirical method, intervention type, and primary outcomes to clarify how our contribution extends prior work. First, our work is related to literature on marketing's role in creating a better world via its contribution to public policy and societal wellbeing (Chandy et al. 2021; Kees and Vallen 2024). An emerging literature at the intersection of marketing and mental health has shown that stigmatising attitudes toward mental illness reduce treatment engagement (Kemp, Davis, and Porter III 2023), that depression correlates with shifts in household shopping behavior (Meckel and Shapiro 2025), and that direct-to-consumer antidepressant advertising can increase prescriptions and reduce absenteeism (Shapiro 2022). Our study contributes to this strand of literature by providing large-scale causal evidence on the psychological and behavioral effects of a national mental health awareness campaign.

Second, our work engages with the broader health marketing literature, which connects message design to behavioral intentions and outcomes. Keller and Lehmann (2008)'s meta-analysis of health communication campaigns in lab settings demonstrates that emotional and tailored appeals can effectively influence behavioral intentions across domains like smoking cessation and disease prevention. Building on this foundation, recent studies have examined advertising's impact on actual health behaviors across diverse contexts, including smoking cessation (Avery et al. 2007), birth rates (Kim and KC 2020a), hospital choice (Kim and KC 2020b), social distancing during the COVID-19 pandemic (Ghosh Dastidar, Sunder, and Shah 2023), prescription medication use (Hristakeva 2025), and high tech medical procedures (Yoon and Kim 2024). These studies primarily focus on advertising placed by commercial firms, whereas Athey et al. (2023) take a different approach by evaluating the impact of social media ads deployed by public health organizations on COVID-19 beliefs and vaccine uptake. Our study extends this literature by evaluating a peer-support–oriented campaign that is neither firm-initiated nor tailored to a specific demographic by leveraging high-frequency national data to capture both psychological wellbeing and behavioral responses to these messages.

Finally, our work relates to the small literature on the effects of health awareness and mass media campaigns on health or behavioural outcomes (Wakefield, Loken, and Hornik 2010). Jacobsen and Jacobsen (2011) examined the effect of breast cancer awareness month on diagnosis rates and found a significant positive influence in the years when breast cancer advocacy was expanding rapidly but had little impact in later years. Similarly, Anderson (2010) examined the effect of a campaign to deter youth from methamphetamine use and found it had no discernible impact. More broadly, evaluations of health awareness campaigns have commonly measured outcomes in terms of self-reported awareness or online activity (e.g., Google searches or Twitter activity relating to the campaign), rather than health outcomes, with most finding positive associations (Vernon, Gottesman, and Warren 2021).[1] Our findings add to this literature by reporting impacts on mental health outcomes and behaviors at both the individual and population level.

THE R U OK? DAY CAMPAIGN

Anxiety and depressive disorders are among the top five contributors to disease burden in terms of age-standardized disability adjusted life years, costing the Australian healthcare system approximately one billion dollars annually and leading to an estimated twelve billion dollars in yearly productivity losses (Lee et al. 2017; McCallum et al. 2018; Schofield et al. 2019; Australian Institute of Health and Welfare 2024a). Suicide is the leading cause of death among young Australians aged 15–44, accounting for approximately 25% of deaths annually (Australian Institute of Health and Welfare 2021). In the broader population, the incidence of suicide is 12 deaths per 100,000, which is comparable to North America and Western Europe (Australian Institute of Health and Welfare 2022b; World Health Organization 2021).

Existing work in psychiatry and public health highlights the importance of timing and interpersonal contact in suicide prevention. While mental disorders elevate suicide risk, most individuals with such conditions do not exhibit suicidal behavior (Borges et al. 2008), and suicide itself is shaped by a complex interplay of neurobiological, psychological, and environmental factors (Turecki and Brent 2016). Among these, acute stressors, such as recent adverse life events, can act as powerful triggers. A recent meta-analysis found that experiencing a negative life event in the past month was associated with a tenfold increase in suicide risk, comparable to having a diagnosis of depression or a history of self-harm

[1] Several studies from the public health literature have evaluated suicide prevention campaigns, often targeted to specific population groups (e.g., veterans, police force, college students), and generally find them to be positively associated with self-reported awareness and knowledge of suicides (for systematic reviews see Dumesnil and Verger 2009; Pirkis et al. 2019; Torok et al. 2017). Studies vary in quality, but many do not involve a comparison group or account for seasonal trends or unobserved factors.

and greater than the risk associated with a previous suicide attempt (Favril et al. 2022). Further, the time between the emergence of suicidal thoughts and attempts is often short. Deisenhammer et al. (2009) report that nearly half of suicide attempts occur within ten minutes of ideation, and almost 90% within four weeks. Many attempters report being open to contact from close others in that window, suggesting that even brief peer engagement may offer a critical opportunity for intervention. This insight underpins the design of R U OK? Day, which equips the public to recognize warning signs, reach out to someone who may be struggling, and encourage action or follow-up support. The campaign's emphasis on peer-to-peer contact reflects growing recognition that non-clinical actors may play a meaningful role in suicide prevention, especially when time is limited and distress is recent (Mann et al. 2005).

In response to the high rates of mental illness and suicide, and family experience with suicide, Australian advertiser Gavin Larkin founded the non-profit suicide prevention organisation, 'R U OK?', in 2009. The organisation encourages people to connect and have conversations with others who may be struggling, long before they reach crisis point. Campaign efforts are concentrated around a national day of action called 'R U OK? Day', which occurs on the second Thursday of September each year. On this day, the general population is invited and encouraged to reach out to others who might be experiencing personal difficulties, starting with asking, 'Are you ok?'. Through traditional offline marketing strategies and online promotional material, people are provided resources and tips on how to have meaningful conversations that 'could change a life.' People are advised to ask; listen without judgment; encourage the person to take action, such as seeing a mental health professional; and follow up with the person. R U OK? Day is marked in many workplaces, organisations, and schools by activities and events to raise awareness and distribute resources.

The primary channel through which R U OK? Day could improve mental wellbeing and prevent suicides is through the peer-to-peer support, which the campaign focuses on promoting. People are empowered and encouraged to check in with their peers and ask them what is troubling them. By doing so, people are providing emotional support to their peers, which in the very short-term, can increase perceived connectedness and a sense of belonging and reduce feelings of hopelessness (Van Orden et al. 2010). Peers can also encourage help-seeking behavior, and provide support in making health appointments. Increased engagement with professional health services could lead to positive improvements in mental wellbeing and reduce self-harming behaviors. Additional potential channels are increased awareness of mental health issues, improved public discourse regarding mental illness, and provision of information regarding mental health services. In turn, this can improve mental wellbeing and reduce psychological harm (Yanos et al. 2020).

Two existing studies provide evidence on how people are potentially impacted by the campaign. Using cross-sectional online survey data collected in the two weeks following R U OK? Day in 2014, Mok et al. (2016) found that 66% of the 2000 respondents were aware of R U OK? Day. Of those aware of the day, 19% reported doing something as part of R U OK? Day, of which the most common responses were asking if others were OK via face-to-face or digital channels. Those aged 25–34 were most likely to participate. The study also found that among those aware of R U OK?, 41% stated they believed the campaign made people more likely to seek professional help for things troubling them, and 52% thought it reduced stigma associated with seeking professional help. Negative perceived impacts were low, with 2% believing the campaign made people *less* willing to seek professional help and 3% believing it *increased* stigma. A follow-up study by Ross and Bassilios (2019) after the 2017 R U OK? Day showed that awareness of the campaign increased to 78%, and that overall participation in an R U OK? Day activity increased to 32% of individuals who were aware. In addition to asking if others were OK, common activities included looking into professional services for oneself or someone else. Though not causal, the studies suggest that R U OK? Day prompted meaningful engagement—reaching around 13% of the population in 2014 and nearly 25% by 2017.

We document the approximate intensity or reach of the campaign each year, through

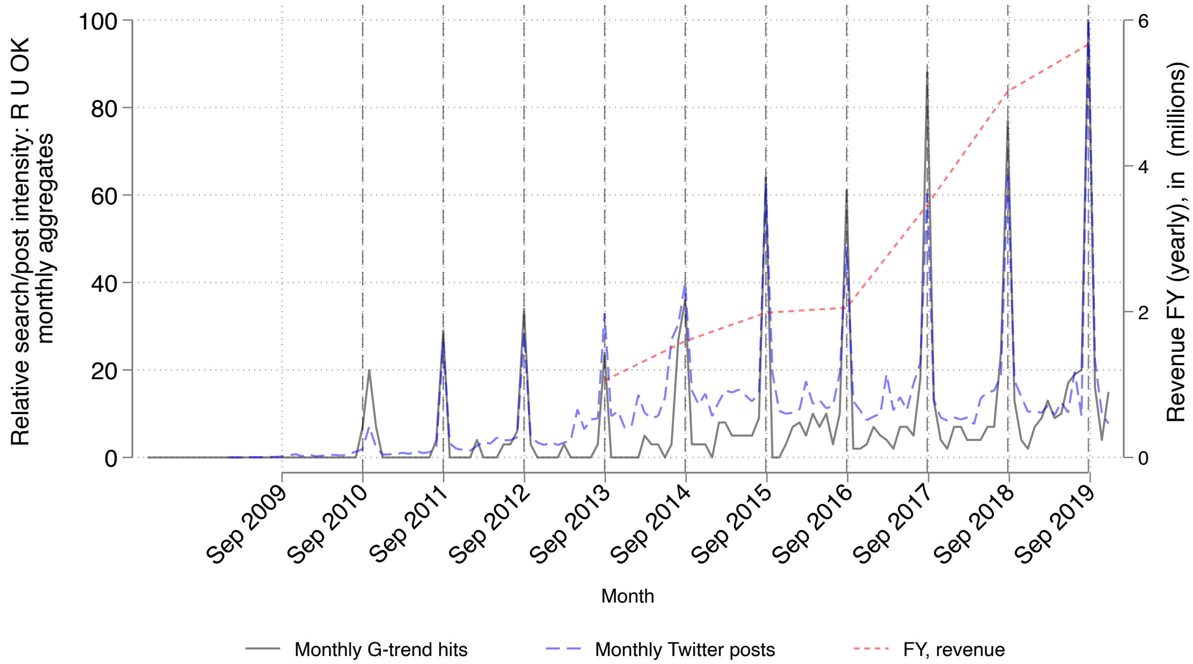


Figure 1: Monthly Google searches "R U OK", Twitter posts, and financial year revenue of the R U OK organisation

Note: Displays the monthly "R U OK" searches on google, twitter posts (divided by maximum monthly posts, 59,968), and the yearly revenue from the organisation's financial reports.
Source: Google Trend search data 2009-2019, Twitter data 2009-2019, R U OK charity financial reports 2013-2019, own calculations.

records of online engagement (from Google searches and Twitter posts) around R U OK? Day. Figure 1 shows that both Google searches (grey line) and Twitter posts (blue dashed line) of "R U OK?" spike each year on R U OK? Day from 2010 to 2019, which indicates that the intensity of the campaign is concentrated on the national day of action. It also shows the intensity of online activity relating to R U OK? Day increased over time, in line with the increase in annual revenue generated by the charity (red dotted line). This figure supports the survey findings by Ross and Bassilios (2019) that awareness of and participation in R U OK? Day increased over time.

Web Appendix Figure C.1 plots the daily internet search intensities for R U OK? Day along with two other large health awareness campaigns, Movember and World Mental Health Day, for the 2019 calendar year. The figure highlights that R U OK? Day's peak search intensity is the largest among the three, providing evidence that it is the most extensive mental health awareness campaign in Australia, albeit one that captures attention over only a short time span.

DATA

In this section, we describe the three primary data sources used in our analysis. The Household, Income, and Labour Dynamics in Australia (HILDA) Survey provides a measure of self-reported mental wellbeing. The Australian Bureau of Statistics' Person Level Integrated Data Asset (PLIDA) includes administrative records on general practitioner (GP) visits and prescribed medications to measure mental healthcare use, and cause of death files to identify deaths from intentional self-harm and accidental poisoning. Google Trends data measures search activity related to suicide planning and prevention. Web Appendix B provides further details on all data sources, including access conditions and coverage.

HILDA Surveys

The Household, Income, and Labour Dynamics in Australia (HILDA) survey is an ongoing annual household-based longitudinal study that commenced in 2001. Each year, all household members aged 15 years and over are surveyed on topics such as earnings, job transitions, family life, and education.[2] While its primary focus is on labor market and income dynamics, the survey also includes detailed measures of self-reported health and well-being. The annual surveys are primarily administered in August, September, and October, coincidentally coinciding with the weeks surrounding R U OK? Day in September. Our analysis focuses on surveys completed within 28 days either side of R U OK? Day to capture the short term effects of the campaign.[3] We use data from 2011 to 2019, spanning nine years during which R U OK? Day was active (see Figure 1).

[2]See Watson and Wooden (2021) for a description of the HILDA survey questionnaires and the data collected in waves 1 to 20.

[3]This surveying pattern is displayed in Figure C.2 of the Web Appendix.

In each HILDA wave, household members self-complete the Short-Form-36 Health Survey Questionnaire (SF-36), a popular instrument for evaluating health-related quality of life.[4] Using responses to nine questions from the SF-36, we construct a mental wellbeing index using principal components analysis. These questions measure mental wellbeing-related symptoms by asking people how frequently during the past four weeks they have had certain feelings, such as being "full of life", "nervous", "so down in the dumps that nothing could cheer you up", and "happy". The six response options for each question range from "all of the time" to "none of the time." The nine questions and the associated weights from the principal component analysis are shown in Table B.2 of the Web Appendix.

Like other longitudinal surveys, HILDA experiences sample attrition associated with disability and ill-health (Pudney and Watson 2013). For the sample years used in this study, we estimate that a one standard deviation decrease in mental wellbeing is associated with a 0.9 percentage point decrease in the likelihood of participating in the next survey wave. While this attrition does not invalidate our identification strategy, it may limit the generalizability of our findings.

Administrative Health Data

We also use the Person-Level Integrated Data Asset (PLIDA), which is a dataset developed by the Australian Bureau of Statistics (ABS) that includes linked de-identified individual records from multiple government agencies. We aggregate PLIDA records to create national daily counts for two mental healthcare measures and two mortality measures over the 28 day window on either side of R U OK? Day for the years 2011 to 2019.

Mental Healthcare Use. Our primary measure from PLIDA is the daily national count of general practitioner (GP) visits resulting in a mental health treatment plan (MHTP).

[4]The self-completion questionnaire can be completed on a different day than the face-to-face interview and mailed back; however, approximately 70% of respondents complete it within three days of the interview. In our analyses, we use the interview date rather than the self-completion date, because the latter is likely to be more non-randomly selected than the former. For example, an occurrence of ill-health may not be sufficient to postpone the scheduled face-to-face interview, but may delay the completion of the self-completion questionnaire.

MHTPs are the most common first step in obtaining professional mental health support in the Australian system. GP visits are subsidized (partially or fully) by Medicare, Australia's universal public health insurer. A MHTP outlines treatment needs and goals, and provide patients access to subsidized therapy from mental health professionals. Our second mental healthcare measure is the daily national count of prescriptions filled for psychiatric medications. These prescriptions include all antidepressants, psychostimulants, antipsychotics, anxiolytics and hypnotics and sedatives, which are commonly used to treat conditions such as psychosis, mood disorder, anxiety, severe insomnia, depression and impaired cognitive abilities. Unlike MHTPs that initiate a pathway to care, prescriptions also reflect ongoing or acute management of mental health conditions, and can be provided by GPs, psychiatrists, and other specialist doctors. The daily counts of MHTPs and prescription medications allow us to assess whether R U OK? Day influences healthcare-seeking behavior. While we capture the most common pathways to professional mental healthcare (Australian Institute of Health and Welfare 2024b), we do not observe smaller providers of mental healthcare, such as community services, privately-funded services, and not-for-profit support services and helplines.

Intentional Deaths & Accidental Poisonings. We utilize the number of deaths per day in Australia, separately for two main cause-of-death classifications – intentional self-harm (suicide) and accidental poisoning – coded according to the ICD-10 classification (WHO 2019, see also Web Appendix B). Determining if an injury was intentional is not always straightforward, but most injury deaths in Australia are certified by a coroner. Accidental poisonings mainly involve pharmaceutical drugs (both prescribed and illegally obtained), but also include poisonings involving alcohol, carbon monoxide, heroin, and other substances (Australian Institute of Health and Welfare 2022a). Our data come from the Cause of Death Unit Record File, which contains characteristics of the person who died (e.g., age and sex) and characteristics of their death (e.g., cause, date, and place where the person usually lived).

Suicide Related Internet Search

Completed suicides are only the 'tip of the iceberg' of self-harm events.[5] For every suicide, many more people plan or attempt suicide. In 2020, an estimated 12.2 million American adults seriously thought about suicide, 3.2 million planned a suicide attempt, and 1.2 million attempted suicide according to the Substance Abuse and Mental Health Services Administration (SAMHSA 2021). Data detailing suicide plans and attempts are rare. Our approach leverages Google internet search data for terms related to suicide planning and prevention. The internet is a common source of information about mental health and suicide (Parker et al. 2017) given the stigma surrounding these issues (Bharadwaj, Pai, and Suziedelyte 2017) and that searching for information online is convenient, anonymous, and time-efficient.[6]

Google Trends provides normalized daily search volumes of terms by geographical region, period, and category. We collect daily Google Trends data for searches at the topic- and search-level originating in Australia for the eight-week period immediately surrounding (and including) R U OK? Day for each of the nine years from 2011-2019.We collect data on the Google Trends topic "suicide" in addition to two more specific search queries. In Google Trends, a "topic" is a predefined category that includes a group of related search queries sharing the same underlying concept across languages, spellings, and phrasings. The "suicide" topic thus captures a broader set of searches than a single keyword query. For example, it may reflect people searching for general information on suicide (e.g., for study or work purposes), people seeking help for suicidal thoughts, or individuals looking for information on methods of suicide. We create two more-specific measures using search-term queries: (i) searches related to suicide prevention ("lifeline" + "help suicide" + "hotline suicide" +

[5] The iceberg model of self-harm (Geulayov et al. 2018) divides self-harm events into: (i) fatal self-harm, an overt and uncommon event (tip of the iceberg); (ii) self-harm that results in a presentation to clinical services, an overt and common event; and (iii) self-harm that occurs in the community, a common but largely hidden event (submerged part of the iceberg).

[6] A similar approach was used by Tefft (2011) to measure depression and anxiety, Frijters et al. (2013) to measure alcohol abuse, and Brodeur et al. (2021) to measure a wide range of mental well-being dimensions (including suicide).

"suicide hotline"); and (ii) searches related to suicide planning ("commit suicide" + "how to suicide" + "painless suicide" + "quick suicide" + "suicide methods"). The prevention search terms were chosen to reflect the help-seeking behaviour of Australians contemplating suicide (or their friends and families), and the planning search terms reflect information-seeking on suicide methods (see Till et al. 2020).

The data returned via Google Trends are separately normalized for each year in our sample. This necessitates a re-scaling approach so that each of the nine time-series is comparable. We follow the scaling approach used in Brodeur et al. (2021), which we detail for our context in Web Appendix B. The resulting outcome variables measure the relative likelihood that a random Australian Google user on a particular day will complete a search for information about suicide.

Data Summary & Model-Free Evidence

We present summary statistics for each outcome variable in Table 1 for our sample period of 28 days before and after the R U OK? Day date in 2011-2019.[7] Within the HILDA sample, the mean of the mental wellbeing index is 67.50, and there is substantial variation in mental wellbeing across individuals (SD: 17.23).[8] On average, 5,880 (SD: 3,551) mental health care treatment plans are initiated per day, and 74,055 (SD: 26,565) prescriptions are filled for mental health-related medications. The mean numbers of deaths due to intentional self harm and accidental poisonings per day equal 7.7 and 3.7, respectively.

Figure 2 presents descriptive evidence of R U OK? Day's relationship with mental wellbeing over HILDA's annual interview period. We plot the average value of the outcome variable for August, September and October, i.e. the months before, during and after the R U OK? Day campaign.[9] The figure shows that average mental wellbeing values remain

[7] A more detailed table of descriptive statistics that includes control variables can be found in Web Appendix Table C.1

[8] See Web Appendix Figure C.3 for the full distribution of the mental wellbeing index.

[9] 95.5% of the HILDA Survey interviews during our sample years are completed in August, September and October. It is for this reason that Figure 2 includes raw monthly averages for these months only, cf. Web Appendix Figure C.2.

Table 1: Descriptive statistics for Mental Health Outcomes

Variables	Mean	SD
A. Data Source: HILDA		
Mental wellbeing principal component	67.50	17.23
B. Data Source: PLIDA		
Healthcare Utilization.		
Mental health treatment plans	5,880	3,551
Mental health prescriptions	74,055	26,565
Deaths.		
Intentional self harm	7.65	2.80
Accidental poisonings	3.72	2.10
C. Data Source: Google Trends		
Suicide topic	17.89	7.56
Suicide prevention	33.98	17.65
Suicide plan	21.23	17.56

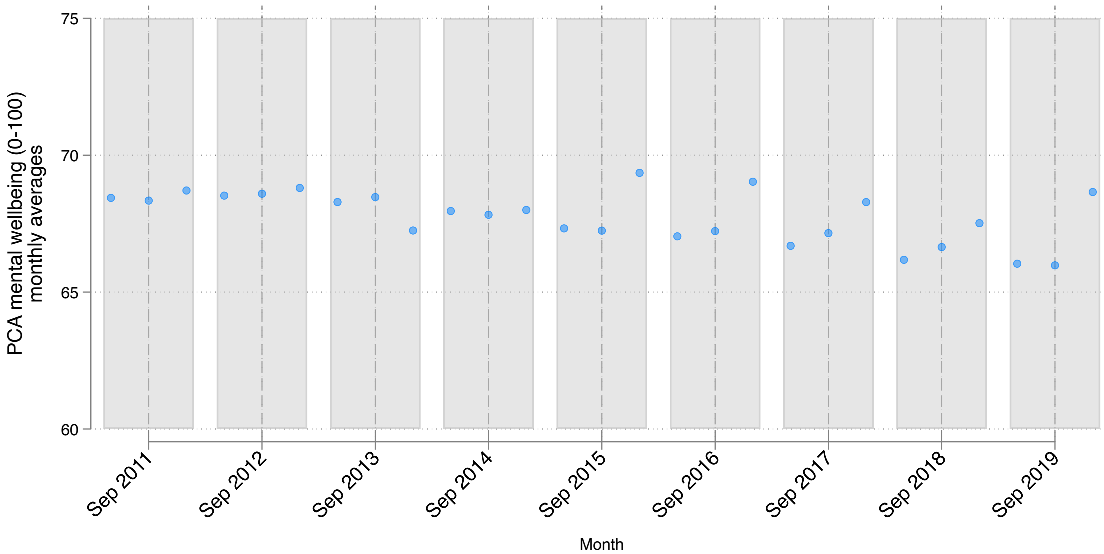
Notes: Table presents means and standard deviations of our main outcome variables. Panel A for the HILDA survey, B for the administrative health care use and death data, and C for the Google Trend search intensity. The HILDA data corresponds to the average of the principal component of the mental wellbeing questions over the survey period, i.e. ± 28 days surrounding R U OK? day. The PLIDA averages are based on the daily counts of the respective variables in the same time periods and Google Trends averages are the daily relative search intensity standardised over the years following (Brodeur et al. 2021).

Source: HILDA 2011-2019 (v19), PLIDA 2011-2019, Google Trends search data 2011-2019.

relatively stable across these three months between 2011 and 2014. This pattern shifts from 2015 onwards, when average mental wellbeing shows a consistent increase following R U OK? Day, suggesting a potential campaign effect.

Figure 3 reports the average monthly number of MHTPs, prescriptions filled and number of suicides across the sample years. Looking across measures of healthcare utilization in panels (a) and (b) shows an increasing trend in the uptake of MHTPs and prescription medication over the sample years but no discernible pattern in utilization across months immediately around R U OK? Day within years. Similarly, Panels (c) and (d) reveal no clear patterns for intentional deaths and accidental poisonings in the months around R U OK? Day. Prima facie, one might interpret the evidence in Figures 2 and 3 as evidence that the campaign only impacts mental wellbeing in later years with no impacts on healthcare

Figure 2: Model-Free Evidence of the Impact of R U OK? Day - Mental Wellbeing.



Notes: Figure shows the raw monthly averages of the principal component of elicited mental wellbeing from the HILDA survey. The gray bars indicate the survey years; each first dot represents August, the second September (which is the month in which R U OK? Day takes place), and the third October.

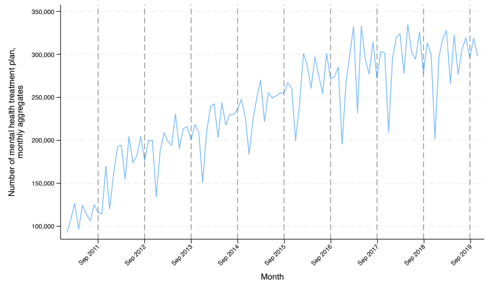
Source: HILDA survey 2011-2019 (v19), own calculations.

utilization or suicides. However, this interpretation requires caution as underlying trends and seasonality in the data need to be accounted for in our analysis.

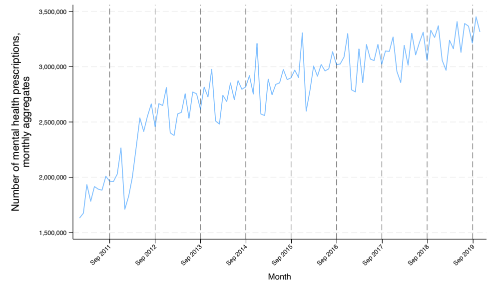
ESTIMATION STRATEGY

We employ a time-based difference-in-differences (DiD) framework to estimate the short-term effects of the R U OK? Day campaign on mental health outcomes in the four weeks immediately following the campaign's focal day. Our approach leverages observations from the first three years (2011–2013) as a control group to account for baseline trends and seasonality in outcome variables, and those from 2014 to 2019 as a treatment group, when public awareness of the campaign was high. This strategy allows us to identify the effects of the campaign by comparing changes in outcomes before and after R U OK? Day in later years relative to changes over the same weeks in earlier years. This empirical design is similar to methods used by Sim et al. (2022), Ada, Abou Nabout, and Feit (2022), Liaukonytė,

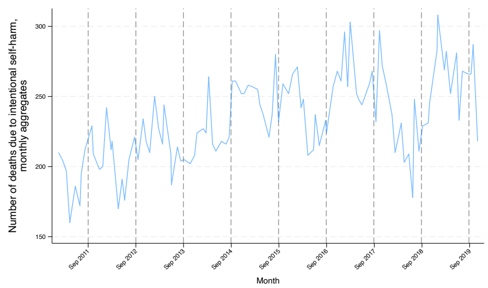
Figure 3: Model-Free Evidence of the Impact of R U OK? Day - Healthcare Utilization & Suicides.



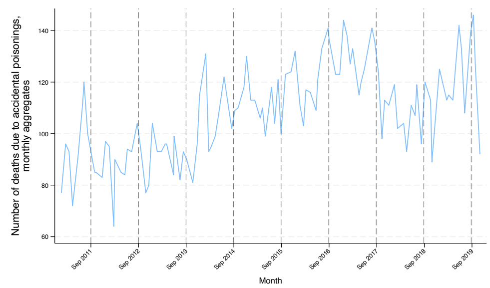
(a) Mental health treatment plan



(b) Mental health prescriptions



(c) Deaths due to intentional self-harm



(d) Deaths due to accidental poisonings

Notes: Figure shows the raw monthly averages of the four main administrative population-level outcome variables. Panel (a) all initiated mental health treatment plans administered by a GP, (b) all dispensed mental health prescriptions (see definition in Web Appendix Table B.1), and death due to (c) intentional self harm and (d) accidental poisoning (ICD-10 code definition).
Source: PLIDA 2011-2019, own calculations.

Tuchman, and Zhu (2023) and Troncoso et al. (2023). Intuitively, it compares changes in outcome variables before and after R U OK? Day in later years with respect to a baseline of changes over the same weeks in the earlier years, when population awareness of the campaign was low.

The estimated effects should be interpreted as intention-to-treat (ITT) effects. While the campaign is population-wide, not every individual would have been directly exposed to the R U OK? Day media campaign, nor would most individuals have participated by asking others if they are OK or being asked themselves. However, the campaign likely generates a population-level influence by raising awareness, shaping public discourse, and encouraging reflections about mental health and wellbeing even among those who do not directly engage with its activities. This broad influence, captured through our design, provides a measure of the campaign's overall impact on mental health outcomes, accounting for both direct and indirect exposure. Importantly, the extent of exposure and responsiveness likely varies across demographic groups. To account for this variation, we will estimate DiD effects separately for key gender-age subgroups, highlighting the heterogeneity of the campaign's impact.

The use of 2011–2013 as a control period is justified by the minimal public awareness of R U OK? Day during these years. While the campaign existed in its early form, its reach was limited, and its visibility in public discourse was low, as evidenced by minimal media coverage and social media engagement (Figure 1). This low baseline awareness implies that any observed differences between the control and treatment periods can be attributed to the campaign's expansion and increased visibility in later years. Consequently, the estimated effects capture the impact of a large, high-visibility campaign relative to a small, low-visibility one. This interpretation is valuable for understanding the effectiveness of increased campaign investments and broader engagement. Furthermore, estimating effects relative to the complete absence of campaigns is neither practical nor meaningful in real-world contexts, as most populations are regularly exposed to various public health initiatives.

We allow our model to estimate separate treatment effects for the earlier (2014–2016) and later (2017–2019) years of the campaign. As shown in Figure 1, R U OK? Day experienced significant growth during this period, with increases in both resources invested and population engagement. Estimating distinct effects for these periods provides important insights. If the estimated effect is larger in the later period, it suggests the campaign became more effective as it expanded and gained traction. Conversely, if effects remain consistent across time, it may indicate diminishing returns to scale.

Our focus on the four-week window surrounding R U OK? Day ensures that the estimated coefficients capture the campaign's short-term effects. Specifically, we compare changes in mental health-related outcomes in the four weeks after R U OK? Day to the four weeks

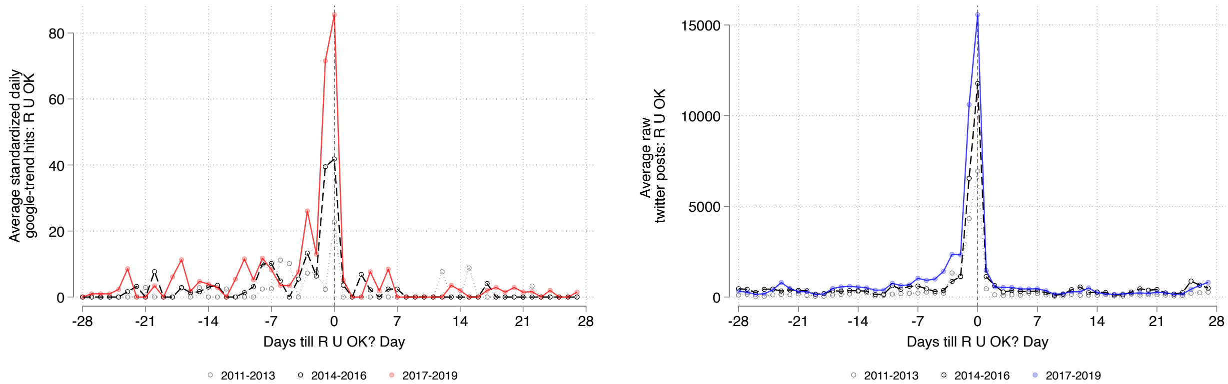


Figure 4: Three year averaged "R U OK" Google searches and Twitter posts relative to R U OK? Day

Note: Left panel displays the three-year averages of the standardized "R U OK" searches on Google, following the standardization of Brodeur et al. (2021). Right panel raw Twitter post counts. See Web Appendix B for further information.
Source: Google trends search data 2011-2019, Twitter data 2011-2019, own calculations.

immediately preceding it. Figure 4 illustrates that attention to the campaign is highly concentrated around its focal day, with interest dissipating quickly thereafter. This concentrated impact makes a one-month window ideal for attributing changes to the campaign while avoiding confounding effects from other population-wide events. Additionally, there are no other major national campaigns or events during this period that could plausibly influence mental health outcomes.

Identification Assumptions. The validity of our DiD framework relies on two key assumptions: (1) the exogeneity of R U OK? Day as a treatment shock, and (2) the parallel trends assumption, which requires that, in the absence of the campaign expansion, mental health outcomes in the treated years (2014–2019) would have evolved similarly to those in the control years (2011–2013). For R U OK? Day to serve as an exogenous shock, its timing and implementation must be independent of other factors influencing population mental health. This assumption is supported by several factors. First, the campaign is an externally introduced intervention designed to prompt conversation and action about mental health. Its timing, as a recurring event in September, is determined by organizational and public awareness goals rather than short-term changes in mental health conditions or external shocks. This predetermined scheduling minimizes concerns about reverse causality between the campaign and mental health outcomes. Second, Figures 1 and 4 illustrate that public engagement with R U OK? Day is highly concentrated around its focal day, with negligible engagement in the weeks leading up to it. This sharp temporal focus indicates that the campaign constitutes a population-level shock to awareness and discourse on mental health. Additionally, prior research has found no evidence of seasonality in suicide rates during the months immediately surrounding the campaign (Burns et al. 2023), supporting the independence of R U OK? Day from trends in mental health outcomes.

Potential threats to exogeneity include changes in external conditions, such as weather or economic factors, which might vary systematically between the treatment and control years. For instance, if later years (2017–2019) consistently experienced different climatic conditions compared to earlier years (2011–2013), these differences might bias the estimated effects. To address these concerns, we include controls for weather, economic conditions, and other potential confounders in our regressions, and our findings remain robust to their inclusion.

The parallel trends assumption requires that, in the absence of the treatment, mental health outcomes in the treated and control years would have followed similar trajectories. We conduct checks to evaluate its plausibility. First, we estimate an event study model, replacing the treatment indicator with a series of weekly leads and lags around R U OK? Day. This allows us to examine whether pre-treatment trends in mental health outcomes differ between the treated and control periods. In the results section below, we show that pre-treatment estimates are close to zero and statistically insignificant, indicating similar trends in outcomes prior to the campaign. Second, we compare mental health outcomes from 2011–2013, when population awareness of R U OK? Day was minimal, to outcomes from earlier pre-campaign years. This comparison reveals no significant differences, further supporting the validity of using 2011–2013 as a counterfactual for later years.

Population-level Daily Counts

The outcome variables on mental health care utilization, suicides and suicide related internet searches are measured daily at an aggregate, national level. We specify a simple, saturated model to analyze how R U OK? Day impacts these variables. Specifically, we use:

$$
\begin{aligned}
y_t = \alpha &+ \tau_1 d_{RUok} \times T_1 + \tau_2 d_{RUok} \times T_2 \\
&+ \delta_{\text{year}} + \delta_{\text{days till RUok}} + \varepsilon_t,
\end{aligned} \tag{1}
$$

where y_{it} denotes an outcome variable of interest and $t = -28, -27, \ldots, 27$ index days relative to R U OK? Day. We define two indicators $T_1 = 1[\text{year}_t \in (2014, 2016)]$ and $T_2 = 1[\text{year}_t \in (2017, 2019)]$ that are switched on for the respective years, and $d_{RUok} = 1[\text{day}_t \geq \text{R U OK? Day}]$ that is switched on when day t is on or within the 28 days after R U OK? Day. Our main interest lies in τ_1 and τ_2. The coefficient τ_1 reports the increase due to higher campaign awareness in 2014-2016 relative to 2011-13, and τ_2 reports the same for 2017-2019. Due to the growth in awareness around the campaign over time, we expect τ_1 to be smaller than τ_2.

To further account for fluctuations that might impact our mental health-related measures, we include a battery of fixed effects in Equation (1). We include year fixed effects, δ_{year} for each year 2012-2019 (2011 omitted) to absorb any nonlinear annual trends, and day-of-the-year fixed effects, $\delta_{\text{days till RUok}}$ (-27 to 27, -28 omitted) to flexibly account for potential seasonality in the sample window. Finally, ε_t is the error term. Unless specified otherwise, the ε_t's covariance matrix is specified to be heteroskedasticity robust.

Self-Reported Mental Wellbeing

We augment equation (1) to estimate the effect of the campaign on mental wellbeing because these data are collected at the individual, rather than national, level. More specifically, we extend the model as follows:

$$
\begin{aligned}
y_{it} = \alpha \quad &+ \quad \tau_1 d_{RUok} \times T_1 + \tau_2 d_{RUok} \times T_2 \\
&+ \quad \delta_{\text{year}} + \delta_{\text{days till RUok}} + \delta_r + \delta_{\text{days since first survey}} + \delta_{sex} + \delta_{age} + x_{it}'\beta + \varepsilon_{it} \qquad (2)
\end{aligned}
$$

where $i = 1, \ldots, n$ indexes individuals and $t = -28, -27, \ldots, 27$ index days relative to R U OK? Day and y_{it} denotes self reported mental wellbeing. Our main interest again lies in τ_1 and τ_2. We extend the model to also include regional fixed effects δ_r, and fixed effects for the days since the annual surveying round commenced $\delta_{\text{days since first survey}}$. We also add fixed effects for an individual's gender and age. In some specifications, we include time-varying individual characteristics x_{it}. The covariance matrix of the individual error terms, ε_{it}, is specified to be cluster-robust, with clustering applied at the individual level.

Non-random Assignment of HILDA Interview Dates. Within our eight-week analysis period, there is a downward trend in the daily number of completed HILDA interviews, reflecting the gradual completion of the interviews overtime (see Figure C.2 in the Web Appendix). The interview date is not randomly assigned in HILDA and is instead negotiated between the respondent and interviewer.[10] This non-random selection could introduce estimation bias if the selection process varied across years such that people with better or worse mental health were more likely to be interviewed post-R U OK? Day in later years. We argue this is not the case for three reasons. First, in the Figure there are no unusual patterns or mass points in the dates that interviews are conducted, and the trends in sample sizes across days appear similar between 2011 and 2019.[11] Second, we show that mental wellbeing is not

[10] Some respondents complete the SF-36 health questions in the HILDA self-completion questionnaire on a date after the face-to-face main interview, as noted above. The self-completion questionnaire is handed out after the interview and can be either immediately completed and given to the interviewer or mailed back on a later day. For all respondents, we use the date of the interview to minimize potential selection.

[11] Web Appendix Figure C.4 of the Web Appendix shows that the item non-response for the mental health questions is smooth around the R U OK? Day and is similar across years.

Table 2: Balance test for observable characteristics: Before vs. After R U OK? Day

Dependent variables: Varying socio-economic characteristics, one regression per line		
	Post R U OK? Day ×	
	2014-2016	2017-2019
	(1)	(2)
Physical health principal component (0-100)	0.096	0.384
	(0.350)	(0.381)
College educated (Yes/No)	-0.000	0.009
	(0.007)	(0.008)
Married/de-facto relationship (Yes/No)	0.003	0.005
	(0.007)	(0.007)
Unemployed (Yes/No)	-0.001	-0.002
	(0.003)	(0.003)
Not-in-labor force (Yes/No)	0.004	-0.004
	(0.006)	(0.006)
Weekly work hours	-0.048	0.052
	(0.279)	(0.296)
Equalized household income/10,000	-0.003	-0.013
	(0.057)	(0.072)
Area-level SEIFA deciles^a (1-10)	0.060	0.111
	(0.042)	(0.045)
Living in metropolitan area^b (Yes/No) urban	-0.042	-0.001
	(0.007)	(0.008)
Missing mental wellbeing score^c (Yes/No)	0.003	-0.003
	(0.004)	(0.004)

Notes: The Table presents coefficient estimates from equation (2). The table reports results for different outcome variables along the row dimension. τ_1 is reported in column (1) and τ_2 in column (2). Conditional age (indicators), sex (indicator), and rurality (indicators)-by-state, and days since the first survey fixed effects, $N = 102,270$. Missings in the covariates are replaced with 0, ^{a} SEIFA refers to the Socio-Economic Indexes for Areas, a set of area-level indices developed by the Australian Bureau of Statistics to rank areas in terms of relative socioeconomic advantage and disadvantage. We use the SEIFA decile ranking as a measure of area-level socioeconomic status, ^{b} excludes covariates related to area (otherwise collinear), ^{c} number of observations uses missing indicator for all potentially answering people (N=151,388). All standard errors are clustered at the individual level.

Source: HILDA 2011-2019 (v19), own calculations.

associated with the day a respondent completes the survey (see Web Appendix Table C.2).[12]

Third, Table 2 shows that the observable characteristics of survey respondents interviewed after R U OK? Day in 2014-2016 and 2017-2019 are similar to those interviewed before the day in terms of their physical health, education, marital status, labor market outcomes, and household income (characteristics unlikely to be caused by R U OK? Day). However, there are significant differences in area-level factors: respondents interviewed after R U OK? Day are more likely to reside in non-metropolitan, higher socioeconomic status neighborhoods. It is possible that these differences are only significant by chance. After adjusting for the multiple hypotheses using the Romano-Wolf correction method (Clarke, Romano, and Wolf 2020), the p-values associated with the area-level differences in Table 2 are all greater than 0.05 (for example, SEIFA: $p = 0.776$ for the years 2014-2016 and $p = 0.132$ for the years 2017-2019). Nevertheless, in our results section we present an extended set of estimates from regressions that include all characteristics in Table 2 as covariates. Estimates from these specifications are consistent with our main findings.

RESULTS

Main Results

Table 3 presents treatment effects for eight mental health outcomes. Panel A reports coefficients for our preferred specification with two distinct treatment periods, 2014-2016 and 2017-2019, while Panel B reports coefficients combining these periods into one treatment period. Estimates in Column (1) indicate that R U OK? Day improved mental wellbeing in 2017-2019 by 0.716 units, equivalent to 4% of one standard deviation (p-value = 0.014). The magnitude of effect is modest in size,[13] and is equivalent to moving a person with median

[12] For this test, we must restrict the sample to those surveyed prior to R U OK? Day, because otherwise the test would capture any effects of R U OK? Day on mental health.

[13] van Agteren et al. (2021) report results from a systematic review and meta analysis of the effect of psychological interventions to improve mental wellbeing showing that some interventions (e.g. Mindfulness-based, CBT and positive psychological interventions) increase mental wellbeing in the general population by about 0.16 - 0.40 of a standard deviation. Our results are lower in magnitude for two reasons: (i) we are capturing a reduced form effect among a general population, and not the direct effect of an intervention on individuals with symptoms of depression, and (ii)

Table 3: R U OK? Day and Mental Health Outcomes

Dependent variables: Principal component of mental wellbeing, and number of daily mental health treatment plan and prescriptions, intentional self harm and accidental poisonings								
		Mental health care		Death due to		Search behavior		
	Mental	Treatment	Prescription	Intentional	Accidental	Suicide-		
	wellbeing	plan	drugs	self harm	poisoning	topic	prevention	plan
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Two treatment periods								
2014-2016	0.167	-281.97	-935.45	-0.123	0.272	4.044	-2.952	-3.869
	(0.275)	(194.14)	(1172.1)	(0.601)	(0.441)	(1.306)	(3.766)	(3.878)
2017-2019	0.716	-243.16	-539.26	-0.160	-0.006	0.712	1.389	4.234
	(0.292)	(222.99)	(1348.8)	(0.570)	(0.425)	(1.590)	(3.703)	(3.755)
Panel B: One treatment period								
2014-2019	0.635	-262.57	-737.36	-0.141	0.133	2.378	-0.782	0.182
	(0.250)	(190.76)	(1123.8)	(0.500)	(0.367)	(1.248)	(3.279)	(3.346)
N	102,270	504	504	504	504	504	504	504
R^2	0.03	0.945	0.963	0.18	0.19	0.31	0.20	0.15
Mean dep.	67.50	5,880	74,055	7.65	3.72	17.89	33.98	21.23
SD dep.	17.23	3,551	26,565	2.80	2.10	7.56	17.65	17.56
Day and year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Days since survey start	✓							
Basic set of covariates	✓							

Notes: The Table presents coefficient estimates from equation (2) for Column (1) with clustered standard errors on the individual-level in parentheses and equation (1) for Columns (2) through (8) with heteroskedasticity robust standard errors in parentheses. For each regression, we present in Panel A the two main coefficients for interaction post-R U OK? Day and the respective year indicators, and in Panel B the estimate for the combined treatment years. The columns also show the N - number of observations, the regression R^2, and the mean and standard deviation of the dependent variable. Column (1) shows the survey elicited principal component of mental wellbeing, (2) the daily counts of mental health treatment plans, (3) prescriptions, (4) deaths due to intentional self harm and (5) accidental poisoning. Google Trend results in Columns (6) to (8) use: (6) the Google Trends topic "suicide,", (7) uses individual search terms related to suicide prevention – 'lifeline + help suicide + hotline suicide + suicide hotline' – and (8) plan – 'commit suicide + how to suicide + painless suicide + quick suicide + suicide methods' – (based on Till et al. 2020)

Source: HILDA 2011-2019 (v19), PLIDA 2011-2019, Google Trends 2011-2019, own calculations.

mental wellbeing (i.e. at the 50th percentile) to the 53rd percentile of mental wellbeing. Comparing this effect to major life events (see Web Appendix Table D.1), the 0.716 estimate represents 16%, 29% and 13% of the mental wellbeing changes from death of a spouse or child, separation from spouse or partner, and major financial worsening, respectively.[14] In the context of related work on the impacts of friends on wellbeing, our results are relatively large. Ho (2016) estimates that making one additional friend reduces feelings of sadness and depression by 1.8% of a standard deviation each. The smaller effect that we find in 2014-2016 supports our argument that R U OK? Day's impact grew with the campaign's size and reach. Results in Panel B, which pools the two time periods support the positive effect on wellbeing, but mask the growth in impact over time.

The impacts of the campaign on mental healthcare utilization are reported in Columns (2) and (3). The estimates show statistically insignificant changes in both mental health treatment plans (Column 2) and filled mental health prescriptions (Column 3) in both the 2014–2016 and 2017–2019 periods. For example, in 2017–2019 the point estimates are −243 treatment plans and −539 prescriptions per day, relative to sample means of 5,880 and 74,055, implying relative effects of −4.1% and −0.73%. The corresponding 95% confidence intervals rule out increases larger than 3.3% for treatment plans and 2.8% for prescriptions. Importantly, the minimum detectable effects for these outcomes are relatively small: 624 treatment plans and 3,776 prescriptions (10.6% and 5.1% of the respective means), indicating these healthcare estimates are sufficiently precise to rule out moderate short-run effects. These results suggest that R U OK? Day did not meaningfully increase use of mental health services in the short run.

Estimates presented in Column (4) of Table 3 report the effect of R U OK? Day on the total number of deaths due to intentional self-harm. The estimates are small in magnitude and imprecisely estimated in both the 2014-2016 period (-0.123) and the 2017-2019 period

the R U OK? Day campaign is a light-touch intervention compared to the interventions included in their analysis.

[14] Web Appendix Table D.1 presents further comparisons and reports the impact of life events on mental wellbeing separately by gender, and Web Appendix Table D.2 accounts for these events in our main model and provides additional correlational benchmarks.

(-0.160). Similar results are found for accidental poisonings in Column (5). These results suggest no discernible impacts on deaths due to intentional self-harm and accidental poisonings. An important caveat to these results is that our statistical power to detect effects on suicide-related outcomes is limited, due to the low frequency and high variance of daily death counts. While our point estimates for the 2017–2019 period are small (–0.160 and -0.006, respectively), the associated confidence intervals are wide. Specifically, the relative effect sizes of the lower bounds of the 95% confidence intervals are –16.7% for intentional self-harm and –22.6% for accidental poisonings. These values indicate that while we can rule out large short-run effects, we cannot rule out the possibility of small-to-moderate reductions in suicide-related outcomes. Ex-post power calculations reinforce this point: assuming a conventional significance level of 5% and power of 80%, the minimal detectable effect sizes over 2017–2019 are 1.60 for intentional self-harm and 1.19 for accidental poisoning (Bloom 1995).

Completed suicides represent a small fraction of outcomes associated with suicidality. People who commit suicide represent about 10 percent of the population who have thought about committing suicide in a given year, and approximately a third of those who plan a suicide make an attempt, according to the Substance Abuse and Mental Health Services Administration (SAMHSA 2021). We use internet search terms related to suicide planning and prevention to measure interest in suicide, suicide planning, and help-seeking in the months around R U OK? Day. The estimated effects of R U OK? Day in 2014-2016 and 2017-2019 on the topic, prevention, and planning variables are presented in Columns (6) to (8) of Table 3. The estimated effects for 2017-2019 are positive for each outcome – indicating increased suicide-related search activity – but have large confidence intervals.[15] For the 2014-2016 period, it is estimated that R U OK? Day significantly increased interest

[15] In Table 3, a clear difference across columns is the size of the standard errors across the Google Trends results. The standard errors are roughly three times larger in columns (7) and (8) than in column (6). This is explained by the much higher volatility in the prevention and planning variables – which were based on search-term queries – than in the suicide topic variable – which is generated by Google Trends. This volatility limits the statistical power to detect effects.

in the suicide topic (p-value equals 0.002) while reducing interest in suicide prevention and planning. Overall, we find little evidence that the campaign reduced suicidal ideation, as represented in Google searches.

Dynamic Impacts. R U OK? Day may generate different temporal effects. We disaggregate our treatment variables into four weekly pre-R U OK? Day indicators and four weekly post-R U OK? Day indicators to estimate an event study design (covering the same sampling period as shown in Table 3). Figure 5 presents these dynamic effects across the five mental health outcomes reported in Columns (1) to (5) in Table 3, relative to the -1 reference week.[16] Estimated effects of the campaign for years 2014-2016 are shown in black and estimates for years 2017-2019 are shown in grey.

Panel A reveals the strongest temporal pattern, with mental wellbeing scores in 2017-2019 showing increasing positive effects that peak at 1.76 units (10% of a standard deviation) in week 3 and 2.93 units (17% of a standard deviation) in week 4, while 2014-2016 estimates remain close to zero. The healthcare utilization measures in Panels B and C exhibit substantial weekly variation with wide confidence intervals that consistently include zero, showing no systematic pattern in either time period. Similarly, mortality outcomes in Panels D and E demonstrate imprecise estimates throughout the observation window, with confidence intervals overlapping zero in all weeks for both intentional self-harm and accidental poisonings.[17]

Multiple explanations can rationalize the dynamic pattern found for self-reported mental wellbeing. The growth over time likely reflects HILDA survey's measurement approach, which asks respondents about their mental wellbeing "during the past 4 weeks." While our analysis implicitly assumes respondents report their current mental wellbeing, they may instead follow the survey instructions and consider all days in the past four weeks equally. In

[16] Event study coefficients for the Google Trend outcomes can be found in Web Appendix D.1.

[17] We show in Web Appendix Figure E.1. that extending the observation window from four weeks to eight weeks shows no discernible effect of R U OK Day up to eight weeks after the campaign, which suggests that there is no evidence that the campaign had a lagged effect on reducing suicides. This is perhaps not surprising given the short time (<4 weeks) between most suicidal thoughts and attempts (Deisenhammer et al. 2009). These results also do not suggest the campaign led to people temporarily delaying suicides until the following month.

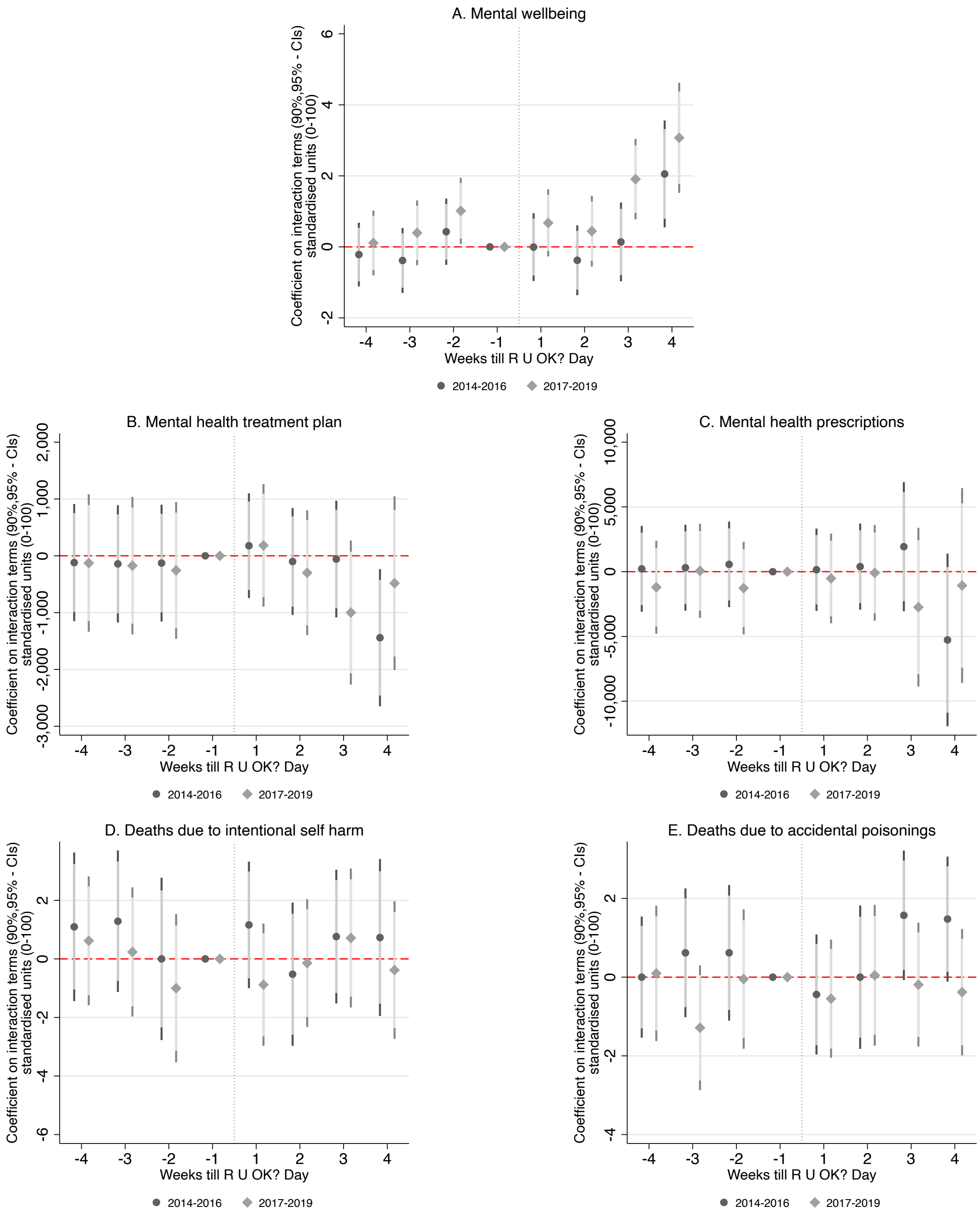


Figure 5: Dynamic Impacts of R U OK? Day Over Time

Note: The Figures displays coefficient estimates of regressions analogous to (1) and (2) in Table 3 disaggregated by four weeks-till pre- and post-R U OK? Day, for the five main outcomes, confidence bands show the 95% (dark) and 90% (light) confidence intervals. Dark grey shows the first treatment period from 2014-2016, and light grey shows the latter treatment period from 2017-2019; week -1 is used as a reference period.

Source: HILDA 2011-2019 (v19), PLIDA 2011-2019, own calculations.

this scenario, an immediate permanent improvement in mental wellbeing would appear as a gradual increase in our measures (see Figure C.5 in the Web Appendix for a visualization).[18] An additional explanation is that the effects of R U OK? Day on mental wellbeing are cumulative over the four week window. This pattern is robust to an extended version of the event study model that includes all observable characteristics listed in Table 2, including location and SES. The revised estimates, presented in Web Appendix Figure D.2, shows a nearly identical pattern to the original event study, suggesting that differences in respondent characteristics across survey weeks and survey years are not driving the increasing wellbeing effect.[19]

Robustness of Main Results

This section tests robustness by ruling out pre-trends across five outcomes and confirming that the mental wellbeing effect holds under alternative baselines. We also show that our identification is not driven by time-varying factors, using instances when R U OK? Day was disrupted by heavy rain. Further evidence supporting robustness across alternative regression specifications, extending the time window to three months after the focal day, and evidence supporting the null-effect results for mental health care utilization and suicide is provided in Web Appendix E.

Pre-trends & Choice of Baseline Period. If the parallel trends assumption holds, event study coefficients for the weeks before R U OK? Day should hover around zero, indicating no systematic differences in mental health outcomes between treated and control groups before the campaign. Figure 5 presents the estimated coefficients for three pre and four post R U OK? Day terms. Across each outcome, there does not appear to be evidence of pre-existing trends. Joint significance tests for the three lead terms confirm this observation: we fail to

[18] Another possibility is that respondents follow the peak-end rule (Redelmeier and Kahneman 1996) and give extra weight to particularly bad mental wellbeing days (peak) and recent days (end). If particularly bad days are more likely to have occurred before R U OK? Day, then days further back in time, will receive higher weights, and self-assessed mental well-being may grow according to a convex function.

[19] Web Appendix D.3 shows event study results split by gender.

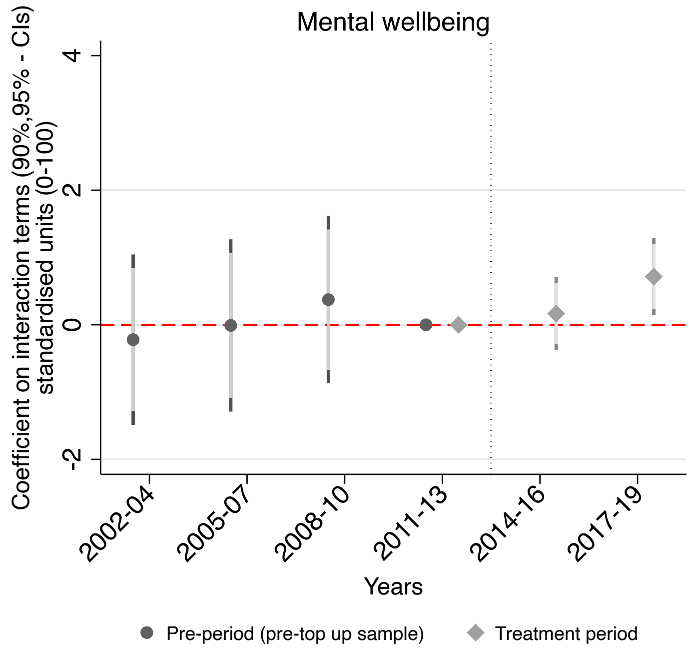


Figure 6: R U OK? Day effect over the years, including pre-campaign years

Note: The Figure presents coefficient estimates of two regressions, both using 2011-2013 as the reference category, grey for treatment year, analogous to those in Table 3 - Column 1, and analogous regressions before the campaign (black) using the pre-top-up sample only (19,471 individuals vs 21,974 individuals in the later period), which reduces sample size and consequently increases standard errors. Confidence bands show the 95% (dark) and 90% (light) confidence intervals.

Source: HILDA 2002-2019 (v19), own calculations.

reject the null hypothesis that all lead coefficients are jointly equal to zero for each outcome variable.

While our main specification uses 2011–2013 as the control period due to its temporal proximity to the treatment periods, HILDA data is available as far back as 2001.[20] To validate our identification strategy, we introduced placebo R U OK? Day dates back to 2001 (the second Thursday of each September) and estimated placebo effects for 2002–2004, 2005–2007, and 2008–2010, relative to 2011–2013. The coefficients, presented in Figure 6, are statistically insignificant for all three placebo periods,[21] implying there was no differential

[20] An additional reason for using 2011-2013 as the control period is that HILDA included a top-up sample of 4,009 new respondents in 2011 (Watson and Wooden 2013).

[21] See Web Appendix Table E.5 for the complete regression results.

mental wellbeing effect of R U OK? Day in 2011–2013 compared with years prior to the campaign's launch. These null results support our causal interpretation of the estimated treatment effects: the impact of scaling up a small, low-visibility campaign to a large, high-visibility one.

Table 4: Heavy Rain, R U OK? Day and Self-Reported Mental Wellbeing

Dependent variable: Principal components of mental wellbeing			
	All	Females	Males
	(1)	(2)	(3)
2014-2016	0.245	0.172	0.361
	(0.281)	(0.399)	(0.395)
Heavy rain on R U OK? Day	-0.433	-0.524	-0.332
	(0.512)	(0.736)	(0.706)
2017-2019	0.796	0.645	0.977
	(0.295)	(0.413)	(0.421)
Heavy rain on R U OK? Day	-0.956	-1.273	-0.345
	(0.688)	(0.968)	(0.969)
N	102,207	54,082	48,125
R^2	0.03	0.02	0.02
Mean dep.	67.50	65.94	69.26
SD dep.	17.23	17.61	16.62
Day and year fixed effects	✓	✓	✓
Days since survey start	✓	✓	✓
Basic set of covariates	✓	✓	✓
Weather on day of survey in postcode	✓	✓	✓
Weather on R U OK? day in postcode	✓	✓	✓
Joint tests for significance on rain days *(p-values)*			
2014-2016	0.73	0.65	0.97
2017-2019	0.82	0.53	0.53

Notes: See Table E.1, see notes therein, tests difference when heavy rain (indicator for highest 10th percentile) on R U OK? Day in the local postcode. The regressions further control for the weather on the day of the survey, the weather on the respective R U OK? Day in the individual's postcode, and the indicator for heavy rain. Shown are the main coefficients from the main model and their interaction with the heavy rain indicator. P-values are based on an F-test of the sum of the two coefficients and correspond to the R U OK? Day effect if it was rained out.
Source: HILDA 2011-2019 (v19), BoM 2011-2019, own calculations.

Rain as an Exogenous Shock to Campaign Impact. Heavy rain on R U OK? Day likely reduces the campaign's effectiveness by disrupting events and activities that promote conversations and awareness.[22] Campaign materials displayed in public spaces might also receive less attention if rain discourages people from being outdoors. Furthermore, poor weather can negatively affect mood and social behavior, reducing individuals' engagement with the campaign's message. Collectively, these factors imply a small or zero effect for people residing in locations with heavy rainfall on R U OK? Day. We use this exogenous variable to test our identification strategy. If our DiD model is misspecified due to time-varying shocks across days and years, we would expect to find significant (but spurious) wellbeing effects in locations and years when R U OK? Day was interrupted by heavy rain. In contrast, if the DiD model is correctly specified, we should find small effects when R U OK? Day was impacted by heavy rain. Table 4 explores this notion by interacting the R U OK? Day variable with an indicator for heavy rain in the respondent's local area, while also controlling for the direct impacts of weather on wellbeing. The results show that without heavy rain, the average effect on mental health (0.746 units) aligns with our baseline estimate (0.716 units). In contrast, when R U OK? Day is rained out, the campaign has no detectable effect on mental wellbeing (p-values: 0.73 in 2014–2016, 0.82 in 2017–2019). This lack of effect supports the robustness of our identification approach, as it suggests that the observed impacts are not driven by confounding time-varying factors.

Demographic Differences

The effects of R U OK? Day on mental wellbeing outcomes may be heterogeneous across subpopulations. In this section we explore how estimated effects vary across demographic groups defined by gender (male/female) and age (15-24, 25-49, and 50+) (Chen, Sridhar, and Mittal 2021). We expect effects to vary across these groups because of differential exposure

[22] Hungerman and Moorthy (2023) documents the long-term effects of bad weather on Earth Day in 1970, leading to weaker levels of environmental support 10–20 years later. In contrast, we focus on the short-term effects of heavy rain on the campaign's impact in the current year.

to the R U OK? Day media campaign. Advertising for R U OK? Day is a mix of national TV advertising, billboards, and advertising placed directly in schools and workplaces. As a result, different demographic groups will encounter information about the campaign at different intensities. Effects may also vary across these groups because - conditional on exposure - the R U OK? Day campaign will influence them differently. For example, the campaign may have a larger impact on men because this group has a lower propensity to discuss mental health issues with peers in the absence of encouragement.

To explore heterogeneity in the campaign's impact for each mental health outcome, we repeated our difference-in-differences approach, running separate regressions for each demographic group. Figure 7 displays the estimated coefficients for mental wellbeing, mental health care utilization, and deaths, disaggregated by gender and age groups for the 2017–2019 period.[23] The first panel indicates significant mental wellbeing improvements concentrated among men, for whom there was a 0.897 increase overall (p-value = 0.031) and a 1.485 effect for men aged 25-49 years (p-value = 0.021). The magnitude of the latter effect is 9% of a standard deviation (or moving from the 50th to 55th percentile of mental wellbeing), which is approximately double the average effect estimated in Table 3. The effects for females, although positive in sign, are statistically insignificant. The subsequent panels reveal muted, statistically insignificant results for mental healthcare utilization and deaths for all demographic cohorts.

Our findings suggest that the impact of R U OK? Day is more pronounced in improving mental wellbeing among men, especially during the campaign's later years. A potential explanation for this result is that men are less likely to use mental healthcare when experiencing psychological distress than women (Wang et al. 2007); thus, R U OK? Day may disproportionately encourage men to seek treatment. But our results suggest that this is not the case, at least in the short-run, as the mental healthcare utilization of men does not increase within four weeks after the campaign. Another explanation is that men are less likely

[23] See Web Appendix Table F.1 and F.2 for the full set of regression coefficients, including those for the 2014-2016 period.

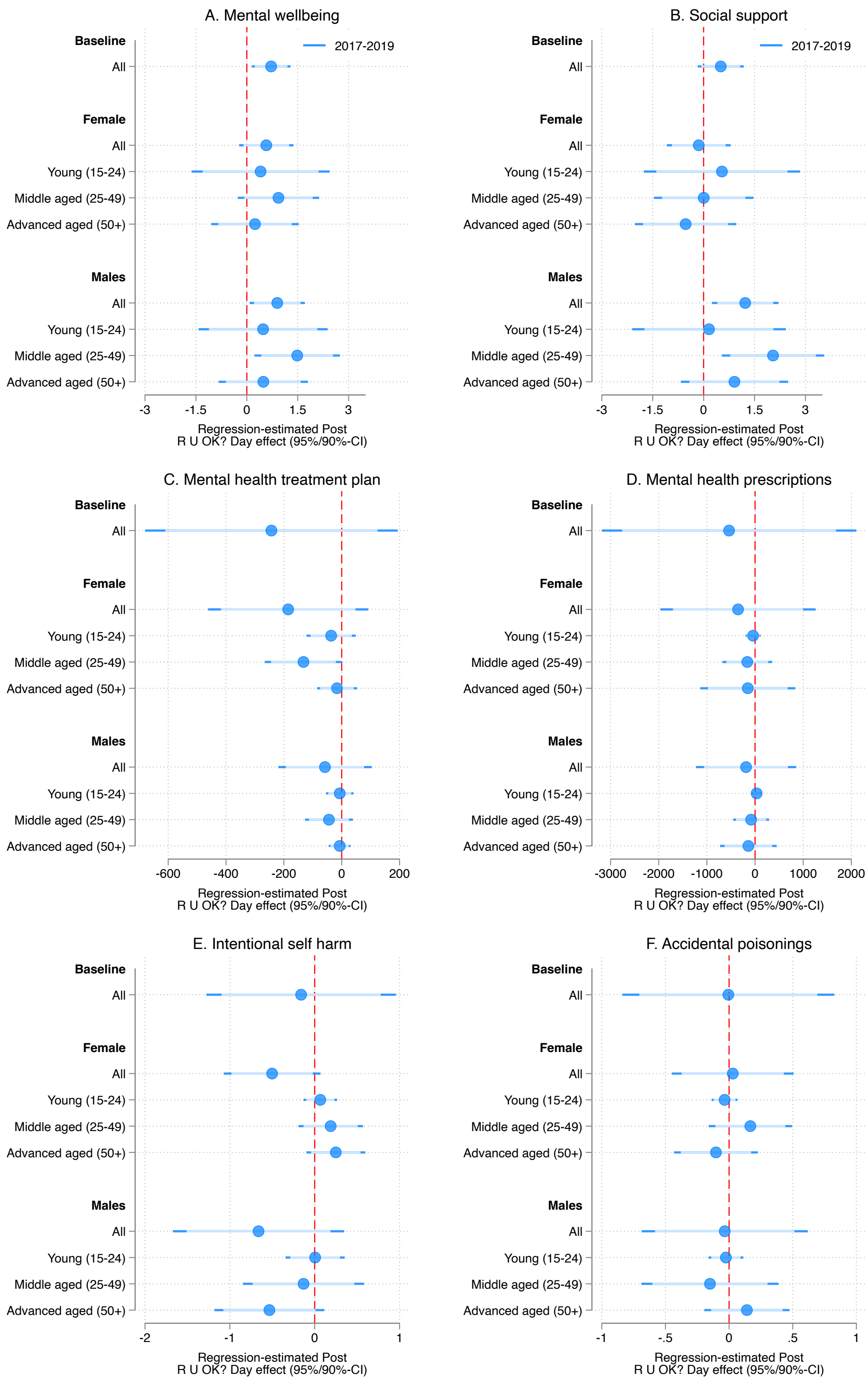


Figure 7: Heterogeneous Impacts of R U OK? Day

Note: The Figure shows coefficient estimates for the second treatment period (2017-2019) analogous to those in Table 3 - Baseline All replicates these for the five main outcomes and for the principal component of social support (top right panel). The remaining coefficients repeat the regressions for subsamples (female/male, and by three age groups (15-24/25-49/50+). Confidence bands based on individual clustered and robust standard errors (for the national data sets) show the 95% (dark) and 90% (light) confidence intervals. See Web Appendix Table F.1 and F.2 for the full set of results.

Source: HILDA 2011-2019 (v19), PLIDA 2011-2019, own calculations.

to express emotions related to weakness, helplessness, and sadness because of traditional masculinity norms (Möller-Leimkühler 2002). So even in the absence of mental healthcare, men may gain more from the peer-to-peer discussions that the campaign encourages. The campaign emphasizes this as a primary mechanism, and there exists correlational evidence in other contexts linking social support to improved mental wellbeing (Olstad, Sexton, and Søgaard 2001; Takizawa et al. 2006; Santini et al. 2016).

We explore whether changes in a proxy measure of peer support align with the heterogeneous mental wellbeing effects observed across age and gender cohorts. This serves two purposes. First, the results provide a validity check for the gender- and age-based heterogeneity findings on mental wellbeing and offer insights into why the effects are stronger for males. Second, it can offer suggestive evidence of one channel that R U OK? Day impacts mental wellbeing.

While the HILDA surveys, from which we derive our mental wellbeing measures, do not directly capture whether individuals provide or receive health-related peer support, they include related questions. In each wave of the survey, people are asked about their strength of agreement (on a 1 to 7 scale) with the following statements: (i) I don't have anyone that I can confide in; (ii) when I need someone to help me out, I can usually find someone; (iii) I have no one to lean on in times of trouble; (iv) I often need help from other people but can't get it, and (v) there is someone who can always cheer me up when I'm down. We construct a social support index by conducting a principal components analysis using responses to these five statements (see Table B.2 in the Web Appendix for more detail) and repeat our main analysis, estimating equation (2) with this index as the outcome variable.

The social support estimation results for 2017-2019 are shown in the upper right panel of Figure 7 (see also Web Appendix Table F.1). The estimated effects are generally small for women, but statistically significant for men. For instance, R U OK? Day in 2017-2019 is estimated to have increased social support received by men by 1.23 units (6.1% of a standard deviation, which is equivalent to a movement from the 50th to the 56th percentile of social support). This gender difference in the effect of the campaign on social support might be driven by existing differences in social support systems between the genders. Existing research has highlighted that men's friendship circles have shrunk over the last 30 years and that men are low exchangers of social support and invest less in maintaining friendships (Cox 2021; Reeves 2022; Liebler and Sandefur 2002). R U OK? Day can then serve as a 'nudge' for men to (re-)connect with their friends and check in with each other. Corresponding with the results documented in Figure 7, in which middle-aged men experienced the largest mental wellbeing effects, we find the largest social support increase for the same group: an improvement of 2.04 units (10.1% of a standard deviation, which is equivalent to a movement from the 50th to 58th percentile of social support).

The estimates for 2014-2016 show a similar pattern of increased social support and mental wellbeing among men (reported in Web Appendix Table F.1). Taken together, the results provide mixed evidence on whether self-assessed social support, as influenced by R U OK? Day, improves mental wellbeing among men. Multiple explanations can rationalize this pattern. One possibility is a change in the composition of individuals reached by the campaign in later years, as the more intense efforts engaged populations for whom the relationship between social support and mental well-being was stronger. Alternatively, while the campaign consistently improved self-assessed social support across all years, the larger scale and visibility of the campaign in later years may have been necessary to translate these improvements into measurable mental wellbeing gains. This could reflect the greater ability of a high-intensity campaign to amplify psychological impacts.

CONCLUDING REMARKS

In this paper, we examined the effectiveness of R U OK? Day, a nationwide suicide-prevention campaign in Australia that promotes peer-to-peer conversations as a form of early intervention. Drawing on detailed administrative and survey data within a temporal difference-in-differences framework, we evaluated short-term intent-to-treat effects on outcomes that align with the campaign's stated objectives: self-reported mental wellbeing, mental healthcare utilisation, and suicide-related deaths. These outcomes are not necessarily the domains in which short-run effects are theoretically most likely, but they represent the outcomes the campaign explicitly aimed to influence. These outcomes also speak directly to the design and evaluation problem that these types of campaigns face, namely what outcomes can shift in the short run in response to the campaign, versus what requires additional steps to translate intention into action.

Our results do not point to a simple conclusion, but instead present a nuanced picture of how the campaign affects both subjective wellbeing and behavioural outcomes. First, the campaign has a small but statistically significant effect on mental wellbeing, with an average increase of 4% of a standard deviation in active years, equivalent to moving an individual from the 50th to the 53rd percentile of mental wellbeing. Second, these wellbeing improvements are concentrated among males aged 25–49, a group known to be at elevated risk of poor mental health outcomes, with an effect size of 9% of a standard deviation. While modest on average, this shift is meaningful at population scale. Third, we find no evidence that the campaign meaningfully increased mental healthcare use, with the estimates precise enough to rule out moderate-sized effects. Fourth, we observe no statistically significant changes in suicide-related deaths. While these outcomes are rare and power is limited, the estimates allow us to rule out large short-run effects. Taken together, the estimates suggest a short-run distinction between small-to-moderate changes in psychological outcomes and no detectable changes in behavioural follow-through.

The wellbeing estimates are robust across extensive robustness checks, examination using rain as an exogenous shock and the consistency in finding the strongest effects for males aged 25-49 for both mental wellbeing and perceived social support all give confidence in the robustness of the mental wellbeing estimates. These results suggest a plausible story that the R U OK? day campaign operates through peer engagement and support, as intended, and this contributes to improved mental wellbeing. That increased social support improves mental wellbeing is well-documented in the psychology literature (Thoits 2011). While our data do not allow us to directly observe whether individuals in our sample initiated or received such interactions, a cross-sectional survey conducted shortly after R U OK? Day found that the most common way that respondents participated in R U OK? Day activities was by reaching out to ask others if they were OK (Mok et al. 2016). It may also be possible for mental wellbeing to be improved by R U OK? Day through other short-term pathways, such as increased awareness of mental health issues, community events and improved public discourse regarding mental illness, which can all help to reduce stigma, reduce feelings of shame and increase social connectedness. (Corrigan et al. 2012; Rüsch et al. 2014)

While the campaign appears effective in increasing perceived social support and improving wellbeing, it does not lead to measurable changes in behavior of the intended recipients of the campaign, such as increased engagement with formal mental health services. This distinction between the campaign's emotional and behavioural effects, cannot be explained by imprecision in the healthcare estimates. Unlike the suicide estimates, for which a lack of power invites caution in the interpretation of the null results, our estimates for healthcare use are precise and we can confidently interpret this as no evidence of any increase in mental health services or prescription medication.

There are several possible explanations for why the campaign increased wellbeing but did not produce detectable changes in formal mental healthcare use. First, it has been shown that stigma, perceived ineffectiveness of treatment or a belief that one's mental health concerns are not severe enough to warrant professional help, may deter individuals from seeking formal care (Andrade et al. 2014; Prins et al. 2011; Tapp et al. 2018), and it is possible that the campaign did not have any impact on shifting these attitudinal factors. Second, individuals with poor mental wellbeing often face greater difficulties in taking action (DiClemente, Nidecker, and Bellack 2008), which may make behavior change inherently harder to achieve through "soft-touch" awareness campaigns or peer-initiated conversations. Third, structural barriers, such as long wait times, high costs, a complex health system and limited access to healthcare professionals, likely also play a role in limiting the behavioral impacts (Knapp et al. 2006; Smith, Paparo, and Wootton 2021). Fourth, a person in distress may feel temporarily better from peers checking-in with them, momentarily reducing their sense of urgency to seek professional help. This potentially counteracts any positive impacts of the campaign in supporting and empowering individuals to seek help. We do not think this fourth reason is driving our results because we would expect to see a delayed increase in mental healthcare use, after the temporary reduction in distress has passed. In additional analyses (in Web Appendix Table E.3), we do not see any changes to mental healthcare use even if we extend our observation period to 8 or 12 weeks post R U OK? Day. Our distinct findings across mental health outcomes are not unique. A systematic review and meta-analysis of one-to-one peer support in mental health services finds that these interventions may have a modest positive impact on psychosocial outcomes such as empowerment and social network support, but no evidence of impact on clinical symptoms (White et al. 2020).

Our results provide some of the first causal evidence on the effectiveness of a large-scale public health campaign built around peer-to-peer engagement. Marketers are increasingly contributing to public policy goals (Chandy et al. 2021; Moorman et al. 2019), and the idea that advertising influences health outcomes is gaining recognition—though existing evidence primarily focuses on firm-led efforts. For example, U.S.-based firm-led campaigns often rely on sustained, high-budget messaging (e.g., Kim and KC 2020a,b; Shapiro 2022; Yoon and Kim 2024). In contrast, R U OK? Day is a brief, single-day initiative with attention concentrated over a one-month window, and its core call to action—asking "Are you OK?"—places the burden of initiation on peers rather than directly prompting recipients to seek care. A practical implication for campaign designers is that brief, population-scale advertising may generate psychological responses without translating into measurable behavioural follow-through. As a result, actual behavior change depends not just on exposure but on interpersonal dynamics, which may help explain the limited observed effects on service use and mortality. These results highlight the value of evaluating campaigns across both psychological responses and behavioural follow-through, and suggest that when the objective is service uptake or other downstream outcomes, peer-script advertising may need to be paired with complementary mechanisms—such as clear next-step prompts, facilitated connections to services, and coordination with providers to ensure capacity.

These findings also speak to a broader class of peer-based public health campaigns, both within and beyond mental health, that use advertising to encourage ordinary people to initiate supportive conversations. R U OK? Day shares design features with international efforts such as World Mental Health Day, World Suicide Prevention Day, Bell Let's Talk Day (Canada), Time to Change (UK), and Dobara Poocho (India). Similar peer-oriented strategies also appear in campaigns targeting other health behaviors—from the Great American Smokeout to Let's Move! and the WHO's World Breastfeeding Week. Our results suggest that while peer-to-peer support may enhance psychological outcomes, achieving sustained behavioural change is likely to depend on complementary interventions that reduce barriers to care, including both structural constraints (e.g., access and capacity) and attitudinal factors (e.g., stigma and perceived need).

Our analysis is limited by the lack of direct data on the campaign's core mechanism—peer-to-peer engagement. We cannot observe whether individuals participated in R U OK? Day by asking, being asked, or checking back in, nor can we assess potential shifts toward other forms of support such as helplines, community counselling, or private psychological care. Our reliance on Medicare data means that while we capture the most common sources of mental healthcare in Australia (Australian Institute of Health and Welfare 2024b), changes in non-Medicare support systems are not captured. Additionally, we do not observe acute mental healthcare contacts such as emergency department presentations, which could reflect crisis intervention. Finally, our design is tailored to detect short run responses in the weeks around the campaign in line with the campaign's stated theory of change; it is not designed to isolate gradual, cumulative changes in norms or stigma that may unfold over longer horizons.

Our work can be extended in several directions. First, an important extension is to examine cumulative, longer-run effects in settings that generate credible variation in repeated exposure intensity—so that longer-run changes in norms, stigma, or help-seeking can be separated from underlying time trends. This could include exploiting differential campaign intensity across media markets, discrete expansions in spending or reach, or platform-level shocks that change who is exposed, rather than relying on nationally uniform annual rollouts. Second, investigating how peer-based campaigns can better bridge the gap between social support and clinical care represents a promising avenue—for example, by studying referral prompts, partnerships with helplines and providers, or integrated messaging that reduces barriers to help seeking and clarifies concrete next steps. Third, evaluating alternative message framing, delivery channels, and segmentation strategies could help similar public health campaigns improve reach and resonance across groups, and better align campaign design with the outcomes they aim to influence.

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WEB APPENDICES FOR "ARE YOU OKAY? EFFECTS OF A NATIONAL PEER-SUPPORT CAMPAIGN ON MENTAL HEALTH"

Contents of the Web Appendix

Web Appendix A Related Literature

Table A.1: Comparison of Related Studies on Advertising-Based Health Interventions

Research	Health Focus	Methods	Type	Advertisement	Outcomes
Avery et al. (2007)	Smoking cessation	Observational	P	Smoking cessation product	Quitting smoking, Attempts to quit
Kim & KC (2020a)	Fertility	Quasi-experiment	P	Erectile dysfunction drugs	Birth rates
Kim & KC (2020b)	Hospital choice	Quasi-experiment	P	Hospital	Hospital selection, Mortality
Shapiro (2022)	Mental Health	Quasi-experiment	P	Antidepressant use	Prescriptions, workplace absenteeism
Hristakeva (2025)	Drug Injury	Quasi-experiment	P	Drug Injury Lawsuits	Anticoagulant prescriptions, Inpatient visits
Yoon & Kim (2024)	Robotic Surgery	Quasi-experiment	P	Robotic Surgeru Ads	Procedure uptake
Ghosh Dastidar, Sunder, and Shah (2023)	Pandemic Response	Quasi-experiment	S	COVID-19 Ads	Social distancing
Athey et al. (2023)	Vaccination	RCT	S	Vaccine uptake	Vaccine uptake, beliefs
Jacobson & Jacobsen (2011)	Breast Cancer	Quasi-experiment	A	Screening encouragement	Cancer screening
This paper	Mental health	Quasi-experiment	S	Peer-support script	Mental wellbeing, mental health services, & suicide

Notes: Type codes classify the advertised object: P = product/service; S = public health action; A = awareness/information.

Web Appendix B Additional data information

PLIDA data

We use the medical and official death records provided by the Australian Bureau of Statistics [ABS] via the PLIDA data covering the years 2011-2019. The data includes individual information, age, gender, cause of death, and broad regions. For the mental healthcare use outcomes, we use Medicare (Australia's universal health insurance provider) billing data that captures the whole population in Australia. We use all claimed items at the daily level for mental health treatment plans initiated by a general practitioner, as well as all prescriptions related to mental health outcomes. We follow Case and Deaton (2015) and use the leading cause of death and the International Statistical Classification of Diseases and Related Health Problems 10th Revision (ICD10); see Web Appendix Table B.1 for details. We use both intentional self-harm and accidental poisoning (as well as the broader measure of death of despair). We then calculate the daily aggregates across Australia, overall, by gender and by gender×three broad age groups.

Twitter data

We use the Academic API from Twitter to obtain tweets related to R U, OK? Day in the years 2011 to 2019. The Academic API provides access to the entire history of publicly available tweets at the time of a query. Tweets are collected and returned based on whether they match a Boolean query. We include the terms 'ruok', 'ruokay', 'ruokay' and their hashtag equivalents along with and tweets that mention or originate from the leading mental health organisations that operate within Australia: Lifeline, Beyond Blue, SaneAustralia, Suicide Prevention AU, and Black Dog Institute. We restrict tweets to be composed in the English language. The tweets were collected on September 30th, 2021. The API returns the tweet metadata, including the date and time of each tweet we use in our analysis. We compute the number of tweets per calendar day by counting all tweets posted within each 24-hour period.

Google Trend data

Next, to assess whether people are searching for suicide topics or support, we use Google trend data. Google is by far the most widely used search engine in Australia and thus captures any (major) change in search behaviour. The search data measures the over a given period relative to the highest search in that period. Therefore, adjustments need to be made on a daily level. In Figure 1, we first used the keyword "R U OK" on the monthly level, which is straightforward to use; 2019 was the highest number of searches. Thus, all others are measured relative to this (if higher than a certain small threshold; otherwise, google does not report the search volume).

When assessing searches on a daily level across years can not be extracted at once. Thus, they need to be standardized to be comparable. We do this following Brodeur et al. (2021) procedure. Since the monthly data has a common scale, these can be used to rescale the daily data – that only has the same scale within a given year. First, we extract the daily counts from the 1st of August till the 31st of October for each year 2011-2019 separately and standardize them by the monthly data. For example, the monthly average of the daily data in August is divided by the monthly August data and then rescaled to 100.

We use suicides (topic), the distinction between planning suicide and seeking suicide help via keywords following Till et al. (2020), health services, and mental health (both categories).[24]

[24] There are three types of searches in G-Trend data. First is searching for `keywords` that correspond to searches for exactly these words, which we use, for example, for the "R U OK". Second, one can use `topics`; these are a pre-specified collection of keyword searches that might vary over time to reflect the changing nature of what people search for; we use this for the "Suicide" topic (the suicide topic is "/m/06z5s" for example, these can be looked up via pytrends package in python). Finally, the broadest level is `categories`, which are a few pre-specified categories, such as medical facilities and services that capture people searching for doctors.

HILDA data

HILDA is a large representative yearly household panel survey that is, by chance, sampled between August and October each year. The sample size is presented in Figure C.2. Thus, we can use our framework to assess whether there were any changes in mental wellbeing following the awareness campaign (that may have grown over time). Since here we have the individual data, we use detailed personal information in this part of the analysis.

We focus on two primary outcomes, mental wellbeing and social support. To this end, we calculate two principal components one for the mental health questionnaire of the SF36 (and rescaled to 100) and analogously for selected questions capturing social support (cf. Web Appendix Table B.1). In some analyses, we add covariates to the specification motivated above (as discussed below).

Table B.1: Description of Variables

Variable	Description
PLIDA data	
Coroner data	
Intentional deaths	Following Case and Deaton (2015), we use International Statistical Classification of Diseases and Related Health Problems 10th Revision (ICD10) codes suicide: X60-84, Y87.0
Accidental poisonings	International Statistical Classification of Diseases and Related Health Problems 10th Revision (ICD10) codes: X40-45, Y10-15, Y45, 47, 49
Mental health care use	
Mental health treatment plan	Daily count of all Medicare (Medial Benefits Scheme - MBS) covered mental health treatment plan initiations by a general practitioner
Mental health prescriptions	Daily count of all Medicare (Pharmaceutical Benefits Scheme - PBS) covered mental health drugs: Antipsychotics, Anxiolytics, Hypnotics and sedatives, Psychostimulants, and Antidepressants
Google trend data	
R U OK? - monthly	Monthly searches extracted at once (14.06.21)
R U OK? - daily	Daily searches, adjusted via Brodeur et al. (2021) procedure. Aug, Sept, Oct, for each year and standardized by monthly searches.
Suicides topic	Scrapes daily searches in topic /m/06z5s - Suicides, adjusted as above
Suicide plan	'commit suicide + how to suicide + painless suicide + quick suicide + suicide methods' − adjusted as above
Suicide prevention	'lifeline + help suicide + hotline suicide + suicide hotline' − adjusted as above
HILDA data	
Mental wellbeing principal component	These questions are about how you feel and how things have been with you during the past 4 weeks. For each question, please give the one answer that comes closest to the way you have been feeling. How much of the time during the past 4 weeks:
	Feel full of life
	Been a nervous person
	Felt so down in the dumps nothing could cheer you up
	Felt calm and peaceful
	Have a lot of energy
	Felt down
	Felt worn out
	Been a happy person
	Felt tired

Variable	Description
Social support principal component	The following statements have been used by many people to describe how much support they get from others. How much do you agree or disagree with each? The more you agree, the higher the number of the box you should cross. The more you disagree, the lower the number of the box you should cross.
	I dont have anyone that I can confide in
	When I need someone to help me out, I can usually find someone
	I have no one to lean on in times of trouble
	I often need help from other people but can't get it
	There is someone who can always cheer me up when I'm down
Covariates	
Female	Whether or not individual identifies as female in survey
Age	Age in years of the individual
Rurality	Indicators house hold lives in rural area: 0-5 Major city - Remote
States	Indicators house hold lives in state or territory
Days since survey start	Indicators for days since first within year survey date date
Extended Covariates	
College	Indicator whether individual has a college degree
Married	Indicator whether individual is married/de-facto
Unemployed	Indicator whether individual is unemployed
Not-in-labor-force	Indicator whether individual is not-in-labor-force
Weekly hours worked	Count of hours worked in usual week
Equivalised hh income	Total household income, adjusted by nr. adults and children
Seifa	Deciles of area deprivation - SEIFA (education & occupation)
Precipitation	Precipitation is for the 24 hours before 9 am (local time), in mm. It is estimated using inverse distance weighting using all rainfall stations from the Australian Bureau of Meteorology (BoM) within 50km of the postcode centroid; see Ireland, Johnston, and Knott (2023) for more details.
Maximum temperature	Maximum temperature in 24 hours after 9 am (local time) in Degrees C, closest weather station, as above
LS weather	Life-satisfaction weighted weather: pre-sample regression index of weather on life satisfaction
Weather on R U OK? Day	Same definition as above but on the actual R U OK? Day (rather than the day of the survey) in the zip code, and an indicator for whether the rain was in the highest percentile of the precipitation distribution
Local unemployment rate	SA4 level unemployment rate in the respective year from Australian Bureau of Statistics (ABS).

Table B.2: Principal components mental wellbeing and social support (rescaled to 0-100)

Questions (Likert 1-7)	Weights	Rel. Weight
Mental wellbeing		
Rev-SCQ:A9a Vitality: Feel full of life	0.350	0.117
SCQ:A9b Mental Health: Been a nervous person	0.279	0.093
SCQ:A9c Mental Health: Felt so down in the dumps nothing could cheer you up	0.322	0.108
Rev-SCQ:A9d Mental Health: Felt calm and peaceful	0.345	0.115
Rev-SCQ:A9e Vitality: Have a lot of energy	0.340	0.114
SCQ:A9f Mental Health: Felt down	0.355	0.119
SCQ:A9g Vitality: Felt worn out	0.329	0.110
Rev-SCQ:A9h Mental Health: Been a happy person	0.348	0.116
SCQ:A9i Vitality: Felt tired	0.326	0.109
Component 1: 4.97391 Eigenvalue, Difference 3.89; Component 2: 1.089 (0.181)		
Social support		
There is someone who can always cheer me up	0.373	0.168
When I need someone to help me out, I can usually find someone	0.448	0.201
Rev: I have no one to lean on	0.498	0.224
Rev: Often I need help from others but can get it	0.415	0.187
Rev: I don't have anyone I can confide in	0.490	0.220
Component 1: 2.801 Eigenvalue, Difference 1.94 ; Component 2: 0.085 (0.221)		

Notes: "Rev-" stands for reversed questions.
Source: HILDA 2011-2019 (v19), own calculations.

Web Appendix C Additional descriptive information

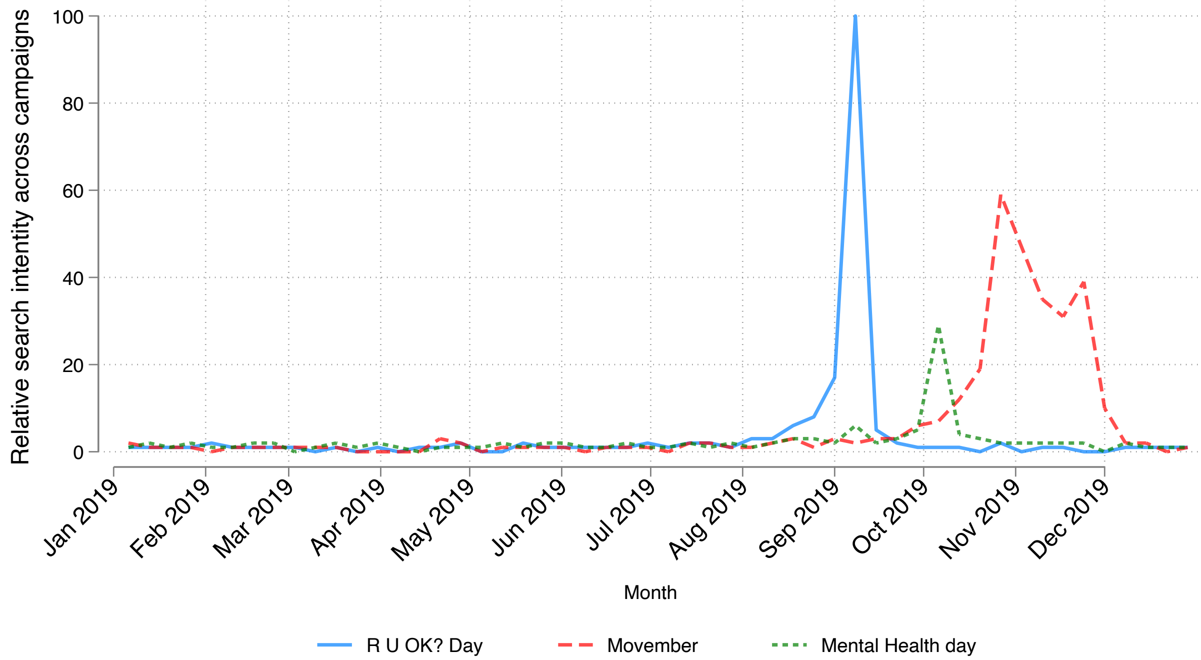

Figure C.1: Size of the campaign relative to other campaigns in the last sample year

Note: Displays Google trends for our last treatment year 2019 (1. Jan - 31. Dec), for the three search terms: "R U OK? day", "Mental Health day", and "Movember" in Australia.
Source: Google Trends search data (extracted 2023).

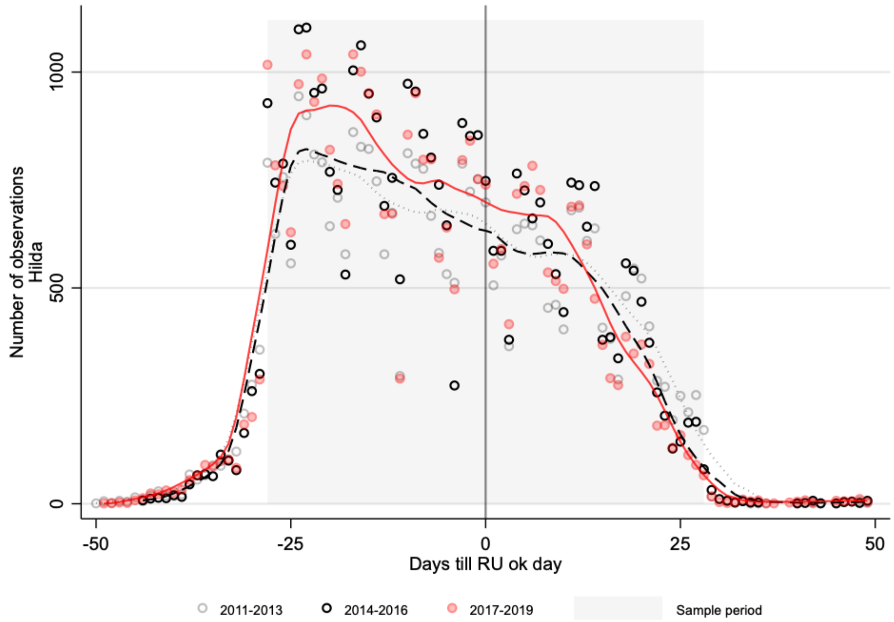

Figure C.2: Sampling time and observations HILDA across years relative to R U OK? Day

Note: Displays the raw three-year sums of the number of valid observations (N=109,340) in the overall HILDA population for each day relative to the yearly specific R U OK? Day. Lines are based on locally weighted regressions of the three-year sums on days till R U OK? Day. The gray background depicts the sample period we use in the later regression analysis

Source: HILDA 2011-2019 (v19), own calculations.

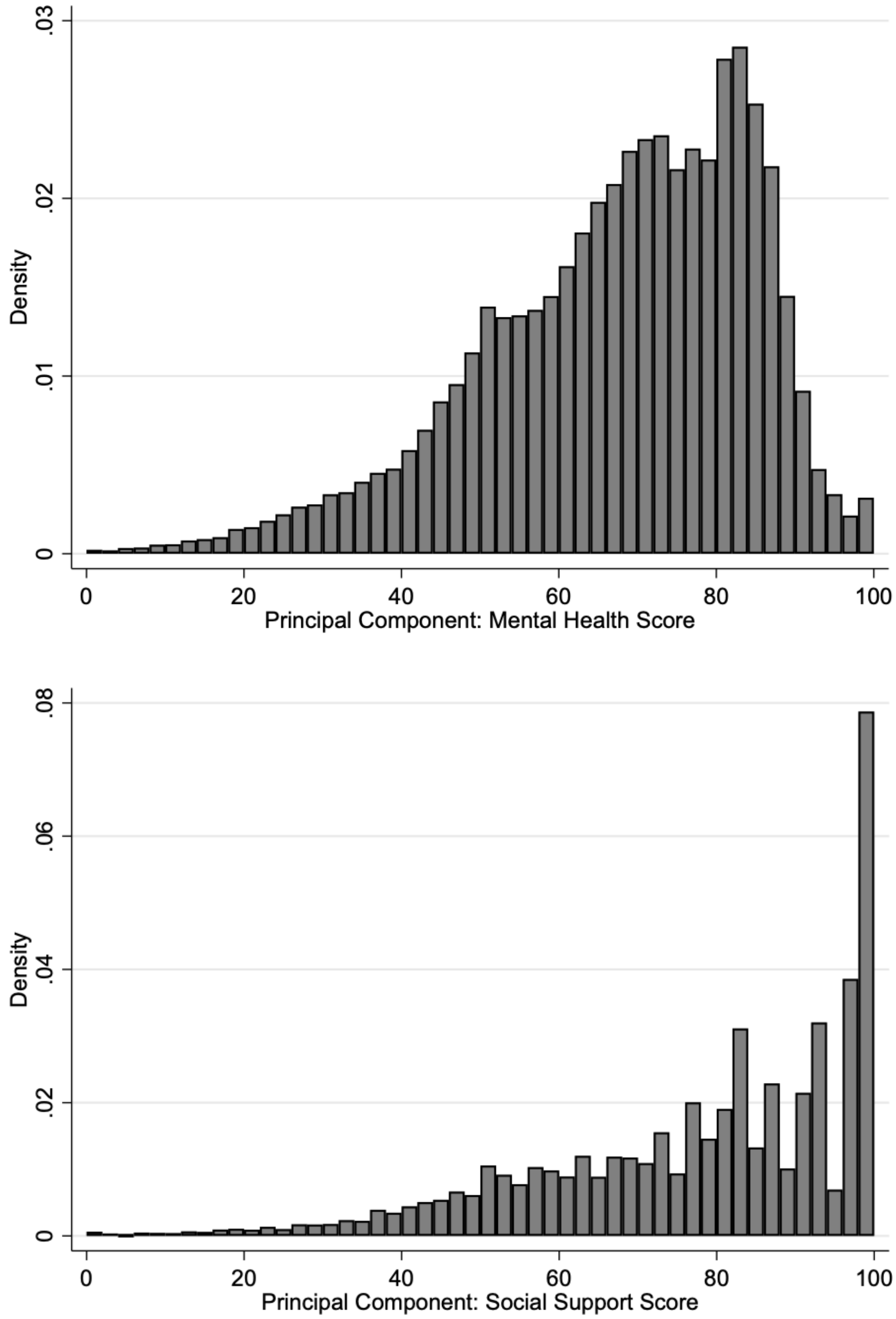

Figure C.3: Histograms of mental wellbeing & peer support indices

Note: Displays the raw distribution of standardized PCA-based mental wellbeing outcome (Top panel) and PCA-based social support score (bottom panel).
Source: HILDA 2011-2019 (v19), own calculations.

Table C.1: Descriptive statistics

Variables	Mean	SD
HILDA survey data		
Mental wellbeing principal component	67.50	17.23
Social support principal component	77.43	20.25
Covariates		
Female share	0.53	0.50
Age in years	44.88	18.69
Rurality (shares)		
Major Cities	0.66	0.47
Inner Regional	0.22	0.41
Outer Regional	0.11	0.31
Remote	0.01	0.11
Very Remote	0.00	0.05
States (shares)		
New South Wales	0.27	0.45
Victoria	0.23	0.42
Queensland	0.23	0.42
South Australia	0.10	0.30
Western Australia	0.10	0.30
Tasmania	0.03	0.16
Northern Territory	0.01	0.09
Australian Capital Territory	0.02	0.15
Extended Covariates		
College share	0.42	0.49
Married share	0.65	0.48
Unemployed share	0.04	0.19
Not-in-labor-force	0.31	0.46
Weekly hours worked share	22.52	20.60
Equivalised hh income (in 10'000)	4.45	3.92
Seifa decile	5.60	2.87
Precipitation (std)	0.00	0.97
Maximum temperature (std)	0.02	0.99
LS weather (std)	0.01	1.00
PLIDA - Medicare data		
Mental health treatment plans	5,880	3,551
Mental health prescriptions	74,055	26,565
PLIDA - Coroner data		
Intentional deaths	7.7	2.8
Accidental poisonings	3.7	2.1
Google trend data		
Suicides topic	17.89	7.56
Suicide prevention	33.98	17.65
Suicide plan	21.23	17.56

Notes: See Table 1, and notes therein. The table additionally presents descriptions of the main covariates used.

Source: HILDA 2011-2019, PLIDA 2011-2019, Google Trends search data 2011-2019.

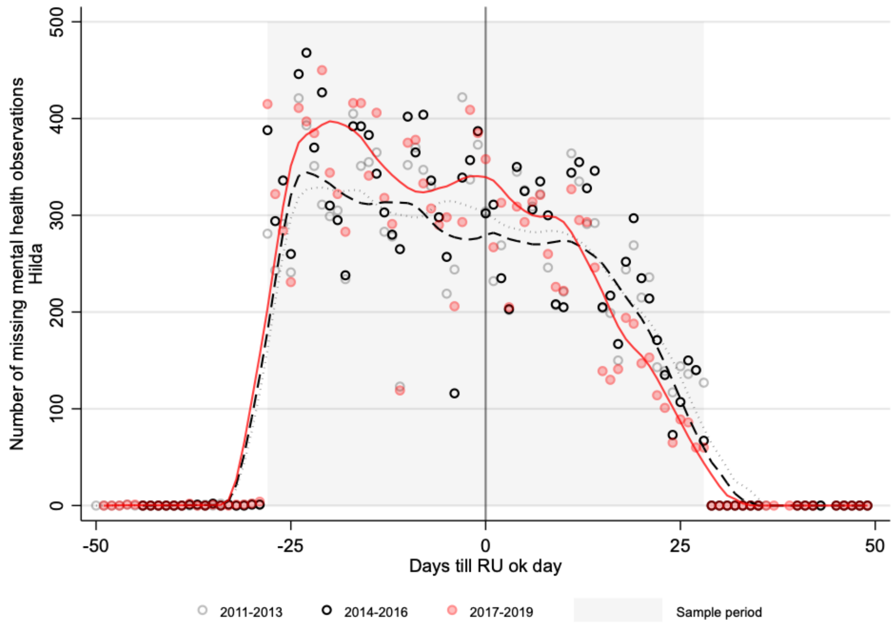

Figure C.4: Sampling time and observations HILDA across years relative to R U OK? Day: Item non-responses

Note: Displays the raw three-year sums of the number of invalid item responses for mental wellbeing questions, the gray background depicts the sample used in the later analysis.
Source: HILDA 2011-2019 (v19), own calculations.

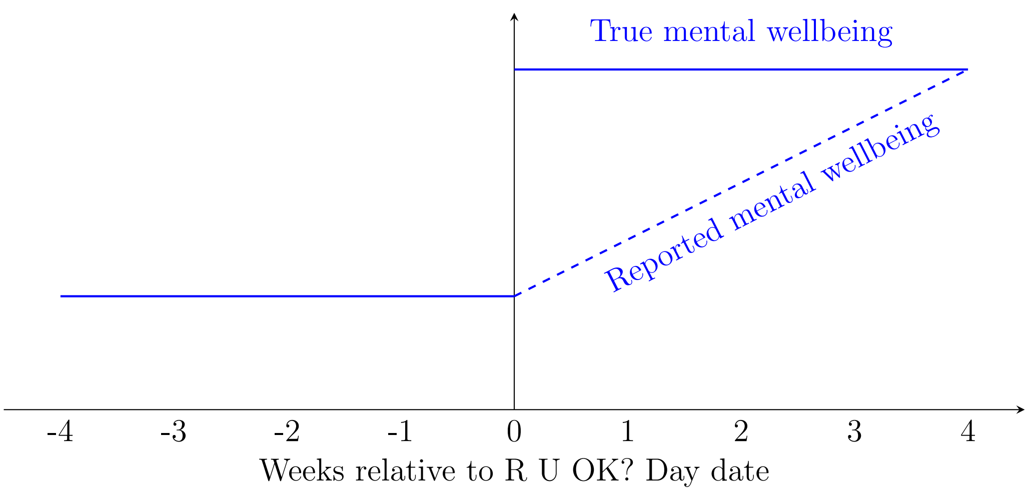

Figure C.5: Hypothetical reporting time dependence

Table C.2: Predicting survey timing from mental wellbeing

Dependent variables: Day of survey completion				
	All	2011-2013	2014-2016	2017-2019
	(1)	(2)	(3)	(4)
Day of survey				
PCA Mental wellbeing (0-100)	0.001	-0.002	0.002	0.002
	(0.002)	(0.004)	(0.003)	(0.003)
N	63,719	19,391	22,427	21,901
R^2	0.12	0.13	0.02	0.20
Mean dep.	30.53	30.89	29.38	31.40
SD dep.	8.51	8.54	8.01	8.85
Age fixed effects	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
State-by-rurality fixed effects	✓	✓	✓	✓

Notes: Table shows coefficient estimates from a regression of the PCA mental wellbeing score on the number of days from survey initiation (first survey) till the survey is completed by the individual, controlling for age, gender, state-by-rurality fixed-effects; standard errors are clustered on the individual level. The sample is based only on pre-R U, OK? Day observations that are not affected by the treatment. Column (1) uses the whole sample period, (2) for the first treatment period – the control in our main specification from 2011-2013; Column (3) for the first treatment period (2014-2016) and (4) for the second treatment period (2017-2019).

Source: HILDA 2011-2019 (v19), own calculations.

Web Appendix D Additional estimation results

Table D.1: Fixed effects life event regressions

Dependent variables: Principal components of mental wellbeing			
	All	Females	Males
	(1)	(2)	(3)
Mental wellbeing (0-100)			
Life event: Death of a spouse or child	-4.432	-5.250	-2.977
	(0.555)	(0.691)	(0.917)
Life event: Major financial worsening	-5.244	-5.183	-5.323
	(0.323)	(0.448)	(0.464)
Life event: Fired	-0.102	-0.917	0.474
	(0.248)	(0.419)	(0.303)
Life event: Married	0.815	0.796	0.839
	(0.266)	(0.382)	(0.369)
Life event: Separated	-2.449	-1.705	-3.417
	(0.285)	(0.388)	(0.417)
N	102,348	54,154	48,192
R^2	0.72	0.71	0.73
Mean dep.	67.62	66.06	69.38
SD dep.	17.19	17.57	16.58
Individual fixed effects	✓	✓	✓
Age fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓

Notes: To benchmark our main causal estimates, we estimate individual fixed effects panel regressions for major life events on our main survey outcome of interest, accounting for age and year fixed effects. Standard errors in parentheses are clustered on the individual level. Column (1) for the overall sample, (2) for females, and (3) for male subsamples.

Source: HILDA 2011-2019 (v19), own calculations.

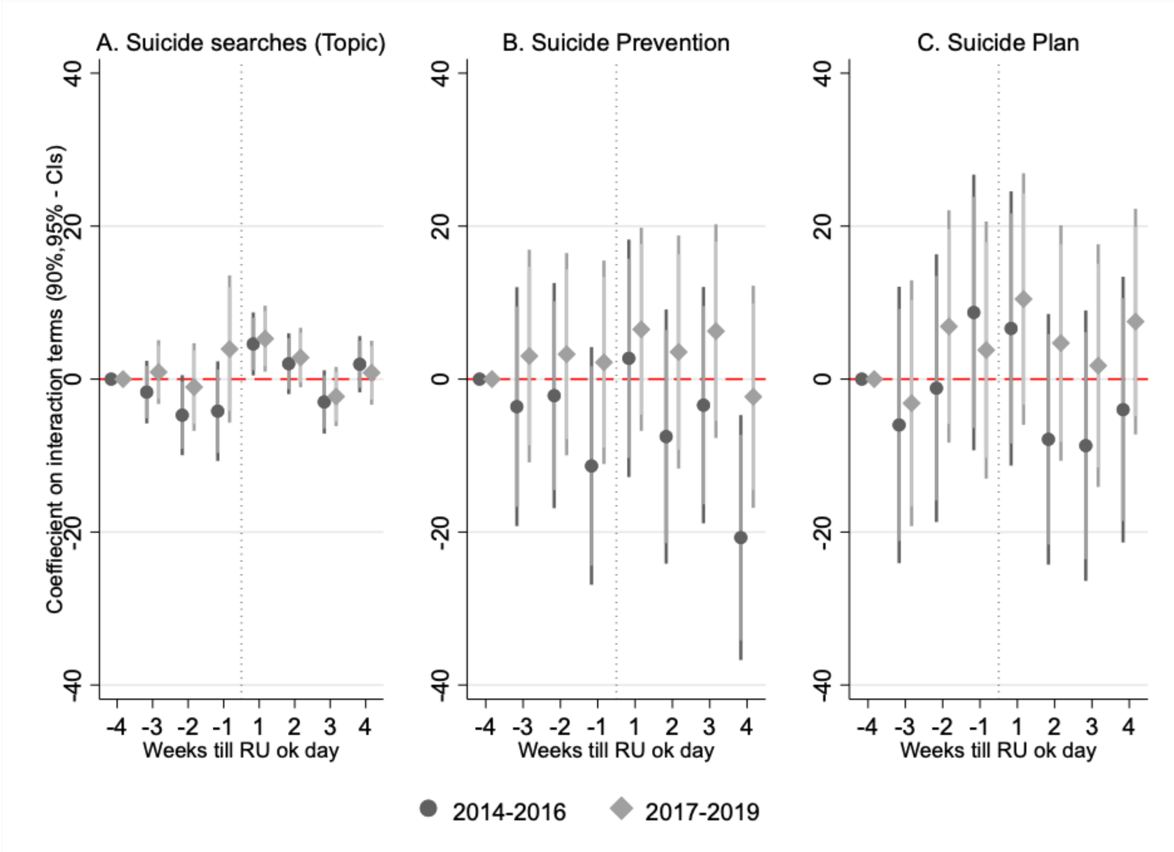

Figure D.1: Google Trend Coefficient by weeks relative to 2011-2013

Note: Displays the coefficient estimates analogous to Table 3, but interacting the treatment indicators with weeks till R U OK? rather than the post-R U OK? indicators, Panel A. corresponds to the Tables Column (6), B to (7), and C to (8).
Source: Google Trends, 2011-2019, own calculations.

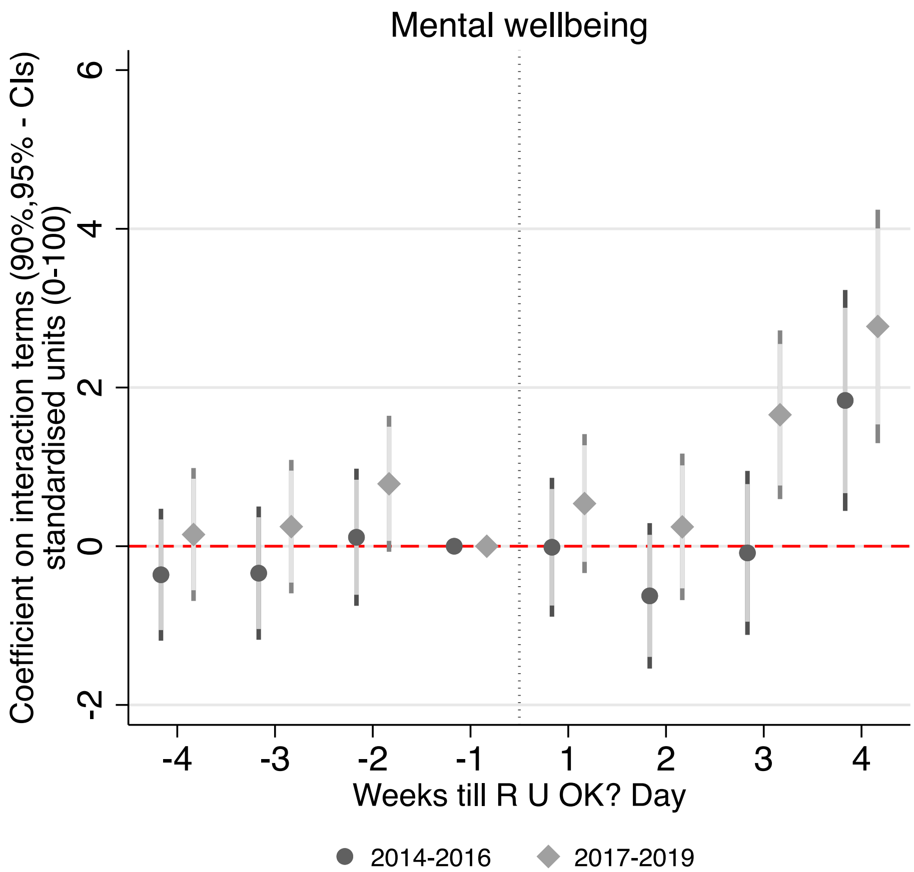

Figure D.2: Over weeks, including extended set of covariates

Note: The Figures displays coefficient estimates of regressions analogous to (2) in Table E.1 Column 2 disaggregated by four weeks-till pre- and post-R U OK? Day, for the mental wellbeing outcome, confidence bands show the 95% (dark) and 90% (light) confidence intervals. Dark grey shows the first treatment period from 2014-2016, and light grey shows the latter treatment period from 2017-2019; week -4 is used as a reference period. Figure 5, in the main text, reports the event study coefficients without the additional control variables.
Source: HILDA 2011-2019 (v19), BoM 2011-2019 own calculations.

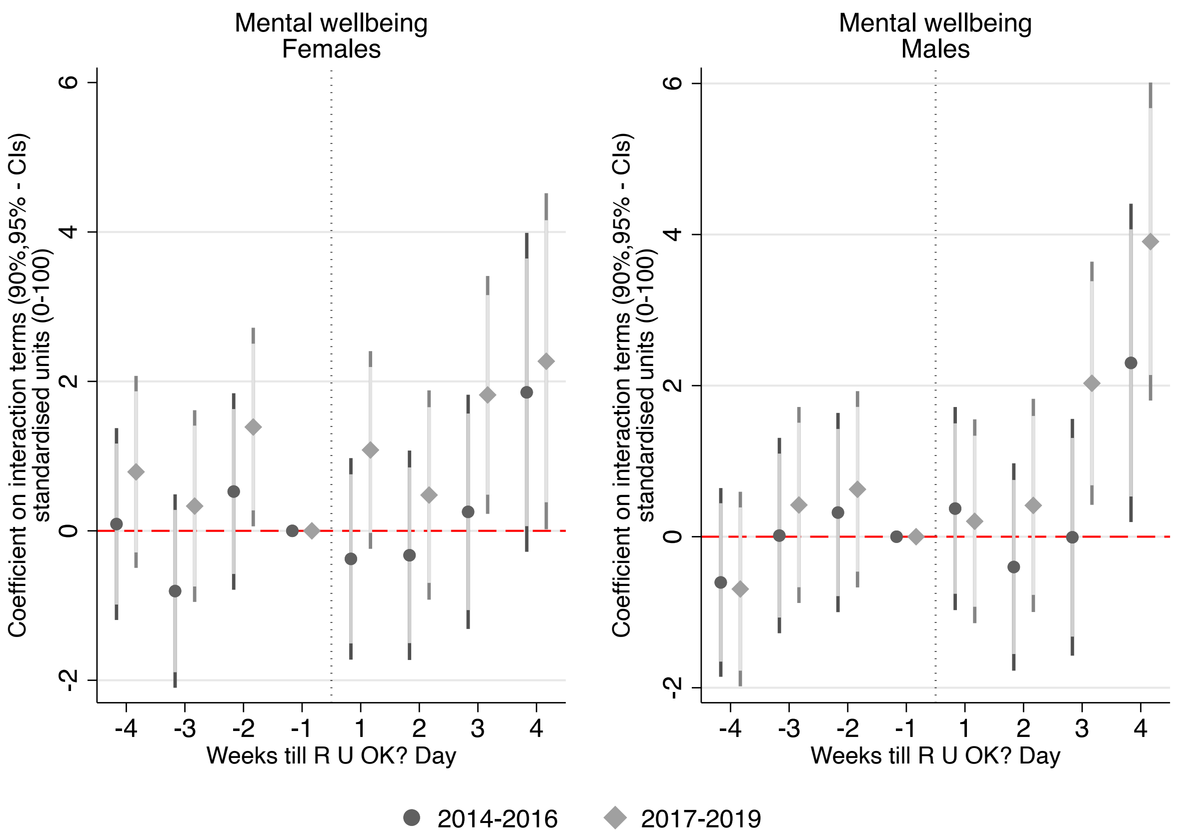

Figure D.3: Over weeks, by sex

Note: See Figure 5 and notes therein, estimated separately via subsample regressions by sex.

Source: HILDA 2011-2019 (v19), own calculations.

Table D.2: Extended regression results for Table 3 - Column 1

Dependent variables: Principal component of mental wellbeing			
	Base	Extended Covariates	Life Events
	(1)	(2)	(3)
2014-2016	0.167	0.121	0.042
	(0.275)	(0.256)	(0.256)
2017-2019	0.716	0.582	0.607
	(0.292)	(0.269)	(0.268)
Physical health principal component (0-100)		0.258	0.255
		(0.004)	(0.004)
College educated (Yes/No)		-0.111	-0.156
		(0.215)	(0.212)
Married/de-facto relationship (Yes/No)		2.723	2.101
		(0.221)	(0.225)
Unemployed (Yes/No)		-3.093	-2.039
		(0.377)	(0.375)
Not-in-labor force (Yes/No)		-2.406	-2.297
		(0.268)	(0.265)
Weekly work hours		0.034	0.029
		(0.006)	(0.006)
Equalized household income/10,000		0.121	0.114
		(0.019)	(0.019)
Area-level Seifa deciles (1-10)		0.197	0.176
		(0.036)	(0.036)
Life event: Death of a spouse or child			-5.082
			(0.678)
Life event: Major financial worsening			-11.029
			(0.440)
Life event: Fired			-1.377
			(0.339)
Life event: Married			2.276
			(0.360)
Life event: Separated			-4.446
			(0.371)
N	102,270	102,270	100,995
R^2	0.03	0.17	0.18
Mean dep.	67.50	67.50	67.56
SD dep.	17.23	17.23	17.22
Day and year fixed effects	✓	✓	✓
Days since survey start	✓	✓	✓
Basic set of covariates	✓	✓	✓
Extended set of covariates		✓	✓
Life events			✓

Notes: The Table presents selected coefficients estimates from equation (1) and individual-level cluster-robust standard errors, conditional age (indicators), sex (indicator), and rurality (indicators)-by-state fixed effects, and a day since the start of survey fixed effects, in Column (1) as in Table 3, and in (2) using the extended set of covariates from Table E.1 Column (2). In Column (3), the regressions additionally account for major life events used in Table D.1.
Source: HILDA 2011-2019 (v19), BoM 2011-2019, own calculations.

Web Appendix E Robustness checks

Alternative Regression Specifications. Table E.1 presents the results of robustness checks across a range of alternative regression specifications. Column (1) reproduces the average mental wellbeing effect documented in Table 3 as a baseline comparison. Column (2) extends the specification by including additional respondent characteristics and neighborhood factors. Column (3) changes the estimation sample by excluding the week immediately preceding R U OK? Day, which aims to address concerns about anticipatory effects due to early messaging, ensuring a cleaner distinction between pre- and post-treatment periods. Column (4) incorporates interviewer-fixed effects to account for potential survey biases stemming from differences in how interviewers engage with respondents. Across each specification, our estimates remain stable and qualitatively consistent.

Column (5) of Table E.1 examines the role of two contextual factors that could potentially threaten the validity of our identification strategy by impacting within-year dynamics in mental wellbeing across years. First, if local unemployment rates varied differently across weeks during later years (2017–2019) compared to earlier years (2011–2013), this could create bias in our results by influencing mental wellbeing independent of the campaign.[25] Second, if weather conditions in the post-R U OK? Day period were consistently warmer or drier in later years (2017–2019) compared to earlier years (2011–2013), this could bias our results.[26] To address these concerns, column (5) includes controls for local weather conditions and unemployment rates, mitigating the potential confounding effects of these broader environmental and economic variables on the relationship between R U OK? Day and mental wellbeing outcomes. The results highlight that neither weather nor unemployment explains the observed effects.

While survey timing does not appear to be systematically associated with observable individual characteristics (Table 2), concerns may remain about correlations with unobserved heterogeneity. With individual fixed effects, identification would come from within-person changes in mental wellbeing when the same respondent is interviewed on different sides of the R U OK? Day cutoff (pre vs post). In our panel, however, interview timing for a given respondent is relatively stable across waves/years, so only a limited subset of respondents "switch" between being observed before versus after the campaign. Consequently, an individual fixed effects specification would rely on a much smaller effective identifying sample and is not well-suited as our main approach. Instead, Column (6) reports a robustness check based on a residualized mental health outcome. Specifically, we first regress mental wellbeing on a second-degree polynomial in age and area fixed effects (state-by-rurality), and—where available—individual fixed effects for respondents observed more than once. We then estimate the campaign effect using this residualized outcome. The resulting estimates are consistent with the main specification, suggesting the wellbeing effect is not driven by age/area composition or time-invariant respondent differences.

We examine alternative weighting strategies to address the temporal reference in the HILDA survey's mental wellbeing questions, which ask respondents to evaluate their feelings "during the past 4 weeks," blending recent and more distant experiences. Since survey interviews occur on different days, the timing of R U OK? Day relative to each respondent's reference period varies, occurring just days before the survey for some and weeks earlier for others. This variability may dilute the immediate effects of R U OK? Day if responses reflect earlier experiences. Column (7) of Table E.1 provides estimates from a model that assumes people weight recent experiences (e.g. yesterday) more highly than experiences from the further past (e.g. 28 days ago) when considering their mental wellbeing. Column (8) assumes that people weight experiences from all days in the past 4 weeks equally. Both approaches increase the estimated effect on mental health for 2017–2019, with point estimates rising to 1.564 units (9.1% of a standard deviation) and 2.249 units (13.1% of a standard deviation). These effects correspond to a move from 50th to 54th percentile and 56th percentile respectively, which is equivalent to 29% and 50% of the mental wellbeing effect caused by a major financial worsening. These results demonstrate that alternate weighting strategies do not alter the direction or significance of

[25] Existing research research establishes a relationship between unemployment and lower levels of mental health (Paul and Moser 2009; Schmitz 2011; Frijters, Johnston, and Shields 2014; Strandh et al. 2014; Moscone, Tosetti, and Vittadini 2016).

[26] Prior research highlights that weather conditions, including temperature and precipitation, influence mental health outcomes, including suicide rates (Burke et al. 2018; Mullins and White 2019; Li, Ferreira, and Smith 2020; Hailemariam, Churchill, and Appau 2023; Hua, Qiu, and Tan 2023; Thompson et al. 2023).

Table E.1: R U OK? Day and Self-Reported Mental Wellbeing - Robustness

Dependent variables: Principal component of mental wellbeing								Weighting	
	Base	+ Co-variates	Two-way clustering	Exclude week -1	Interviewer FEs	Weather Unempl.	Residualised	Recency	Equal
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Two treatment periods									
2014-2016	0.167	0.121	0.167	0.150	0.117	0.173	-0.213	0.559	0.971
	(0.275)	(0.256)	(0.262)	(0.290)	(0.276)	(0.276)	(0.247)	(0.394)	(0.513)
2017-2019	0.716	0.582	0.716	0.620	0.618	0.746	0.670	1.564	2.249
	(0.292)	(0.269)	(0.299)	(0.311)	(0.292)	(0.292)	(0.275)	(0.421)	(0.548)
B. One treatment period									
2014-2019	0.658	0.532	0.635	0.585	0.563	0.683	0.772	1.051	1.589
	(0.247)	(0.229)	(0.258)	(0.262)	(0.248)	(0.248)	(0.236)	(0.354)	(0.460)
N	102,270	102,270	102,270	88,069	102,264	102,270	102,270	102,270	102,270
R^2	0.03	0.17	0.03	0.03	0.04	0.03	0.03	0.03	0.03
Mean dep.	67.50	67.50	67.50	67.56	67.50	67.50	67.50	67.50	67.50
SD dep.	17.23	17.23	17.23	17.19	17.23	17.23	17.23	17.23	17.23
Day and year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Days since survey start	✓	✓	✓	✓	✓	✓	✓	✓	✓
Basic set of covariates	✓	✓	✓	✓	✓	✓	✓	✓	✓
Extended set of covariates		✓						✓	

Notes: The Table presents coefficient estimates from equation (2), analogous to Table 3. Column (1) repeats the mental wellbeing baseline results for reference; Column (2) includes extended covariates from Table 2; selected estimates are presented in Web Appendix Table D.2; Column (3) reports results using two-way clustered standard errors (individual and year); Column (4) excludes the week before R U OK? Day; Column (5) includes interviewer fixed effects; Column (6) includes weather and unemployment; Column (7) uses a residualised mental health measure, via an auxiliary first stage regression - regressing out individual fixed effects, second order polynomial in age and state-by-rurality fixed effects; and Columns (8) and (9) reflect different weighting strategies.

Source: HILDA 2011–2019 (v19), BoM 2011–2019, ABS 2011–2019, own calculations.

observed effects and highlight the potential conservatism of our unweighted estimates. Since prior research offers little guidance on how respondents weigh recent versus distant experiences, we adopted the simpler, conservative approach for our main analysis.

Using Reported Survey Responses. Our primary analysis uses a mental wellbeing index constructed from nine standardized questions from the Health Survey Questionnaire component of HILDA. To demonstrate that our results are not driven by the construction of the index, Web Appendix Table E.2 presents estimates for each individual mental wellbeing question. The estimates demonstrate positive and statistically significant improvements in mental wellbeing attributable to R U OK? Day during the 2017-2019 period, with all showing a positive tendency in the second period and five out of nine questions showing significant positive effects. Specifically, we observe improvements in feeling full of life, calm and peaceful, having energy, feeling less worn out, and being a happy person. Furthermore, similar to our main analysis, there are no statistically significant mental wellbeing effects found across all responses for the 2014-2016 period. These findings provide further support for R U OK? Day positively impacting mental wellbeing by revealing consistent, modest improvements across multiple mental wellbeing dimensions.

Expanding the Post-Event Time Window. We extend the event study window to 8 and 12 weeks post-campaign for the administrative outcomes—mental healthcare use and suicide-related deaths—where data are available over a longer post-event horizon. We cannot do the same for the mental wellbeing outcome, as 95.5% of respondents in the HILDA sample are interviewed within August, September, and October, leaving too few observations beyond four weeks post R U OK? Day. The results are presented in Table E.3 and show no evidence of delayed effects for the administrative outcomes.

Additional Robustness Checks for Death Outcomes. We conduct further robustness analyses for death outcomes in Web Appendix Table E.4. Specifically, we estimate Poisson regressions to account for the count nature of the outcome variables and estimate regressions with the outcomes in per-capita form (dividing the outcome by population size). We also present estimates for a broader definition of deaths of despair (Case and Deaton 2015), which includes deaths related to alcohol consumption. All the estimates indicate that R U OK? Day did not significantly reduce deaths.

Table E.2: R U OK? Day and Self-Reported Mental Wellbeing - Subscales

Dependent variables: Standardised subscales of mental wellbeing questions (1 - All of the time to 6 - None of the time)									
	Feel full of life^r	Been a nervous person	Nothing can chear up	Felt calm and peaceful^r	Have a lot of energy^r	Felt down	Felt worn out	Been a happy person^r	Felt tired
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
2014-2016	0.023	-0.000	0.001	0.001	0.024	-0.007	0.003	0.021	-0.004
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
2017-2019	0.042	0.018	0.011	0.035	0.054	0.016	0.033	0.044	0.022
	(0.017)	(0.016)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
N	102,270	102,270	102,270	102,270	102,270	102,270	102,270	102,270	102,270
R^2	0.03	0.06	0.02	0.02	0.05	0.03	0.04	0.01	0.05
Mean dep.	0.00	0.00	0.01	0.00	0.00	0.01	0.00	0.00	0.00
SD dep.	1.00	1.00	0.99	1.00	1.00	1.00	1.00	1.00	1.00
Day and year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Days since survey start	✓	✓	✓	✓	✓	✓	✓	✓	✓
Basic set of covariates	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The Table presents coefficients estimates analogous to Table 3, but replaces the principal component with its sub-components, c.f. Table B.2 for the precise questions, all range from all the time to none of the time and the ^{r} - in the column header - denotes when they are reversed, i.e. all coded positively, meaning higher values correspond to better mental wellbeing. The outcomes are standardized to be mean 0 and standard deviation 1, as shown in the bottom, to be comparable across columns.

Source: HILDA 2011-2019 (v19), own calculations.

Table E.3: R U OK? Day and Outcomes with Longer Post-Event Timing

Dependent variables: Outcome levels at 4, 8, and 12 weeks after event						
	Mental health treatment plan			Mental health prescriptions		
	4 weeks	8 weeks	12 weeks	4 weeks	8 weeks	12 weeks
Panel A. Mental health outcomes						
2014–2016	-281.97	-170.81	-244.73	-935.45	-684.78	-619.83
	(194.14)	(157.49)	(144.20)	(1172.12)	(822.86)	(716.69)
2017–2019	-243.16	-173.41	-307.82	-539.26	-453.40	-523.96
	(222.99)	(175.78)	(159.66)	(1348.80)	(922.41)	(797.49)
R^2	0.945	0.950	0.955	0.963	0.969	0.973
Mean dep.	5,880	5,997	6,065	74,055	74,419	75,132
	Intentional self-harm			Accidental Poisonings		
	4 weeks	8 weeks	12 weeks	4 weeks	8 weeks	12 weeks
Panel B. Suicide-related mortality						
2014–2016	-0.123	-0.115	-0.048	0.272	0.203	0.237
	(0.601)	(0.537)	(0.506)	(0.441)	(0.367)	(0.343)
2017–2019	-0.160	0.255	-0.007	-0.006	-0.171	-0.337
	(0.570)	(0.499)	(0.468)	(0.425)	(0.364)	(0.348)
R^2	0.173	0.174	0.176	0.195	0.187	0.180
Mean dep.	7.65	7.77	7.81	3.72	3.60	3.54
N	504	756	1,008	504	756	1,008

Notes: This table presents coefficient estimates from equation (2), estimated separately for 4, 8, and 12 weeks post R U OK? Day. See Table 3 and notes therein.

Source: PLIDA 2011–2019, own calculations.

Table E.4: Robustness - PLIDA death results

Dependent variables: Number of deaths per day by ICD-codes							
	Intentional self harm			Accidental poisoning			Deaths
	Base	Poisson	Per capita	Base	Poisson	Per capita	of despair
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
2014-2016	-0.203	-0.026	-0.130	0.271	0.088	0.273	0.264
	(0.600)	(0.075)	(0.602)	(0.441)	(0.117)	(0.443)	(0.847)
2017-2019	-0.218	-0.028	-0.165	0.006	0.024	-0.004	0.394
	(0.573)	(0.072)	(0.570)	(0.425)	(0.112)	(0.426)	(0.844)
N	504	504	504	504	504	504	504
R^2	0.18		0.17	0.19		.20	0.23
Day and year fixed effects	✓	✓	✓	✓	✓	✓	✓

Notes: Table presents coefficient estimates for death outcomes presented in Table 3 columns (4) and (5), represented in Columns (1) and (4) here. Columns (2) and (5) use Poisson regressions rather than linear regressions, (3) and (6) present outcomes as shares of the population. Column (7) combines the counts from (1) and (4) and includes other deaths associated with despair (Case and Deaton 2015).

Source: PLIDA 2011-2019, own calculations.

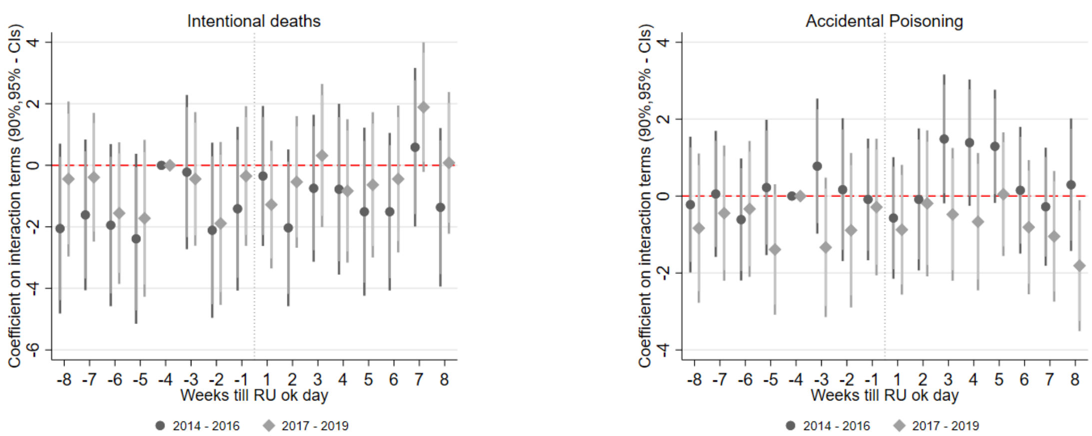

Figure E.1: Coefficient by weeks relative to 2011-2013, 2 months around cutoff

Note: Displays the coefficient estimates analogous to Table 5 Panels (d) and (e) see notes therein. They display 8 weeks on either side rather than 4.

Source: PLIDA 2011-2019, own calculations.

Table E.5: Pre-trend (non-top up sample)

Dependent variables: PCA mental wellbeing			
	All	Females	Males
	(1)	(2)	(3)
2002-2004	-0.175	-0.483	0.170
	(0.487)	(0.692)	(0.680)
2005-2007	-0.047	-0.573	0.564
	(0.486)	(0.692)	(0.677)
2008-2010	0.424	-0.070	1.035
	(0.470)	(0.668)	(0.657)
2011-2013		omitted cat.	
2014-2016	0.274	0.182	0.403
	(0.270)	(0.382)	(0.379)
2017-2019	0.668	0.526	0.844
	(0.290)	(0.405)	(0.413)
N	182,134	96,531	85,603
R^2	0.02	0.02	0.02
Day and year fixed effects	✓	✓	✓
Days since survey start	✓	✓	✓
Basic set of covariates	✓	✓	✓

Notes: The Table presents coefficients estimates from Figure 6 – see notes therein – in Column (1) and by (2) females, and (3) males for the non-top up sample only.

Source: HILDA 2002-2019 (v19), own calculations.

Web Appendix F Demographic Differences Regressions

Table F.1: Heterogeneity by age and sex: Survey outcomes

Dependent variables: Principal components of mental wellbeing and social support									
		Females				Males			
	all	all	15-24	25-49	50+	all	15-24	25-49	50+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Mental wellbeing									
2014-2016	0.167	0.092	1.213	0.062	-0.283	0.288	-0.834	1.159	-0.134
	(0.275)	(0.390)	(0.976)	(0.605)	(0.613)	(0.387)	(0.923)	(0.602)	(0.620)
2017-2019	0.716	0.574	0.406	0.932	0.241	0.897	0.479	1.485	0.486
	(0.292)	(0.409)	(1.038)	(0.612)	(0.657)	(0.416)	(0.970)	(0.642)	(0.671)
N	102,270	54,119	9,101	22,847	22,171	48,151	8,371	20,151	19,629
R^2	0.03	0.02	0.05	0.02	0.03	0.02	0.04	0.02	0.03
Mean dep.	67.50	65.93	63.71	65.06	67.75	69.26	69.73	68.03	70.32
SD dep.	17.23	17.61	18.21	17.23	17.57	16.62	16.09	16.55	16.83
Panel B. Social support									
2014-2016	0.298	-0.500	1.127	-0.421	-1.281	1.227	-0.899	2.183	1.123
	(0.328)	(0.460)	(1.086)	(0.734)	(0.710)	(0.468)	(1.086)	(0.733)	(0.750)
2017-2019	0.507	-0.141	0.543	0.007	-0.529	1.226	0.167	2.043	0.906
	(0.346)	(0.480)	(1.175)	(0.747)	(0.763)	(0.500)	(1.155)	(0.767)	(0.806)
N	102,974	54,531	9,144	22,990	22,397	48,443	8,427	20,257	19,759
R^2	0.02	0.01	0.04	0.02	0.02	0.02	0.04	0.03	0.02
Mean dep.	77.43	79.04	79.43	79.25	78.67	75.61	77.55	75.56	74.85
SD dep.	20.25	20.30	19.92	20.36	20.39	20.03	19.11	20.14	20.24
Day and year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Days since survey start	✓	✓	✓	✓	✓	✓	✓	✓	✓
Basic set of covariates	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Analogous to Table 3 and subsamples described in Figure 7 see notes therein. More information on the construction of the social support principal component can be found in Table B.2.

Source: HILDA 2011-2019 (v19), own calculations.

Table F.2: Heterogeneity by age and sex: National administrative datasets

Dependent variables: Number mental health care service use and deaths due to mental health-related conditions

		Females				Males			
	all	all	15-24	25-49	50+	all	15-24	25-49	50+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Mental health treatment plan									
2014-2016	-281.97	-190.71	-54.91	-99.82	-35.98	-91.26	-20.33	-46.07	-24.87
	(194.14)	(122.59)	(33.97)	(59.66)	(33.11)	(72.53)	(19.08)	(37.30)	(18.65)
2017-2019	-243.16	-185.17	-36.53	-132.29	-16.35	-57.99	-6.80	-44.22	-6.97
	(222.99)	(141.45)	(43.46)	(68.14)	(35.32)	(82.33)	(24.11)	(42.21)	(19.33)
N	504	504	504	504	504	504	504	504	504
R^2	0.95	0.95	0.90	0.95	0.96	0.94	0.90	0.94	0.96
Mean dep.	5,880	3,824	695	1,892	1,237	2,056	378	1,053	625
SD dep.	3,551	2,310	495	1,125	729	1,244	271	630	365
Panel B. Mental health prescriptions									
2014-2016	-935.45	-654.94	-42.74	-206.90	-405.30	-280.51	6.74	-90.59	-196.66
	(1172.1)	(715.14)	(59.11)	(222.19)	(455.09)	(460.54)	(36.32)	(162.95)	(270.24)
2017-2019	-539.26	-352.71	-40.72	-161.15	-150.84	-186.55	33.45	-81.33	-138.68
	(1348.8)	(822.14)	(80.76)	(264.40)	(503.33)	(529.87)	(49.11)	(190.59)	(300.18)
N	504	504	504	504	504	504	504	504	504
R^2	0.96	0.96	0.96	0.97	0.97	0.96	0.95	0.96	0.96
Mean dep.	74,055	46,647	2,570	15,206	28,872	27,407	1,930	9,830	15,648
SD dep.	26,565	16,534	1,395	5,247	10,516	10,045	801	3,611	5,842
Panel C. Intentional self harm									
2014-2016	-0.123	0.003	-0.006	-0.095	0.104	-0.125	-0.168	0.134	-0.091
	(0.601)	(0.298)	(0.112)	(0.204)	(0.181)	(0.511)	(0.171)	(0.368)	(0.333)
2017-2019	-0.160	0.502	0.066	0.188	0.249	-0.662	0.005	-0.132	-0.534
	(0.570)	(0.291)	(0.101)	(0.194)	(0.177)	(0.515)	(0.178)	(0.364)	(0.330)
N	504	504	504	504	504	504	504	504	504
R^2	0.17	0.16	0.17	0.21	0.14	0.16	0.17	0.14	0.13
Mean dep.	7.70	2.00	0.29	0.90	0.76	5.70	0.72	2.80	2.20
SD dep.	2.80	1.40	0.50	0.97	0.84	2.40	0.84	1.70	1.50
Panel D. Accidental poisoning									
2014-2016	0.272	0.038	-0.013	0.063	-0.011	0.234	0.095	0.122	0.017
	(0.441)	(0.241)	(0.048)	(0.170)	(0.158)	(0.337)	(0.073)	(0.273)	(0.176)
2017-2019	-0.006	0.029	-0.035	0.167	-0.103	-0.034	-0.025	-0.150	0.140
	(0.425)	(0.244)	(0.052)	(0.167)	(0.168)	(0.333)	(0.070)	(0.275)	(0.171)
N	504	504	504	504	504	504	504	504	504
R^2	0.20	0.14	0.16	0.15	0.08	0.21	0.09	0.19	0.17
Mean dep.	3.70	1.20	0.06	0.63	0.52	2.50	0.10	1.70	0.70
SD dep.	2.10	1.10	0.23	0.79	0.74	1.60	0.32	1.30	0.86
Day and year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Analogous to Table 3 and subsamples described in Figure 7 see notes therein.
Source: PLIDA 2011-2019, own calculations.