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YOUR NAME HERE[1], YOUR CO-AUTHOR HERE[2]

[1] Victoria University of Wellington, New Zealand ORCID 0000-0000-0000-0000
[2] Georgia State University, Matheny Center for the Study of Stress, Trauma, and Resilience ORCID 0000-0000-0000-0000

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Abstract

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KEYWORDS: *Causal Inference*; *Cross-validation*; *Longitudinal*; *Machine Learning*; *Semi-parametric*; *Targeted Learning*; *TMLE*; **OTHERS**; **FEWER**.

Introduction

A central question in the scientific study of XXXX is whether YYYY fosters ZZZZZ (De Coulanges 1903; Johnson 2005; Norenzayan *et al.* 2016; Schloss and Murray 2011; Sosis and Bressler 2003; Swanson 1967; Watts *et al.* 2015; Watts *et al.* 2016; Wheatley 1971; Whitehouse *et al.* 2023). However, quantifying causal effects for YYYY, and many other social behaviours presents significant challenges.

Investigators have limited scope to randomise YYYY on ZZZZZ. On the other hand, valid causal inferences from non-experimental or observational data must combine high-resolution repeated-measures time-series data with robust methods for causal inference. Few studies meet this standard…

An encouraging recent attempt to obtain valid causal inference is PPPP's thoughtful investigation of the relationships between religious attendance, beliefs, and affiliation on blood donations among pregnant women and their partners who were residents of Bristol, United Kingdom, in the early 1990s and participated in the Avon Longitudinal Study of Parents and Children, $N = 13,477$ mothers and $N = 13,424$ partners (Major-Smith 2023). PPPP's study begins with a careful overview of the threats to causal inference from confounding and selection bias….

Although we may sometimes use cross-sectional associations to obtain credible suggestions about causality, we cannot typically attach causal interpretations, at least not without strong assumptions about the relative order and timing of events (VanderWeele 2021). Indeed, below we report an analysis restricted to baseline New Zealand Attitudes and Values Study data that observes a 2.65 times overstatement for the effect of perfectionism on anxiety and a 2.43 times overstatement for perfectionism's effect on depression…

Here, to obtain causal inferences from time-series data, we leverage comprehensive panel data from a **SYNTHETIC DATASET** for 19,997 participants in the New Zealand Attitudes and Values Study from 2018-2021 to quantify the effects of clearly defined interventions in religious attendance across the population of New Zealanders on two features of mental health: "Depression" and "Anxiety" These outcomes are called "counterfactual" or "potential" outcomes, terms we use interchangeably (Pearl 2009; Robins 1986; Rubin 2005; Splawa-Neyman *et al.* 1990; Van Der Laan and Rose 2018).[1]

A fundamental challenge in observational studies is to ensure *balance* between the variables in interventions or "treatments" to be compared that might affect both treatment and the potential outcomes under treatment

[1] Philosophical disagreements about the meanings assigned to "potential" and "counterfactual" outcomes do not affect our use.

(Shiba and Kawahara 2021). We call the state of imbalance *confounding*, and the strategy for ensuring balance, *confounding control.* In this study, we express the interventions on religious service as "modified treatment policies" (Díaz *et al.* 2021, 2023; Haneuse and Rotnitzky 2013; Hoffman *et al.* 2023). We obtain causal inferences by contrasting inferred population averages under different modified treatment policies.

Our initial causal contrast investigates: "What would be the average difference across the New Zealand population if everyone were to become one unit greater on a 1-7 ordinal scale in Perfectionism versus the status quo" This contrast addresses the practically interesting question …

A second analysis investigates whether there are differences in the causal effects of perfectionism among those born in New Zealand and those born oveseas. A considerable body of acculturation research suggestions… yadda yadda

Note that our approach does not focus on testing specific hypotheses; instead, we aim to compute our pre-specified causal contrasts with high accuracy by combining appropriate time-series data and robust methods for causal inference (Hernán and Greenland 2024).

Method

Sample

Data were *SIMULATED* from responses to the New Zealand Attitudes and Values Study (NZAVS), an annual longitudinal national probability panel study of social attitudes, personality, ideology, and health outcomes in New Zealand. Chris G. Sibley started the New Zealand Attitudes and Values Study in 2009, which has grown to include a community of over fifty researchers. In this simulated dataset there were 20,000 New Zealand residents. The New Zealand Attitudes and Values Study operates independently of political or corporate funding and is based in a university setting. Data summaries for our study sample on all measures used in this study are found in **Appendices B-D**. For more details about the New Zealand Attitudes and Values Study see: OSF.IO/75SNB.

Treatment Indicator

Perfectionism was assessed using a three item "almost perfect scale":

- *Yadda*
- *Yadda*
- *Dadda*

Measures of Well-Being

Kessler-6. Yadda…

- Anxiety: *"Yadda*

- Depression: *Yadda*

Subgroup Analysis

We assessed group differences in these effects ….

We provide comprehensive details of all measures in **Appendix A**.

Causal Interventions

We define three targeted causal contrasts (*causal estimands*) as interventions on prespecified modified treatment policies (refer to Haneuse and Rotnitzky (2013); Díaz *et al.* (2021); Díaz *et al.* (2023)). Let A_t denote the treatment – monthly frequency of religious service. There are three time points: $t \in 0, 1, 2$, where $t = 0$ denotes the baseline wave, $t = 1$, the treatment wave, and $t = 2$ at the end of the study. $\mathbf{d}(\cdot)$ denotes a modified treatment policy $f_{\mathbf{d}}$.

When a treatment is fixed to a level defined by the modified treatment policy, perhaps contrary to a participant's observed level of treatment, we use the lowercase symbol a_1. Here, the functions defined by modified treatment policies $f_{\mathbf{d}}$ are interventions that fix A_1 to a_1.

1. **Regular Religious Service Treatment**: Administer treatment that leads to a +1 unit greater perfectionism to everyone in the adult population from 1-7 on the perfectionism scale. If an individual's perfectionism is within one unit of the top of the range, adminster the maximum value at the range:

$$\mathbf{d}^{\lambda}(a_1) = \begin{cases} 7 & \text{if } a_1 < 6 \\ a_1 + 1 & \text{otherwise} \end{cases}$$

3. **Status Quo – No Treatment**: Apply no treatment. Each expected mean outcome is calculated using each individual's natural (observed) value of religious service attendance.

$$\mathbf{d}(a_1) = a_1$$

Causal Contrasts

From these policies, we compute the following causal contrasts.

Target Contrast B: 'Regular vs. Status Quo': What is the marginal effect of the treatment in New Zealand compared with its status quo?

$$\text{Regular Religious Service vs. No Treatment} = E[Y(\mathbf{d}^{\lambda}) - Y(\mathbf{d})]$$

This contrast reflects a policy-relevant hypothetical experiment examining the effect of shifting everyone's perfectionism up by one point, allowing us to quantitatively assess how much a society in which everyone attends would differ from a society in its current state.

Identification Assumptions

To consistently estimate a causal effect, investigators must satisfy three assumptions:

1. **Causal consistency:** potential outcomes must correspond with observed outcomes under the treatments in the data. Essentially, we assume potential outcomes do not depend on how the treatment was administered, conditional on measured covariates (VanderWeele 2009; VanderWeele and Hernan 2013).
2. **Exchangeability**: given observed covariates, we assume treatment assignment is independent of the potential outcomes to be contrasted. In other words, there is "no unmeasured confounding" (Chatton *et al.* 2020; Hernan and Robins 2024).
3. **Positivity:** every individual must have a non-zero chance of receiving the treatment, regardless of their covariate values Westreich and Cole (2010). We evaluate this assumption in each study by examining changes in religious service attendance from baseline (NZAVS time 10) to the treatment wave (NZAVS time 11). For further discussion of these assumptions in the context of NZAVS studies, see Bulbulia *et al.* (2023).

Target Population

The target population for this study comprises New Zealand residents as represented in the baseline wave of the **SIMULATED** New Zealand Attitudes and Values Study (NZAVS) during the years 2018-2019, weighted by New Zealand Census weights for age, gender, and ethnicity (refer to Sibley (2021)). The NZAVS is a national probability study designed to reflect the broader New Zealand population accurately. Despite its comprehensive scope, the NZAVS does have some limitations in its demographic representation. Notably, it tends to slightly under-sample males and individuals of Asian descent while slightly over-sampling females and Māori (the indigenous peoples of New Zealand). To address these disparities and enhance the accuracy of our findings, we apply constructed survey weights to address the gender imbalance, which was presented largest of threat to external validity. These sample weights were integrated into statistical models using the `weights` option in `lmtp` (Williams and Díaz 2021), following protocols stated in Bulbulia (2024a).

Eligibility Criteria

To be included in the analysis of this study, participants needed to meet the following eligibility criteria:

Inclusion Criteria

- Enrolled in the **SIMULATED** 2018 wave of the New Zealand Attitudes and Values Study (NZAVS time 10).
- Missing covariate data at baseline was permitted, and the data was subjected to imputation methods to reduce bias. Only information obtained at baseline was used for such imputation (refer to Zhang *et al.* (2023)). Participants may have been lost to follow-up the end of study NZAVS time 11 or 12. We constructed inverse probability of censoring weights for missing responses at time 11. We adjusted for attrition and non-response at time 12 automatically by specifying a censoring indicator to `lmtp` when estimating outcomes as described below.

Exclusion Criteria

- Missing data in the perfectionism scale at baseline, wave 10 of the **SIMULATED** New Zealand Attitudes and Values Study.

A total of 19,997 **SIMULATED** individuals met these criteria and were included in the study.

Causal Identification

Table 1 presents three Single World Intervention Graphs (SWIGs) that describe our confounding control (identification strategy) (Richardson *et al.* 2023; Richardson and Robins 2013, 2023; Robins and Richardson 2010; Shpitser *et al.* 2022; Shpitser and Tchetgen 2016). Our approach consistently applies the same identification strategy across all functions estimated in this study. Unlike standard causal diagrams, SWIGs allow us to *separately* read the factorisation of the conditional dependencies for the distribution of each set of counterfactual outcomes under each modified treatment policy (Richardson and Robins 2013). Note, that the natural value of the treatment A is obtained both from its observed instances and from baseline historical data, including the baseline treatment. This method ensures that our analysis accurately captures the causal effects of flexible treatment regimes that rely on levels of religious service attendance at the treatment wave, while ensuring balance for each treatment function that we compare (Díaz *et al.* 2021; Muñoz and Van Der Laan 2012; Young *et al.* 2014).

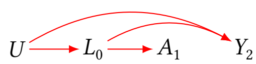
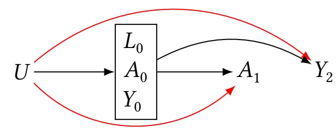
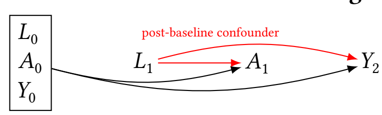
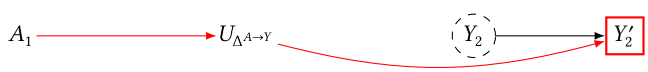
Confounding Control

To manage confounding in our analysis, we implement VanderWeele (2019)'s *modified disjunctive cause criterion* by following these steps:

1. **Identified all common causes** of both the treatment and outcomes to ensure a comprehensive approach to confounding control.
2. **Excluded instrumental variables** that affect the exposure but not the outcome. Instrumental variables do not contribute to controlling confounding and can reduce the efficiency of the estimates.

Table 1: This table presents a three-wave panel design as described in VanderWeele *et al.* (2020). By adjusting for a rich array of baseline covariates, we considerably reduce the... threats to confounding control in a three-wave panel design. Bulbulia (2024a).

Confounding Control in a Three-Wave Panel Study

Assumed Causation	Confounding Threat	Control Strategy
Unmeasured confounder: A_1 and Y_2 share both measured and unmeasured common causes $\{L_0, U\}$; **Confounding control strategy**: condition on all measured baseline confounders that are associated with the treatment, outcome or both, or that are proxies of unmeasured confounders; condition on the baseline measure of the exposure and outcome, such that any residual confounding must be orthogonal to the baseline exposure and outcome: $\boxed{L_0\ A_0\ Y_0}$.		
Post-baseline confounding		
Problem: post-baseline confounding **Response**: condition on L_1 if not a mediator.		
Bias in Attrition		
Problem: Treatment affects censoring. **Response**: Inverse Probability of Censoring Weighting		
Bias in Attrition		
Another problem: Confounding by common cause of treatment and outcome in the censored. **Response**: Inverse Probability of Censoring Weighting		

A denotes the treatment.
Y denotes the outcome.
U denotes an unmeasured confounder.
L denotes a measured confounder.
X_t: **Time-indexed node**: denotes true/known relative chronology
$\boxed{L}$ **black box** denotes conditioning on variable L.
$\rightarrow$ **black arrow** denotes an assumed causal pathway.
$\rightarrow$ **red arrow** denotes a pathway of bias.
$\boxed{L}$ **red box** denotes case when conditioning on L induces bias.
Threat: even after conditioning on the baseline exposure and outcome there is unmeasured confounding.

3. **Included proxies for unmeasured confounders** affecting both exposure and outcome. According to the principles of d-separation, using proxies allows us to control for their associated unmeasured confounders indirectly.
4. **Controlled for baseline exposure** and **baseline outcome**. Both are used as proxies for unmeasured common causes, enhancing the robustness of our causal estimates.

Appendix B details the covariates we included for confounding control. These methods adhere to the guidelines provided in (Bulbulia 2024a) and were pre-specified in our study protocol https://osf.io/ce4t9/.

Missing Data

To mitigate bias from missing data, we implement the following strategies:

Baseline missingness: we employed the `ppm` algorithm from the `mice` package in R (Van Buuren 2018) to impute missing baseline data. This method allowed us to reconstruct incomplete datasets by estimating a plausible value for missing observation. Because we could only pass one data set to the `lmtp`, we employed single imputation. About 2% of covariate values were missing at baseline. Eligibility for the study required fully observed baseline treatment measures as well as treatment wave treatment measures. Again, we only used baseline data to impute baseline missingness (refer to Zhang *et al.* (2023)).

treatment-wave missingness in time 11 (treatment wave): to adjust for censoring in the treatment wave, we estimated inverse probability of censoring weights by predicting loss-to follow up from all indicators, including the baseline values of the treatment and outcomes. We used same superlearners employed in the causal estimation models (`ranger`, `xgboost`, `glmnet`) and impliented 10-fold cross validation.

Outcome missingness in time 12 (outcome wave): to address confounding and selection bias arising from missing responses and panel attrition, we applied censoring weights obtained using nonparametric machine learning ensembles afforded by the `lmtp` package (and its dependencies) in R (Williams and Díaz 2021).

Statistical Estimator

We perform statistical estimation using semi-parametric Targeted Learning, specifically a Targeted Minimum Loss-based Estimation (TMLE) estimator. TMLE is a robust method that combines machine learning techniques with traditional statistical models to estimate causal effects while providing valid statistical uncertainty measures for these estimates (Laan and Gruber 2012; Van der Laan 2014).

TMLE operates through a two-step process that involves modelling both the outcome and treatment (exposure). Initially, TMLE employs machine learning algorithms to flexibly model the relationship between treatments, covariates, and outcomes. This flexibility allows TMLE to account for complex, high-dimensional covariate spaces *efficiently* without imposing restrictive model assumptions (Laan *et al.* 2014; Van Der Laan and Rose 2011, 2018). The outcome of this step is a set of initial estimates for these relationships.

The second step of TMLE involves "targeting" these initial estimates by incorporating information about the observed data distribution to improve the accuracy of the causal effect estimate. TMLE achieves this precision through an iterative updating process, which adjusts the initial estimates towards the true causal effect. This updating process is guided by the efficient influence function, ensuring that the final TMLE estimate is as close as possible, given the measures and data, to the targeted causal effect while still being robust to model-misspecification in either the outcome or the treatment model (Laan *et al.* 2014).

Again, a central feature of TMLE is its double-robustness property. If either the treatment model or the outcome model is correctly specified, the TMLE estimator will consistently estimate the causal effect. Additionally, we used cross-validation to avoid over-fitting, following the pre-stated protocols in Bulbulia (2024a). The integration of TMLE and machine learning technologies reduces the dependence on restrictive modelling assumptions and introduces an additional layer of robustness. For further details of the specific targeted learning strategy we favour, see (Díaz *et al.* 2021; Hoffman *et al.* 2022, 2023). We perform estimation using the `lmtp` package

(Williams and Díaz 2021). We used the `superlearner` library for semi-parametric estimation with the predefined libraries `SL.ranger`, `SL.glmnet`, and `SL.xgboost` (Chen *et al.* 2023; Polley *et al.* 2023; Wright and Ziegler 2017). We created graphs, tables and output reports using the `margot` package (Bulbulia 2024b).

Sensitivity Analysis Using the E-value

To assess the sensitivity of results to unmeasured confounding, we report VanderWeele and Ding's "E-value" in all analyses (VanderWeele and Ding 2017). The E-value quantifies the minimum strength of association (on the risk ratio scale) that an unmeasured confounder would need to have with both the exposure and the outcome (after considering the measured covariates) to explain away the observed exposure-outcome association (Linden *et al.* 2020; VanderWeele *et al.* 2020). To evaluate the strength of evidence, we use the bound of the E-value 95% confidence interval closest to 1.

Scope of Interventions

To illustrate the magnitude of the shift interventions we contrast, we provide histograms in Figure 1, that display the distribution of treatments during the treatment wave. Figure 1: The intervention less than one unit at the top of the range is presented in colour.

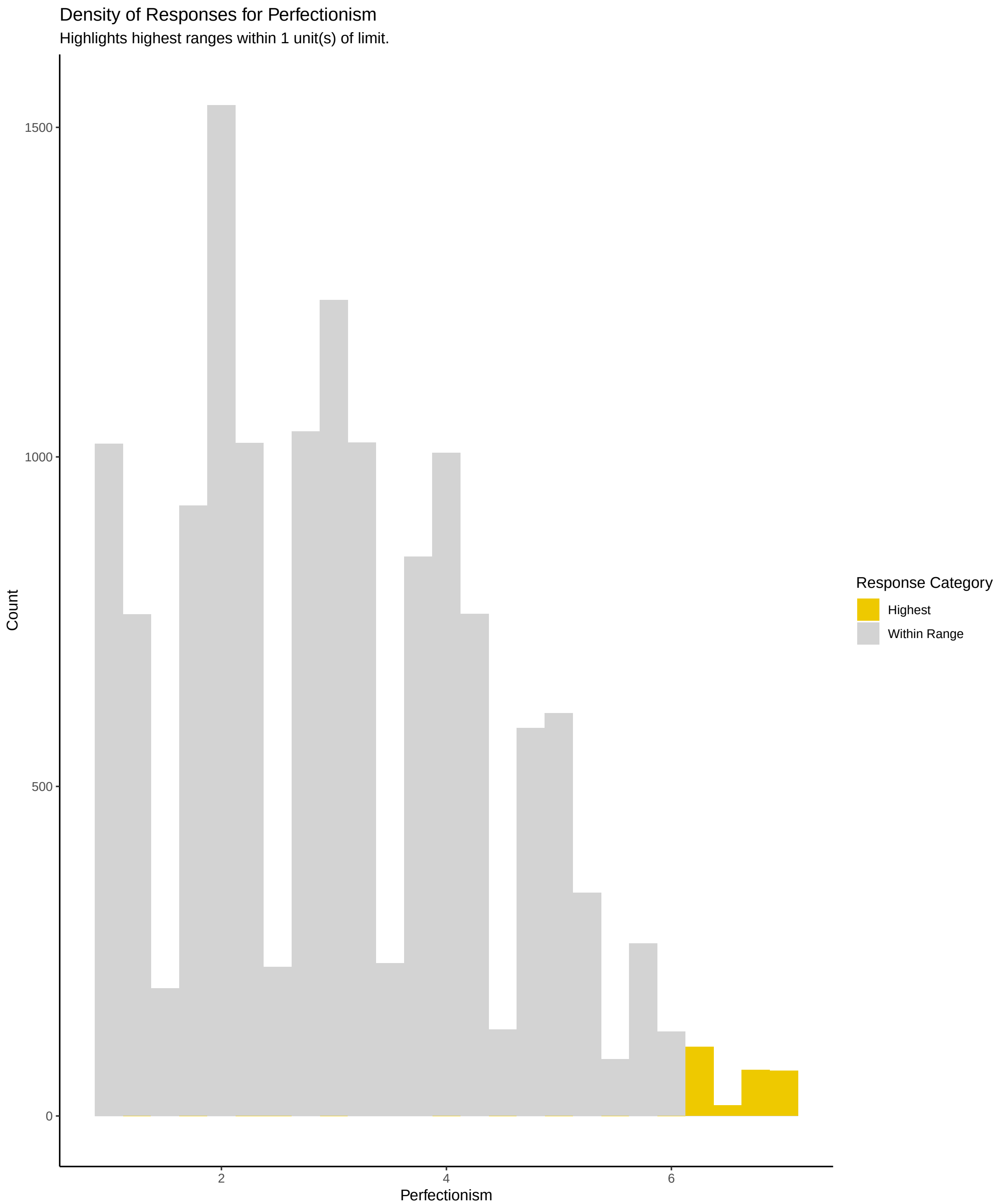


Figure 1: This figure shows a histogram of responses to religious service frequency in the baseline + 1 wave. Responses above eight were assigned to eight, and values were rounded to the nearest whole number. The red dashed line shows the population average. (A) Responses in the gold bars are shifted to four on the Regular Religious Service intervention. All those responses in grey (four and above) remain unchanged. (B) On the zero-intervention, responses in the blue bars denote those shifted under the zero-intervention treatment.

Evidence for Change in the Treatment Variable

Table 2 clarifies the change in the treatment variable from the baseline wave to the baseline + 1 wave across the sample. Assessing change in a variable is essential for evaluating the positivity assumption and recovering evidence for the incident exposure effect of the treatment variable (Danaei *et al.* 2012; Hernan and Robins 2024; VanderWeele *et al.* 2020). We find that state 4 (weekly attendance) and state 0 present the highest overall. However, movement between these states reveals they are not deterministic. States 1, 2, 3, and 5 exhibit more frequent jumps in and out of these states, suggesting lower stability and/or measurement error.

Table 2: This transition matrix captures shifts in states across across the treatment intervals. Each cell in the matrix represents the count of individuals transitioning from one state to another. The rows correspond to the treatment at baseline (From), and the columns correspond to the state at the following wave (To). **Diagonal entries** (in **bold**) correspond to the number of individuals who remained in their initial state across both waves. **Off-diagonal entries** correspond to the transitions of individuals from their baseline state to a different state in the treatment wave. A higher number on the diagonal relative to the off-diagonal entries in the same row indicates greater stability in a state. Conversely, higher off-diagonal numbers suggest more frequent shifts from the baseline state to other states.

From	State 1	State 2	State 3	State 4	State 5	State 6	State 7
State 1	**893**	484	194	40	16	3	2
State 2	657	**1737**	904	283	78	9	1
State 3	237	1073	**1368**	768	245	40	5
State 4	66	335	803	**1076**	523	108	10
State 5	24	77	253	531	**579**	223	26
State 6	7	9	38	106	205	**216**	53
State 7	2	1	5	8	25	45	**48**

Results

Study 1: Causal Effects of Perfectionism on Anxiety and Depression

Results for the treatment contrasts between perfectionism and the status quo on mental health are displayed in Figure 2 and Table 3. These results are measured on the causal difference scale.

Table 3: This table reports the results of model estimates for the causal effects of a one unit increase in perfectionism vs the status quo depression and anxiety at the end of the study. Contrasts are expressed in standard deviation units.

	E[Y(1)]-E[Y(0)]	2.5 %	97.5 %	E_Value	E_Val_bound
marginal: anxiety	0.115	0.096	0.135	1.460	1.405
marginal: depression	0.141	0.120	0.162	1.531	1.473

A Longitudinal Modified Treatment Policy (LMTP) calculates the expected outcome difference between treatment and contrast conditions over a sequential regime of treatments for a prespecified target population.

For 'marginal: depression', the effect estimate is 0.141 [0.12, 0.162]. The E-value for this estimate is 1.531, with a lower bound of 1.473. At this lower bound, unmeasured confounders would need a minimum association strength with both the intervention sequence and outcome of 1.473 to negate the observed effect. Weaker confounding would not overturn it. We infer **evidence for causality**.

For 'marginal: anxiety', the effect estimate is 0.115 [0.096, 0.135]. The E-value for this estimate is 1.46, with a lower bound of 1.405. At this lower bound, unmeasured confounders would need a minimum association strength with both the intervention sequence and outcome of 1.405 to negate the observed effect. Weaker confounding would not overturn it. We infer **evidence for causality**.

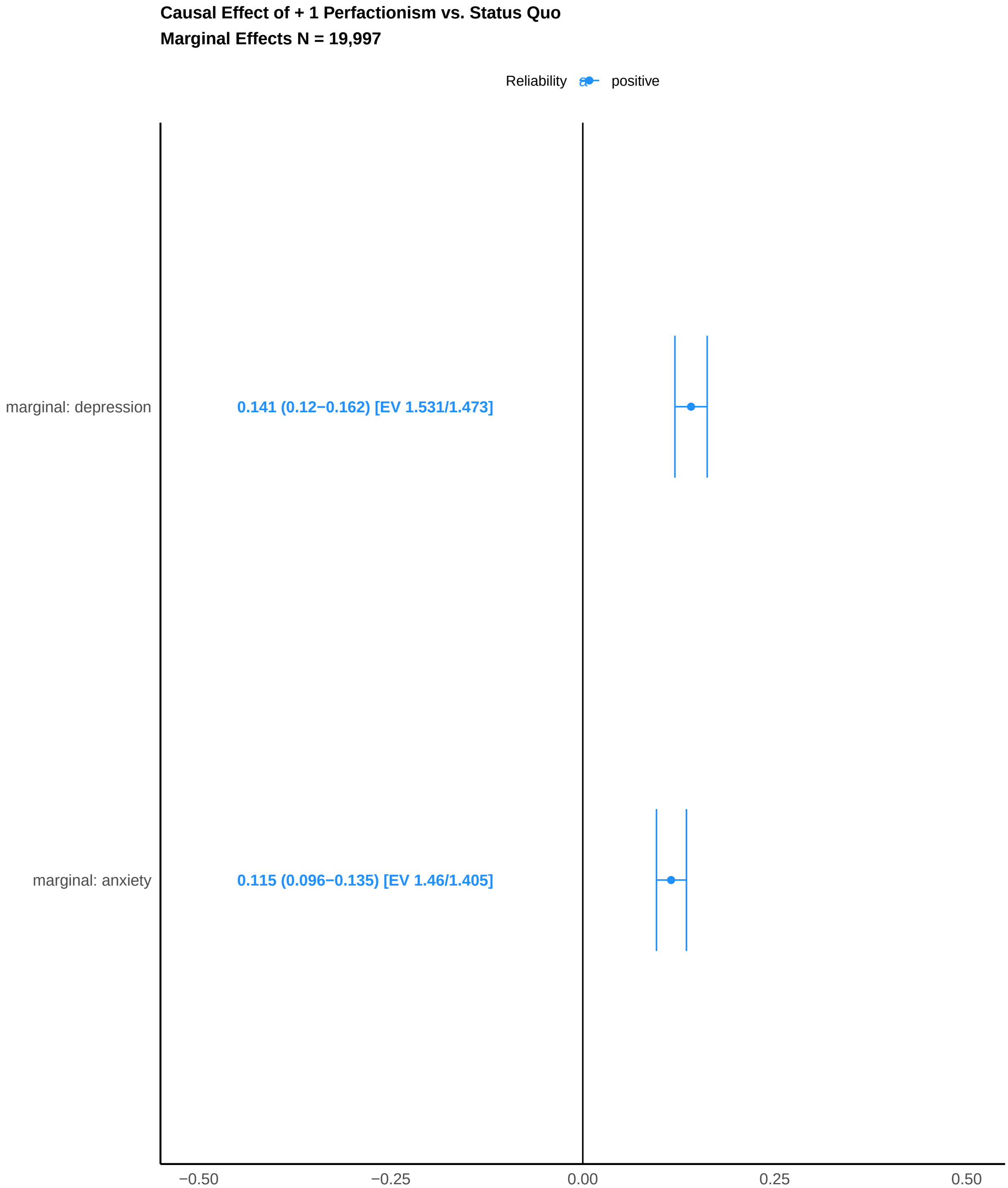


Figure 2: This figure reports the results of model estimates for the causal effects of a one unit increase in perfectionism vs the status quo depression and anxiety at the end of the study. Contrasts are expressed in standard deviation units.

Subgroup Analysis: Those Born in NZ

Figure 3 present results for the treatment contrasts within the two subgroups, evaluating the effect of a one unit increase in perfectionism vs the status qu. These results are presented on the difference scale in standardized units.

Table 4 presents the results for those born in New Zealand. Table 5 presents results for those born overseas.

Table 4: This table reports the results of model estimates for in the population of residents born in New Zealand. We again estimate the causal effects of a one unit increase in perfectionism vs the status quo depression and anxiety at the end of the study. Contrasts are expressed in standard deviation units..

	E[Y(1)]-E[Y(0)]	2.5 %	97.5 %	E_Value	E_Val_bound
born NZ: anxiety	0.113	0.093	0.134	1.455	1.394
born NZ: depression	0.145	0.121	0.168	1.542	1.478

For 'born NZ: depression', the effect estimate is 0.145 [0.121, 0.168]. The E-value for this estimate is 1.542, with a lower bound of 1.478. At this lower bound, unmeasured confounders would need a minimum association strength with both the intervention sequence and outcome of 1.478 to negate the observed effect. Weaker confounding would not overturn it. We infer **evidence for causality**.

For 'born NZ: anxiety', the effect estimate is 0.113 [0.093, 0.134]. The E-value for this estimate is 1.455, with a lower bound of 1.394. At this lower bound, unmeasured confounders would need a minimum association strength with both the intervention sequence and outcome of 1.394 to negate the observed effect. Weaker confounding would not overturn it. We infer **evidence for causality**.

Table 5: This table reports the results of model estimates for in the population of residents not born in New Zealand (born overseas). We again estimate the causal effects of a one unit increase in perfectionism vs the status quo depression and anxiety at the end of the study. Contrasts are expressed in standard deviation units.

	E[Y(1)]-E[Y(0)]	2.5 %	97.5 %	E_Value	E_Val_bound
born overseas: anxiety	0.113	0.075	0.152	1.455	1.342
born overseas: depression	0.143	0.101	0.185	1.537	1.424

For 'born overseas: depression', the effect estimate is 0.143 [0.101, 0.185]. The E-value for this estimate is 1.537, with a lower bound of 1.424. At this lower bound, unmeasured confounders would need a minimum association strength with both the intervention sequence and outcome of 1.424 to negate the observed effect. Weaker confounding would not overturn it. We infer **evidence for causality**.

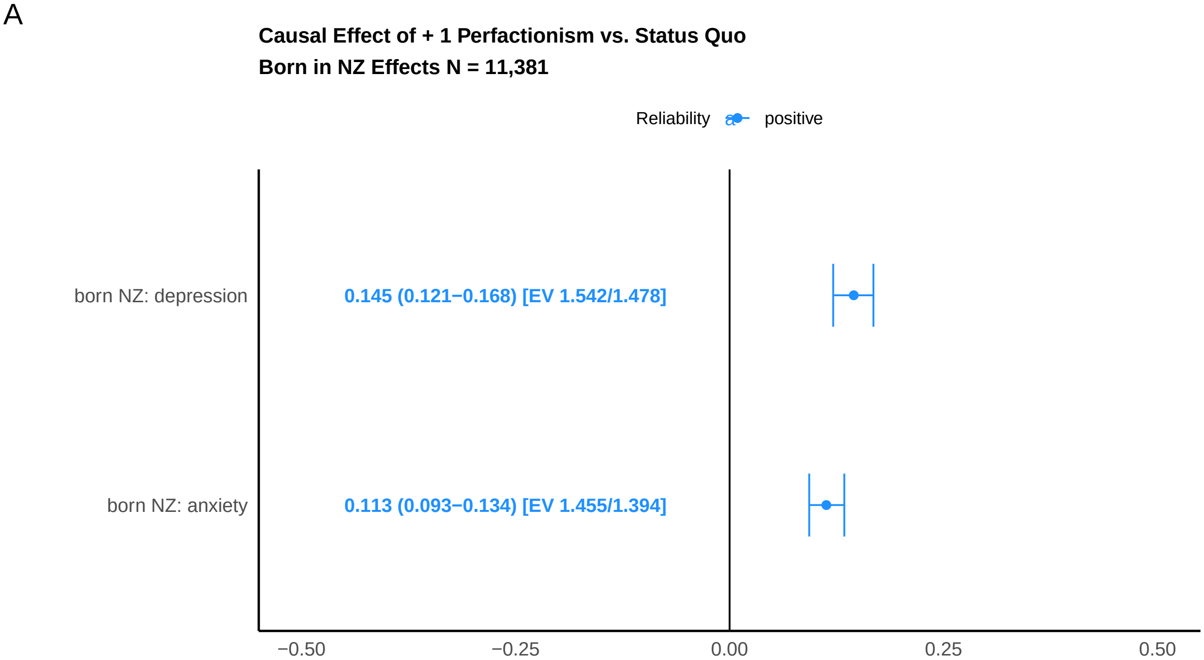
For 'born overseas: anxiety', the effect estimate is 0.113 [0.075, 0.152]. The E-value for this estimate is 1.455, with a lower bound of 1.342. At this lower bound, unmeasured confounders would need a minimum association strength with both the intervention sequence and outcome of 1.342 to negate the observed effect. Weaker confounding would not overturn it. We infer **evidence for causality**.

Findings

Comparing the difference in the mean outcomes between to the two groups for anxiety: The difference in means is 2e-04 with a standard error of 0.0223 and a 95% CI of [-0.0436, 0.0439].. We therefore do not detect a reliable difference between those born in New Zealand and those born elsewhere.

Group Comparisons: Perfectionism on Distress

A



B

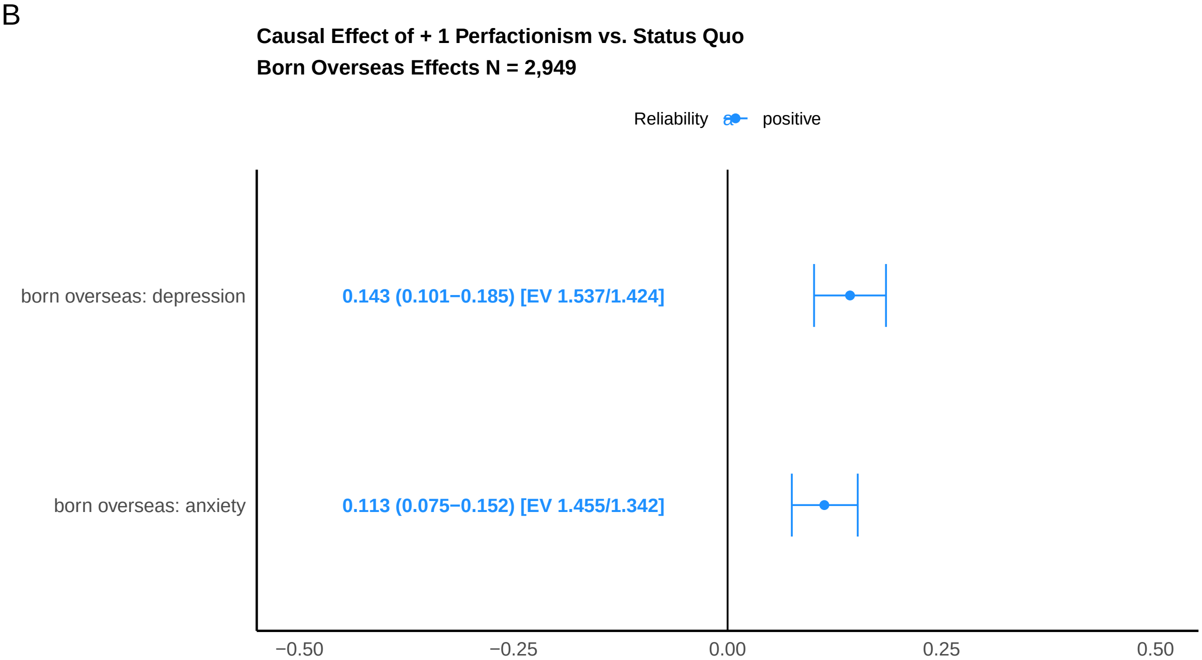


Figure 3: This figure reports the results of for each sub-group analysis for the causal effects of a one unit increase in perfectionism vs the status quo depression and anxiety at the end of the study. Contrasts are expressed in standard deviation units.

Comparing the difference in the mean outcomes between to the two groups for depression: The difference in means is 2e-04 with a standard error of 0.0223 and a 95% CI of [-0.0436, 0.0439].. We therefore do not detect a reliable difference between those born in New Zealand and those born elsewhere.

Additional Study: Comparison of Causal Inference Results with Cross-Sectional Regressions

To better evaluate the contributions of our methodology to current practice, we conducted a series of cross-sectional analyses using the baseline wave data. We quantified the statistical associations between religious service attendance and our focal prosocial outcomes. We included all regression covariates from the causal models (including sample weights) for each analysis, obviously omitting the outcome measured at baseline, i.e. the response variable.

Cross-sectional anxiety result: the change in expected kessler-6 anxiety for a one-unit increase in perfectionism is b = 0.31; (95% CI 0.30, 0.32). This result is 2.65 per cent greater than the effect estimated from the '+1 vs. status quo' causal contrast (0.115), indicating an **overstatement in the cross-sectional regression model.**

Cross-sectional depression result: the change in expected kessler-6 depression for a one-unit increase in perfectionism is b = 0.34; (95% CI 0.33, 0.35). This result is 2.43 per cent greater than the effect estimated from the '+1 vs. status quo' causal contrast (0.141), indicating an **overstatement in the cross-sectional regression model.**

These findings underscore that the results of cross-sectional regressions, although suggestive, can considerably diverge from those obtained from the causal analysis of panel data.

Discussion

Considerations

First, as stated above, our causal inferences turn on three assumptions, which are worth revisiting:

(i). **Unmeasured confounding**: although we employ robust methods for causal inference, our results depend on the effectiveness of our strategy to control for confounding. Our sensitivity analyses address the potential impacts of unmeasured confounders. Nevertheless, the presence and influence of such confounders are uncertain and unverifiable from our data.

(ii). **Causal consistency**: the observed outcomes must correspond to the counterfactual treatments contrasted. Although "religious service attendance" may appear conditionally independent of the outcomes given baseline covariates, interpreting these interventions remains challenging. "Religious service attendance" varies widely, encompassing everything from informal gatherings at homes to formal services in cathedrals across New Zealand's religious diversity. This type of "treatment" does not mirror a straightforward medical intervention like a vaccine. The heterogeneity of "religious service" limits the clarity of our results, an issue no amount of data or analysis can resolve because "religious service" reflects a broad spectrum of community activities.

(iii). **Positivity**: we have confirmed that religious service attendance varies within our sample and have used semi-parametric models with ensemble learning and cross-validation to prevent data over-extrapolation and model over-fitting. Conceptually, it is crucial for valid causal inference that every potential level of "treatment" to religious services is realistically possible (refer to discussion in VanderWeele (2017)). Although there are instances realised in our data of secular individuals initiating religious service and frequent attendees stopping (refer to Table 2), it might stretch credulity too far to imagine that such changes are possible for everyone.

Second, our study confronts the spectre of **measurement error**: both direct and correlated measurement errors can introduce biases, either by implying effects where none exist or by attenuating true effects (VanderWeele and Hernán 2012). Importantly, evaluating prosociality using multiple measures while also controlling for these measures and the treatment at baseline helps to mitigate measurement error concerns. Nevertheless, unknown combinations of measurement error might nevertheless bias our results. The outcomes and estimates we report here are best-considered approximations.

Third, we do not examine **treatment effect heterogeneity**: identifying which subgroups experience the strongest responses remains a task for future research. Such investigations are crucial for making informed policy decisions and tailoring advice relevant to those subgroups of the population who might benefit most. Perhaps the most obvious stratum is religious affiliates (VanderWeele 2017).

Fourth, the **transportability of our findings remains unclear**: New Zealand is our target population. Our findings generalise to this population. However, the transportability of our findings to other settings—whether our results generalise beyond our targeted New Zealand population—remains an open question, a matter for future investigations.

Observations and Recommendations

Ethics

{authors blinded}

Data Availability

{authors blinded}

Acknowledgements

{authors blinded}

Author Statement

{authors blinded}

Appendix A: Measures

Age (waves: 1-15)

We asked participants' ages in an open-ended question ("What is your age?" or "What is your date of birth?").

Born in New Zealand

Charitable Donations (Study 1 outcome)

Using one item from Hoverd and Sibley (2010), we asked participants, "How much money have you donated to charity in the last year?".

Charitable Volunteering (Study 1 outcome)

We measured hours of volunteering using one item from Sibley *et al.* (2011): "Hours spent … voluntary/charitable work."

Children Number (waves: 1-3, 4-15)

We measured the number of children using one item from Bulbulia *et al.* (2015). We asked participants, "How many children have you given birth to, fathered, or adopted. How many children have you given birth to, fathered, or adopted?" or "How many children have you given birth to, fathered, or adopted. How many children have you given birth to, fathered, and/or parented?" (waves: 12-15).

Disability

We assessed disability with a one-item indicator adapted from Verbrugge (1997). It asks, "Do you have a health condition or disability that limits you and that has lasted for 6+ months?" (1 = Yes, 0 = No).

Education Attainment (waves: 1, 4-15)

We asked participants, "What is your highest level of qualification?". We coded participants' highest finished degree according to the New Zealand Qualifications Authority. Ordinal-Rank 0-10 NZREG codes (with overseas school quals coded as Level 3, and all other ancillary categories coded as missing) See:https://www.nzqa.govt.nz/assets/Studying-in-NZ/New-Zealand-Qualification-Framework/requirements-nzqf.pdf

Employment (waves: 1-3, 4-11)

We asked participants, "Are you currently employed? (This includes self-employed or casual work)".

Ethnicity

Based on the New Zealand Census, we asked participants, "Which ethnic group(s) do you belong to?". The responses were: (1) New Zealand European; (2) Māori; (3) Samoan; (4) Cook Island Māori; (5) Tongan; (6) Niuean; (7) Chinese; (8) Indian; (9) Other such as DUTCH, JAPANESE, TOKELAUAN. Please state:. We coded their answers into four groups: Maori, Pacific, Asian, and Euro (except for Time 3, which used an open-ended measure).

Fatigue

We assessed subjective fatigue by asking participants, "During the last 30 days, how often did … you feel exhausted?" Responses were collected on an ordinal scale (0 = None of The Time, 1 = A little of The Time, 2 = Some of The Time, 3 = Most of The Time, 4 = All of The Time).

Honesty-Humility-Modesty Facet (waves: 10-14)

Participants indicated the extent to which they agree with the following four statements from Campbell *et al.* (2004) , and Sibley *et al.* (2011) (1 = Strongly Disagree to 7 = Strongly Agree)

```
i.  I want people to know that I am an important person of high status, (Waves: 1, 10-14)
ii. I am an ordinary person who is no better than others.
iii. I wouldn't want people to treat me as though I were superior to them.
iv. I think that I am entitled to more respect than the average person is.
```

Hours of Childcare

We measured hours of exercising using one item from Sibley *et al.* (2011): 'Hours spent … looking after children."

To stabilise this indicator, we took the natural log of the response + 1.

Hours of Housework

We measured hours of exercising using one item from Sibley *et al.* (2011): "Hours spent … housework/cooking"

To stabilise this indicator, we took the natural log of the response + 1.

Hours of Exercise

We measured hours of exercising using one item from Sibley *et al.* (2011): "Hours spent … exercising/physical activity"

To stabilise this indicator, we took the natural log of the response + 1.

Hours of Childcare

We measured hours of exercising using one item from Sibley *et al.* (2011): 'Hours spent … looking after children."

To stabilise this indicator, we took the natural log of the response + 1.

Hours of Exercise

We measured hours of exercising using one item from Sibley *et al.* (2011): "Hours spent … exercising/physical activity"

To stabilise this indicator, we took the natural log of the response + 1.

Hours of Housework

We measured hours of exercising using one item from Sibley *et al.* (2011): "Hours spent … housework/cooking"

To stabilise this indicator, we took the natural log of the response + 1.

Hours of Sleep

Participants were asked, "During the past month, on average, how many hours of *actual sleep* did you get per night?".

Hours of Work

We measured work hours using one item from Sibley *et al.* (2011): "Hours spent … working in paid employment."

To stabilise this indicator, we took the natural log of the response + 1.

Income (waves: 1-3, 4-15)

Participants were asked, "Please estimate your total household income (before tax) for the year XXXX". To stabilise this indicator, we first took the natural log of the response + 1, and then centred and standardised the log-transformed indicator.

Kessler-6: Psychological Distress (waves: 2-3,4-15)

We measured psychological distress using the Kessler-6 scale (kessler2002?), which exhibits strong diagnostic concordance for moderate and severe psychological distress in large, crosscultural samples (kessler2010?; prochaska2012?). Participants rated during the past 30 days, how often did… (1) "… you feel hopeless"; (2) "… you feel so depressed that nothing could cheer you up"; (3) "… you feel restless or fidgety"; (4)"… you feel that everything was an effort"; (5) "… you feel worthless"; (6) " you feel nervous?" Ordinal response alternatives for the Kessler-6 are: "None of the time"; "A little of the time"; "Some of the time"; "Most of the time"; "All of the time."

Male Gender (waves: 1-15)

We asked participants' gender in an open-ended question: "what is your gender?" or "Are you male or female?" (waves: 1-5). Female was coded as 0, Male as 1, and gender diverse coded as 3 (Fraser *et al.* 2020). (or 0.5 = neither female nor male)

Here, we coded all those who responded as Male as 1, and those who did not as 0.

Mini-IPIP 6 (waves: 1-3,4-15)

We measured participants' personalities with the Mini International Personality Item Pool 6 (Mini-IPIP6) (Sibley *et al.* 2011), which consists of six dimensions and each dimension is measured with four items:

1. agreeableness,
   i. I sympathize with others' feelings.
   ii. I am not interested in other people's problems. (r)
   iii. I feel others' emotions.
   iv. I am not really interested in others. (r)
2. conscientiousness,
   i. I get chores done right away.
   ii. I like order.
   iii. I make a mess of things. (r)
   iv. I often forget to put things back in their proper place. (r)
3. extraversion,
   i. I am the life of the party.
   ii. I don't talk a lot. (r)
   iii. I keep in the background. (r)
   iv. I talk to a lot of different people at parties.
4. honesty-humility,
   i. I feel entitled to more of everything. (r)
   ii. I deserve more things in life. (r)
   iii. I would like to be seen driving around in a very expensive car. (r)
   iv. I would get a lot of pleasure from owning expensive luxury goods. (r)

5. neuroticism, and
    i. I have frequent mood swings.
    ii. I am relaxed most of the time. (r)
    iii. I get upset easily.
    iv. I seldom feel blue. (r)
6. openness to experience
    i. I have a vivid imagination.
    ii. I have difficulty understanding abstract ideas. (r)
    iii. I do not have a good imagination. (r)
    iv. I am not interested in abstract ideas. (r)

Each dimension was assessed with four items and participants rated the accuracy of each item as it applies to them from 1 (Very Inaccurate) to 7 (Very Accurate). Items marked with (r) are reverse coded.

NZ-Born (waves: 1-2,4-15)

We asked participants, "Which country were you born in?" or "Where were you born? (please be specific, e.g., which town/city?)" (waves: 6-15).

NZ Deprivation Index (waves: 1-15)

We used the NZ Deprivation Index to assign each participant a score based on where they live (Atkinson *et al.* 2019). This score combines data such as income, home ownership, employment, qualifications, family structure, housing, and access to transport and communication for an area into one deprivation score.

NZSEI Occupational Prestige and Status (waves: 8-15)

We assessed occupational prestige and status using the New Zealand Socio-economic Index 13 (NZSEI-13) (Fahy *et al.* 2017a). This index uses the income, age, and education of a reference group, in this case the 2013 New Zealand census, to calculate a score for each occupational group. Scores range from 10 (Lowest) to 90 (Highest). This list of index scores for occupational groups was used to assign each participant an NZSEI-13 score based on their occupation.

We assessed occupational prestige and status using the New Zealand Socio-economic Index 13 (NZSEI-13) (Fahy *et al.* 2017b). This index uses the income, age, and education of a reference group, in this case, the 2013 New Zealand census, to calculate a score for each occupational group. Scores range from 10 (Lowest) to 90 (Highest). This list of index scores for occupational groups was used to assign each participant an NZSEI-13 score based on their occupation.

Opt-in

The New Zealand Attitudes and Values Study allows opt-ins to the study. Because the opt-in population may differ from those sampled randomly from the New Zealand electoral roll; although the opt-in rate is low, we include an indicator (yes/no) for this variable.

Partner (No/Yes)

"What is your relationship status?" (e.g., single, married, de-facto, civil union, widowed, living together, etc.)

Politically Conservative

We measured participants' political conservative orientation using a single item adapted from Jost (2006).

"Please rate how politically liberal versus conservative you see yourself as being."

(1 = Extremely Liberal to 7 = Extremely Conservative)

Religious Service Attendance

If participants answered *yes* to "Do you identify with a religion and/or spiritual group?" we measured their frequency of church attendence using one item from Sibley and Bulbulia (2012): "how many times did you attend a church or place of worship in the last month?". Those participants who were not religious were imputed a score of "0".

Rural/Urban Codes

Participants residence locations were coded according to a five-level ordinal categorisation ranging from "Urban" to Rural, see Sibley (2021).

Short-Form Health

Participants' subjective health was measured using one item ("Do you have a health condition or disability that limits you, and that has lasted for 6+ months?"; 1 = Yes, 0 = No) adapted from Verbrugge (1997).

Sample Origin

Wave enrolled in NZAVS, see Sibley (2021).

Support received: money (waves 10-12) (Study 4 outcomes)

The NZAVS has a 'revealed' measure of received help and support measured in hours of support in the previous week. The items are:

Please estimate how much help you have received from the following sources in the last week?

- *family...MONEY (hours)*
- *friends...MONEY (hours)*
- *members of my community...MONEY (hours)*

Because this measure is highly variable, we convert responses to binary indicators: *0 = none/1 any*

Support received: time (waves 10-13) (Study 3 outcomes)

Please estimate how much help you have received from the following sources in the last week.

- *family...TIME (hours)*
- *friends...TIME (hours)*
- *members of my community...TIME (hours)*

Because this measure is highly variable, we convert responses to binary indicators: *0 = none/1 any*

Total Siblings

Participants were asked the following questions related to sibling counts:

- Were you the 1st born, 2nd born, or 3rd born, etc, child of your mother?
- Do you have siblings?
- How many older sisters do you have?
- How many younger sisters do you have?
- How many older brothers do you have?
- How many younger brothers do you have?

A single score was obtained from sibling counts by summing responses to the "How many…" items. From these scores, an ordered factor was created ranging from 0 to 7, where participants with more than 7 siblings were grouped into the highest category.

Appendix B. Baseline Demographic Statistics

Table 6: Baseline demographic statistics

Demographic Variables	N = 19,997
Age	NA
Mean (SD)	49 (14)
Range	18, 98
IQR	39, 60
Male	7,425 (37%)
Edu	NA
Mean (SD)	5.29 (2.72)
Range	0.00, 10.00
IQR	3.00, 7.00
Unknown	510
Eth Cat	NA
euro	15,850 (80%)
maori	2,280 (12%)
pacific	498 (2.5%)
asian	1,107 (5.6%)
Unknown	262
Partner	14,501 (75%)
Unknown	649
Employed	15,884 (80%)
Unknown	39
Born Nz	15,436 (78%)
Unknown	232
Neighbourhood Community	NA
Mean (SD)	4.20 (1.67)
Range	1.00, 7.00
IQR	2.99, 5.95
Unknown	113
Household Inc log	NA
Mean (SD)	11.39 (0.76)
Range	0.69, 14.40
IQR	11.00, 11.92
Unknown	1,529
Parent	14,037 (71%)
Unknown	156
Religion Religious	7,148 (36%)
Unknown	301
Urban	12,291 (62%)
Unknown	129

Table 6 baseline demographic statistics for couples who met inclusion criteria.

Appendix C: Treatment Statistics

Table 7: Exposures at baseline and baseline + 1 (treatment) wave

Exposure Variables by Wave	**2018**, N = 19,997	**2019**, N = 19,997
Perfectionism	3.02 (2.03, 4.04)	2.99 (2.00, 4.02)
Unknown	0	5,558

tbl-table-exposures-code presents baseline (NZAVS time 10) and exposure wave (NZAVS time 11) statistics for the exposure variable: perfectionism (range 1-7).

Imbalance of Confounding Covariates Treatments

Figure 4 shows imbalance of covariates on the treatment at the treatment wave. The variable on which there is strongest imbalance is the baseline measure of religious service attendance. It is important to adjust for this measure both for confounding control and to better estimate an incident exposure effect for the religious service at the treatment wave (in contrast to merely estimating a prevalence effect). See VanderWeele *et al.* (2020).

Appendix D: Baseline and End of Study Outcome Statistics

Table 8: Outcomes at baseline and end-of-study

Outcome Variables by Wave	**2018**, N = 19,997	**2020**, N = 19,997
Kessler Latent Anxiety	1.04 (0.66, 1.68)	1.03 (0.65, 1.67)
Unknown	203	6,768
Kessler Latent Depression	0.33 (0.01, 0.98)	0.32 (0.01, 0.97)
Unknown	207	6,765

Table 8 presents baseline and end-of-study descriptive statistics for the outcome variables.

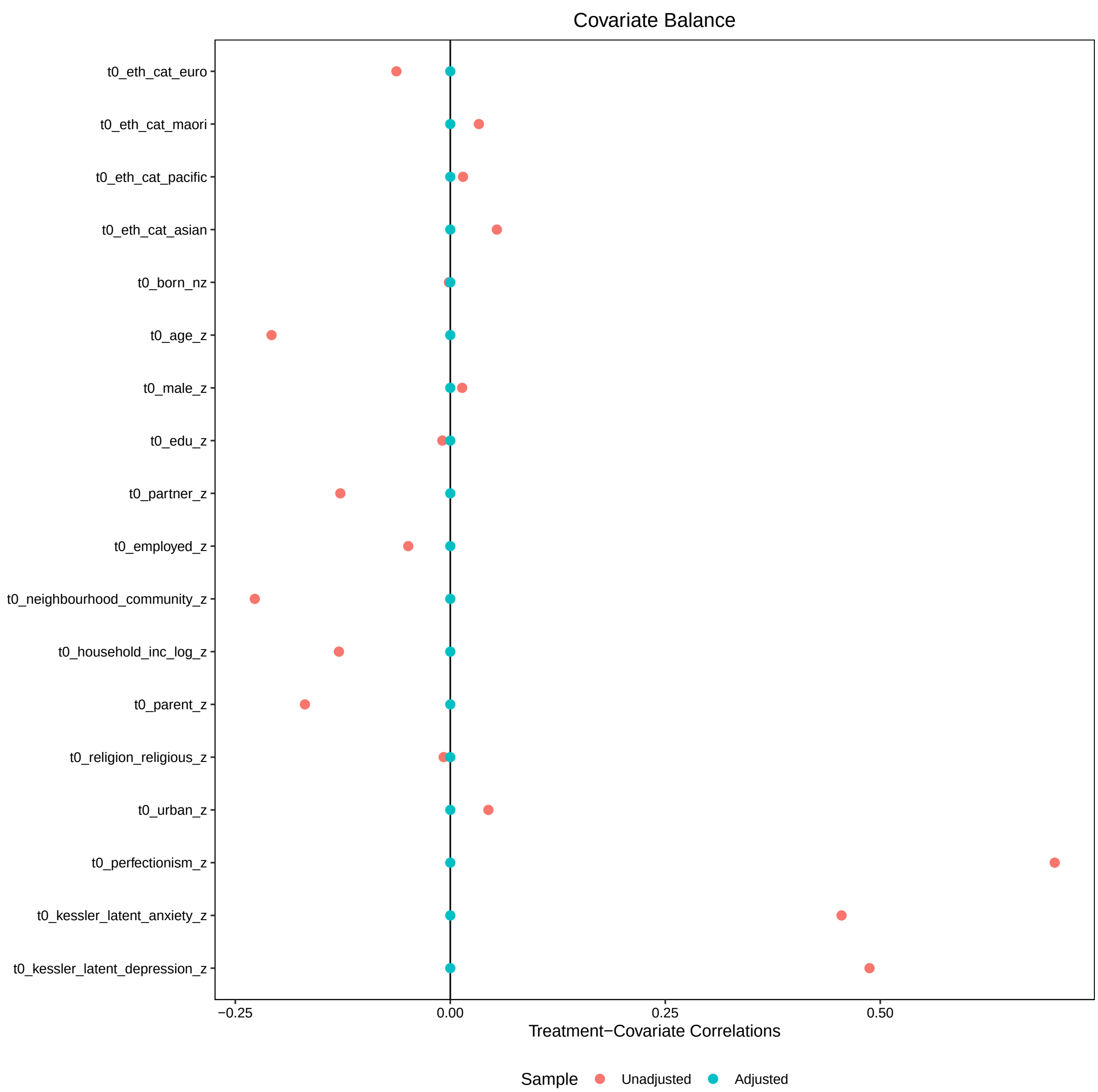


Figure 4: This figure shows the imbalance in covariates on the treatment

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