Your Title Here

YOUR NAME HERE

YOUR CO-AUTHOR HERE

2024-05-20

Abstract

Your abstract herr.

**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, mothers and 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]](#footnote-20)

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 (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](https://doi.org/10.17605/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); Dı́az *et al.* (2021); Díaz *et al.* (2023)). Let denote the treatment – monthly frequency of religious service. There are three time points: , where denotes the baseline wave, , the treatment wave, and at the end of the study. denotes a modified treatment policy . 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 . Here, the functions defined by modified treatment policies are interventions that fix to .

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

### 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?

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

[Table 1](#tbl-02) presents three Causal Directed Acyclic Graph (DAG) that describe our confounding control (identification strategy). 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 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 (Dı́az *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.
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](#appendix-demographics) 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](#fig-hist), that display the distribution of treatments during the treatment wave. [Figure 1](#fig-hist): 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](#tbl-transition) 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** | |

–>

–>

–>

–>

–>

–>

–>

–>

–>

–>

–>

–>

–>

–>

–>

–>

–>

–>

–> –>

–>

–>

–>

–>

–>

–>

–>

–>

–>

–>

–>

–> –> –>

–>

–>

–>

–>

–>

–> –> –> –> –> –>

–>

–> –>

–>

–> –> –> –> –> –>

–> –> –>

–>

## References

Bulbulia, J (2024a) A practical guide to causal inference in three-wave panel studies. *PsyArXiv Preprints*. doi:[10.31234/osf.io/uyg3d](https://doi.org/10.31234/osf.io/uyg3d).

Bulbulia, JA (2024b) *Margot: MARGinal observational treatment-effects*. doi:[10.5281/zenodo.10907724](https://doi.org/10.5281/zenodo.10907724).

Bulbulia, JA, Afzali, MU, Yogeeswaran, K, and Sibley, CG (2023) Long-term causal effects of far-right terrorism in New Zealand. *PNAS Nexus*, **2**(8), pgad242.

Chatton, A, Le Borgne, F, Leyrat, C, … Foucher, Y (2020) G-computation, propensity score-based methods, and targeted maximum likelihood estimator for causal inference with different covariates sets: a comparative simulation study. *Scientific Reports*, **10**(1), 9219. doi:[10.1038/s41598-020-65917-x](https://doi.org/10.1038/s41598-020-65917-x).

Chen, T, He, T, Benesty, M, … Yuan, J (2023) *Xgboost: Extreme gradient boosting*. Retrieved from <https://CRAN.R-project.org/package=xgboost>

Danaei, G, Tavakkoli, M, and Hernán, MA (2012) Bias in observational studies of prevalent users: lessons for comparative effectiveness research from a meta-analysis of statins. *American Journal of Epidemiology*, **175**(4), 250–262. doi:[10.1093/aje/kwr301](https://doi.org/10.1093/aje/kwr301).

De Coulanges, F (1903) *La cité antique: Étude sur le culte, le droit, les institutions de la grèce et de rome*, Hachette.

Díaz, I, Williams, N, Hoffman, KL, and Schenck, EJ (2021) Non-parametric causal effects based on longitudinal modified treatment policies. *Journal of the American Statistical Association*. doi:[10.1080/01621459.2021.1955691](https://doi.org/10.1080/01621459.2021.1955691).

Díaz, I, Williams, N, Hoffman, KL, and Schenck, EJ (2023) Nonparametric causal effects based on longitudinal modified treatment policies. *Journal of the American Statistical Association*, **118**(542), 846–857. doi:[10.1080/01621459.2021.1955691](https://doi.org/10.1080/01621459.2021.1955691).

Dı́az, I, Hejazi, NS, Rudolph, KE, and Der Laan, MJ van (2021) Nonparametric efficient causal mediation with intermediate confounders. *Biometrika*, **108**(3), 627–641.

Haneuse, S, and Rotnitzky, A (2013) Estimation of the effect of interventions that modify the received treatment. *Statistics in Medicine*, **32**(30), 5260–5277.

Hernan, MA, and Robins, JM (2024) *Causal inference: What if?*, Taylor & Francis. Retrieved from <https://www.hsph.harvard.edu/miguel-hernan/causal-inference-book/>

Hernán, MA, and Greenland, S (2024) Why stating hypotheses in grant applications is unnecessary. *JAMA*, **331**(4), 285–286.

Hoffman, KL, Salazar-Barreto, D, Rudolph, KE, and Díaz, I (2023) Introducing longitudinal modified treatment policies: A unified framework for studying complex exposures. doi:[10.48550/arXiv.2304.09460](https://doi.org/10.48550/arXiv.2304.09460).

Hoffman, KL, Schenck, EJ, Satlin, MJ, … Díaz, I (2022) Comparison of a target trial emulation framework vs cox regression to estimate the association of corticosteroids with COVID-19 mortality. *JAMA Network Open*, **5**(10), e2234425. doi:[10.1001/jamanetworkopen.2022.34425](https://doi.org/10.1001/jamanetworkopen.2022.34425).

Johnson, DD (2005) God’s punishment and public goods: A test of the supernatural punishment hypothesis in 186 world cultures. *Human Nature*, **16**, 410–446.

Laan, MJ van der, and Gruber, S (2012) Targeted minimum loss based estimation of causal effects of multiple time point interventions. *The International Journal of Biostatistics*, **8**(1).

Laan, MJ van der, Luedtke, AR, and Dı́az, I (2014) Discussion of identification, estimation and approximation of risk under interventions that depend on the natural value of treatment using observational data, by Jessica Young, Miguel Hernán, and James Robins. *Epidemiologic Methods*, **3**(1), 21–31.

Linden, A, Mathur, MB, and VanderWeele, TJ (2020) Conducting sensitivity analysis for unmeasured confounding in observational studies using e-values: The evalue package. *The Stata Journal*, **20**(1), 162–175.

Major-Smith, D (2023) Exploring causality from observational data: An example assessing whether religiosity promotes cooperation. *Evolutionary Human Sciences*, **5**, e22.

Muñoz, ID, and Van Der Laan, M (2012) Population intervention causal effects based on stochastic interventions. *Biometrics*, **68**(2), 541–549.

Norenzayan, A, Shariff, AF, Gervais, WM, … Henrich, J (2016) The cultural evolution of prosocial religions. *Behavioral and Brain Sciences*, **39**, e1. doi:[10.1017/S0140525X14001356](https://doi.org/10.1017/S0140525X14001356).

Pearl, J (2009) [*Causal inference in statistics: An overview*](https://doi.org/10.1214/09-SS057).

Polley, E, LeDell, E, Kennedy, C, and Laan, M van der (2023) *SuperLearner: Super learner prediction*. Retrieved from <https://CRAN.R-project.org/package=SuperLearner>

Richardson, TS, and Robins, JM (2013) Single world intervention graphs: A primer. In *Second UAI workshop on causal structure learning, Bellevue, Washington*, Citeseer. Retrieved from <https://citeseerx.ist.psu.edu/document?repid=rep1&type=pdf&doi=07bbcb458109d2663acc0d098e8913892389a2a7>

Robins, J (1986) A new approach to causal inference in mortality studies with a sustained exposure period—application to control of the healthy worker survivor effect. *Mathematical Modelling*, **7**(9-12), 1393–1512.

Rubin, DB (2005) Causal inference using potential outcomes: Design, modeling, decisions. *Journal of the American Statistical Association*, **100**(469), 322–331. Retrieved from <https://www.jstor.org/stable/27590541>

Schloss, JP, and Murray, MJ (2011) Evolutionary accounts of belief in supernatural punishment: A critical review. *Religion, Brain & Behavior*, **1**(1), 46–99.

Shiba, K, and Kawahara, T (2021) Using propensity scores for causal inference: Pitfalls and tips. *Journal of Epidemiology*, **31**(8), 457–463.

Sibley, CG (2021) [*Sampling procedure and sample details for the new zealand attitudes and values study*](https://doi.org/10.31234/osf.io/wgqvy).

Sosis, R, and Bressler, ER (2003) Cooperation and commune longevity: A test of the costly signaling theory of religion. *Cross-Cultural Research*, **37**(2), 211–239.

Splawa-Neyman, J, Dabrowska, DM, and Speed, TP (1990) On the application of probability theory to agricultural experiments. Essay on principles. Section 9. *Statistical Science*, 465–472.

Swanson, GE (1967) Religion and regime: A sociological account of the Reformation.

Van Buuren, S (2018) *Flexible imputation of missing data*, CRC press.

Van der Laan, MJ (2014) Targeted estimation of nuisance parameters to obtain valid statistical inference. *The International Journal of Biostatistics*, **10**(1), 29–57.

Van Der Laan, MJ, and Rose, S (2011) *Targeted Learning: Causal Inference for Observational and Experimental Data*, New York, NY: Springer. Retrieved from <https://link.springer.com/10.1007/978-1-4419-9782-1>

Van Der Laan, MJ, and Rose, S (2018) *Targeted Learning in Data Science: Causal Inference for Complex Longitudinal Studies*, Cham: Springer International Publishing. Retrieved from <http://link.springer.com/10.1007/978-3-319-65304-4>

VanderWeele, TJ (2009) Concerning the consistency assumption in causal inference. *Epidemiology*, **20**(6), 880. doi:[10.1097/EDE.0b013e3181bd5638](https://doi.org/10.1097/EDE.0b013e3181bd5638).

VanderWeele, TJ (2019) Principles of confounder selection. *European Journal of Epidemiology*, **34**(3), 211–219.

VanderWeele, TJ (2021) Can sophisticated study designs with regression analyses of observational data provide causal inferences? *JAMA Psychiatry*, **78**(3), 244–246.

VanderWeele, TJ, and Ding, P (2017) Sensitivity analysis in observational research: Introducing the e-value. *Annals of Internal Medicine*, **167**(4), 268–274. doi:[10.7326/M16-2607](https://doi.org/10.7326/M16-2607).

VanderWeele, TJ, and Hernan, MA (2013) Causal inference under multiple versions of treatment. *Journal of Causal Inference*, **1**(1), 1–20.

VanderWeele, TJ, Mathur, MB, and Chen, Y (2020) Outcome-wide longitudinal designs for causal inference: A new template for empirical studies. *Statistical Science*, **35**(3), 437–466.

Watts, J, Bulbulia, J. A., Gray, RD, and Atkinson, QD (2016) Clarity and causality needed in claims about big gods., **39**, 41–42. doi:[DOI:10.1017/S0140525X15000576](https://doi.org/DOI:10.1017/S0140525X15000576).

Watts, J, Greenhill, SJ, Atkinson, QD, Currie, TE, Bulbulia, J, and Gray, RD (2015) *Broad supernatural punishment but not moralizing high gods precede the evolution of political complexity in Austronesia* *Proceedings of the Royal Society B: Biological Sciences*, Vol. 282, The Royal Society, 20142556.

Westreich, D, and Cole, SR (2010) Invited commentary: positivity in practice. *American Journal of Epidemiology*, **171**(6). doi:[10.1093/aje/kwp436](https://doi.org/10.1093/aje/kwp436).

Wheatley, P (1971) *The pivot of the four quarters : A preliminary enquiry into the origins and character of the ancient chinese city*, Edinburgh University Press. Retrieved from <https://cir.nii.ac.jp/crid/1130000795717727104>

Whitehouse, H, Francois, P, Savage, PE, … Turchin, P (2023) Testing the big gods hypothesis with global historical data: A review and retake. *Religion, Brain & Behavior*, **13**(2), 124–166.

Williams, NT, and Díaz, I (2021) *lmtp: Non-parametric causal effects of feasible interventions based on modified treatment policies*. doi:[10.5281/zenodo.3874931](https://doi.org/10.5281/zenodo.3874931).

Wright, MN, and Ziegler, A (2017) ranger: A fast implementation of random forests for high dimensional data in C++ and R. *Journal of Statistical Software*, **77**(1), 1–17. doi:[10.18637/jss.v077.i01](https://doi.org/10.18637/jss.v077.i01).

Young, JG, Hernán, MA, and Robins, JM (2014) Identification, estimation and approximation of risk under interventions that depend on the natural value of treatment using observational data. *Epidemiologic Methods*, **3**(1), 1–19.

Zhang, J, Dashti, SG, Carlin, JB, Lee, KJ, and Moreno-Betancur, M (2023) Should multiple imputation be stratified by exposure group when estimating causal effects via outcome regression in observational studies? *BMC Medical Research Methodology*, **23**(1), 42.

1. Philosophical disagreements about the meanings assigned to “potential” and “counterfactual” outcomes do not affect our use. [↑](#footnote-ref-20)