Reference-based imputation methods based on conditional mean imputation

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Abstract

Clinical trials with longitudinal outcomes typically include missing data due to missed assessments or structural missingness of outcomes after intercurrent events handled with a hypothetical strategy. Approaches based on Bayesian random multiple imputation and Rubin’s rule for pooling results across multiple imputed datasets are increasingly used in order to align the analysis of these trials with the targeted estimand. We propose and justify deterministic conditional mean imputation combined with the jackknife for inference as an alternative approach. The method is applicable to imputations under a missing-at-random assumption as well as for reference-based imputation approaches. In an application and a simulation study, we demonstrate that it provides consistent treatment effect estimates with the Bayesian approach and reliable frequentist inference with accurate standard error estimation and type I error control. A further advantage of the method is that it does not rely on random sampling and is therefore replicable and unaffected by Monte Carlo error.

1 Introduction

The ICH E9(R1) addendum on estimands stresses the importance of a precise description of the targeted treatment effect reflecting the clinical question posed by the trial objectives [ICH E9 working group, 2019]. One important attribute of an estimand is a list of possible intercurrent events (ICEs), i.e. events occurring after treatment initiation that affect either the interpretation or the existence of the measurements associated with the clinical question of interest, and the definition of appropriate strategies to deal with ICEs. The two most relevant strategies for the purpose of this article are the hypothetical strategy and the treatment policy strategy. For the hypothetical strategy, a scenario is envisaged in which the ICE would not occur. For the treatment policy strategy, the occurrence of the ICE is considered irrelevant in defining the treatment effect of interest and the value for the outcome of interest is used regardless of whether or not the ICE occurred. The ICH E9(R1) addendum distinguishes between ICEs and missing data. However, there are many connections between them. Under the hypothetical scenario, outcome values after the ICE are not directly observable and treated using models for missing data. Under a treatment policy scenario, the outcome assessments after the ICE are observable but may be incomplete because study drop out cannot be prevented. In addition, intermittently missing outcome assessments may also occur due to missed visits.

For more than a decade, a likelihood-based mixed effects model repeated measures (MMRM) approach has been the de facto standard for the primary analysis of continuous longitudinal endpoints in pharmaceutical clinical trials [Mallinckrod et al., 2008]. MMRM typically models the longitudinal outcomes at scheduled visits using a multivariate normal model with the visit time treated as a categorical variable in each treatment group, adjustment for baseline covariates, and an unstructured covariance matrix. MMRM models provide valid treatment effect estimates if the missing data are missing at random (MAR) and the complete data...
model is correctly specified. Informally, this means that missing outcome values can be accounted for by baseline covariates, the assigned treatment group, and observed outcomes. The MAR assumption is often a good starting point for implementing a hypothetical strategy [Mallinckrodt et al., 2020]. However, it is typically less plausible when implementing a treatment policy strategy for an ICE such as study treatment discontinuation. For this latter setting as well as for settings which include multiple ICES handled with different strategies, methods based on imputation of missing data may be more suitable than MMRM [Cro et al., 2020]. For example, it may be feasible to impute missing data after study treatment discontinuation based on subjects from the same treatment group who also discontinued treatment but continued to be followed up [Guizzaro et al., 2021, Polverejan and Dragalin [2020]]. An alternative which is especially attractive if post-discontinuation data are sparse is to use reference-based imputation methods [Carpenter et al., 2013, Cro et al. [2020], Mallinckrodt et al. [2020]]. These methods formalize the idea to impute missing data in the intervention group based on data from a control or reference group. A typical approach to implement these imputation methods is to use multiple imputation based on Bayesian posterior draws of model parameters combined with Rubin’s rules to make inferences.

In this article, conditional mean imputation for MAR and reference-based imputation of missing data is introduced, justified, and explored in a clinical trial application and a simulation study. The proposed approach differs from the conventional multiple imputation methods in several ways. First, it estimates the parameters of the imputation model using maximum likelihood rather than via Bayesian posterior draws [von Hippel and Bartlett, 2021]. Second, it uses a single conditional mean imputation rather than random imputations. Third, inference is based on resampling methods, i.e. the jackknife or the bootstrap, and targets the repeated sampling variance rather than the variance estimated by Rubin’s formula which over-estimates the frequentist variance for reference-based imputation methods [Seaman et al., 2014, Liu and Pang [2016], Tang [2017], Bartlett [2021]]. Unlike the conventional approach, it avoids the specification of prior distributions and the complexity of Markov chain Monte Carlo (MCMC) sampling and leads to deterministic treatment effect estimates (i.e. they are free from Monte Carlo sampling error) and, in the case of the jackknife, also to deterministic confidence intervals and p-values.

2 Conditional mean imputation method

In brief, the procedure is implemented in four steps. First, a MMRM imputation model is fitted to the observed outcome data. Second, conditional mean imputation of missing data is performed based on the parameter estimates from the MMRM imputation model and the chosen imputation method, e.g. reference-based imputation. Third, the completed data is analysed using a simple ANCOVA model. Finally, inference is performed based on resampling techniques. All four steps are described and justified below.

2.1 Alignment between the estimand and the missing data strategy

Assume that the data are from a randomized two-group trial of an intervention versus control with n subjects in total and that each subject i (i = 1, . . . , n) has J scheduled follow-up visits at which the outcome of interest is assessed. Denote the observed outcome vector of length J for subject i by Yi (with missing assessments coded as NA (not available)) and its non-missing and missing components by Yi! and Yi?, respectively.

Missing data in Yi may occur due to missed outcome assessment or may be caused by unobserved counterfactual outcomes relevant to the chosen estimand. Observed outcome data after an ICE handled using a hypothetical strategy is not compatible with this strategy. Therefore, we assume that all post ICE data after ICES handled using a hypothetical strategy are already set to NA in Yi. Intermittent missing data and data after such ICES will be assumed to be MAR (conditional on baseline covariates and observed outcomes). Observed outcome data after an ICE handled using a treatment policy strategy is compatible with the chosen strategy and therefore relevant for the analysis. Such post-ICE data are increasingly systematically collected in RCTs. However, depending on the study context, it may be challenging to retain subjects in the trial after ICES such as study treatment discontinuation and missing post-ICE data may be common. Two options may be applied for imputation of missing post-ICE data in this setting: MAR imputation after inclusion of time-varying covariates and reference-based imputation methods.
Imputation under a MAR assumption after inclusion of appropriate internal time-varying covariates associated with the ICE (e.g. time-varying indicators of treatment compliance, discontinuation or initiation of rescue treatment) have been proposed by Guizzaro et al. [2021]. In this approach, post-ICE outcomes are included in every step of the analysis, including in the fitting of the imputation model. The approach allows that ICEs may impact post-ICE outcomes but that otherwise missingness is non-informative. The approach also assumes that deviations in outcomes after the ICE are correctly modeled by these time varying covariates and that sufficient post-ICE data are available to reliably estimate the effect of ICEs on outcome data.

Depending on the disease area and the anticipated mechanism of action of the intervention, it may be plausible to assume that subjects in the intervention group behave similarly to subjects in the control group after the ICE treatment discontinuation. In such settings, reference-based imputation methods are valuable. They are implemented by combining estimated mean trajectories (and covariance matrices) from both treatment groups when determining the imputation distribution for a subject as described in Section 2.3.

2.2 Imputation model

The purpose of the imputation model is to estimate mean trajectories and covariance matrices for each treatment group in the absence of ICEs handled using reference-based imputation methods. Conventionally, publications on reference-based imputation methods have implicitly assumed that the corresponding post-ICE data is missing for all subjects [Carpenter et al., 2013]. We will also allow the situation where post-ICE data is available for some subjects but needs to be imputed using reference-based methods for others. However, any observed data after ICEs for which reference-based imputation methods are specified are not compatible with the imputation model described below and they will therefore be removed and considered as missing for the purpose of estimating the imputation model, and for this purpose only. If we did not exclude these data, then the imputation model would mistakenly estimate mean trajectories based on a mixture of observed pre- and post-ICE data which are not relevant for reference-based imputations. However, any observed post-ICE data will be included as is in the subsequent imputation and analysis steps. For subjects who experience an ICE that is to be handled by reference-based imputation of missing data, denote the visit after which the ICE occurs by \( t_i < J \). For all other subjects, set \( t_i = \infty \). A subject’s outcome vector after setting observed outcomes after time \( t_i \) to \( \text{NA} \) is denoted as \( Y'_{i} \) and the corresponding data vector after removal of \( \text{NA} \) elements as \( Y''_{i} \).

The imputation model of the longitudinal outcomes \( Y'_{i} \) assumes that the mean structure is a linear function of covariates. At a minimum, the covariates will include the treatment group, the (categorical) visit, and treatment-by-visit interactions. Typically, other covariates including the baseline outcome will also be added. Interaction terms between the treatment group and baseline covariates as well as three-way interactions between treatment group, baseline covariates, and visit can also be included. Indeed, such interactions are recommended if they are suspected to be present based on clinical considerations [Sullivan et al., 2018].

External time-varying covariates (e.g. calendar time of the visit) as well as internal time-varying covariates (e.g. time-varying indicators of treatment discontinuation or initiation of rescue treatment) may in principle also be included if indicated [Guizzaro et al., 2021]. Missing covariate values are not allowed. This means that the values of time-varying covariates must be non-missing at every visit regardless of whether the outcome is measured or missing.

Denote the \( J \times p \) design matrix for subject \( i \) by \( X_{i} \) and the same matrix after removal of rows corresponding to missing outcomes in \( Y'_{i} \) by \( X'_{i} \). Here \( p \) is the number of parameters in the mean structure of the model for the elements of \( Y''_{i} \). The MMRM imputation model for the observed outcomes is defined as:

\[
Y'_{i} = X'_{i}\beta + \epsilon_{i} \quad \text{with} \quad \epsilon_{i} \sim N(0, \Sigma_{iii})
\]

where \( \beta \) is the vector of regression coefficients and \( \Sigma_{iii} \) is a covariance matrix which is obtained from the complete-data \( J \times J \)-covariance matrix \( \Sigma \) by omitting rows and columns corresponding to missing outcome assessments for subject \( i \). Typically, a common unstructured covariance matrix across treatment groups is assumed for \( \Sigma \). However, separate covariance structures per treatment group are also possible and recommended in case heteroscedasticity between treatment groups is suspected.
2.3 Conditional mean imputation step

Conditional mean imputation is based on either maximum likelihood or restricted maximum likelihood (REML) estimates of the regression coefficients and covariance matrices from the imputation model which are denoted by \( \hat{\beta} \) and \( \hat{\Sigma} \). As demonstrated by others, ML-based imputation is often more efficient and avoids the specification of prior distributions and the usage of Markov chain Monte Carlo (MCMC) methods which are required for imputation methods based on Bayesian posterior draws of imputation parameters [Wang and Robins, 1998, von Hippel and Bartlett [2021]].

In order to impute missing data for a subject, the marginal imputation distribution is first defined. For MAR imputation, the marginal imputation distribution for subject \( i \) is a multivariate normal distribution with mean \( \mu_i = X_i \hat{\beta} \) and covariance \( \hat{\Sigma}_i = \hat{\Sigma} \). For subjects randomized to the control group, the same marginal imputation distribution as for MAR imputation is also used for reference-based imputation methods. For a subject \( i \) randomized to the intervention group for whom a reference-based imputation method is used after visit \( \hat{t}_i < J \), denote the predicted mean based on the assigned treatment as \( \mu_i = X_i \hat{\beta} \). Denote the corresponding design matrix assuming that (counter to fact) the subject had been randomized to the control group (but using the subject’s observed baseline characteristics and time-varying covariates) by \( X_{i,ref} \) and denote the corresponding predicted mean from the imputation model by \( \mu_{ref,i} = X_{i,ref} \hat{\beta} \). Then, the mean of the marginal imputation distribution depends on the chosen reference-based imputation method [Carpenter et al., 2013, Section 4.3]:

1. Copy to reference (CR): \( \mu_i = \mu_{ref,i} \).
2. Jump to reference (J2R): \( \mu_i = (\mu_i[1], \ldots, \mu_i[\hat{t}_i], \mu_{ref,i}[\hat{t}_i + 1], \ldots, \mu_{ref,i}[J])^T \).
3. Copy increments in reference (CIR): \( \mu_i = (\mu_i[1], \ldots, \mu_i[\hat{t}_i], \mu_i[\hat{t}_i] + (\mu_{ref,i}[\hat{t}_i + 1] - \mu_{ref,i}[\hat{t}_i]), \ldots, \mu_i[\hat{t}_i] + (\mu_{ref,i}[J] - \mu_{ref,i}[\hat{t}_i]))^T \).

If the same covariance matrix is assumed for both treatment groups, then the covariance matrix of the marginal imputation distribution for reference-based imputation methods is also equal to \( \hat{\Sigma}_i = \hat{\Sigma} \). Otherwise, the relevant covariance matrix is derived based on the estimated covariance matrices from the intervention and control groups as described in Carpenter et al. [2013, Section 4.3].

Intuitively, the marginal imputation distribution under CR assumes that subjects who discontinue the intervention did not get any benefit from the intervention, J2R assumes that they did get benefit but that the benefit is lost immediately after discontinuation, whereas CIR assumes that the benefit accrued up to discontinuation is retained but that there is no additional residual benefit after the ICE. In practice, the most conservative imputation method is J2R while CIR is the least conservative. J2R and CIR may be seen as relatively conservative imputation strategies for a symptomatic or a disease-modifying intervention, respectively. A more general family of reference-based imputation methods has been presented by White et al. [2020].

Imputation of \( Y_i \) is then according to the distribution of \( Y_i | Y_{i!!} \) which is a multivariate normal distribution with mean \( E(Y_i | Y_{i!!}) = \mu_i + \Sigma_i \Sigma_i^{-1} (Y_{i!!} - \mu_i) \), where the subscripts “??” and “!!” denote the selection of elements of \( \mu_i \) and rows and columns of \( \Sigma_i \) corresponding to missing and observed outcomes of \( Y_i \), respectively. Note that \( Y_i \) includes observed post-ICE data, hence they are being conditioned on in the construction of the imputed data. Rather than performing multiple random imputations of the missing data from the conditional distribution of \( Y_i | Y_{i!!} \), we perform a single deterministic conditional mean imputation, i.e. we impute \( Y_{i!!} \) by \( E(Y_i | Y_{i!!}) \). As demonstrated in the next section, analyzing the deterministically imputed dataset using conditional mean imputation with an ANCOVA model is equivalent to performing infinitely many random imputations, analyzing each randomly imputed dataset using ANCOVA, and then pooling the resulting treatment effect estimates.

2.4 Analysis step

The imputed dataset using conditional mean imputation (as described above) can be analyzed using an analysis of covariance (ANCOVA) model with the outcome (or the change in the outcome from baseline) at a specific visit \( j \) as the dependent variable, the assigned treatment group as the primary covariate and, typically, adjustment for the same baseline covariates as for the imputation model. The treatment effect
estimator based on conditional mean imputation is then the regression coefficient corresponding to the treatment assignment and is denoted by $\hat{\theta}_{\text{CMI}}$. In many trials, the outcome at the final visit $J$ will be the primary outcome, but the method can also be applied to outcomes from another visit.

This treatment effect estimator is generated conditional on the REML estimate of the imputation model whereas reference-based imputation methods typically rely on Bayesian posterior draws from the imputation model [Carpenter et al., 2013]. As demonstrated in von Hippel and Bartlett [2021], point estimates based on REML estimation yield slightly more efficient point estimates than methods based on Bayesian posterior draws.

von Hippel and Bartlett [2021] further proposed to perform $M$ random imputations based on the REML estimate, analyze them, and use the average of the treatment effect estimates across random imputations as the overall treatment effect estimator. We denote the treatment effect from a single randomly imputed dataset $m$ as $\hat{\theta}_{SI,m}$ and the overall average treatment effect as $\hat{\theta}_{MI,M} = \frac{1}{M} \sum_{m=1}^{M} \hat{\theta}_{SI,m}$. In contrast, our estimator $\hat{\theta}_{\text{CMI}}$ is based on a single imputed dataset using conditional mean imputation. However, in our particular setting of a continuous outcome analysed by ANCOVA, it is easy to show that the proposed estimator $\hat{\theta}_{\text{CMI}}$ is identical to the pooled treatment effect under multiple random imputation with an infinite number of imputations or, more precisely, that

$$\hat{\theta}_{MI,M} \to \hat{\theta}_{\text{CMI}} \text{ as } M \to \infty \text{ (a.s.)}$$

(1)

Thus, in this particular modeling setup, the estimator based on conditional mean imputation corresponds to a computationally efficient implementation of the proposal by von Hippel and Bartlett [2021].

To prove this, let $Z_m$ and $Z_{\text{CMI}}$ correspond to the imputed outcome vector from the visit relevant to the primary endpoint for all subjects based on the $m^{th}$ randomly imputed dataset and conditional mean imputation, respectively. Then

$$\sum_{m=1}^{M} \frac{1}{M} Z_m \to Z_{\text{CMI}} \text{ as } M \to \infty \text{ (a.s.)}$$

by the strong law of large numbers (and because the randomly imputed outcomes $Z_m$ are realizations of the conditional mean imputation plus mean zero noise). Let $X$ be the design matrix for the ANCOVA analysis model (which is identical for all multiple imputed datasets). Denote the vector of estimated regression coefficients from the ANCOVA model by $\hat{\beta}_{SI,m}$ and $\hat{\beta}_{\text{CMI}}$ for the $m^{th}$ randomly imputed datasets and the conditional mean imputed dataset, respectively. Then

$$\hat{\beta}_{MI,M} = \frac{1}{M} \sum_{m=1}^{M} \hat{\beta}_{SI,m} = \frac{1}{M} \sum_{m=1}^{M} (X^t X)^{-1} X^t Z_m = (X^t X)^{-1} X^t \sum_{m=1}^{M} \frac{1}{M} Z_m \to (X^t X)^{-1} X^t Z_{\text{CMI}} = \hat{\beta}_{\text{CMI}}$$

as $M \to \infty \text{ (a.s.)}$. This proves (1) because the treatment effect estimators $\hat{\theta}_{MI,M}$ and $\hat{\theta}_{\text{CMI}}$ correspond to the same component from the regression coefficient vectors $\hat{\beta}_{SI,M}$ and $\hat{\beta}_{\text{CMI}}$, respectively.

The proof relies on the fact that the ANCOVA estimator is a linear function of the outcome vector. For complete data, the ANCOVA estimator leads to identical parameter estimates as an MMRM model of all longitudinal outcomes with an arbitrary common covariance structure across treatment groups if covariate-visit interactions are included in the analysis model for all covariates [Amemiya, 1985, p. 197]. Hence, the same proof also applies to such MMRM models. We expect that conditional mean imputation is also valid if a general MMRM model is used for the analysis but more involved argument would be required to formally justify this.

### 2.5 Resampling-based inference step

The most frequently used method for inference with multiple imputed data is Rubin’s variance formula. However, Rubin’s formula is not applicable to imputations methods generated conditional on the REML
estimate [von Hippel and Bartlett, 2021]. Moreover, Rubin’s formula overestimates the repeated sampling variance for reference-based imputation methods [Seaman et al., 2014, Liu and Pang [2016], Tang [2017], Bartlett [2021]]. Rather than deriving complex analytic formulas for inference in our setting, we propose to use resampling methods such as the jackknife and the bootstrap [Efron and Tibshirani, 1994].

The jackknife requires $n$ estimates of the treatment effect based on data with one subject removed from the original dataset each time. Denote the treatment effect estimate based on omission of subject $i$ by $\hat{\theta}_{(-i)}$ ($i = 1, \ldots, n$). Then, the jackknife standard error is defined as

$$se_{jack} = \left( \frac{n-1}{n} \sum_{i=1}^{n} (\hat{\theta}_{(-i)} - \bar{\theta}_{(\cdot)})^2 \right)^{1/2}$$

where $\bar{\theta}_{(\cdot)}$ denotes the mean of all jackknife estimates. Tests of the null hypothesis $H_0 : \theta = \theta_0$ (and corresponding confidence intervals) are then based on the $Z$-score $Z = (\hat{\theta} - \theta_0)/se_{jack}$ using a normal approximation.

Consistency of the jackknife method for variance estimation has been demonstrated for REML estimators in linear mixed effects models [Jiang and Nguyen, 2007, Chapter 1.4.4] and for M estimators [Shao and Tu, 1995, Example 2.5] under suitable regularity conditions. As described, the conditional mean imputation is a simple transformation of the REML estimator of the imputation model, the observed data, and missing data indicators, and the ANCOVA analysis model is a linear transformation of the imputed outcomes. Therefore, our procedure can also be represented as an M-estimator, i.e. as a stacked set of estimating equations for estimating the variance components and parameters indexing the mean function of the MMRM model and those for the analysis model.

Bootstrap methods can be used as an alternative to the jackknife and have already been recommended for inference with multiple imputed missing data [von Hippel and Bartlett, 2021, Bartlett and Hughes [2020]]. In most trials, the sample size $n$ should be sufficiently large to ensure that bootstrap standard errors and normal approximations are reliable. An advantage of the bootstrap over the jackknife is that if there are concerns about the adequacy of the normal approximation, then more precise bootstrap confidence intervals (e.g. percentile or BCa intervals) and tests can be used for inference instead [Efron and Tibshirani, 1994]. The disadvantages of the bootstrap are that the inference is no longer deterministic and that it is more computationally intensive as the required number of bootstrap samples $B$ is typically much larger than the sample size $n$. As a minimum, $B = 999$ is recommended [Davison and Hinkley, 1997, page 202] and, as described in the Appendix, substantially higher values may be required to obtain accurate $p$-values which are minimally affected by Monte Carlo error.

3 Application

We applied the proposed methods to a publicly available example data set from an antidepressant clinical trial of an active drug versus placebo which was also used in Mallinckrodt et al. [2013] and Tang [2017]. The relevant endpoint is the Hamilton 17-item rating scale for depression (HAMD17) which was assessed at baseline and at weeks 1, 2, 4, and 6. Study drug discontinuation occurred in 24% (20/84) for the active drug and 26% (23/88) for placebo. All data after study drug discontinuation were missing and there was a single additional intermittent missing observation. The imputation model had the mean change from baseline in the HAMD17 score as the outcome, included the treatment group, the (categorical) visit, treatment-by-visit interactions, the baseline HAMD17 score, and baseline HAMD17-by-visit interactions as covariates, and assumed a common unstructured covariance matrix in both groups. The dataset included one intermittent missing value which was imputed under a MAR assumption. For imputing missing data after study drug discontinuation, we explored MAR and reference-based imputation methods. The ANCOVA analysis model also adjusted for the baseline HAMD17 value. The results for conventional Bayesian multiple imputation combined with Rubin’s rule (with $M=1,000$ imputations) and conditional mean imputation using jackknife or bootstrap (with $B=10,000$ bootstrap samples) standard errors, respectively, are reported in Table 1.

As shown in Table 1, conventional Bayesian multiple imputation methods and conditional mean imputation...
result in nearly identical estimates of the treatment effect for all imputation approaches. However, for reference-based imputation methods, the resampling-based standard errors of the treatment effect from conditional mean imputations are considerably smaller than those from Rubin’s rule. The reported treatment effects estimates and standard errors are very similar to those obtained in Tang [2017] using a different implementation. The treatment effect estimate and the standard error for conditional mean imputation were smallest for J2R and the bootstrap produced slightly smaller standard error estimates than the jackknife for all methods. The estimated computing time to obtain all four reported treatment effect estimates and corresponding standard errors with our R software implementation were 14.48 minutes for Bayesian multiple imputation \((M=1,000)\), 5.16 minutes for conditional mean imputation plus jackknifing, and 312 minutes (5.2 hours) for conditional mean imputation plus bootstrapping \((B=10,000\), i.e. on average 1.87 seconds per bootstrap sample) on a single core of a low-power laptop with an Intel Core i5-8365U processor (1.60 GHz), 16 GB RAM, and Windows 10 Enterprise.

### 4 Simulation study

To further explore the statistical properties of the methods, we conducted a simulation study of a trial of an active drug versus placebo with 100 subjects per group and bi-monthly visits from baseline until 12 months. The other simulation parameters were chosen as follows:

- The mean outcome trajectory in the placebo group (and in the active group under the null hypothesis) increased linearly from 50 at baseline to 60 at 12 months.
- The mean outcome trajectory in the active group under the alternative hypothesis was identical to the placebo group up to month 4. From month 4 onward, the slope decreased by 50% to 5 points/year.
- The covariance structure of the baseline and follow-up values in both groups was implied by a random intercept and slope model with a standard deviation of 5 for both the intercept and the slope, and a correlation of 0.25. In addition, an independent residual error with standard deviation 2.5 was added to each assessment. This implies marginal standard deviations ranging from 5.59 at baseline to 8.29 at the last visit in both groups.
- The probability of study drug discontinuation after each visit was calculated according to a logistic
model which also depended on the observed outcome at that visit. Specifically, a visit-wise discontin-
uation probability of 1.5% and 2.5% in the placebo and active group, respectively, was specified if the
observed outcome was 50 or lower. For outcomes above 50, the odds of discontinuation further increase
by 50% for each 10 point increase.

- Study drug discontinuation had no effect on the mean trajectory in the placebo group. In the active
group, subjects who discontinued followed the slope of the mean trajectory from the placebo group
from that time point onward (CIR).

- Study drop-out at the study drug discontinuation visit occurred with a probability of 75% leading to
missing outcome data from that time point onward.

Under the alternative, the simulation parameters imply a true treatment effect of -2.59 (7.41 vs 10.00) under
a treatment policy strategy which is the target of this simulation study. The treatment discontinuation
probabilities were 34% and 24% in the active and placebo groups, respectively, under the null hypothesis
and 31% and 24% under the alternative hypothesis.

Reference-based imputation methods of missing data were compared to MAR imputation. Observed post-
discontinuation data was included in the analysis model but excluded from the imputation model for both
reference-based and MAR-based imputation methods. Imputation and analysis models were as described for
the antidepressant clinical trial application. Results for conventional Bayesian multiple imputation combined
with Rubin’s rule were based on \( M = 100 \) random imputation and bootstrap standard errors were calculated
for \( B = 1,000 \) bootstrap samples per simulation scenario.

Results of the simulations under the null hypothesis based on 100,000 simulated data sets are presented in
Table 2. This choice of the number of simulations provides a Monte Carlo standard error for type I error
estimates of approximately 0.07%. As expected, the mean treatment effect estimate was 0 for all imputation
methods and scenarios. For Bayesian multiple imputation, the observed standard deviation of treatment
effect estimates across simulation runs \( (SD \hat{\theta}) \) was substantially smaller than the mean treatment effect
standard errors from Rubin’s rule (mean \( \hat{\text{se}} \hat{\theta} \)) for reference-based imputation methods. Consequently, the
simulated type I error ranged from 0.9%-2.5% and was substantially below the nominal type I error of 5%.

It is sometimes reported that MMRM models such as our imputation model using an unstructured covariance
matrix result in convergence problems [Mallinckrodt et al., 2008]. In this case, a simplified correlation struc-
ture, e.g. a Toeplitz or autoregressive structure, may be applied. In our simulations with a relatively small
sample size and a substantial proportion of subjects with treatment discontinuations and missing data, the
imputation model failed to converge in only 8/100,000 (0.008%) simulated datasets and these were excluded
from our summaries in Table 1. Jackknifing did not result in any additional convergence problems and con-
vergence problems occurred only for 1/(100,000-8)*1,000 bootstrap samples. In case convergence problems
occurred for a bootstrap sample, we replaced it by a new bootstrap sample which seems unproblematic due
to the minimal number of observed convergence problems.

Results of the simulations under the alternative hypothesis based on 10,000 simulated data sets (providing
a Monte Carlo standard error for power estimates of <0.5%) are reported in Table 3. The mean treatment
effect estimate under a CIR assumption was identical to the simulation truth of -2.59 for all three methods.
MAR imputation gave inflated treatment effect estimates whereas CR and J2R were conservative. Rubin’s
rule overestimated the frequentist standard error for all reference-based methods leading to a substantial
reduction in the estimated power compared to methods based on conditional mean imputation plus re-
sampling. The frequentist standard error was lowest for J2R. Surprisingly, this led to a larger simulated
power for J2R than for CIR or CR. Inference based on bootstrapping led to small power gains of 0.3-0.4%
across the four scenarios compared to the jackknife.
Table 2: Results from the simulation study under the null hypothesis (for a nominal two-sided significance level of 5%).

| Imputation | Parameter   | Bayes+Rubin Conditional mean (jackknife) | Bayes+Rubin Conditional mean (bootstrap) |
|------------|-------------|----------------------------------------|----------------------------------------|
|            |             | Mean $\hat{\theta}$ | SD $\hat{\theta}$ | Mean $\hat{s}\hat{e}_{\hat{\theta}}$ | Type I error | Mean $\hat{\theta}$ | SD $\hat{\theta}$ | Mean $\hat{s}\hat{e}_{\hat{\theta}}$ | Type I error | Mean $\hat{\theta}$ | SD $\hat{\theta}$ | Mean $\hat{s}\hat{e}_{\hat{\theta}}$ | Type I error |
| MAR        | Mean $\hat{\theta}$ | 0.00 | 0.93 | 0.92 | 5.0% | 0.00 | 0.93 | 0.92 | 5.3% |
| J2R        | Mean $\hat{\theta}$ | 0.00 | 0.69 | 0.93 | 0.9% | 0.00 | 0.69 | 0.69 | 5.1% |
| CR         | Mean $\hat{\theta}$ | 0.00 | 0.80 | 0.91 | 2.5% | 0.00 | 0.80 | 0.80 | 5.2% |
| CIR        | Mean $\hat{\theta}$ | 0.00 | 0.80 | 0.92 | 2.4% | 0.00 | 0.80 | 0.79 | 5.2% |

Table 3: Results from the simulation study under the alternative hypothesis (for a nominal two-sided significance level of 5%).

| Method | Parameter   | Bayes+Rubin Conditional mean (jackknife) | Bayes+Rubin Conditional mean (bootstrap) |
|--------|-------------|----------------------------------------|----------------------------------------|
|        |             | Mean $\hat{\theta}$ | SD $\hat{\theta}$ | Mean $\hat{s}\hat{e}_{\hat{\theta}}$ | Power | Mean $\hat{\theta}$ | SD $\hat{\theta}$ | Mean $\hat{s}\hat{e}_{\hat{\theta}}$ | Power |
| MAR    | Mean $\hat{\theta}$ | -3.14 | 0.93 | 0.93 | 92.1% | -3.14 | 0.92 | 0.92 | 92.4% |
| J2R    | Mean $\hat{\theta}$ | -2.38 | 0.74 | 0.93 | 74.9% | -2.38 | 0.74 | 0.75 | 74.7% |
| CR     | Mean $\hat{\theta}$ | -2.55 | 0.83 | 0.93 | 80.4% | -2.55 | 0.83 | 0.84 | 87.1% |
| CIR    | Mean $\hat{\theta}$ | -2.59 | 0.82 | 0.93 | 82.0% | -2.59 | 0.83 | 0.82 | 88.7% |
5 Discussion

We introduced and justified conditional mean imputation based on the REML estimates of the parameters of the imputation model for MAR and reference-based imputation methods with longitudinal multivariate normal outcomes. For inference, we explored the use of the jackknife and the bootstrap. The data example and the simulations illustrated that this leads to nearly identical treatment effect estimates compared to established methods based on multiple imputation using Bayesian posterior draws form the imputation model parameters and Rubin’s rules. The resampling methods produced standard errors which were consistent with the observed repeated sampling variation. The jackknife demonstrated exact protection of the type I error in simulations with a relatively low sample size \( n = 100 \) per group and a substantial amount of missing data (>25% of subjects with treatment discontinuations) whereas the bootstrap based on a normal approximation showed a slightly increased type I error rate compared to the nominal value (up to 5.3% at a nominal 5% significance level). The method based on Bayesian posterior draws and Rubin’s rule had inflated standard errors relative to the true repeated sampling variance, very conservative observed type I error rates, and a substantially decreased statistical power for reference-based imputation methods.

It is well known that Rubin’s rule does not provide frequentist consistent estimates of the standard error for reference-based imputation methods [Seaman et al., 2014, Liu and Pang [2016], Tang [2017], Cro et al. [2019], Bartlett [2021]]. Intuitively, this occurs because reference-based imputation methods borrow information from the reference group for imputations in the intervention group. This creates a positive correlation between outcomes in the two treatment groups and leads to a reduction in the frequentist variance of the resulting treatment effect contrast which is not captured by Rubin’s variance estimator. Formally, the discrepancy is due to uncongeniality between the imputation and analysis models in reference-based imputation methods [Meng, 1994, Bartlett [2021]]. Cro et al. [2019] argued that Rubin’s rule is nevertheless valid for reference-based imputation methods because it is approximately information-anchored, i.e. standard error estimates from reference-based imputation methods are similar to those observed under MAR imputation. In contrast, several other authors have implicitly or explicitly favored inference that is correct from a frequentist repeated-sampling perspective [Seaman et al., 2014, Liu and Pang [2016], Tang [2017], Bartlett [2021]].

To us, information anchoring is a sensible concept for sensitivity analyses, whereas for a primary analyses, we feel that that it is more important to adhere to the principles of frequentist inference. Analyses of data with missing observations generally rely on unverifiable missing data assumptions and the assumptions for reference-based imputation methods are relatively strong. Therefore, these assumptions need to be clinically justified as appropriate or at least conservative for the considered disease area and the anticipated mechanism of action of the intervention. Furthermore, appropriate sensitivity analyses should be planned. While we have not explicitly discussed it, \( \delta \)-based imputation approaches which adjust imputed datasets prior to the analysis from the reference group for imputations in the intervention group. This creates a positive correlation between outcomes in the two treatment groups and leads to a reduction in the frequentist variance of the treatment effect contrast which is not captured by Rubin’s variance estimator. Formally, the discrepancy is due to uncongeniality between the imputation and analysis models in reference-based imputation methods [Efron and Stein, 1981] whereas bootstrap variances are more susceptible to downwards bias [Shao and Tu, 1995, Section 3.4.3].

As a consequence, the jackknife demonstrated exact type I error protection whereas the bootstrap led to slight type I error inflation. If the bootstrap is applied, we recommend a large number of bootstrap samples (at least \( B = 10,000 \)) and, especially for smaller sample sizes, the usage of more accurate methods for confidence intervals and testing than the normal approximation based on the bootstrap standard error. A further advantage of the jackknife is that the proposed conditional mean imputation approach leads to deterministic
treatment effect estimates, standard errors, and inference. This is particularly important in a regulatory setting where it is important to ascertain whether a calculated p-value which is close to the critical boundary of 5% is truly below or above that threshold rather than being uncertain about this because of Monte Carlo error.

In summary, we believe that conditional mean imputation for MAR and reference-based imputation of missing data combined with the jackknife is a useful complementary approach to Bayesian multiple imputation for longitudinal multivariate normal outcomes. It provides replicable parameter estimates and standard errors and correct frequentist inference.

Software implementation

All presented methods have been implemented in an R package ‘rbmi’ which will be uploaded to CRAN in early 2022. The data analysis of the antidepressant clinical trial is described in the ‘Quick start’ vignette of the package. All simulation code has been uploaded to github and can be found at https://github.com/nociale/Simulation_condmean_manuscript.

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A Appendix: Additional details on bootstrap methods

A.1 Bootstrap confidence intervals (CI) and tests for the treatment effect

A large number of bootstrap methods for CI estimation and testing have been proposed in the literature. We first discuss methods based on the bootstrap standard error and the normal approximation and percentile bootstrap methods [Efron and Tibshirani, 1994, Davison and Hinkley [1997]].

Denote the treatment effect estimates from B bootstrap samples by \( \hat{\theta}^*_b \) \( (b = 1, \ldots, B) \). The bootstrap standard error \( \hat{SE}_{boot} \) is defined as the empirical standard deviation of the bootstrapped treatment effect estimates. The corresponding two-sided normal approximation \( 1 - \alpha \) CI is defined as \( \hat{\theta} \pm z_{1-\alpha/2} \cdot \hat{SE}_{boot} \) where \( \hat{\theta} \) is the treatment effect estimate in the original dataset, i.e. \( \hat{\theta} = \hat{\theta}_{CMI} \). Tests of the null hypothesis \( H_0 : \theta = \theta_0 \) are then based on the Z-score \( Z = (\hat{\theta} - \theta_0) / \hat{SE}_{boot} \) using a standard normal approximation.

Alternatively, percentile bootstrap methods can be used for inference: For this method, the \( 1 - \alpha \) CI is defined as the interval from the \( (B + 1)\alpha/2^{th} \) to the \( (B + 1)(1 - \alpha/2)^{th} \) ordered bootstrap treatment effect estimate [Davison and Hinkley, 1997, page 202f]. Typically, \( B \) is chosen such that \( (B + 1)\alpha/2 \) is an integer value. If this is not the case, the corresponding empirical bootstrap quantiles can be calculated via interpolation. The advantage of the percentile method over the standard normal approximation is that it is also valid if the treatment effect estimator itself is not normally distributed around the population treatment effect but if a monotone transformation exists which normalizes the estimator [Efron and Tibshirani, 1994, page 173f]. For example, this is the case for t-distributed test statistics.

A one-sided bootstrap p-value for a test of the null hypothesis \( H_0 : \theta = \theta_0 \) versus the alternative \( H_1 : \theta > \theta_0 \) can be defined via inversion of the percentile CI as \( \hat{p} = (\# \{ \hat{\theta}^*_b < \theta_0 \} + 1) / (B + 1) \) [Efron and Tibshirani, 1994,
The one-sided test in the other direction can be defined in the same way, and the corresponding two-sided p-value is defined as 2 times the lower of the two one-sided p-values.

More complex bootstrap confidence intervals also exist and the BCa (bias-corrected and accelerated) CI is often recommended [Carpenter and Bithell, 2000]. This would be also possible but not trivial in our setting: Typically, \( a \) is estimated based on the skewness of the score function [Davison and Hinkley, 1997, page 205] (which is not available in closed form in our setting) or on a jackknife approximation to it [Carpenter and Bithell, 2000] (which is computationally intensive in our setting).

Permutation tests and associated bootstrap tests which draw bootstrap samples under the null hypothesis of equality between the two randomized treatment groups [Efron and Tibshirani, 1994, chapters 15 and 16; Davison and Hinkley, 1997, chapter 4] could also be applied but have not been explored in our setting.

### A.2 How many bootstrap samples?

As a general rule of thumb, Davison and Hinkley [1997] (pages 156 and 202) advise to use \( B = 999 \) or more for CI estimation and testing. However, as illustrated below, substantially larger values of \( B \) may be required for accurate estimation of p-values. Let \( \hat{p}_\infty \) denote the one-sided p-value that would be obtained under the theoretical bootstrap distribution (including all possible bootstrap samples) and \( \hat{p}_B \) the corresponding p-value based on \( B \) bootstrap samples. The approximate distribution of \( \hat{p}_B \) is:

- \( \hat{p}_B \sim \Phi(\Phi^{-1}(\hat{p}_\infty) / \sqrt{Z/(B-1)}) \) with \( Z \sim \chi^2(df = B - 1) \) for the normal approximation
- \( \hat{p}_B \sim (Z + 1)/(B + 1) \) with \( Z \sim Bin(B, \hat{p}_\infty) \) for the inverted bootstrap percentile method

Based on these approximations, the probabilities below were evaluated:

| Method                  | B=999 | B=9,999 | B=99,999 |
|-------------------------|-------|---------|----------|
| **Normal approximation**|       |         |          |
| 95% range for \( \hat{p}_B \) if \( \hat{p}_\infty = 2.5\% \) | 2.02% to 3.02% | 2.34% to 2.66% | 2.45% to 2.55% |
| Probability of \( \hat{p}_B \leq 2.5\% \) if \( \hat{p}_\infty = 2\% \) | 98.37% | 100.00% | 100.00% |
| Probability of \( \hat{p}_B > 2.5\% \) if \( \hat{p}_\infty = 3\% \) | 96.42% | 100.00% | 100.00% |
| **Percentiles method**  |       |         |          |
| 95% range for \( \hat{p}_B \) if \( \hat{p}_\infty = 2.5\% \) | 1.70% to 3.60% | 2.21% to 2.82% | 2.40% to 2.60% |
| Probability of \( \hat{p}_B \leq 2.5\% \) if \( \hat{p}_\infty = 2\% \) | 84.66% | 99.97% | 100.00% |
| Probability of \( \hat{p}_B > 2.5\% \) if \( \hat{p}_\infty = 3\% \) | 84.54% | 99.88% | 100.00% |

These tables demonstrate that the approximation of the bootstrap distribution with a finite number of bootstrap samples \( B \) requires a larger \( B \) for high accuracy of the percentile method (which is valid under weaker assumptions) than for the method based on the bootstrap standard error (which relies on a normal approximation).

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