Multiple imputation of partially observed covariates in discrete-time survival analysis

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1 Introduction

Many phenomena in the social or the medical sciences can be characterized as events, meaning that a qualitative change occurs at some point in time. Typical research questions focus on whether, when, and under which circumstances events occur. For instance, a sociologically relevant event could be a divorce or a job offer after a period of unemployment. When analyzing such time-to-event (or "survival" time) data, we cannot rely on the simple linear regression model since the time-to-event is missing (censored) for parts of the population. E.g., some married people never experience divorce, and while everybody dies, data collection will almost certainly not continue until this point for all observations. Different analytical approaches were developed to deal with the censoring problem, like for example Cox regression for continuous-time survival analysis (Cox, 1972). Cox (1972) also extended the proportional hazard model for *discrete-time* survival analysis, analyzing the conditional odds of an event at a particular time point, given survival up to that point. Data have to be first transformed for discrete-time survival analysis, from the familiar person format (one row for each person/observational unit) to person-period format (one row for each period in which we observed a person).

A challenge that arises in the application of these survival models (as well as in other models) is that often, one or more covariates have missing data. Simplistic approaches like complete case (CC) analysis/listwise deletion (LD) and unconditional mean imputation are still used at the moment in the social sciences (e.g. Tzeng and Mare, 1995; Böttcher, 2006; Cooke, 2006; Arranz Becker and Lois, 2010; Manning

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et al., 2016; Cooper et al., 2018; Stoddard and Veliz, 2019). However, these simplistic approaches can be highly inefficient (listwise deletion) or lead to severely biased variance estimates (e.g., mean imputation). Also, point estimates can be biased after listwise deletion if the missingness mechanism depends on the outcome (Hughes et al., 2019).

Another approach for handling missing data is multiple imputation (Rubin, 1987, 1996; Schafer, 1997; van Buuren et al., 1999). Multiple imputation (MI) leads to unbiased point and variance estimates if certain conditions concerning the missingness mechanism and the imputation model met (Allison, 2000, 302). However, while there has been research on how to best impute missing covariate values for Cox regressions (van Buuren et al., 1999; White and Royston, 2009; Clark and Altman, 2003; Keogh and Morris, 2018) and the Cox cure model (Beesley et al., 2016) as well as the relative survival model (Nur et al., 2009), the imputation of covariates in discrete-time survival analysis is still understudied. For time-varying covariates, (Murad et al., 2019) showed that MI approaches using information from the previous and current time point seems sufficient in most situations. However, in discrete-time survival analysis, not only time-varying covariates are used but also time-invariant covariates. *Our novel contribution to the literature will be to explore how to specify a suitable imputation model for partly missing time-invariant covariates in a discrete-time survival analysis.*

This is not an easy or straightforward task. Kenward and Carpenter (2007, 205) shows that including outcome information in the imputation model for partially observed covariates is crucial for unbiased estimates. However, with discrete-time survival models, the two outcome variables (event and time-to-event) are not fully observed due to censoring. Nor is it clear in which format the imputation procedure should be carried out: with data in person format or person-period format? And how should the relationship between the time-to-event variable and the covariates be modeled for imputation?

It is a relevant issue since discrete-time survival analysis is widely used in the social sciences, especially in the family sociology (as an example for many see Barber, 2001; Schoen et al., 2002; Cooke, 2006; Nomaguchi, 2006; Arranz Becker and Lois, 2013) but also in the medical sciences (Murad et al., 2019).

In this article, we start with an introduction to the Discrete-Time Survival Analysis Model (DTSAM) and the derivation of the analysis model. We also cover the two data formats: person format and person-period format (see Section 2). We will also briefly discuss the problematic implications of missing covariates (see Section 2.3). We then continue with a brief introduction to multiple imputation before presenting different possible imputation approaches, divided by data format and specification of the imputation model (see Section 3). We conducted four simulations with discrete

survival data and varying degrees of unobserved heterogeneity (see Section 4). Using data from the German Family Panel pairfam, we also provide an applied example with real-world data (see Section 5). We close with a discussion of further results and steps (see Section 6).

2 Discrete-time Survival Analysis Model

2.1 The model

In the following section, we will introduce the formalization of a discrete-time Survival Analysis Model (DTSAM) by [Singer and Willett \(1993\)](#).

We speak of discrete survival time when the time-to-event can only take distinct values, e.g., one, two, three, or more years/semesters/weeks. Occasionally, discrete survival data are “truly discrete” ([Kleinbaum and Klein, 2012, 325](#)); the event can only happen at distinct values of time (e.g., fertility modeling, particularly the time from puberty to first childbirth). However, in most cases, discrete data are the result of interval-censoring. Events might happen in a continuous range of time, but they were observed only in grouped form instead of continuous-time data, e.g., only the year of divorce is recorded and not the month and day ([Kleinbaum and Klein, 2012, 318](#)).

Let T be a discrete random variable that indicates the period j when the event occurs for a randomly selected individual from the population. E.g., T could be the time until the divorce in a person’s first marriage. The reader should note we look at non-repeatable events here and that event occurrence is thus inherently conditional. For instance, a person can only experience the divorce of their first marriage if he or she did not already experience it in any of the periods before j . We aim at describing T by a *conditional probability density function*. The conditional probability that an event will occur in each period given that it has not occurred earlier is called discrete-time hazard h_j .

Researchers are usually interested in whether the risk of event occurrence differs systematically between observations. E.g., in a study of divorce risks, the risk might depend on age at begin of marriage or differences in social status between partners. We consider K of such predictors X_k . For now, we examine predictors that do not vary over time, i.e., *time-invariant predictors*. We now have to distinguish between different individuals, each with their predictor values X_{ki} for K predictors X_k .

We model the individual hazard h_{ij} as depending on periods P_j and predictors X_k through a logit link (see [Singer and Willett \(1993, 166\)](#), also [Cox \(1972\)](#); [Allison \(1982\)](#))

Our model to be estimated is the following model (Singer and Willett, 2003, 317):

$$\log_e \left(\frac{h_{ij}}{1 - h_{ij}} \right) = (\alpha_1 P_{1ij} + \alpha_2 P_{2ij} + \dots + \alpha_J P_{Jij}) + \quad (1)$$

$$(\beta_1 X_{1ij} + \beta_2 X_{2ij} + \dots + \beta_K X_{Kij}). \quad (2)$$

This model contains not a single intercept but instead, a set of alpha parameters $[\alpha_1, \alpha_2, \dots, \alpha_J]$ that act as intercepts for a specific period. The β s postulates how much the logit-hazards shift with a unit shift in the parameters, e.g., how much an additional year in age difference between partners shifts the logit of the hazard of divorce.

2.2 The data: Transformations and the person-period format

Having established our model, we take a closer look at our data and the data format needed for a DTSAM.

In general, data comes in person format, and there is a separate row for each observational unit (or person). Apart from the covariates X_k , there are three other possible outcome variables. First, there is the variable T , the time-to-event. However, as we discussed already, we would almost certainly have a lot of missing data in T due to censoring. Instead of the variable T , usually the variable Y is included, the *time until the last observation*. This variable is observed, regardless of whether the event occurred or censoring happened. We can also add an event indicator E , set to one if the event occurred, and set to zero if the observation is censored.

To estimate a DTSAM (our *substantive* model), we need the data set in the so-called *person-period format* (see Table 1) (Singer and Willett, 1993, 171). In the person-period (PP) format, there is a separate row for each period in which a unit/person was observed. Apart from the covariates, we usually have a variable indicating the observed period P column and again an event indicator E . The event indicator is only set to one if the event happened for this unit in this specific period. Usually, the time-until-the-last-observation Y is not included separately in person-period format, but we will need it for certain imputation approaches. Y as an added variable in person-period format is exclusively used for imputation, not for analysis.

The dichotomous event indicator E is treated as a collection of independent values with a hypothesized logistic dependence on predictors (Singer and Willett, 1993, 174). This model implicitly asserts that the variables exhaust all the sources of individual variation in the hazard rate. E.g., the model implies that the variation in hazards is *only* due to differences in the independent variables and period and that the model is correctly specified. The survival model literature describes these models as having no unobserved heterogeneity (Allison, 1982)[82].

The omission of a critical predictor of the outcome from the model is equivalent to mixing hazard profiles for the different populations defined by the dropped predictor values. The pooled hazard profile does not have to look like any member of the general population. As an example, assume that all members of the population have a flat risk profile. Still, the height of the risk profile differs between members. Over time, members with a high risk drop out of the population. If we do not include the predictor that shifts the members' risk, the aggregate profile will show a risk profile that decreases with time (also [Singer and Willett \(1993, 185\)](#)).

Building a model that exhausts *all sources of individual variation* is practically not feasible. We do not only have to worry about possible confounders missing from our model, but we have to worry about *all* direct predictors of survival. However, even in the absence of important predictors, parameters can still be interpreted as an average across population hazard profiles ([Xue and Brookmeyer, 1996](#)). E.g., if we include obesity as a risk factor for diabetes in our model and there is unobserved heterogeneity, we would estimate the log odds ratio for all people with obesity versus those without. If there would be no unobserved heterogeneity, we could *additionally* interpret the regression coefficient as the log odds ratio for an individual before versus after developing obesity. Apart from complicating the interpretation of factors, [Allison \(1982, 83\)](#) also remarks that in case of unobserved heterogeneity, “one would expect this dependence among the observations to lead to inefficient coefficient estimates and estimated standard errors that are biased downward”. As unobserved heterogeneity is not entirely avoidable, we will generate the data sets in our simulations with varying degrees of unobserved heterogeneity. However, before testing the different MI approaches in this section, we take a closer look at the implications of missing data for discrete-time survival analysis.

2.3 Implications of missing data for discrete-time survival analysis

Survey data are often subject to missing data, and we have to decide how to treat partially observed covariates in discrete-time survival analysis. We want to note that we are only looking at missing data in the covariates in this article, not at missing data in the time-to-event variable. Multiple imputation is not suited for missing data due to censoring ([Allison, 2010](#)).

Before exploring different possible imputation strategies for imputing partly missing covariates in discrete-time survival analysis, we take a more general look at the implications of missing data in regression analysis to demonstrate the need for a suitable MI strategy.

One way to deal with missing data in regression analysis is to drop the incomplete

observations, i.e., listwise deletion (LD) and complete case analysis. The first and most basic implication is that we possibly have huge efficiency losses (Carpenter and Kenward, 2013, 9). Only the observations with all the variables observed will be used, i.e., even just one missing covariate among many will lead to the complete exclusion of the observation. This gets especially problematic if we aim at keeping unobserved heterogeneity down and include not only possible confounders but also other essential predictors.

However, efficiency losses are one only side of the coin, bias being the other one. Table 2 gives an overview under which conditions logistic regression coefficients will be biased after listwise deletion. X_1 and X_2 are here two independent variables, Y is the dependent binary outcome variable. For now, it is only essential on which variables the completeness of a case depends (e.g., the outcome and X_1), not what kind of missingness mechanism is behind the nonresponse.

From Table 2, we can see that in case of logistic regression, say DTSAM, the coefficient of an independent variable X_1 is only biased if the completeness of an observation depends

1. on the binary outcome
2. and on the covariate X_1 itself.

Concerning the bias, it does not make a difference for analysis results after listwise deletion whether X_1 is missing not at random (MNAR) dependent on X_1 and the outcome or whether X_2 is missing at random (MAR) dependent on X_1 and the outcome (Carpenter and Kenward (2013, 32)).

Therefore, if we have to assume that the estimated coefficient of our variable X_1 will be biased after listwise deletion (LD), we need other approaches to handle missing data. Nevertheless, even if we are confident that our LD analysis will not be biased, we will lose information from incomplete cases. Thus, listwise deletion cannot be an adequate solution in most cases, and we will look into multiple imputation as a possible remedy for these unwanted effects of missing data.

3 Multiple imputation

3.1 Multiple imputation in general

One of the most popular approaches to tackle missing data is multiple imputation (Little and Rubin, 2002). Multiple imputation (MI) allows for the analysis of incomplete data sets through substituting missing values. We substitute by “imputing” values of a variable based on other variables, mostly those of the analysis model.

Concerning analyses, these imputed values are not treated the same as observed values, since this would lead to biased variance estimates. Therefore, we impute several times to avoid treating imputed values as observed. Each data set is then analyzed separately, and estimates and standard errors are combined across imputations using rules developed by [Rubin \(1987\)](#).

If we are now to impute missing values, we need models of the distribution of the missing values. There are two main approaches, the joint modeling (JM) approach and the fully conditional specification (FCS) or chained equations approach ([van Buuren, 2007](#))¹. Joint modeling MI ([Schafer, 1997](#)) draws missing values simultaneously for all incomplete variables using a multivariate distribution. However, specifying such a joint model is often challenging, for example for categorical variables. In contrast, FCS “splits” the problem into a series of univariate problems ([van Buuren, 2007](#)). FCS involves specifying a series of univariate models for the conditional distribution of each partially observed variable, given all the other variables ([White et al., 2011](#)). It is more flexible than the joint model approach since adequate regression models can be selected for every variable (e.g., linear regression for continuous partially observed variables, logistic regression for binary partially observed variables).

When specifying the imputation model, it is crucial to account for the *substantive model*. The *substantive model* is the model that the researcher is primarily interested in, often also called the *analytical model*. The associations to be examined in the substantive model must also be represented in the imputation model; if not, bias towards null will be the likely consequence ([Fay, 1992](#)). The imputation and substantive model should be compatible. *Compatible* means that there exists a joint model with conditionals that equal the imputation model and the substantive model (see [Bartlett et al. \(2014\)](#) and for the related term of congeniality see [Meng \(1994\)](#)). In addition to JM and FCS, a variation of FCS that allows to easier specify a compatible imputation model was recently developed by [Bartlett et al. \(2014\)](#). It is called *Substantive Model Compatible - Fully Conditional Specification* or SMC-FCS. SMC-FCS is used when it is hard to find a compatible standard FCS imputation model if the substantive model is either non-linear (e.g., a Cox regression), or contains non-linear (e.g., squared or interaction) terms. SMC-FCS can also be used for models without non-linear terms. Like with FCS, separate models are specified for the partially observed variables. What differentiates it from regular FCS, is that the conditional distribution of a variable (given the other variables) is *combined with the specified substantive model* to define an imputation model.² This combination ensures that the missing data are drawn from models *compatible* with the specified substantive model.

3.2 Handling missing covariates values in case of a DTSAM as a substantive model

Representing the associations in the substantive model is not straightforward in case of a DTSAM and its complicated outcome structure. Generally we have two “outcome” variables: the event indicator E and the time-until-the-last-observation Y . Also, it is not clear whether to impute in person or person-period format. Imputing, while the data set is still in person format, would lead to imputed values that are identical, say consistent, for all observed periods. However, it is not possible to specify a compatible imputation model in that case since the substantive analysis model - a DTSAM - is estimated with the data set in person-period format. Imputing with the data set in person-period format would, however, lead to values for time-invariant variables that potentially differ between persons.

Since there is no clear-cut FCS solution to this problem, we will explore several imputation FCS approaches/approximations, differing (1) in the data format used, (2) which general imputation approach is used and (3) the imputation model specification. The data format, in which we impute, has two possible forms: person format and person-period format (1). We also examine both FCS and SMC-FCS approaches (2). Our imputation models also differ whether we include variables for different periods or (censored) survival times or if we also treat the (uncensored) time-to-event T as a partially observed covariate (Beesley et al., 2016) and impute conditional on this partly imputed variable (3).

3.2.1 FCS specifications with data in person format

We begin with the imputation approaches in *person (P) format*, i.e., before transforming the data into person-period format. We present several approaches, some of which are taken from the literature on MI with continuous survival (cure) data (Beesley et al., 2016). They mainly differ in the implemented conditioning on the (censored) time-to-event. For all presented imputation models, we always also condition on the other covariates. Imputation is done through the package `mice` in R (R Core Team, 2019) with single level normal imputation (van Buuren, 2007). All code is available at https://osf.io/txvey/?view_only=04116ea9a8934b5aaf9a41059c254213.

Let X_k be one of K incomplete continuous time-invariant random variables ($k = 1, \dots, K$) and let X be $X = (X_1, \dots, X_K)$. Let $X_{-k} = (X_1, \dots, X_{k-1}, X_{k+1}, X_K)$. Let Z_m be one of M complete variables ($m = 1, \dots, M$) and let $Z = (Z_1, \dots, Z_M)$. Time-varying covariates are included in wide format/separate variables for each time point both in person and person-period format (only for imputation, not for the analysis after).

In the following, we will mainly discuss how to include the information included in the

event indicator E as well as the completely observed time-until-the-last-observation variable Y or the uncensored but incompletely observed time-to-event variable T . The first approach (FCS P $Y + E$) uses dummy variables Y_j (j periods, $j = 1, 2, \dots, J$) for each possible period j . Let Y be $Y = (Y_1, \dots, Y_J)$. The aim is to allow for enough flexibility in the relationship between Y and the other covariates.

The imputation model is:

$$X_k = [Y, E, X_{-k}, Z]\beta + e, e \sim N(0, I\sigma^2) \quad (3)$$

with persons as observational units.

This specification is only possible if the number of discrete-time points is not too large, the hazard is not expected to be near zero in some periods, and the risk sets are sufficiently large for each time point. Other specifications of the relationship between (censored) time-to-event are possible in these cases, like, for example, linear, quadratic, cubic, or higher-order polynomials. It is also possible to use the logarithm of time (Klein et al., 2013) or to use step functions for grouped periods. For our simulation, however, we did not simulate more than 15 possible time points. Thus, we used dummy variables to keep the specification as general as possible.

Another approach in person format (FCS P $\log(T)$) is to treat the time-to-event T as a partially observed covariate (Beesley et al., 2016). Therefore, it is imputed like other partially observed covariates. We impute the partially observed covariates on the other covariates and the partly imputed time-to-event T , not the time-until-the-last-observation Y and the event indicator E .

The imputation model is thus

$$X_k = [\log(T), E, X_{-k}, Z]\beta + e, e \sim N(0, I\sigma^2) \quad (4)$$

with persons as observational units.

3.2.2 FCS specifications with data in person-period format

It is also possible to impute after transforming the data, i.e., in person-period (PP) format. One should note that persons will possibly receive varying imputed values for time-invariant covariates. As for the approaches in person format, we always condition on all other covariates while imputing. Imputation is again done with `mice` in R.

For our first approach (FCS PP $P + E$) in person-period format, we impute conditional on the event indicator E and dummy variables P_j for every period j ($j = 1, \dots, J$). Let P be $P = (P_1, \dots, P_J)$. Here, we do not condition on the time-until-the-last-observation Y since it is, in general, not included as an extra variable in the person-

period format for event analyses.

The imputation model is thus

$$X_k = [P, E, X_{-k}, Z]\beta + e, e \sim N(0, I\sigma^2) \quad (5)$$

with periods in persons as single units.

However, we fear to lose important information, especially for the rows belonging to the first few periods. If we do not include the time-until the-last-observation (Y), we do not directly condition on the censored “survival” time. Therefore, we also try imputing conditional on the time-until-the-last-observation instead of the current period. We use dummy variables Y_j (FCS PP $Y + E$).

The imputation model is thus

$$X_k = [Y, E, X_{-k}, Z]\beta + e, e \sim N(0, I\sigma^2) \quad (6)$$

with periods in persons as observational units.

3.2.3 SMC-FCS specifications with data in person-period format

After bringing forth several possible FCS approximations, we also examine SMC-FCS and its performance in this setting. SMC-FCS has shown excellent results in different simulation settings (Bartlett et al., 2014; Beesley et al., 2016). We will explore how this approach fares with a DTSAM as a substantive model. In contrast to the other imputation approaches, we now have to supply a “substantive” (the model we are interested in/the analytical model) model.

When we supply the substantive model (see Equation 2) to `smcfcs` (Bartlett and Keogh, 2019) and impute in person-period format (SMC-FCS PP $P + E$), we effectively condition our imputations on the current period P of the row aside from the other covariates and the event indicator E . Therefore, the imputation model does include all variables that are also part of the substantive model, plus the data is also in the same data format. However, the imputation model does not depict the clustered structure of the observation, and it does *not* include the time-until-the-last-observation Y directly.

We also review another approximation (SMC-FCS PP $Y + E$). We regress the event indicator E on the covariates, but we also include the time-until-the-last-observation. We do this to account for relationships between the covariates and the censored time-to-event duration. While we recognize that this is a departure from the original SMC-FCS approach concerning the data format, we think it is worthwhile to test this approximation. It also allows modeling part of the clustering of periods in persons, relevant in the case of unobserved heterogeneity, which we model in three of our four

simulation studies (see next section).

4 Simulation Study

4.1 Data-Generating mechanisms

We now want to explore the performance of the imputation approaches through a series of simulations. We will first present the details of the different simulation studies (data-generating mechanisms, the introduction of missingness, performance measures) before discussing the results.

For all simulations, we create five continuous covariates, X_k ($k = 1, \dots, 5$). These covariates are drawn from a multivariate normal distribution with means 0 and variances 1. The covariates are moderately correlated with each other ($r=0.1$).

Concerning the generation of the survival times, we use the familiar DTSAM model (see Equation 2) that will be estimated later on after the introduction of missingness and subsequent imputation. This yields (truly) discrete survival data that fulfill the assumption of proportional odds for the DTSAM, the substantial model. If we do not add a frailty term to the generation of survival times, the generated data also meets the central DTSAM assumption of no unobserved heterogeneity. However, as several authors have noticed, the assumption of unobserved heterogeneity is highly unrealistic (Allison, 1982, 83). Therefore, we added three more simulations, each with a higher amount of unobserved heterogeneity. The frailty term is in all cases normally distributed with mean 0 and variance 0.25 (respective 1 and 4). We simulate 15 possible time points; after the last time point, all observations are censored. We also randomly censor 10% of all observations. We use $\alpha = (-5.00, -4.72, -4.44, \dots, -0.8)$ and $\beta = (0.8, 2.2, -0.5, 0.3, -1.4)$ as parameter vectors (see Equation 2).

For every scenario, we generate 1000 simulated data sets to prevent the Monte Carlo error from masking differences between methods. For each of the four simulation scenarios, we drew 1000 samples with 2000 persons each. Data set length in person-period format will vary.

4.2 Missingness

To introduce Missing at Random (MAR) data in each of the simulations, we set 30% of the observations for each covariate with probabilities depending on the other covariates and the censored survival time missing. Therefore, the completeness of an observation row depends both on the outcome and all the covariates, which leads to biased coefficient estimates after listwise deletion (see Section 2.3).

We avoid perfect predictors and the accompanying computational problems (White et al., 2011, 394), i.e., that the complete cases include only failures or non-failures for a specific time point. We avoid these problem through including 30 “fail safe” observations (two for each possible time point, one with event indicator one and one

with event indicator zero) in all analysed data sets used in the simulations (Full data, data sets with missing data, data sets with imputed values).

After introducing different possible imputation strategies and creating data sets with missing observations, we are now able to examine the performance of the different imputation approaches.

4.3 Methods compared and performance measures

We perform multiple imputation of partly observed covariates in discrete-time survival analysis using the imputation specifications described in Section 2.2. For each simulation and method, we created five imputed data sets. This approach corresponds to the respective defaults for both MI packages, `mice` and `smcfcs`. We then compute the mean coefficient, relative bias, mean squared error (MSE), confidence interval (CI) length, and coverage for estimated DTSAM parameters across 1000 Monte Carlo repetitions for each imputation model specification.

Performance is often evaluated only for regression point coefficients and variance coefficients. But logistic regression coefficients have come under scrutiny since they do not behave like linear regression coefficients. Logistic regression coefficients are influenced by unobserved heterogeneity, i.e., omitted variables (Mood, 2009, 67). To allow comparisons between models with different covariate specifications, researchers often use average marginal effects (AME) for interpreting substantial results. An AME is the average effect of an independent variable on the predicted probability (Mood, 2009, 75), i.e., the hazard in case of a DTSAM. Due to the popularity of AMEs, we provide a comparison of the AMEs mean for all approaches.

4.4 Simulation results

Figure 1 displays the bias for the estimates of β_1 , for no unobserved heterogeneity and raising unobserved heterogeneity with frailty term variance $\sigma^2 = 0.25$, respective $\sigma^2 = 1$ and $\sigma^2 = 4$.

We first concentrate on the imputation methods under no unobserved heterogeneity on the left. The assumptions of no unobserved heterogeneity and proportional odds are thus fulfilled for the analysis model with full data. Listwise deletion overestimates the true coefficient, and the CI length and MSE is about double the one of the full data, so there is room for improvement in terms of bias and efficiency. If we now turn to the different imputation methods and their performance, we notice profound differences. We register the highest bias for the imputation approach FCS P $\log(T)$, where we first imputed the missing time-to-event T and used this variable (apart from the covariates) to impute our covariates. Like in the case of the Cox model, this approach is completely inadequate (Beesley et al., 2016, 4711). The second and

third-to-worst approaches are the two FCS approaches in person-period format (5. FCS PP $P + E$ and 6. FCS PP $Y + E$), with (censored) survival times respective the current period included in the imputation model.

A little better but still not satisfying is the FCS approach in person format using the information from the (censored) time to survival and the event indicator (3. FCS P $Y + E$). This leaves us with the two SMC-FCS approaches (7. SMC-FCS PP $Y + E$ and 8. FCS PP $P + E$). The compatible model (with the period information) performs better than the incompatible one with the censored time-to-event. The same is true if we add unobserved heterogeneity to the data-generating process (frailty term variance $\sigma^2 = 0.25$ or $\sigma^2 = 1$, $\sigma^2 = 4$), even though the compatible model ignores the clustered structure of the periods in persons. The assumption of no unobserved heterogeneity is thus not fulfilled, a very common violation, which is why we explore the performance of imputation approaches under this condition. We also notice that with rising heterogeneity, the true coefficient is drawn towards zero. It is not surprising since the logistic regression coefficients are drawn towards zero by unobserved heterogeneity, i.e., omitted variables (Mood, 2009). Nevertheless, the ranks of the different imputation approaches in relative bias stay the same; the SMC-FCS approaches in person-period format fare very well while the other approaches do not perform sufficiently in terms of relative bias.

We can confirm the strong performance of the SMC-FCS approaches (7. and 8.) concerning MSE and coverage if we look at other performance measures. While some of the performance measures of FCS approaches perform as well as the compatible SMC-FCS approach, if heterogeneity is high, they do not perform better than SMC-FCS.

Since Average Marginal Effects are often used in the comparison of different DTSAM models (e.g. Wagner et al., 2019, 84), we also included the Average Marginal Effects in our performance evaluation. The first thing noticeable is that there is no recognizable difference between the two SMC-FCS approaches anymore. We can conclude that the difference in coefficient performance is due to the SMC-FCS imputation with the included time-until-the-last-observation introducing too much unobserved heterogeneity into the imputed data set. This, in turn, leads to coefficient estimates that are biased towards the null but leaves the AMEs approximately unbiased.

From all that we have seen so far, the compatible SMC-FCS model outperforms or matches at least all other imputation models and is therefore recommended. The currently most common approach of listwise deletion leads to high efficiency losses and possibly high bias, which should be avoided. The amount of heterogeneity that can be modeled in a DTSAM will vary strongly with available variables, but the compatible SMC-FCS performs well *under all four conditions*.

Apart from genuinely discrete survival data, DTSAMs with a logistic link are often

used to analyze interval-censored survival data. Therefore, we based another scenario on the continuous Weibull distribution for survival times and rounded variables to create interval-censored survival times (Lee and Go, 1997), results are presented in Table form in the Appendix.³ The most important take-away from this additional simulation is that the conjugate SMC-FCS imputation model (8. SMC-FCS PP $P + E$) again shows excellent properties in terms of (relative) bias, MSE, CI length, and coverage. The FCS approaches in person-period format, however, perform completely inadequately (high bias and coverage near zero).

5 Applied example with the German Family Panel pairfam

To conclude, we want to give an applied example under real-world data conditions. We conduct an example analysis from the field of relationship and family research with data from the German Family Panel pairfam (Brüderl et al., 2017).

The 2008-launched German Family Panel ("Panel Analysis of Intimate Relationships and Family Dynamics") is a longitudinal study for research on relationships and families in Germany. The data is collected from a nationwide random sample of the three birth cohorts 1971-73, 1981-83, 1991-93, and their partners, parents, and children. For our real-world example, we used data from the data set *biopart*, which includes prospective and retrospective information on the anchor's relationships, including relationships, cohabitation, and marriage history. We used the data set 8.0.0 (Brüderl et al., 2017), which includes updated information from the survey waves 1-9 (for more details on the panel see Huinink et al. (2011)).

We use a simple substantive model from the field of relationship stability research. We want to note that our goal in this article is not to estimate any causal model but instead explore how our imputation approaches fare with a real data set. Let us assume that we are interested in the relationship between the probability that a couple i splits up and several time-invariant independent variables.

We first include six indicators $j, j = \{1, 2, 3, 4, 5, 6\}$ for grouped periods, each representing five years of a relationship and the last one all years after 25 years of a relationship. These variables include the time point at which the relationship started (*begin* in years since 1900), the age in years when the anchor began the relationship (*age*), the difference in ages of the two relationship partners (*difference age*), an indicator whether the partners are married (*married*) and whether the parents separated during the anchor's childhood (*parents separation*).

$$\begin{aligned}
\text{separated} \sim & \\
& \sum_{j=1}^6 \alpha_j P_j + \beta_1 \text{begin} + \beta_2 \text{age} + \beta_3 \text{difference age} \\
& + \beta_4 \text{married} + \beta_5 \text{parents separation}
\end{aligned} \tag{7}$$

We reduced the data set to the fully observed first reported relationships of all anchors. This leaves us with a sample of 2,173 relationships. Transforming the data set to the relationship-year format leads us to 26,554 rows, i.e., observed periods. For our data set with missing data, we deleted 30% percent of the observations for three of the variables, e.g. the *age*, *begin* and *parents separation* values. The values are MAR missing, i.e., depending on the censored time to survival, whether the relationship failed, whether the two partners are married to each other, and the two respective other variables with missing data. We then imputed using the same approaches we already tested in simulations.

Examining the results in Figure 2, we notice that the coefficient estimate for *begin* after listwise deletion has a higher variance. We also see that after imputing with our various approaches that the imputation approach FCS P $\log(T)$ (4.) is again performing differently and worse than the rest of the imputation approaches, the coefficient of *begin* is way off, the 95%-confidence interval does not even touch the interval of the full data. Also, the FCS approach in person-period format with period dummies (5. FCS PP $P + E$) seems to perform especially poorly, confidence intervals are very wide. Fortunately, the SMC-FCS approach with a compatible imputation model (8. SMC-FCS PP $P + E$) again performs adequately in a realistic setting. All in all, the findings resemble those we have also found in the simulations before. Some imputation approaches are not advisable, e.g., treating the time-to-event variable as partly unobserved. Imputing in person-period format with FCS can not be recommended, and SMC-FCS with a compatible imputation model performs adequately.

6 Summary

Results

The analysis of discrete-time survival data (like many other forms of data analysis) is often challenged by missing data in one or more covariates. Negative consequences of such missing data range from efficiency losses to bias. A popular approach to circumvent these unwanted effects of missing data is multiple imputation (MI). However, with multiple imputation, it is crucial to include outcome information in the model for imputing partially observed covariates. Unfortunately, this is not

straightforward in case of discrete-time survival data, since we (a) usually have a partly observed (left- or right-censored or both) outcome, (b) do not have only one outcome variable, but two: the event indicator and the time-to-event and (c) have to decide whether to impute while the data set is still in person format or after transformation in person-period format, especially if we look at time-invariant information.

In this article, we have tested different approaches for imputing missing covariates in the discrete-time survival model setting. For this purpose, we performed four different simulations, which differ in the amount of unobserved heterogeneity. The investigated methods are partly from the previous literature on the imputation of time-constant variables for the Cox model ([van Buuren et al., 1999](#); [White and Royston, 2009](#); [Clark and Altman, 2003](#); [Keogh and Morris, 2018](#); [Beesley et al., 2016](#)). They use mostly FCS MI, but we also took a closer look at SMC-FCS ([Bartlett et al., 2014](#)) and its performance. We also provided an applied example using pairfam data ([Brüderl et al., 2017](#)).

What have we learned so far? We agree with [Beesley et al. \(2016\)](#) that treating censored survival times as partly unobserved and imputing other covariates depending on the multiply imputed missing survival times yields insufficient results in all cases. While [Beesley et al. \(2016\)](#) observed this for the Cox (cure) model, we confirmed it for discrete-time survival analyses.

Furthermore, the performance of imputation methods in person-period format with FCS is disappointing. Apart from the inherent incoherence between the imputed values for different times in the same person, coverage and relative bias are insufficient. The SMC-FCS approach using a compatible imputation model performs best in our simulations with and without unobserved heterogeneity and is therefore strongly recommended.

Limitations and Future Research

We want to note that the approach that fared best in our simulation (using SMF-FCS with a compatible imputation model) is also in principle suitable for the imputation of time-varying covariates since imputation is done in person-period format. I.e., it is possible to impute different values for different observed periods of the same person. However, research to confirm adequate performance in the case of time-varying covariates is still to be undertaken.

Another question is how to deal with time-varying effects of covariates, both time-invariant and time-varying. [Keogh and Morris \(2018\)](#) has shown that a variation of the SMC-FCS approach also performs sufficiently in the case of time-varying covariates. Again, an evaluation for discrete-time survival models has not yet been conducted.

Conclusions

We recommend using the new SMC-FCS approach proposed by (Bartlett et al., 2014) with a compatible imputation model. This approach performed at least as well as and usually better than other (SMC-)FCS approaches we explored in this article. This is true for in the case in which the data set fulfilled the assumptions of a DTSAM. It is also true in the case of unobserved heterogeneity of varying degrees and interval-rounded survival data with proportional hazards instead of proportional odds. SMC-FCS implemented both in Stata (Bartlett, 2015) and in R (Bartlett and Keogh, 2019), and the specification of the substantive (analytical) model is easy to do. The SMC-FCS approach has already shown excellent performance in case of included quadratic covariates and interaction effects in linear and logistic regression as well as the imputation of covariates in Cox (cure) regressions (Bartlett et al., 2014; Bartlett and Taylor, 2016; Beesley et al., 2016; Keogh and Morris, 2018). To conclude, we also recommend it for the imputation of time-invariant covariates in discrete-time survival analysis.

Notes

¹FCS is known under a multitude of names, e.g., also as imputation by chained equations or “ice”.

²This complicates the imputation, and rejection sampling has to be used. Rejection sampling is more computationally intensive than direct sampling methods.

³The created data fulfill the criteria of proportional hazards. In the case of a DTSAM, our assumption is however, that the odds are proportional (it is also called the proportional odds model). If the hazard probabilities are small (<0.2), however, this represents no problem since $h_{ij} \approx h_{ij}/(1 - h_{ij})$.

7 Tables

Table 1: Exemplary data set in person-period (PP) format. Usually the time-until-the-last-observation Y is not included, but we will need it for certain imputation approaches. Even then, it is only used for imputation, not for analysis. X_1 - X_5 are time-invariant variables.

Obs	X_1	X_2	X_3	X_4	X_5	P	E	Y
1	9,3	3	female	rural	6	1	0	4
1	9,3	3	female	rural	6	2	0	4
1	9,3	3	female	rural	6	3	0	4
1	9,3	3	female	rural	6	4	1	4
2	4,1	2	male	urban	3	1	0	6
2	4,1	2	male	urban	3	2	0	6
2	4,1	2	male	urban	3	3	0	6
2	4,1	2	male	urban	3	4	0	6
2	4,1	2	male	urban	3	5	0	6
2	4,1	2	male	urban	3	6	0	6
3	2,1	4	male	urban	4	1	1	1
...	...	...	...	...	...	...	...	...
...	...	...	...	...	...	...	...	...
n	...	...	...	...	...	...	...	...
n	...	...	...	...	...	...	...	...

Table 2: Bias in case of logistic regression using complete records. Adapted from [Carpenter and Kenward \(2013, 32\)](#)

Mechanism depends on	Biased Coefficients?		
	Constant	Coeff. X_1	Coeff. X_2
Y	Yes	No	No
X_1	No	No	No
X_2	No	No	No
X_1, X_2	No	No	No
Y, X_1	Yes	Yes	No
Y, X_2	Yes	No	Yes
Y, X_1, X_2	Yes	Yes	Yes

8 Figures

Figure 1: Performance measures for coefficient β_1 (for X_1). Het.= Unobserved Heterogeneity. PP=person-period Format. P= Person Format. FCS = Fully Conditional Specification. SMC-FCS = Substantive Model Compatible - Fully Conditional Specification. Grey areas 95%-confidence interval for performance measures. 1000 Monte-Carlo (MC) repetitions per simulation. Results are also available in Table Format in the Appendix Section 9.3

Figure 2: Selected estimated coefficients for a DTSAM of the separation of the first fully observed reported relationships. Pairfam data set 8.0.0 with MAR missing data introduced by the authors. For the specification of the DTSAM see Equation 7.

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9 Appendix

9.1 Weibull survival times

For the scenario based on the Weibull distribution, we used the method by [Bender et al. \(2005\)](#) for creating survival data. [Bender et al. \(2005\)](#) show how the cumulative baseline hazard H_0 can be inverted and individual-specific survival times be created by the following formula (in case of the Weibull distribution):

$$T = \left(-\frac{\log(U)}{\lambda \exp(\gamma'x)} \right)^{1/\nu}. \quad (8)$$

U is a variable following a uniform distribution on the interval from 0 to 1.

We choose as γ -parameter vector $(0.8, 2.2, -0.5, 0.3, -1.4)$, as scale parameter λ 0.001 and as shape parameter ν 5. All survival times $T > 15$ are censored at period $j = 15$. We rounded the survival times to whole numbers.

Table 3: Evaluation measures for the coefficient β_5 (log-odds scale) of X_5 . Simulation with Weibull survival times

	FCS/ SMC-FCS	Data For- mat	Included Survival Time Vars	Bias	Rel. Bias	MSE	Cov	SE	CI len.	AME
1			Full data	0.00	-0.00	0.05	0.96	0.05	0.21	-0.10
2			Listwise deletion	-0.08	0.07	0.13	0.86	0.09	0.37	-0.09
3	FCS	P	$\sum Y_j + E$	0.01	-0.01	0.07	0.92	0.07	0.26	-0.10
4	FCS	P	$\log(T)$	0.38	-0.32	0.39	0.00	0.06	0.25	-0.07
5	FCS	PP	$\sum P_j + E$	0.30	-0.25	0.30	0.00	0.06	0.22	-0.07
6	FCS	PP	$\sum Y_j + E$	0.35	-0.29	0.35	0.00	0.06	0.22	-0.07
7	SMC-FCS	PP	$\sum Y_j; E$	-0.05	0.05	0.08	0.86	0.06	0.25	-0.10
8	SMC-FCS	PP	$\sum P_j; E$	0.02	-0.01	0.07	0.91	0.06	0.25	-0.09

9.2 Computational times

Table 4: Computational Time in Seconds for 5 Imputations. data set is the reduced pairfam data set described in Section 5.

Imputation Package	Data For- mat	Included Survival Time Vari- ables	Time in Seconds for 5 Imputa- tions
mice	P	$\log(T)$	2
mice	P	$\sum Y_j + E$	2
mice	PP	$\sum P_j + E$	110
mice	PP	$\sum Y_j + E$	94
smcfcs	PP	$\sum Y_j + E$	65
smcfcs	PP	$\sum P_j + E$	65

9.3 Performance measures in Table format

Table 5: Performance measures for coefficient β_1 (for X_1). Simulation with $\sigma = 0$.

Method	1. FD	2. LD	3. FCS P Y + E	4. FCS P log(T)	5. FCS PP P + E	6. FCS PP Y + E	7. smcfs PP Y + E	8. smcfs PP P + E
Mean Coefficient	0.825	0.884	0.716	0.515	0.630	0.563	0.765	0.791
Mean Coefficient - MC Error	0.001	0.003	0.002	0.002	0.001	0.001	0.002	0.002
Bias	0.000	0.059	-0.109	-0.310	-0.195	-0.262	-0.060	-0.034
Bias- MC Error	0.001	0.003	0.002	0.002	0.001	0.001	0.002	0.002
Rel.Bias	0.000	0.071	-0.132	-0.376	-0.237	-0.317	-0.073	-0.042
Rel.Bias- MC Error	0.002	0.003	0.002	0.002	0.002	0.002	0.002	0.002
MSE	0.044	0.100	0.122	0.315	0.201	0.266	0.082	0.067
MSE - MC Error	0.001	0.003	0.002	0.002	0.001	0.001	0.002	0.002
Cov	0.953	0.874	0.518	0.001	0.032	0.002	0.764	0.880
Cov - MC Error	0.007	0.010	0.016	0.001	0.006	0.001	0.013	0.010
SE	0.045	0.078	0.059	0.055	0.050	0.048	0.053	0.055
SE - MC Error	0	0	0	0	0	0	0	0
Mean length CI	0.177	0.305	0.231	0.217	0.196	0.187	0.208	0.216
Mean len. CI - MC Error	0.000	0.001	0.001	0.001	0.001	0.001	0.001	0.001
AME	0.040	0.032	0.036	0.028	0.032	0.029	0.038	0.038
AME - MC Error	0	0	0	0	0	0	0	0

Table 6: Performance measures for coefficient β_1 (for X_1). Simulation with $\sigma = 0.5$.

Method	1. FD	2. LD	3. FCS P Y + E	4. FCS P log(T)	5. FCS PP P + E	6. FCS PP Y + E	7. smcfs PP Y + E	8. smcfs PP P + E
Mean Coefficient	0.766	0.818	0.677	0.493	0.604	0.543	0.720	0.739
Mean Coefficient - MC Error	0.001	0.002	0.002	0.002	0.001	0.001	0.002	0.002
Bias	0.000	0.052	-0.089	-0.273	-0.162	-0.223	-0.046	-0.027
Bias - MC Error	0.001	0.002	0.002	0.002	0.001	0.001	0.002	0.002
Rel.Bias	0.000	0.067	-0.116	-0.357	-0.211	-0.291	-0.060	-0.035
Rel.Bias - MC Error	0.002	0.003	0.002	0.002	0.002	0.002	0.002	0.002
MSE	0.045	0.093	0.104	0.278	0.168	0.227	0.072	0.064
MSE - MC Error	0.001	0.002	0.002	0.002	0.001	0.001	0.002	0.002
Cov	0.942	0.897	0.655	0.003	0.094	0.005	0.825	0.892
Cov - MC Error	0.007	0.010	0.015	0.002	0.009	0.002	0.012	0.010
SE	0.044	0.076	0.057	0.055	0.049	0.047	0.052	0.053
SE - MC Error	0	0	0	0	0	0	0	0
Mean length CI	0.173	0.297	0.224	0.214	0.192	0.185	0.203	0.209
Mean len. CI - MC Error	0.000	0.001	0.001	0.001	0.001	0.001	0.001	0.001
AME	0.037	0.030	0.034	0.027	0.031	0.028	0.036	0.036
AME - MC Error	0	0	0	0	0	0	0	0

Table 7: Performance measures for coefficient β_1 (for X_1). Simulation with $\sigma = 1$.

Method	1. FD	2. LD	3. FCS P Y + E	4. FCS P log(T)	5. FCS PP P + E	6. FCS PP Y + E	7. smcfs PP Y + E	8. smcfs PP P + E
Mean Coefficient	0.641	0.683	0.587	0.437	0.540	0.491	0.613	0.622
Mean Coefficient - MC Error	0.001	0.002	0.002	0.001	0.001	0.001	0.002	0.002
Bias	0.000	0.042	-0.054	-0.204	-0.101	-0.150	-0.027	-0.019
Bias - MC Error	0.001	0.002	0.002	0.001	0.001	0.001	0.002	0.002
Rel.Bias	0.000	0.066	-0.084	-0.318	-0.157	-0.234	-0.043	-0.030
Rel.Bias - MC Error	0.002	0.004	0.002	0.002	0.002	0.002	0.003	0.003
MSE	0.043	0.086	0.074	0.208	0.110	0.156	0.059	0.060
MSE - MC Error	0.001	0.002	0.002	0.001	0.001	0.001	0.002	0.002
Cov	0.944	0.902	0.833	0.013	0.432	0.087	0.880	0.880
Cov - MC Error	0.007	0.009	0.012	0.004	0.016	0.009	0.010	0.010
SE	0.042	0.072	0.053	0.051	0.047	0.046	0.049	0.050
SE - MC Error	0	0	0	0	0	0	0	0
Mean length CI	0.164	0.281	0.209	0.200	0.186	0.179	0.191	0.194
Mean len. CI - MC Error	0.000	0.001	0.001	0.001	0.001	0.001	0.001	0.001
AME	0.033	0.026	0.030	0.024	0.028	0.026	0.032	0.032
AME - MC Error	0	0	0	0	0	0	0	0

Table 8: Performance measures for coefficient β_1 (for X_1). Simulation with $\sigma = 2$.

Method	1. FD	2. LD	3. FCS P Y + E	4. FCS P log(T)	5. FCS PP P + E	6. FCS PP Y + E	7. smcfs PP Y + E	8. smcfs PP P + E
Mean Coefficient	0.433	0.473	0.416	0.321	0.405	0.378	0.424	0.426
Mean Coefficient - MC Error	0.001	0.002	0.001	0.001	0.001	0.001	0.002	0.002
Bias	0.000	0.040	-0.017	-0.112	-0.029	-0.055	-0.009	-0.007
Bias - MC Error	0.001	0.002	0.001	0.001	0.001	0.001	0.002	0.002
Rel.Bias	0.000	0.092	-0.040	-0.259	-0.066	-0.127	-0.020	-0.016
Rel.Bias - MC Error	0.003	0.005	0.003	0.003	0.003	0.003	0.004	0.004
MSE	0.040	0.084	0.050	0.118	0.053	0.069	0.050	0.051
MSE - MC Error	0.001	0.002	0.001	0.001	0.001	0.001	0.002	0.002
Cov	0.941	0.880	0.933	0.263	0.893	0.749	0.916	0.910
Cov - MC Error	0.007	0.010	0.008	0.014	0.010	0.014	0.009	0.009
SE	0.039	0.067	0.047	0.045	0.045	0.043	0.045	0.045
SE - MC Error	0	0	0	0	0	0	0	0
Mean length CI	0.151	0.264	0.185	0.177	0.175	0.170	0.176	0.177
Mean length CI - MC Error	0.000	0.001	0.001	0.001	0.001	0.000	0.001	0.001
AME	0.023	0.018	0.023	0.018	0.022	0.021	0.023	0.023
AME - MC Error	0	0	0	0	0	0	0	0