On the Importance of Correcting Reported End Dates of Labor Market Programs[1]

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Abstract

With administrative data becoming increasingly important for empirical research, the quality of crucial variables of process generated data is of growing interest. This paper investigates reported end dates of further training programs in the German Integrated Employment Biographies Sample (IEBS) to gain insights on how to deal with this sensitive part of the IEBS in future studies and on how measurement error in program end dates affects evaluation results. Error-proneness of reported end dates in the IEBS is discussed, corrections are introduced and their impact on evaluation results is studied for different estimation frameworks using sensitivity analysis. Though there is considerable measurement error in the end dates that can be corrected, the effect on evaluation results is modest.

Zusammenfassung

Die Qualität wichtiger Variablen aus prozessgenerierten Datensätzen ist für die Wissenschaft von zunehmendem Interesse, da sich empirische Studien verstärkt auf Verwaltungsdaten stützen. Die vorliegende Studie untersucht das berichtete Enddatum von Maßnahmen zur Förderung der beruflichen Weiterbildung in der Stichprobe der Integrierten Erwerbsbiographien (IEBS). Ziel ist es erstens herauszufinden, wie in zukünftigen Studien mit diesem fehleranfälligen Teil der IEBS umgegangen werden sollte und zweitens wie Messfehler in Maßnahmenenddaten Evaluationsergebnisse beeinflussen. Zunächst wird die Fehleranfälligkeit der Maßnahmenenddaten diskutiert, dann werden Korrekturmechanismen eingeführt und schließlich wird ihr Einfluss auf Evaluationsergebnisse bei Verwendung verschiedener Analyserahmen untersucht. Obwohl der kor-

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rigierbare Messfehler erheblich ist, hat er nur einen geringen Einfluss auf die Evaluationsergebnisse.

1. Introduction

Large administrative data sets are becoming increasingly available for empirical research. Therefore the quality of crucial variables of process generated data is of growing interest to researchers. This paper investigates a sensitive part of a new German data set: the reported end dates of labor market programs in the Integrated Employment Biographies Sample (IEBS). The IEBS covers about 1.4 million individuals and rich, daily information on employment, job search, transfer payments and active labor market programs. It has therefore become a very important data set for microeconometric labor market policy evaluation in Germany. It is the basis for the ongoing government conducted evaluation of recent years' labor market reforms. The data are considered highly reliable, but end dates of labor market measures are an exception to this: a considerable part of reported end dates is later than the end of actual participation. The impact of this measurement error on evaluation results is analyzed in this paper for the example of further training.

Because measurement error in end dates may influence evaluation results through several channels, it is difficult to predict ex ante how results will be affected. But the IEBS has the advantage that due to its special feature of including data from different administrative processes, it is possible to correct almost all relevant end dates of further training programs. This study introduces three approaches to deal with error-prone end dates, a "naive", approach, a standard approach and a mechanism to explicitly correct end dates. These three approaches are used to study through which channels and to what degree upward measurement error in end dates influences results. For this objective employment rates in a framework with a simple treatment variable (as typical for matching studies) and a duration model with time-varying treatment variables are estimated. A setting with typical features of evaluation studies like employment as the outcome of interest, an evaluation period starting with the start of the program, and the consideration of program effects as opposed to pure threat effects is chosen. There are two aims of this exercise. The first is to learn more about the quality of a sensitive variable in the IEBS and to gain knowledge on how to handle the problem in future studies using the IEBS. The second is to get insights on how measurement error in end dates of treatments influences evaluation results in empirical studies in general. This might be helpful for studies using other administrative data sets, which are supposed to suffer from

measurement errors in end dates that cannot be corrected. To the best of my knowledge, there is no guidance in the literature on this problem.

The remainder of this paper is structured as follows: section two presents the data set and discusses the relevance of treatment end dates and why they are a sensitive part of the IEBS. Section three discusses possible corrections and introduces three procedures to deal with error-prone end dates of further training programs, and presents their impact on the sample used for the empirical analysis. Sections four and five investigate the sensitivity of evaluation results in two different frameworks. Section six concludes and links the conclusions of this paper to the validity of existing studies on further training using the IEBS.

2. End Dates of Labor Market Programs in the IEBS

2.1 Data Set

The IEBS consists of a 2.2 % random sample of individuals data drawn from the universe of data records collected in four different administrative processes: the IAB Employment History (*Beschäftigten-Historik*), the IAB Benefit Recipient History (*Leistungsempfänger-Historik*), the Data on Job Search Originating from the Applicants Pool Database (*Bewerberangebot*), and the Particpants-in-measures Data (*Maßnahme-Teilnehmer-Gesamtdaten-bank*).[2] This study uses version 2.05 of the IEBS and focusses on unemployment periods beginning in between February 2000 and January 2002.[3] The data contains detailed daily information on employment subject to social security contributions, receipt of transfer payments during unemployment, job search, and participation in different programs of active labor market policy. Thus, the IEBS is particularly useful to evaluate different parts of German active labor market policies in detail. It is the data set that is mainly used for the evaluations of the so-called *Hartz-Reformen*, several major labor market reforms of recent years.[4] In addition, the IEBS has already been used for several further evaluation studies, for example Biewen et al. (2006); Biewen

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[2] For detailed information on the IEBS see Hummel et al. (2005) and Bender et al. (2005). Information in English can be found on the website of the Research Data Center (FDZ) of the Federal Employment Office (BA) (http://fdz.iab.de/en), in particular the documentation „The German Integrated Employment Biographies Sample IEBS" by P. Jacobebbinghaus and S. Seth. The website also describes the conditions under which researchers may use the IEBS and the process to get the permission.

[3] The data used here has been supplemented with some additional information compared to the standard version.

[4] Compare the report of the federal government (Bericht 2006 der Bundesregierung zur Wirksamkeit moderner Dienstleistungen am Arbeitsmarkt) for an overview of the results.

et. al. (2007); Boockmann et al. (2007); Jaenichen / Stephan (2007); Lechner / Wunsch (2006); Schneider / Uhlendorff (2006). Certainly further studies will follow as the data set is unique in Germany concerning its largeness and richness in detailed information on employment biographies and as it will be updated in the future to always include recent years.

The first of the four administrative data sources, the IAB Employment History, consists of social insurance register data for employees subject to contributions to the public social security system. It covers the time period from 1990 to 2004. The main feature of these data is detailed daily information on the employment status of each recorded individual. In evaluation studies this information can be used to account for the labor market history of individuals as well as to measure employment outcomes. For each employment spell, in addition to start and end dates, data from the Employment History contains information on personal as well as job and firm characteristics such as wage, industry or occupation.

The IAB Benefit Recipient History, the second data source, includes daily spells of unemployment benefit, unemployment assistance and subsistence allowance payments the individuals received between January 1990 and June 2004. In addition to the sort of the payment and the start and end dates of periods of transfer receipt the spells contain further information like sanctions, periods of disqualification from benefit receipt and personal characteristics. The Benefit Recipient History is important as it provides information on the periods during which individuals were out of employment and therefore not covered by the Employment History.

The third data source included in the IEBS is the so-called Data on Job Search Originating from the Applicants Pool Database, which contains rich information on individuals searching for jobs covering the period January 1997 to June 2004. The spells include detailed information concerning job search, regional information and personal characteristics, in particular on educational qualifications, nationality, and marital status. They also provide information on whether the applicant wishes to change occupation, how many job proposals he or she already got, and about health problems that might influence employment chances.

The Participants-in-measures Data, the fourth data source, contains diverse information on participation in public sector sponsored labor market programs for example training programs, job-creation measures, integration subsidies, business start-up allowances covering the period January 2000 to July 2004. Similar to the other sources, information comes in the form of spells indicating the start and end dates at the daily level, the type of the program as well as additional information on the program such as the planned end date, whether the participant entered the program with a delay, and whether the program was successfully completed.

2.2 Relevance of End Dates

There exist several studies on measurement error in the treatment variable. Molinari (2005) develops limits for treatment effects in the case that the treatment variable has missings in survey data. Battistin / Sianesi (2006) characterize the bias if treatment status is mismeasured and provide bounds. Lewbel (2004) develops GMM estimators for the scenario that the treatment variable is measured with error and an instrument that influences the probability of treatment but is conditional independent of the misclassification probabilities and the average treatment effect is available. For the case where no such instrument is available bounds are developed. The problem analyzed in this paper is different in two respects. First, the problem itself is more complicated, because it is not the treatment indicator which is mismeasured but the program end dates. Measurement error in end dates can affect the treatment indicator but it may also affect the results through other channels as discussed below. But second, using the IEBS data it is possible to correct the end dates. Therefore the approach of this paper is to develop procedures to correct the end dates and then to analyze through which channels and to what extent wrong end dates influence results.

Through which channels upward measurement error of end dates potentially influences employment effects depends on the evaluation design. Using descriptive analysis of employment rates or matching with a simple treatment variable, program end dates have no direct effect on the results but may bias them indirectly through outcome measurement and through the treatment indicator. First, if the outcome is measured as regular employment or non-employment (including every other status including program participation), too late end dates of programs lead to a contradiction: the researcher observes program spells and regular employment spells in parallel for some time. A decision whether to count this time as employment or program participation (and thus non-employment) is necessary and will influence employment rates and treatment effects. Second, end dates define the actual length of program participation, which can be relevant for the decision if a program has been attended long enough to be counted for evaluation. Too late end dates can lead to measurement error in the treatment indicator: it may indicate participation, although it should indicate non-participation, as in reality the participant did not attend long enough.

Measurement error in the end dates influences the results more directly in estimation designs in which it is of importance if a participant is in a program at a certain point in time or in which it is relevant whether a program has been completed or not, thus in frameworks with a time-varying treatment variable. An example for this is a duration analysis approach in which attending an uncompleted program and having attended a program in the past are considered separately.[5] In conclusion, there exist different channels through which mea-

surement errors in program end dates may bias evaluation results and it is therefore difficult to predict the direction and magnitude of a potential bias.

2.3 Error-proneness of End Dates for Labor Market Programs

The reliability of the IEBS data was checked carefully by Bender et al. (2004, 2005). Concerning calendar dates, their conclusion is that start and end dates in the employment and benefit data are very highly reliable. Calendar dates seem to be less reliable in the Particpants-in-measures Data and the Data on Job Search Originating from the Applicants Pool.[6] Bender et al. (2005) point out that the end dates of further training and retraining programs are error-prone. It is possible that end dates of other programs in the Participants-in-measures Data like short-term training or job creation measures suffer from similar measurement error that leads to comparable biases. But note that data constellations pointing at wrong end dates vary for different programs. Job-creation measures for example are expected to have an employment spell in parallel, whereas this is implausible for further training programs as discussed in section 3.1. The analysis of passive policies like the receipt of unemployment benefits and unemployment assistance is not supposed to suffer from similar biases, because the information on benefit receipt originates from the IAB Benefit Recipient History and not from the Participants-in-measures Data.

There are two aspects which determine the reliability of administrative data. One is how the information is registered during the administrative process itself. The other is what rules the providers of the data use to define which piece of information of the administrative data bases will finally appear in the scientific data set.[7] The reason why end dates for labor market programs are a sensitive part of the IEBS seems to be the concurrence of at least two problems: First, the correct reporting of end dates of individual program participation is not always directly relevant for payments (Bernhard et al. 2006, 5), mainly because for part of the measures the employment office pays per measure and not per person. This is in contrast to for instance the dates of benefit spells, which

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[5] Measuring treatments by dose (for instance using days of treatment as a treatment variable) is another framework in which the end date is of direct importance, but only if dose is measured in realized duration and not in planned duration.

[6] For job search data the measurement error seems to be quite severe, but it is possible to circumvent this problem by defining the labor market status using benefit and employment data.

[7] Jaenichen et al. (2005) analyze inconsistencies of the participation data that are related to the end date problem. One of their conclusions is that both aspects are relevant, but the problems in the registering of the data themselves might be the major problem. Kruppe / Oertel (2003) provide detailed information on aspects of the data creation. The rules to create the Participants-in-Measure Data have been changed between the IEB versions 3 and 4 (see Waller, 2007).

are directly and even technically linked to payments and thus much more reliable in the data. Second, the end of program participation often changes after the date is first registered. This can be due to drop-out of the program, non-attendance, change of course or shift of the course. If then the registered date is not corrected or if the correction does not reach the data set provided to the researcher, the end date of participation in the IEBS will be incorrect.[8]

3. Empirical Approach: The Example of Further Training

Concerning data checks and corrections, the IEBS has a great advantage: the fact that it includes data of different administrative processes can be exploited to check plausibility and correct implausible information. It is thus possible to correct end dates and to analyze if and how errors in treatment end dates lead to biased estimation results. The impact of upward measurement error in treatment end dates will be investigated in this paper for the example of further training programs. Further training is an important part of active labor market policy in Germany.[9] It has already been evaluated using the IEBS several times (see e.g. Biewen et al. (2007); Lechner/Wunsch (2006); Schneider/Uhlendorff (2006), and Schneider et al. (2007)). For investigating the error-proneness of end dates, further training has the advantage that participants receive subsistence allowance while they are in the program and this information helps to correct end dates.

3.1 Plausibility Checks

This section discusses which information in the data indicates a wrong end date. A constellation that is a clear contradiction is a regular employment spell that starts while the participant of further training is still attending the program.[10] This is a contradiction (Bernhard, 2006, 25), because once a person is regularly employed, he or she cannot continue the program.[11] As calendar dates in the IAB Employment History are much more reliable than in the Par-

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[8] Start dates are more reliable than end dates, probably because drop-outs are irrelevant and because they lie in the nearer future, so that fewer changes occur. In case of non-attendance start and end dates are per definition incorrect. In this case the correction of the end date leads to non-participation in a program and thereby also to a correction of the start date.

[9] See Bundesagentur für Arbeit (2006).

[10] In this study regular employment is defined as non-minor unsubsidized employment on the first labor market with a minimum length of two weeks.

[11] A participant of an active labor market program to whom a job is offered must take the job and may not continue the program, due to the rule of priority to job placement (*Vorrang der Vermittlung), SGB III § 4.*

ticpants-in-measures Data, the employment information indicates that the correct end date of program participation is at the latest one day before regular employment starts. A second major possibility for corrections is provided by subsistence allowance (*Unterhaltsgeld*) spells. Subsistence allowance are payments of the labor agency to cover living costs of the participants of further training programs. They are a subsidy to unemployment benefit or unemployment assistance for the time of the program. With very few exceptions all participants of further training receive subsistence allowance for the complete time of the program, a fact that proves true in the data. Dates of subsistence allowance spells are very reliable. Thus, if a subsistence allowance spell that has started in parallel to a further training spell finishes before the training spell, one can conclude that the end date of the program spell is wrong. Furthermore two variables of the Participants-in-measures Data can be used: a variable indicating that someone did never attend (*Maßnahmeerfolg: Nichtantritt*) and a variable indicating the date a participant notifies a dropout (*FbW Abmeldedatum*). These two variables have many missings, but used with a lot of caution they can help to correct end dates in some cases.

There is other information in the data one might be tempted to use, but which would lead to false corrections in some cases. First, one should not use the length of program spells. The law provides rules for the length of certain programs, but despite of this in practice there exist - though rarely - much longer programs. Therefore one should not change end dates in the data just because a spell is surprisingly long. Second, while regular employment parallel to training programs is a contradiction, employment of a few hours only is possible (Bernhard et al. 2006, 24) and is no hint for a wrong end date.[12]

3.2 Three Procedures to Deal with Error-prone End Dates

This section introduces three ways to deal with the error-prone end dates. They are illustrated using the fictitious example shown in the upper left diagram of figure 1: the individual in the example becomes unemployed (out of regular employment) at day zero and receives unemployment benefit. He or she starts a further training program at day 40 of his or her unemployment period and receives subsistence allowance in parallel. The receipt of subsistence allowance ends on day 100 and the individual takes up regular employment on day 140. The reported end date of the further training spell is on day 180, but according to the argumentation of the last section the correct end of program participation would be day 100, because this is when the subsistence allowance spell ends.

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[12] For rarer data constellations that indicate or do not indicate a wrong end date and for some aspects to be cautious about when relying on subsistence allowance spells see Waller (2007).
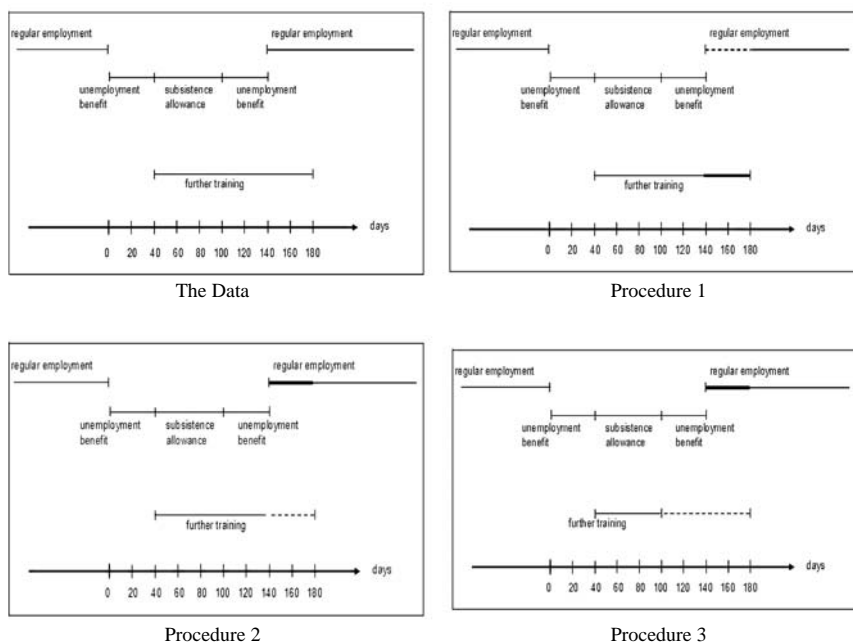
Figure 1: Illustration of the Three Procedures using a Fictive Example

3.2.1 Procedure 1

The underlying idea of procedure 1 is that program participation is the most important information in a data set mainly created for evaluation studies. Therefore participation spells are taken as they are in the data. But if a partici-pation spell conflicts with a regular employment spell for some time, the re-searcher is forced to take a decision.[13] The rule of procedure 1 is to always give priority to the program spell. There are two important situations where program and regular employment spells may conflict and the rule applies. The first is shown in the upper right diagram of figure 1: in between day 140 and day 180 the regular employment and the further training spell are in parallel in the data. To measure the outcome the status of the person must be defined (employed or not employed, e.g. in a program). The rule of procedure 1 gives priority to the program spell, thus the status of the person is defined as attend-ing a program (and thus not employed).

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[13] All procedures assume that it is impossible to start regular employment but con-tinue a further training program, as argued above. If one assumed that this was possible, one would explicitly allow for this situation when generating treatment and outcome variables. Results should then be similar to the results of procedure 2.

The second situation where the rule applies is the same data constellation (regular employment and a continuing program spell) but not for a program within the unemployment spell to be evaluated but for a former program in a former unemployment period. The rule of procedure 1 gives priority to the program spell and thus the concerned days until the program spell ends are not counted as employment. If an evaluation study focusses only on unemployment periods before which the individuals have been employed a certain amount of days, the rule of procedure 1 will in some cases prevent an unemployment spell to be used. This is because the criterion of sufficient pre-employment is not fulfilled when not counting the concerned days as employment.

In short, the basic assumption of Procedure 1 is that program spells are more reliable than employment spells. To get the results of procedure 1 this assumption does not necessarily have to be implemented explicitly. The results of procedure 1 will just appear when one first checks if there is program participation at a certain day and second if there is employment, without having checked for contradictions before. Procedure 1 is called the "naive" procedure, because a close look at examples in the data reveals that dates of employment spells are more reliable than end dates of program spells.[14]

3.2.2 Procedure 2

The basic assumption underlying procedure 2 is that regular employment spells are more valid than programm spells. The rationale is that employment dates in the IEBS are very reliable, because the length of the spells is directly relevant for pension payment. Thus in procedure 2 regular employment spells are given priority in case they conflict with program spells. The rule to give priority to employment information is always applied when the researcher is forced to take a decision, thus in the two situations described above (measurement of the outcome as well as measurement of labor market status before the unemployment period in focus). The lower left diagram of figure 1 illustrates that procedure 2 gives priority to the employment spell for the time employment and program spells are parallel.

But note that no ex ante correction of the program end dates is implemented. Procedure 2 involves no explicit data correction. Only when the employment status of the individual on a certain day is to be defined, the researcher in the first step checks for regular employment and if there is none he or she

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[14] One hint for this is for instance, that subsistence allowance and employment spells almost never conflict, whereas it occurs quite often that the end of program spells does not fit to the end of subsistence allowance. Another hint are examples in which several annual employment spells follow each other in a regular way, while the program spell is still continuing in parallel. Furthermore, Bernhard et al. (2006, 46) advise the researcher to give priority to employment spells.

secondly checks for program participation. Thus in case of conflict regular employment will be counted. Procedure 1 and procedure 2 have in common that there is no explicit data correction at the beginning of data preparation. They just use one rule whenever a conflict appears. And the rule of procedure 2 (priority to regular employment spells) is the contrary of the rule of procedure 1 (priority to program spells). Because procedure 2 does not implement an explicit data correction, but uses its rule only when the employment status must be defined, the wrong end dates themselves are not changed in the data and when for instance the length of a program is calculated the wrong end date is used. Procedure 2 is called the standard procedure, because it seems to be the best choice if one does not want to implement an explicit correction mechanism.

3.2.3 Procedure 3

Procedure 3 uses the same rule for the definition of the employment status as procedure 2, but in addition a mechanism to correct end dates of further training programs is implemented at the beginning of the data preparation: First, an end date of a further training spell is changed if the subsistence allowance spell ends before the further training spell. The end date of the program is set to the last day of receipt of subsistence allowance. This is illustrated in the lower right diagram of figure 1: the program end date is set from the date of day 180 to the date of day 100. Second, in the rare cases where there is no subsistence allowance spell, and only then, other correction possibilities like the variable indicating that someone did never attend (*Maßnahmeerfolg: Nichtantritt*) or the variable indicating the date a participant notifies a drop-out (*FbW Abmeldedatum*) are used if they are filled and indicate that a correction is necessary. Third, if a regular employment spell starts before the end of the program, the day before the start of employment is set as the corrected program end date (if no stronger correction has been implemented before).[15] When implementing the corrections, several technical particularities of the IEBS as well as some special regulations (in particular concerning programs partly sponsored through the European Social Funds) must be taken into account. See Waller (2007) for more details on the corrections and a description of how the corrections may be implemented. Procedure 3 relies on all assumptions of the corrections, in particular on the reliability of subsistence allowance spells. In conclusion, procedure 3 is the only procedures presented involving explicitly changing information in the data, that means setting some end dates to different dates at the beginning of data preparation.

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[15] Waller (2007) implements a fourth procedure because procedure 3 could be biased as using employment spells for changing end dates (and thus possibly not counting the program anymore, because participation becomes too short) leads to counting programs of those who find employment less often. It turns out that this bias is negligible.

3.3 Treatment and Sample

Further training (FT) programs are defined in this study as those measures that train profession skills and last typically several months up to a year. Other training programs, like the longer retraining (*Umschulung*), which leads to a new degree within the German vocational training system or short-term training (*Trainingsmaßnahme*) are not analyzed here.[16] The effect of participating in a program (as opposed to a possible threat effect of the announcement to be assigned to a program) shall be evaluated and therefore programs are counted only if the unemployed has participated a minimal amount of days. The limit has been set considering program aims and the distribution of planned program durations to 28 days.[17]

The sample chosen for the empirical analysis consists of unemployment periods of women (aged between 25 and 53 years) living in West Germany which start in between February 2000 and the end of January 2002 after continuous regular employment of at least three months. Entering unemployment is defined as quitting regular employment and subsequently being in contact with the labor agency (not necessarily immediately) either through benefit receipt, program participation or a job search spell.[18] Only program participation out of such an unemployment period is counted.

It turns out that it is possible to check almost all end dates of relevant treatments using procedure 3. For the sample described above only 2.5 % of the end dates of FT programs in focus can neither be confirmed nor corrected. For some of them there are hints in the data that the end date is correct or not, but the information seems not reliable enough to be used for corrections.

Table 1 gives the number of valid unemployment spells, valid FT treatments and the duration of the programs for each procedure. There are less valid employment spells (and as a consequence also less valid treatments) using procedure 1 due to the condition of entering unemployment out of three months of employment. This condition is met a little less often in procedure 1, because also participation spells of earlier programs dominate earlier employment spells (compare section 3.2.1). Less programs are valid in procedure 3 than in procedure 2, because due to the corrections more program spells are affected by the minimal attendance criterion of 28 days. If the durations of the program spells are compared considering only those that are valid in every procedure

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[16] See Waller (2007) for an investigation of the end dates of retraining.

[17] If one person has several participation spells within one unemployment spell, the spells are connected if there are at most 14 days in between two spells. If a person participated in several programs within one unemployment period with an interruption of more than two weeks, the first program is evaluated.

[18] Note that this implies that the same individual may appear more than once in the evaluation sample. Approximately ten percent of the individuals are represented by more than one unemployment spell according to the above definition.

(second last line), the average length is considerably shortest for procedure 3, where the end dates are explicitly corrected. Considering the average length of those programs valid in the respective procedure, but not necessarily in all procedures (the row in the middle of the table), sample differences make this picture less clear. The last line in table 1 reflects the outcome measurement: it gives the length of the program spell, but cut if a validated regular employment spell starts. This length is on average only a bit longer for procedure 2 than for procedure 3, but considerably longer for procedure 1. This is because for 7.3 % of the treatments valid in all procedures regular employment starts on average 128 days before the end of the original program spell.

Table 1

Programs in the different procedures

Procedure	1	2	3
Valid unemployment spells	20165	20439	20435
Valid FT treatments	879	896	884
Average program duration	214.66	215.05	202.54
. . . for programs valid in all procedures	214.67	212.90	200.87
. . . and until validated employment starts only	214.67	202.84	200.04

4. Sensitivity Analysis I: Frameworks
with a Simple Treatment Variable

This section investigates the sensitivity of the use of the different procedures and thus the impact of measurement error in program end dates for frameworks with a simple time constant treatment variable. In these frameworks individuals who start a program within some time window and attend it for a minimal amount of days are counted as participants (in a multiple framework of one of the measures). Individuals who do not attend a program (or who do not attend it long enough) are counted as nonparticipants.

4.1 Impact on Employment Rates of Participants

The channels through which measurement error in end dates influences results in frameworks with a simple treatment variable can be analyzed in studying employment rates estimated using the three procedures. Figure 2 shows the employment rate of FT participants for each month before and after the beginning of treatment (month zero) for each of the three procedures.[19]

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[19] For later months participation rates might be a little underestimated, because employment data of year 2004 is not yet complete.

Figure 3 is just another way of presenting the results by showing the differ-
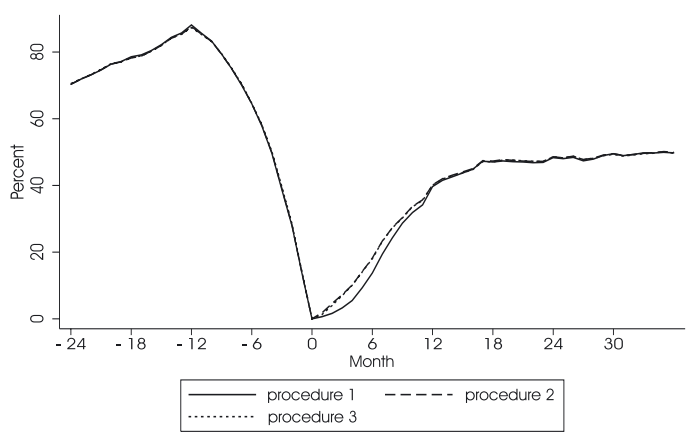ences of the graphs of figure 2.

Figure 2: Employment Rate of FT Participants

Procedure 1 minus Procedure 2   Procedure 3 minus Procedure 2
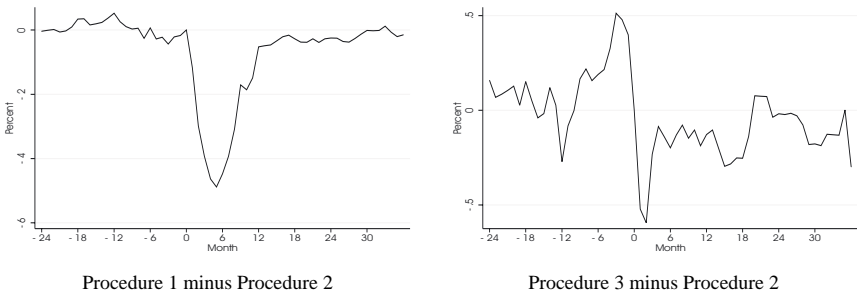
Figure 3: Differences (Employment Rate of FT Participants)

Procedure 1 underestimates the employment rate up to almost 5 percentage
points as compared to procedure 2. This is because when measuring the out-
come, program participation spells are given priority to regular employment
spells. If a researcher uses procedure 1 instead of procedure 2 he or she would
misinterpret the size of the employment rate.[20] Differences between procedure
1 and procedure 2 occur almost exclusively in between the start and the

[20] This is true independently of significance of the difference (because the researcher
would only have the results of procedure 1 and interpret these). But to get an idea on
significance, figure 4 in the appendix gives confidence intervals of the estimated em-
ployment rates. But note that they do not represent a correct test of the difference be-
tween the procedures, because the results are dependent.

planned ends of programs, because the two procedures differ with respect to the priority in case of conflict between employment and participation. These conflicts appear within the planned duration of the programs.[21] Thus using procedure 1 or procedure 2 is only relevant for the time where part of the participants is „locked" into programs.

Explicitly correcting end dates (procedure 3) results in a very slightly smaller employment rate than the standard procedure (procedure 2). The measurement of the outcome is the same for procedure 2 and 3, thus differences in the employment rate can only be due to differences in the validation of programs. Some program spells which are wrongly classified as long enough without corrections are too shortly attended to be evaluated or not attended when using corrections. In other words, the treatment indicator will in some cases indicate participation using procedure 2 and non-participation using procedure 3. If the individuals that are counted wrongly as participants in procedure 2 have on average a higher employment rate, procedure 2 will overestimate the employment rate. This seems to be the case (after the start of the programs) but the impact is very small (see figure 3). The reason becomes clear looking at the numbers of corrections: While end dates change quite often due to corrections (out of the 896 valid treatments in procedure 2 and 3, 13.1 % (117) have an earlier end date due to correction) and the corrections are often quite severe (on average 90 days, 59 days is the median, 10 % have corrections less than 2 days and 5 % more than 266 days), only very few corrections influence the sample and can thus influence the employment rates.[22] Due to the corrections only 15 treatments valid in procedure 2 are not valid in procedure 3 (10 due to correction based on subsistence allowance, 4 due to a very early deregistration date and 1 due to reported non-attendance).

Differences between procedures 2 and 3 last longer than differences between procedures 2 and procedure 1, because the former are due to selection effects and the latter are due to outcome measurement. In conclusion, a considerable amount of end dates is corrected using procedure 3, but this correction has few implications, because the end dates influence the results only through the minimal length criterion, which is rarely concerned by the corrections. This result suggests that in approaches with a simple treatment variable and employment as the outcome variable, it does not make much of a difference if one uses procedure 2 or the more involved procedure 3. But using procedure 1 leads to a downward bias.

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[21] Because of later programs and because of sample differences, there occur minor differences at other times.

[22] The overall sum of corrections in the data is much higher. Here only treatments in focus are counted.

4.2 Impact on Treatment Effects Using Matching

When estimating employment effects using matching, one typically compares the employment status of participants with a fitted employment status of matched non-participants. This is essentially a comparison of the employment rates of participants and weighted non-participants. As the choice of procedure does (almost) not influence the employment rates of nonparticipants, the impact of the choice of procedure on treatment effects using matching is very similar to the impact on employment rates investigated in section 4.1. Therefore the sensitivity on matching results will be discussed only very shortly in this paper.

As shown in section 4.1, Procedure 1 underestimates the employment rate of participants for the time of planned program durations as compared to procedure 2, because of different measurement of the employment status in case of wrong end dates. When using matching this underestimation of the employment status of participants will lead to overestimating a negative treatment effect (lock-in-effect) in the first months after program begin. Waller (2007) shows that indeed procedure 1 overestimates the lock-in-effect up to 5.28 percentage points (in month six after program start at a treatment effect of –17.23 % for procedure 2).[23] The difference vanishes about one year after program start. It may be of importance when a cumulative employment effect is calculated for example for cost-benefit analysis. In this case the researcher should refrain from using procedure 1, because this would lead to a considerable bias in estimation results.

A difference between the matching results using procedure 2 or procedure 3 can only evolve (as for the employment rates in section 4.1) through the minimal attendance criterion. Using procedure 2 some non-participants will be wrongly classified as participants and this influences the samples of participants and non-participants. This may have an impact on matching results, if those misclassified are on average more or less successful than the other participants. In addition, influence through the estimation of the propensity score or the non-participants available for matching are possible. But as the minimal length criterion is very rarely concerned by the corrections (as shown in section 4.1), there are very few changes of the treatment indicator. Thus treatment effects differ only negligibly (up to one percentage point according to Waller, 2007). Therefore in a matching framework with the features described above, an explicit correction of end dates as implemented in procedure 3 does not seem to be necessary unless the exact magnitude of the treatment effect is needed.

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[23] For women in West Germany the employment effect of taking an FT program within the first three months of an unemployment period against not taking a program at least until then is estimated. The outcome variable is the probability of regular employment in each month after the start of the program.

5. Sensitivity Analysis II: Framework
with Time-varying Treatment Variables

Apart from matching methods, duration models are very popular for the estimation of program effects.[24] A simple approach is to compare the Kaplan-Meier survivor functions in unemployment of participants and non-participants. Figure 5 in the appendix shows the survivor function for FT participants (program start within the first year of unemployment) and non-participants. Day zero is the first day of an unemployment period and the hazard is defined as leaving this unemployment period towards regular employment.[25] Comparing the slopes of the survivor functions of participants and non-participants, the slope of the survivor function of participants is much less steep in the beginning, reflecting the lock-in-effect. But later on, when participants have finished the program and intensify their search for employment again, the slope becomes steeper. The Kaplan-Meier estimates show this effect only very roughly, because participants start and leave the programs at different points in time (and because no observable characteristics are controlled for). But parametric duration analysis (e.g. a proportional hazard model) allows to separate these two phases of the program effects by including treatment variables that change over time. In such a framework the impact of the measurement error is likely to be different from the frameworks with a simple treatment variable because of the importance of the program end date when defining the treatment variables over time.

To investigate the sensitivity of reported end dates a proportional hazard model is estimated. A Weibull distribution is chosen to allow for duration dependence. The duration is again defined as starting with the beginning of a valid unemployment spell and ending with a new regular employment spell. In addition to personal and regional characteristics and information on the individual's labor market history that are supposed to influence the hazard rate (see appendix for the final specification), three time-varying covariates are included in the estimation.[26] The day an individual enters the program under consideration the dummy variable "lock" changes to one. Once she leaves a completed program (defined as having participated for least 80 % of the planned duration), the "lock" dummy changes to zero again and a second dum-

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[24] Recent examples of studies estimating employment effects of further training programs with German administrative data using duration analysis are Hujer et al. (2006); Schneider / Uhlendorf (2006), and Schneider et al. (2007).

[25] Sample, minimal length criterion, unemployment and regular employment defined as before. The differences between the procedures are similar to the differences in employment rates discussed in section 4.1 for participants and not visible for non-participants (therefore only the results for procedure 2 are shown).

[26] A time-varying covariate is interpreted as a measure of the effect of a one unit change in the covariate at time t on the log hazard (see Lancaster, 1990).

my ("treatfin") is set to one, indicating that this individual has finished a program.[27] In case the individual leaves an uncompleted program, "lock" is also set to zero and a third dummy ("postdrop") is set to one, indicating that the individual has dropped out of a program in the past. These three time-varying dummies allow to study separately how programs bind the unemployed on the one hand and the time after a completed program on the other hand.[28] The distinction between having completed a program or having dropped out is implemented, because these two situations represent different conditions for finding employment. The coefficients may not be interpreted as treatment effects, they just describe some aspects of the complex process that is going on. Particular problems preventing a causal interpretation are the potential endogeneity of the program end date and the relation between the dummies "lock" and "treatfin".

Table 2

Hazard ratios for time-varying dummies

Procedure	1	2	3
lock	0.086 [0.059; 0.126] (879) (27)	0.129 [0.094; 0.177] (896) (39)	0.291 [0.235; 0.360] (884) (86)
treatfin	1.829 [1.671; 2.001] (811)	1.831 [1.674; 2.004] (814)	1.669 [1.516; 1.836] (731)
postdrop	0.711 [0.452; 1.116] (41)	0.794 [0.517; 1.220] (43)	0.745 [0.530; 1.053] (67)

Extract of the results of the PH model estimating the hazard to regular employment. In squared brackets the 95 % confidence intervals of the hazard-rates are given. The numbers in parentheses are the numbers of individuals that are in the respective state for at least one day of their duration. The second parentheses of "lock" give the numbers of individuals who do only reach "lock", that is leave to employment (or are censored) out of an unfinished treatment. The whole number of individuals varies between 16773 and 18772.

In this framework the influence of the program end date on the results is still indirect, as the end date itself is neither regressor nor outcome variable. But measurement error in the end date may lead to measurement error in the covariates, the coefficients of which shall be interpreted. Table 2 shows the hazard ratios (the exponentiated coefficients) for the dummies "lock", "treatfin" and "postdrop" for FT programs for the three procedures. For the coeffi-

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[27] The last day of a completed program is already considered as "treatfin" (if the individual leaves directly to employment), because regarding the effect of a finished program, starting a job directly after a completed program or having days of unemployment in between is considered the same given the length of the whole unemployment duration.

[28] This framework is inspired by Schneider / Uhlendorf (2006) and Schneider et al. (2007), who distinguish between a lock-in-effect and a post program effect.

cients, including those of the additional covariates and standard errors, see appendix. A hazard ratio of 0.291 for "lock" means that the hazard rate for those being currently in an unfinished program is just 29.1 % of the hazard rate of those not being in a program. As one would expect, "lock" has a negative and highly significant effect: attending a non finished program comes along with a drastic reduction in leaving unemployment. This is also visible from the numbers in the brackets. Whereas 884 women enter an FT program (procedure 3), only 86 end their duration out of the uncompleted program. Using the procedures with no explicit corrections, much less individuals are assessed to end their duration out of an unfinished program. This influences the hazard ratios of "lock": they differ 4.3 percentage points between procedures 1 and 2 and 16.2 percentage points between procedures 2 and 3. Thus the difference between procedures 2 and 3 is more important than between procedures 2 and 1.[29]

The large majority of those assessed to take a program finish it and "treatfin" has a significant positive effect on the hazard rate. As discussed above, this is not to be interpreted as a positive treatment effect, it just says that individuals having finished a program leave unemployment more often than others. The hazard ratios are similar for procedures 1 and 2 but differ 16.2 percentage points between procedures 2 and 3. The reason for this difference is that a procedure without an explicit correction of end dates misclassifies individuals to have finished a program, while they should be classified as being unemployed after an unfinished program or leaving to employment out of an unfinished program (as one can also see from the numbers in brackets). A second effect is that in procedure 2 too many individuals are assessed as leaving directly out of an unfinished program, while in reality they have left the program even before and should be classified as "postdrop" equal to one and "lock" equal to zero. This effect leads c.p. to a too high hazard ratio of "lock" and a too low hazard ratio for "postdrop" using procedure 2. The coefficients of "postdrop" are not significant.

In sum, the results show that in a framework with time-varying treatment variables it can be of importance to explicitly correct end dates when preparing the data as done in procedure 3. In the above duration framework the end date affects the results more directly than in the frameworks of section 4, because it is of importance if the end date of the program lies in the past when

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[29] The confidence intervals of the estimates for procedure 2 and 3 do not overlap. This is a hint that the results might be significantly different. But it is not possible to tell for certain, because the estimates are not independent. The question if the results are significantly different or not is of minor importance for the analysis in this study: A researcher using the IEBS would interpret the results he or she gets using for instance procedure 2, provided the confidence interval is not too large. Had he or she used procedure 3, he or she would use these results and the conclusions about the size of the effect would be different.

the individual starts employment and if the end date of the program lies considerably before the planned end date (dropout). But also in this framework, measurement error in end dates changes only the magnitude but not the direction of the results.

6. Conclusion

Program end dates are a sensitive part of the German Integrated Employment Biographies Sample. Mainly due to early drop-out not corrected in the data, a considerable part of end dates of further training programs are later than the actual end of participation. In this paper three procedures how to deal with the error-prone end dates are presented, a "naive" procedure, a standard procedure and a procedure that explicitly corrects the data before the analysis. The influence of the different procedures on evaluation results is studied in a framework with a simple treatment variable (like typically used in matching) and in a duration framework with time-varying treatment variables. In conclusion, for typical matching studies it does not seem necessary to explicitly correct the data before using it. But especially if there is interest in the size of the lock-in-effect, one should refrain from using the "naive" procedure (giving priority to program data when measuring the outcome), because it considerably overestimates the lock-in-effect. This may be a particular problem when treatment effects are averaged over time to get one number for program comparison or for cost-benefit analysis. In frameworks with time-varying treatment variables, like in the duration model investigated in section 5, reported end dates are more important for the generation of the treatment variables. Therefore, if one is interested in the size of the coefficients of the treatment variables, it may be necessary to correct the reported end dates before the analysis.

Concerning the studies that have already evaluated further training programs using the IEBS, the measurement error in program end dates should not be a problem for the conclusions Lechner and Wunsch (2006) draw. They calculate cumulated effects using their matching results to compare programs, but do not interpret the exact magnitude of these effects. Thus in case they gave priority to program data, this might have biased the size of the cumulated effects a little, but would not have changed their conclusions. For the results of the microeconometric analysis of further training in the context of the so called *Hartz-Reformen* (Schneider et al. 2007) the error-prone end dates of the IEBS might be a (very small) problem. From the description on how the outcome variable is generated (Schneider et al. (2007, 104) it seems as if programs spells have been given priority to employment spells (procedure 1). Provided the authors have not done some end dates corrections not mentioned in the report before the generation of the outcome variable, the results should suffer from the problems which occur when using procedure 1. The authors

use matching results for a cost-benefit analysis. They calculate a cost-benefit effect cumulated over the time from program start until the end of the observation period (144), thus they include results for the lock-in-period for which procedure 1 overestimates the negative effect. Therefore the estimate of the cost-benefit relation in the report might be a bit too negative. In addition Schneider et al. (2007) estimate a duration model with time-varying treatment variables. The model is somewhat different from the one estimated above, but the way measurement error in end dates influences the results should be similar. Thus, if Schneider et al. (2007) have not corrected the end dates before their analysis, the size of the results might be a little biased. But this is not a problem for their conclusions, because the authors do not interpret the coefficients themselves but focus on the reform effect. In conclusion, the overall small effect of error-prone end dates on evaluation results is good news for researchers using the IEBS and also for those using different administrative data sets in which reported program end dates cannot be corrected but may nevertheless be error-prone.
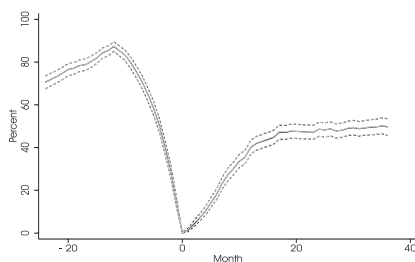
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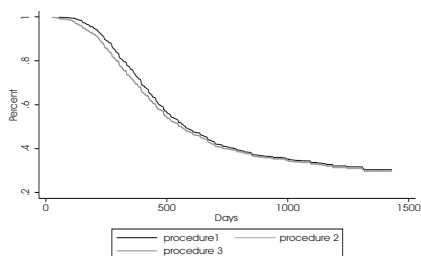
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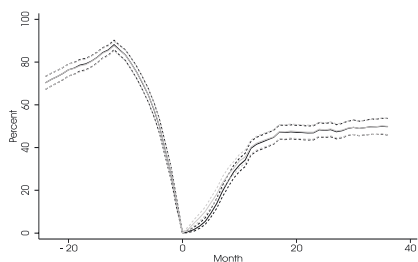
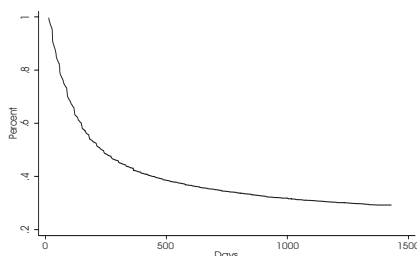Procedure 1 in black, Procedure 2 in grey   Procedure 3 in black, Procedure 2 in grey

Figure 4: Employment Rate of FT Participants with Confidence Intervals

FT Participants   Non-participants

Figure 5: Survival until New Regular Employment

Table 3

Coefficients of PH model

Covariate (exact definition upon request)	Procedure 1	Procedure 2	Procedure 3
lock (currently attending the program)	–2.451 (0.193)***	–2.048 (0.161)***	–1.235 (0.108)***
treatfin (has attended and completed)	0.604 (0.046)***	0.605 (0.046)***	0.512 (0.049)***
postdrop (has dropped out)	–0.342 (0.230)	–0.231 (0.219)	0.291 (0.175)*
age 25 – 29	0.169 (0.031)***	0.170 (0.031)***	0.169 (0.031)***
age 30 – 34	0.156 (0.030)***	0.161 (0.030)***	0.158 (0.030)***
age 35 – 39	0.198 (0.030)***	0.203 (0.029)***	0.200 (0.029)***
age 40 – 44	0.280 (0.030)***	0.290 (0.030)***	0.285 (0.030)***
days in employment last 3 years	0 (0.000)***	0 (0.000)***	0.000 (0.000)***
log last daily censored wage	0.051 (0.015)***	0.050 (0.015)***	0.050 (0.015)***
unemployment benefit last 3 years	0.328 (0.023)***	0.324 (0.023)***	0.322 (0.023)***

Continued Table 3

Covariate (exact definition upon request)	Procedure 1	Procedure 2	Procedure 3
unemployment assistance last 3 years	–0.152 (0.034)***	–0.146 (0.033)***	–0.140 (0.033)***
days out of sample last 3 years	0.00009 (0.000)	0.00009 (0.000)	0.0008 (0.000)
days subsistence allowance last 3 years	0.0002 (0.000)	0.0002 (0.000)	0.0002 (0.000)
unemployment rate home district	–1.175 (0.262)***	–1.570 (0.262)***	–1.752 (0.262)***
foreigner	–0.109 (0.033)***	–0.110 (0.033)***	–0.108 (0.033)***
region3 (IAB classification)	0.038 (0.027)	0.029 (0.027)	0.029 (0.027)
region4 (IAB classification)	0.153 (0.034)***	0.139 (0.034)***	0.134 (0.034)***
region5 (IAB classification)	0.205 (0.029)***	0.185 (0.029)***	0.184 (0.029)***
health problem (no impact)	–0.298 (0.047)***	–0.291 (0.046)***	–0.294 (0.046)***
health problem (impact on placement)	–0.438 (0.053)***	–0.435 (0.052)***	–0.439 (0.052)***
no degree	–0.038 (0.048)	–0.041 (0.048)	–0.041 (0.048)
vocational training degree	–0.018 (0.044)	–0.023 (0.043)	–0.022 (0.043)
9 or 10 years of schooling degree	0.036 (0.040)	0.038 (0.040)	0.041 (0.040)
12 or 13 years of schooling degree	0.115 (0.048)**	0.118 (0.048)**	0.120 (0.048)
living alone	0.843 (0.032)***	0.833 (0.032)***	0.831 (0.032)***
not living alone but not married	0.709 (0.055)***	0.694 (0.055)***	0.691 (0.055)***
single parent	0.608 (0.044)***	0.596 (0.044)***	0.591 (0.044)***
married	0.546 (0.032)***	0.538 (0.031)***	0.543 (0.031)***
at least one child	0.188 (0.025)***	0.184 (0.025)***	0.186 (0.025)***
last job less than full time	–0.113 (0.024)***	–0.106 (0.024)***	–0.107 (0.024)***
last job in agriculture	0.251 (0.073)***	0.253 (0.073)***	0.245 (0.073)***
last job in industry	–0.172 (0.030)***	–0.170 (0.030)***	–0.171 (0.030)***
last job in commerce, traffic, hotel	0.078 (0.024)***	0.080 (0.024)***	–0.077 (0.024)***
last job in financial sector	0.015 (0.028)	0.015 (0.028)	0.015 (0.028)
last job whitecollar-job	0.083 (0.026)***	0.088 (0.026)***	0.090 (0.026)***
no wish to change occupation	0.208 (0.026)***	0.211 (0.025)***	0.212 (0.025)**
end of last job in 2 or 3 / 2000	–0.074 (0.043)*	–0.062 (0.043)***	–0.056 (0.043)
end of last job in 4 / 2000 – 6 / 2000	–0.148 (0.038)***	–0.142 (0.037)***	–0.141 (0.037)***
end of last job in 7 / 2000 – 9 / 2000	–0.129 (0.035)***	–0.122 (0.035)***	–0.119 (0.035)***
end of last job in 10 / 2000 – 12 / 2000	–0.053 (0.033)	–0.057 (0.032)*	–0.053 (0.032)
end of last job in 1 / 2001 – 3 / 2001	–0.073 (0.033)	–0.065 (0.032)	–0.060 (0.032)
end of last job in 4 / 2001 – 6 / 2001	–0.116 (0.036)***	–0.112 (0.036)***	–0.108 (0.036)***
end of last job in 7 / 2001 – 9 / 2001	–0.128 (0.035)***	–0.127 (0.035)***	–0.126 (0.035)***
lack of cooperation	–0.044 (0.032)	–0.047 (0.032)	–0.049 (0.032)
program with social assistance in past	–0.166 (0.068)**	–0.123 (0.060)**	–0.124 (0.060)**
_cons	–5.568 (0.122)***	–5.518 (0.121)***	–5.510 (0.122)***